THE SOCIAL CONTRACT FOR SCIENCE:
CONGRESS, THE NATIONAL INSTITUTES OF HEALTH,
AND THE BOUNDARY BETWEEN POLITICS AND SCIENCE

by

David H. Guston

B.A. in a special divisional major, Yale University (1987)

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ABSTRACT

This dissertation addresses the “problem of politics and science”: what is the nature of the boundary between the sphere of politics and that of science—a sphere traditionally treated as devoid of and separate from politics—and how, in practice, is this boundary negotiated. I examine the problem at three levels of analysis: (1) the most concrete level of congressional-bureaucratic relations and the ability of Congress to influence the scientific bureaucracy; (2) a more abstract level of the social contract for science and the contours of U.S. science and technology policy in the post-World War Two period; and (3) the most abstract level of the relationship between democratic politics and scientific inquiry. In the dissertation, I explain the origins, dynamics, and some implications of novel solutions to the problem of politics and science offered by Congress and the National Institutes of Health (NIH) during the 1980s. Particularly, I examine how Congress and NIH responded to two issues that challenged previous solutions: the integrity and the productivity of scientific research.

I frame congressional-NIH relations in terms of principal-agent theory and the problems of scientific integrity and productivity as problems of moral hazard. Under the ideological guidance of the social contract for science—which in the technological enthusiasm of the postwar period demarcated a sharp boundary between politics and science—the integrity and productivity of science were presumed to be the free and automatic results of interactions among scientists. The sharp boundary of the social contract for science was tested episodically by controversies such as loyalty tests for scientists, programmatic direction like the War on Cancer, and risks from recombinant DNA research, but it remained in tact because none of these challenges sufficiently highlighted the politics within science and formally linked that politics with broader political interests.

But responding to economic and institutional conditions that encouraged them to examine the bases of existing policies, members of Congress in the late 1970s and 1980s scrutinized the integrity and productivity of science with unprecedented detail. They identified the politics within science: the self-interest among scientists and their institutions that, in the one case, prevented them from protecting scientific integrity against fraud and misconduct and that, in the other case, could make science more productive if properly motivated. Congress then encouraged the creation of formal mechanisms for monitoring scientific integrity and applying incentives for scientific productivity that correspond to the principal-agent dynamic and reject the prescriptions of the social contract for science.

In the case of scientific integrity, I argue that scrutiny by Representatives John Dingell (D-MI) and Ted Weiss (D-NY) caused NIH director James Wyngaarden to create an Office of Scientific Integrity (OSI) to monitor and adjudicate scientific misconduct. But Wyngaarden’s “pre-emptive response” in creating OSI is distinguishable from both an “anticipated reaction” dominated by congressional preferences, and from an overtopping bureaucratic dominance. Although
Wyngaarden intended to preserve the sharp boundary between politics and science, subsequent negotiations over the definition of scientific misconduct and the policies and procedures of OSI created a new, more interactive boundary. Contrary to what might have been expected if scientists were protecting the interests of their community, scientists rejected rules relying on professional norms and peer review in favor of the procedural safeguards of administrative law in which scientists merely assist in the adjudication of misconduct.

In the case of scientific productivity, I describe how Congress encouraged federally funded scientists to have a greater impact on technological innovation through legislation that allowed them to profit from patents and exclusive licenses on their inventions. I focus on the implementation at NIH of the Federal Technology Transfer of 1986, which allows federally employed scientists to establish cooperative research and development agreements (CRADAs) with private firms and mandates the sharing of royalties from exclusive licenses with the intramural inventors. I explain how commercial technology transfer, which is coordinated at NIH through its Office of Technology Transfer (OTT), involves the collaboration of scientists and nonscientists in traditionally scientific tasks such as defining a discovery and identifying research partners. In producing patents, licenses, royalties and CRADAs, these collaborations also produce indicators of the productivity of scientific research. Such collaborations are illustrative of the new, more interactive boundary, and the newly established indicators invite further interaction between politics and science.

I draw conclusions from the two cases of scientific integrity and productivity at the three levels of analysis mentioned above. At the level of congressional-bureaucratic relations, I find that Congress is able to exert influence over the scientific bureaucracy by identifying the politics and the interests within science and by linking these with broader political interests. But the extent of congressional influence—the boundary between politics and science—is determined in practice by the details of implementation, for example, the interaction of scientists and nonscientists who collaborate to adjudicate misconduct, or determine what a discovery is. This collaboration, and the formal monitoring and incentive mechanisms that created it, means that at the second level of analysis, a new social contract for science has been created. Under the new social contract for science, politicians no longer rely on the free and automatic provision of scientific integrity and productivity. Through its role in establishing explicit mechanisms to assure scientific integrity and incentives to encourage scientific productivity, Congress, as the immediate surrogate for society at large, has asserted its role as a well-informed and active client of science. In this role as client, Congress and rest of society demand the regulation of some aspects of science, and some fear dire consequences from political intrusion. But this new clientele also assists in the extension of science as well. For example, assistance in maintaining scientific integrity can help reduce strains within the scientific community by providing a fair process for adjudication, and it can thereby help augment the ability and willingness of scientists to rely on their colleagues. Similarly, assistance in technology transfer can expand the domain of the scientific community by stimulating the circulation of ideas, materials, and people across sectoral boundaries. Thus, at the third level of the relationship between democratic politics and scientific inquiry, I find that the traditional distinction between the spheres of politics and science has broken down in the practice of maintaining scientific integrity and productivity, and that both politicians and scientists are taking advantage of increasingly recognized embeddedness of science in the larger society.

Thesis Supervisor: Eugene B. Skolnikoff
Title: Professor of Political Science
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A dissertation is supposed to be a work of independent scholarship; yet there may be no other work in a scholarly career in which the writer is so dependent on colleagues, friends and family, for intellectual, emotional and financial support (not necessarily in any order). Writing acknowledgements is one of the final tasks of the dissertation because, as I have experienced, one continues to accumulate debts until the acid-neutral, 20-pound paper copy has been deposited at the Institute Archives. But it is also one of those sweet tasks held out at the end of a harsher labor. It is a reward to regard others at the end of what is, essentially, an enterprise only completed through the unfortunate disregard of all else.

Since the dissertation is primarily a scholarly undertaking, I will begin with my scholarly debts. I trace my interest in the topic of the social contract for science to a NATO Advanced Study Institute on science policy, held over two crisp October weeks at Il Ciocco in northern Tuscany in 1989. I must thank Ken Keniston for bringing this conference to my attention, and the National Science Foundation for the travel grant to participate. At the conference, I not only met exciting scholars from the U.S. and abroad, but I began to think about science policy in terms of this “social contract for science,” a phrase often used in Il Ciocco by Harvey Brooks and picked up by Arie Rip. I owe Arie a special thanks for two conversations in particular—a late night in easy chairs and an evening walk up a wooded Tuscan hillside—that helped set out this agenda.

I returned to Cambridge with many more thoughts than when I left. That spring I was able to start investigating some of them with Uday S. Mehta on a partial research assistantship from the Department of Political Science. The sometimes winding but always provocative discussions on Hobbes, science, and the social contract in Uday’s smokey office overlooking the Charles seemed to reflect some ideal realm of graduate
education. But despite the fascination of this work with Uday, I still remained uncertain about a dissertation topic for another six months.

In the meantime, I moved to Washington, DC and spent a ten-week fellowship at the National Museum of American History of the Smithsonian Institution. Under the tutelage of Jeffrey Stine, I researched the Allison Commission, a special joint commission of Congress in the 1880s that investigated the organization of the government’s scientific bureaus. The Allison Commission thereby rose into competition with the social contract for science for the dubious honor of being the intellectual focus of my life for the next few years. The competition was short-lived, however. Within a few months of beginning work at the National Academy of Sciences in August 1990, I had heard of the “social contract for science” so frequently that I decided someone might be telling me something. For this preternatural direction, I need to thank Larry McCray, Herb Lin, Vince Ruddy, Frank Richter, Chris Hill, and again Harvey Brooks (and perhaps others).

I cannot express enough appreciation for my eighteen or so months with the Panel on Scientific Responsibility and the Conduct of Research, of the Committee on Science, Engineering, and Public Policy (COSEPUP) at the Academy. The “temple of science” at the end of the Mall was an intriguing context in which to ask questions about the integrity of science and its relationship to the state. Rosemary Chalk was a skillful and knowledgeable study director, Barry Gold a valued colleague and friend, the Panel members (particularly Bernie Gert and Dick Meserve) engaging and challenging, and all of COSEPUP a welcoming family. I owe Larry McCray another round of thanks for Project X, the OBAS follow-up, and more. COSEPUP not only employed me at a time when MIT could not, but allowed me to lay some of the intellectual and professional groundwork necessary for the dissertation. COSEPUP was also quite tolerant of my academic distractions.
Toward the end of my stay at COSEPUP I prepared some of my early thoughts on science and the social contract for presentation at the annual meeting of the APSA in Washington. Although I was not able to present the paper for health reasons, I would also like to thank Frank Laird for his helpful comments on an early draft, and any others, the recollection of whose assistance was lost to the fever of acute hepatitis. It was also shortly after this time that I had a very helpful discussion at Rebecca’s Cafe in Kendall Square about misconduct, boundaries, and the interests of the scientific community with Tom Gieryn, during one of his brief visits to Cambridge.

It was not until after my time with COSEPUP that my field research began in earnest. I conducted a total of sixty-nine interviews, most during a three-month period in the winter of 1991-92, and the remainder during a frantic four-and-one-half days in January 1993. Since I cannot be free with the identities of many of my research subjects, I will not thank any of them by name here. But they were all generous with their time and their perspectives, and my research would have been impossible without their participation. I assure them that I took pains to understand what they had to say rather than what I wanted to hear, and I take full responsibility for any misunderstanding, misinterpretation, inappropriate usage or outright error that might have nevertheless occurred.

Of course, the other group who fits this general description—without whom the dissertation would have come to naught but who are nevertheless absolved from any of its shortcomings—is my thesis committee. Before I thank any one of them, let me say that they all exceeded my nervous expectations, and I hope that anything I write about one will not detract from another. What characteristics I mention here are common to all, not singular to each, only more so (Orwellian, perhaps, but true). My gratitude again goes to Uday, who balanced the compliments and the criticisms of my work (not just the dissertation, but before as well) so well that both were most valuable. Uday seemed to believe from the beginning that what I was working on was as important as I
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There is another group of people, who don’t yet think of themselves as a group (a "latent group"?), whose assistance I must acknowledge. These are former students in the MIT Department of Political Science, people who are close to me and who have, by their recent effort and success in completing their dissertations, inspired me. In particular (and in order of finishing): Bruce Bimber, whose seemingly effortless glide through his dissertation masked a hard-working and lucid intellect; Lourdes Melgar, who taught me more than I can say about the process of writing a dissertation (and other things besides), but significantly the value of determination in the pursuit of a distant and even ambiguous goal; and Steven Flank, who underwent the trials of second colloquium and defense a few short months before me (but finished, after a few
late nights with me, two days before me). Bruce, Lourdes, and Steve also read and commented helpfully on drafts at various points, as did David Hart.

Some of the conceptualization of and research for this dissertation was performed while I was a research assistant (academic year 1991-1992) for the University Science and the Federal Government project, conducted at the MIT STS Program under Ken Keniston and Sheila Widnall, and supported by the Carnegie Corporation of New York and the MIT Office of the Provost. I would like to thank Ken, Sheila, the Carnegie Corporation, Mark Wrighton, the nine speakers, and the twenty-some MIT faculty members who provided a monthly supply of intellectual grist. Judith, Graham, and Sylvia in the STS office assisted me in this enterprise greatly.

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Almost all of the drafting and honing of the dissertation went on in the comfortable and stimulating environs of the Center for Science and International Affairs (CSIA) at the John F. Kennedy School of Government at Harvard University. A predoctoral research fellowship with pleasantly few strings attached to it supported me through the academic year 1992-1993, and the notion of spending a postdoctoral year at CSIA was a dangling carrot before my labor. I must thank Lew Branscomb for extending to me CSIA's research ideal of letting good people work on what they will, and for allowing me to contribute, however modestly, to the Empowering Technology
project. Ted Parsons provided some sound, late-night advice and, he tells me, is also partly responsible for the "revolt" that led to the inclusion of predoctoral fellows in that CSIA research ideal. Lew and Bill Clark offered me a wonderful opportunity and a sometimes necessary (and other times not) distraction by declaring me a co-instructor in STP 301; and I need to add an embarrassed smile of appreciation to STP 301 for the daffodils. Harvey Brooks, again, provided the critical eyes of experience to several key, particularly historical, sections of drafts. Of course CSIA is not just an intellectual environment, but also a social one. I owe thanks to a large number of friends there (and elsewhere in Cambridge) who offered smiles when I was annoyed, encouragement when I was worried, distractions when I was zombified, and company at Unos or the House of Blues: David, Steven, Karen, Jessica, David, Stephen, Karen, Jessica, Rob, Maria, Vicki, Stacey, Sara, Eric, Andrea, Carter, Amy, Peter, and Jean. And none of the work at CSIA--including that of often aloof predocs--would get done were it not for administrators like Susan and Peggy (and Susan informs me that Ash Carter is personally responsible for my location in L358, an office with a spectacular view--including moonrises, sunrises, and fall foliage--that immeasurably assisted my ability to work).

Finally comes the pleasure of thanking those people who, because of their proximity, don't get thanked enough--people whose love and attention may have been taken for granted during the rigors of dissertation writing. Lourdes Melgar has been with me for longer than this project has, and she has been a vital source of advice and direction as it unfolded. Lourdes sustained it and me with her love and encouragement: even when she was alone doing her field work in Monterrey; even through the trials of establishing a home in Washington; even in the throes of her dissertation; and even in the distance of her recent job in Mexico City. Her professional role in MIT's 1993 Commencement is not merely a fortunate coincidence; it has also been a constant motivation.
In a world filled with fractured and alienated relationships, acknowledging a loving and cohesive family is a privilege. My sisters, Debra Guston and Judith Guston, have over the years nurtured my soul and my mind more than they know. Each phone call was truly appreciated, each care package hungrily devoured. One learns implicitly and explicitly through the experience of older siblings, and things that they have taught me are part of my daily life. I'm sure my parents, Herbert and Sheila Guston, have long expected something like this. But with their high expectations have come even higher measures of love, support, and stimulation. I can only hope they understand how much I love them, how much I have appreciated their commitment, and how much I am their son.

David H. Guston
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Chapter I. Introduction: The problem of politics and science

A. Introduction

1. A new appreciation of the link between science and society

Representative George E. Brown, Jr. (D-CA), chairman of the Science, Space, and Technology Committee in the House of Representatives, believes that science policy in the United States has reached a crucial turning point. In a commentary published in a prominent physics journal, Brown (1992) invoked the Czech playwright and statesman Vaclav Havel to describe to his audience of professional physicists, "The objectivity crisis" in American society and its relation to science policy. Brown's commentary was no mere after-dinner address; it was of a piece with his other writing (Brown 1991) and the work of his committee as well (U.S. Congress 1992).

Brown (1992:779) acknowledged the place of science in American culture alongside that of "baseball, Old Glory, and the right to avoid self-incrimination." He juxtaposed these icons with verses from what is widely understood as the gospel of U.S. science policy, Vannevar Bush's (1980 [1945]) *Science: The Endless Frontier,* and from contemporary prophet Leon Lederman's (1991) *Science: The End of the Frontier?* But then, contrary to the teachings of Bush and Lederman, Brown proceeded to castigate scientists for a near-idolatrous pursuit of science for its own sake, and for their misbegotten faith in the connection between this pursuit and the immediate beneficence of technological progress.

Brown did not indulge his fellow politicians, criticizing them for having been selfishly served by the orthodoxy that scientific research will deliver society from its

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3 Elsewhere, Brown (1991:26) has referred to Bush's report as "an almost biblical command."
ills. Brown (1992:780) wrote that “it is easier--politically, economically, socially, scientifically--to support more research than it is to change ourselves.” Rather than take this indulgent path, Brown beseeched scientists and politicians to “define a more constructive role for science in society”:

The problem then is to visualize and create linkages between the search for scientific truth, and the desire to achieve justice in our society....[To do this] the scientific community must seek to establish a new contract with policy makers, based not on demands for autonomy and ever-increasing budgets, but on the implementation of an explicit research agenda rooted in [social] goals....[S]cientists and policy makers must work together to make certain that research programs stay focused on policy goals...[and] that success of research is measured by progress toward a better quality of life for humankind, rather than by number of publications or citations or research grants (Brown 1992:780-81; emphasis added).

A new “contract” is required, Brown (1992:779) argued, because “[s]cience does not exist in a vacuum; it is inextricably linked with nonscientific elements of society--politics, history, economics, luck. This is obvious, though it is often not adequately appreciated.”

Brown’s concern about the links between science and society is contrary to the ideas that have framed science policy in the U.S. since the end of World War II. His argument is demonstrative of broader concerns among policymakers that the position of relative privilege that scientists and scientific research has enjoyed for the last half-century--a privilege of the generous provision of funds under generous terms--has changed.

2. Institutions, science, and politics

   a. three levels of analysis

   The problem this dissertation addresses is one of the interaction of politics and science: how Congress adopted a set of heterodox policies that exemplify Brown’s rhetoric in attempts to strengthen these links between science and society. In addressing this problem of politics and science, the dissertation seeks explanations at
three levels of analysis. First, at the most concrete level, this dissertation is an empirical investigation into a specific congressional-bureaucratic relationship. It seeks to explain the extent of and limits to congressional control of a bureaucratic agent. Second, at a slightly more abstract level, the dissertation analyzes this concept of a "contract" between science and society to which Brown (and many other science policymakers and analysts) refers. The dissertation thereby seeks to explain changes in the broader contours of U.S. science policy in the post-World War Two period. Third, at the most abstract level, the dissertation contemplates the relationship between the ongoing processes of democratic politics and scientific inquiry. Although I seek no hard-and-fast explanations at this level, I believe the evidence presented here will strongly suggest that this relationship is more complicated and more highly interactive than previously thought. I also believe that abstract discussions on science and democracy need to be grounded in empirical studies of such interactions.4

I conceive of these three levels of inquiry or analysis as related--almost "nested" in the way that steps in a computer program are "nested" in order to execute one set of computations within a hierarchy of other sets of computations. The particular congressional-bureaucratic relationship I examine, between Congress and the National Institutes of Health, is one (very significant) element of a set of relationships between Congress and the scientific bureaucracy. This set of relationships is an element of the set of interactions between government and science, and thus an example of the "contract" of which Brown talks. Finally, this "contract" is a part of the most overarching connections and vexing normative questions that run between democratic politics and scientific inquiry. The remainder of this section will present in more depth what I hope to accomplish at each of these levels of analysis.

4 The abstract discussion of most relevance here is the Brown-Havel-Holton nexus alluded to above, to which I would add Latour (1991) and Ezrati (1990). The general concern of all is the relationship of science to democratic politics when science becomes detached, via Romanticism, Uncertainty, or other forces, from Truth.
b. institutions

It is hard, in empirical inquiries as well as in policy, to traffic in concepts like the linkages between science and society. It helps to view these concepts in sets of rules, patterns of interaction—in a word, institutions. This vision does not reify the concepts, but rather it seeks to find examples of the concepts-in-action, and explain how politics and policies are constructed from there. This dissertation views the conceptual relationship between society and science through a pair of complex institutions, the U.S. Congress and the National Institutes of Health (NIH), and through interactions among members of Congress, congressional staff, bureaucrats, and scientists. It also reduces the scope of all possible substantive areas of interaction to two issues that rose to prominence in the 1980s: the integrity and the productivity of scientific practice. The kernel of the dissertation is, therefore, an issue-specific study of congressional-bureaucratic relations. Appropriate to this focus, I investigate how issues of the integrity and productivity of science rose to congressional attention, how members of Congress expressed their interests and concerns in a formal manner, and how the bureaucrats responded to these concerns. The dissertation seeks to discover what the limits of congressional control of the bureaucracy are, and what the range of bureaucratic discretion is, on the “two-way street” (Moe 1987:482) between the congressional principal and the bureaucratic agent. That is, I explain how a principal-agent relationship is created, and what the limits of the such a relationship are.

The cases of the integrity and productivity of science are important in this context for three reasons: first, because of their reflection of congressional interests and abilities, the integrity and productivity of bureaucratic performance are common concerns of congressional overseers, so they can be generalized to other areas of congressional-bureaucratic relations; second, the integrity and productivity of scientific

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5 By integrity of scientific practice, I mean the issue of fraud and misconduct in science. By productivity of scientific practice, I mean the expectation of technological and other benefits flowing from research. See I.C.4 below.
research is a very hard case for congressional oversight and control, because scientists make a strong claim that only they can determine the integrity and productivity of their peers; third, the integrity and productivity of science are crucial aspects of the more abstract levels of analysis.

c. science

The principal-agent relationship between congressional committees and NIH is nested within what Representative Brown called the “contract” between scientists and policymakers. As Brown suggested, accepting the link between science and society means rejecting an old “contract” and negotiating a new one. The old contract is what I call the “social contract for science.” It is a conceptual framework that attempted to partition science and society, rather than to appreciate their linkages. The social contract for science is akin to other “social contracts” invoked by professional communities such as physicians to describe a privileged relationship between the state and the profession of medicine as an autonomous sphere within civil society (Bayles 1983). The *quid pro quo* of such a social contract is the grant of a substantial amount of autonomy in exchange for professional service to a clientele in the public interest.

The social contract for science is different from these professional social contracts, however. Whereas, the latter are defended as the best way to service a clientele and appropriate for a Liberal state; the former is defended as the only way to pursue the truth and necessary for a Liberal state. That is, the social contract for science justifies a privileged position for science by claiming that the scientific community produces verifiable and useful new knowledge if and only if the community is left to be autonomous and self-regulating, much like a free economic market (e.g., Polanyi 1962). Furthermore, although it might be possible to procure professional products and services through explicit contracts, scientists credibly claim that the

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6 It is also plausibly related to social contracts among persons to create systems of authority to govern themselves. See Guston (1991).
greatest benefits of their services and products are available only over the long-run, and therefore cannot be explicitly procured or contracted for.\textsuperscript{7} Though general concepts, integrity and productivity are thus essential for the credibility of the social contract for science: as defenders of the contract argue, any political interference, even to ensure integrity and productivity, in fact threatens to destroy integrity and productivity. By proscribing political interference, the social contract for science demarcates a sharp boundary between politics and science.

When the institutional relationships I study were created immediately after World War Two, these claims of integrity and productivity seemed credible, generally because they were politically expedient for both the scientists and the politicians involved, but also because there was positive evidence from the conduct of scientists during the war to support these claims. The specific institutional relationships that evolved reflected the general and unproblematic acceptance of the integrity and productivity of science. The institutional boundary between politics and science was sharp. But beginning in the late 1970s, vexing cases of scientific misconduct and indications of declining levels of technological innovation made claims of integrity and productivity less credible to the congressional principals than they previously had been. Consequently, the congressional principals became more direct in their attempts to control their scientific agents. The dissertation reveals these attempts and argues that the application of such direct controls is actually the beginning of the new contract that Representative Brown is seeking. Although I focus on the implementation of these controls at NIH, they now exist to some degree across the spectrum of federally funded research. As these controls are applied, the relative privilege of science is reduced and the boundaries between politics and science are renegotiated. In a sense, a social

\textsuperscript{7} The conceptual foundations of the social contract for science will be further elaborated in II.C.3. This claim of an inherent "uncheckability" in the relationship is a primary reason for applying a principal-agent perspective.
contract for science that had looked to some like an entitlement is sliding toward procurement.\textsuperscript{8}

d. politics

Although the social contract for science is based on claims about the nature of science--its integrity and productivity--it is not just a prescription for the organization of science but it is also a prescription for the organization of politics. In other words, the social contract for science is an explicitly political statement. Indeed, at the broadest level, the social contract for science is about the relationship between democratic politics and science, the two most compelling forces of modernity. This dissertation attempts to cast the empirical work in congressional-bureaucratic relations presented here in the light of recent theoretical scholarship on the changing relationship of science and politics.

Representative Brown suggested that politicians in democratic society, as both sponsors of science and beneficiaries of its successes, are also accountable for its failures. Indeed, members of Congress have long been interested and active in trying to assess and ameliorate any untoward externalities of science and technology. But they have now been driven to examine and tinker with aspects of science that have previously been protected as internal and integral to the organization of science. If only to defend themselves from charges of being spendthrifts or wastrels, members of Congress will scrutinize science (as other spheres of operation) for its integrity and productivity, and ultimately create mechanisms to ensure both. Such creations blur the boundary between the spheres of politics and science that had been drawn by the social contract for science.

The motivations and mechanisms that bring about this blurring are suggestive of a much more complex and interactive relationship between science and politics than the

\textsuperscript{8} This language of entitlement and procurement was, in fact, the way some of my interview subjects described the situation.
blurred boundary metaphor portrays. Just as empirical studies in anthropology and sociology suggested to Karl Polanyi (1957 [1944]) that the self-regulating economic market is created by and submerged in social relations, empirical studies in the anthropology and sociology of science suggest that science is also embedded in social relations. My study of congressional-NIH relations offers a venue on the embeddedness of science in politics. From this venue, political science can envision a complex politics of science, like Karl Polanyi's politics of economics, that both extends and constrains science as an autonomous, self-regulating enterprise. This vision of embeddedness rather than separateness as the appropriate description of the relationship between science and society is at the heart of Havel's "end of the modern era" that inspired Brown, as well as Israeli political scientist Yaron Ezrahi's (1990) "postmodern science and postmodern politics" and French sociologist Bruno Latour's (1991) science and technology in the "nonmodern world."

One need not concede to this apocalyptic-sounding, Hegelian language to recognize important consequences. The embeddedness--or even the proximity--of the spheres of science and politics has two immediate effects: it highlights the links of extension and constraint between the two spheres; and it renders visible the politics that is internal to science for the first time. In the microcosm of the relationship between Congress and NIH, we not only can see the evolution of the principal-agent dynamic. But in drawing politics and science nearer, we can join Representative Brown in seeing a "political" science. We can also witness the end of the social contract for science and perhaps glimpse the practical underpinnings of the changing status of science in a pivotal time.

B. Institutions and issues

1. Political science and apolitical science

Political science has been ill-prepared for this view of a “political” science. As Lowi (1992:1) has argued, the discipline is something of a “dependent variable” of the state in that its scholarship has followed changes in the state rather uncritically. There is a lag in the relationship, such that “we [have] failed to catch, characterize, and evaluate the great ideological sea changes accompanying the changes [in the state]” (Lowi 1992:5). As political scientists, we have failed in a similar manner to catch and characterize the changes in congressional-NIH relations, in the social contract for science, and in the broader relationship between politics and science that so interests Representative Brown.

I believe this failure has occurred because political science has generally seen the exercise and extent of political power as its boundary. For the most part, political science has assumed that political power extended to and from, but not into, science. The belief that political power stops where true knowledge begins is not an unusual one. If Thomas Hobbes is a founder of political science, then this belief is foundational to the discipline. Hobbes applies geometry--viewed as the parent and archetype of the sciences--to resolve the chaos of the state of nature into a stable government and civil society. Geometric science holds this potential for Hobbes because he found it fundamentally apolitical:

Which is the cause, that the doctrine of Right and Wrong, is perpetually disputed, both by the Pen and the Sword: Whereas the doctrine of

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10 Political science may have also relied too heavily on other disciplines such as history, which has productively interwoven science, technology, and politics (Reingold 1991; Hughes 1989; Strickland 1989; Bruce 1987; Harden 1986; Kohlstedt and Rossiter 1986; England 1982; Kevles 1978; Strickland 1972; and Dupree 1957). Indeed, historian Susan Wright (1993:82) points to the importance of “an investigation of the political economy of science--the power relations affecting the direction and pace of research and development that crystallize in governmental policy.” Among the most thorough overviews of the postwar interaction between science and politics have come from journalists (Dickson 1988; Greenberg 1967); Smith (1990) and Sapolsky (1990) stand alone as views from political science. In this explanation, the discipline may have underestimated the value added to such work.
Lines, and Figures, is not so; because men care not, in that subject what be truth, as a thing that crosses no mans ambition, profit, or lust. For I doubt not, but if it had been a thing contrary to any mans right of dominion, *That the three Angles of a Triangle should be equal to two Angles of a Square*; that doctrine should have been, if not disputed, yet by the burning of all books of Geometry, suppressed, as farre as he whom it concerned was able. (Hobbes 1983 [1651]:165-66; emphasis in the original).\(^{11}\)

Hobbes’s idea is quintessentially modern and Liberal: the “doctrine of Lines, and Figures” differs from the “doctrine of Right and Wrong” in that persons lack an interest in the former (“it crosses no mans ambition, profit, or lust”); but they maintain an extreme interest in the latter, such that they are prepared to dispute it even by violence.\(^ {12}\) In other words, science and politics exist in two separate spheres.\(^ {13}\) The modernity of Hobbes’s vision is identifying a sphere of knowable, indisputable, objective truths, and distinguishing that sphere from the one of morals, ethics and values. The Liberalism of the vision is claiming that the reason this first sphere is indisputable is because it does not cross “ambition, profit, or lust”--exactly those features in the Hobbesian anatomy of human beings that invite the war of all against all

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11 Sheldon Wolin, in his introduction to Blisset (1972), notes this passage from Hobbes. Although I had not seen Wolin’s introduction when I first made this connection myself, I had been attracted to this aspect of Hobbes in work with Uday S. Mehta, a student of Wolin.

12 One could question Hobbes’s sincerity in making this distinction. He was, after all, involved in the most significant scientific dispute of his day with Robert Boyle over the nature of scientific knowledge in general, and the existence of the vacuum and the quality of Boyle’s air pump in particular (Shapin and Schaffer 1985). Nevertheless, what Boyle and Hobbes disagreed about was how to create this objective, indisputable, and apolitical knowledge; they agreed that such a category existed. One could also question Hobbes’s understanding of how the sciences progress, if not by being advanced through the “ambition” of the persons who draw and calculate. But this understanding is a distinctly recent one. For Hobbes at the beginning of the modern age, as well as for Tocqueville reflecting on its consolidation and for Weber near its culmination, science was a product of leisure and contemplation, not ambition. Tocqueville (1969 [1848]:460) writes that the “higher sciences or the higher parts of all sciences require meditation above everything else.” Weber (1946 [1918]) writes that the practitioners of science, as a vocation, must be insulated or separated from political and economic interests, which interfered with their calling to engage in scientific work. Although this permissive, contemplative space is concomitant to politics, it still needed to be separated from the clash of interests in order to foster productive scientific inquiry. For this reading of Tocqueville, Weber, and other aspects of science and social theory, see Guston (1993).

13 The belief in separate spheres for politics and science did not keep Hobbes (or other Liberals) from deploying a “scientific” argument about politics. Indeed, it seems to have encouraged them to believe that the use of scientific arguments would ameliorate politics to the extent that they would alleviate the reliance of politics on “ambition, profit, or lust.” Lowi (1992) denounces the turn of political science toward formal, economic, scientific methods that results in inquiry without “passion.”
and thereby necessitate politics. Hobbes limits politics, and thus political science, to
the sphere of interests, which is apart from the sphere of science.\footnote{Ambition, profit, just and the like are also the qualities that necessitate politics for other Liberal political thinkers. For example, for Locke, a government as "umpire" is necessary because no person is competent, given human ambitions, to be a judge in his own case. Locke (1963:308), like Hobbes, also circumscribes the sphere of politics and political power to a domain that does not include other relationships established in other social settings, e.g., families, employment, and enslavement. It is exactly this limited conception of the political that many critics of Liberalism, e.g., feminist scholars who see politics in families, fasten on. Ezrahi (1990) describes this general separation of science from politics in Liberal thought as the "autonomy of truth," and he traces its development from the English Enlightenment through seminal American thinkers like Jefferson and Franklin to the American Pragmatists.}

2. Lacunae of the literature

Following Hobbes, and following the Liberal state that adopted the modern vision of an apolitical science,\footnote{See Ezrahi (1990; 1980) for the instrumental and ideological importance of this vision of science to liberal democratic thought and practice.} one could say that the discipline of political science has been interested in science only when science directly impinges on politics. That is, political science generally views science as an exogenous force on politics, for example: when scientists participate in political innovation and public affairs (Polsby 1984; Teich 1974); when science has an impact on political organization and institutions (Lowi and Ginsberg 1976) or on foreign policy concerns (Skolnikoff 1972; 1967); or when scientists bring knowledge to power as an "apolitical elite" (Lakoff 1966; Wood 1964; Dupré and Lakoff 1962).\footnote{Lakoff (1966) and Dupré and Lakoff (1962) are broader than this characterization, but it is the central concern of the works. Both also contain work by journalists and participants. Science advice has probably been the bulkiest portion of the literature. Some recent work in political science has begun to examine the scientific advisory apparatus in the bureaucracy (Smith 1992), but the literature is largely dominated by the recollections and exhortations of science policy participants (Golden 1991; 1988; 1980; Killian 1982; Burger 1980; Kistiakowsky 1976; Wiesner 1965).} Even the exceptional work of Don K. Price proves the rule. Price (1954) praises the institutional arrangements derived from the Second World War that separated politics and science, despite the new funding relationship. His later work (Price 1981; 1965) prescribes policy action that would uphold and maintain this separation. Price's political science, particularly the "spectrum from truth to power," insulates truth from political inquiry by distancing it from the exercise of power.
The only two significant exceptions are: Marlan Blisset’s (1972) *Politics in Science*, which examines the political dynamics of consensus and controversy within the scientific community; and Yaron Ezrahi’s (1990) *The Descent of Icarus*, which examines what could be called the backside of Price’s “spectrum from truth to power,” where science and politics meet in forging cultural authority for both. Blisset’s work is intriguing, but it does not seem to have achieved any visibility in the field. If only to incorporate the two decades of advances in political science and the sociology of science, Blisset’s work on the internal politics of the scientific community should be revisited. But connections between the politics in science and the politics of science also need to be investigated. This connection is, in part, what Ezrahi attempts, but on the grand scale of politics, ideology, and culture. His analysis focuses on how science and liberal democratic thought have, in effect, matured together since the 17th Century. In the last half of the 20th Century, argues Ezrahi, changes in both science and liberal democracy have undermined the way each manufactures authority, and these changes threaten the stability of both. It is an interesting and important task for political science to discover if the connections described by Ezrahi actually function in the practice of science policy relationships such as those between Congress and NIH.

In part because of the institutional connection with the acme of the “apolitical elite,” the President’s science advisor, there has been some attention to presidential decisionmaking with regard to science and technology (Lambright 1985). Where political science has pried a little deeper into science and its association with the executive branch, the results have often languished, unfortunately, in the field of public

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17 Not surprising, since Ezrahi was a student of Price at Harvard.
18 Blisset’s work is in a sense a political counterpart to Polanyi’s (1962) initial market model of “The republic of science” and Tullock’s (1966) more formal but similar, political economic model of *The Organization of Inquiry*. Gilbert and Mulkay (1984) critique Blisset for following an accepted sociological method that privileges the accounts of participants too greatly.
19 Lambright (1985:2) laments, “There is a literature on the presidency, and there is one on science and technology. Seldom do they converge.” Similarly, Lewis M. Branscomb (1988:44) complains that “scientists figure more prominently than Presidents” in the science advising literature.
administration (Lambright 1976; Rosenthal 1973). Rettig's (1977) study of the National Cancer Act of 1971 avoids this fate, however, by following the policy process from agenda-setting to implementation for a specific piece of science legislation. But most of the deepest prying and the most revealing accounts of the relationship between the institutions of government and science, have come from outside the discipline of political science, particularly from the new sociology of science and from the older, controversy studies literature.

Even more rarely has political science touched on any aspect of science and technology in its relationship to the Congress. The most significant (and perhaps only clear) example of such work is Green and Rosenthal's *Government of the Atom* (1963), about the tenure of the Joint Committee on Atomic Energy and its relationship with the Atomic Energy Commission (AEC). More than a quarter-century later, Cohen and Noll (1991) address the question of government-funded, commercial research and development projects in *The Technology Pork Barrel*. Until recently, the question of the use of scientific and technical information by Congress had been relegated to accounts by participants in congressional science advice, parallel to the accounts of science advice to the President. There are now more sophisticated descriptions and

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20 Whereas, Barke (1986) attempts to take on the entire policy-making apparatus of the federal government. Historical and programmatic details of executive agencies involved in research, and views of policy questions, can also be found in the participant-literature. The most thorough source is Brooks (1968), but also see Shannon (1973), Orlans (1968), Keenan (1966), Seitz (1966) among others. For contemporary participant-literature, see Meredith, et al. (1991; and annually).

21 Among the most prominent are Chubin and Hackett (1990), Jasanoff (1990), and Mukerji (1989).

22 Examples include Primack and von Hippel (1974) and virtually the entire corpus of Dorothy Nelkin, although much of Nelkin's work involves science and politics interacting at the nonfederal level.

23 Congress is not the exclusive focus of the volume, however. Furthermore, the authors of the case studies are mainly economists. But given the economic slant of political science currently, and the absolute dearth of studies on science and Congress, I choose to include it here. The work of economists (and organization and management scholars) is becoming increasingly important to science policy. See Rosenberg, Landau, and Mowery (1992), Mansfield (1991), Dresch and Janson (1990; 1987) Mowery and Rosenberg (1989), Link and Tassey (1987), Averch (1985), Clark (1985), and Bozeman, Crow and Link (1984).

24 For example, Gibbons (1988) and Gibbons and Gwin (1986)
models of information use in general (Bimber 1991; Krehbiel 1991) and even of scientific and technical information in particular (Bimber 1992).\textsuperscript{25}

Almost lost in the literature are a tiny number of case studies in the (generally sparse) literature about congressional oversight that specifically address the oversight of what could be called science-based agencies: Jahnige (1968) writes about the oversight of the National Aeronautics and Space Administration (NASA); and Foreman (1988) examines oversight of the Environmental Protection Agency (EPA), the Food and Drug Administration (FDA), and the Occupational Safety and Health Administration (OSHA).\textsuperscript{26}

At their best, these works do politicize the technical content of the issues under congressional scrutiny. Green and Rosenthal recognize the possible policy consequences of settling technical disputes in the tension between the nonexperts at the Joint Committee and the experts in AEC. Cohen and Noll identify the political burdens that politicians intend research and development programs to carry. Both works find that congressional structure—a joint committee, and decentralization—has a significant and predictable influence on policy outcomes even in areas with high technical content. Bimber (1992) acknowledges the uncertain and constructed elements of scientific knowledge. Jahnige affirms the ability of even a subcommittee that is at times intellectually outclassed to wield influence. Foreman (1988:7) finds that the oversight of regulatory policy is complicated by the “radical uncertainty” presented in part by technical inputs. However, these concerns about the political content of the technical are marginal rather than central to these efforts.

\textsuperscript{25} Study of the science advisory bodies external to government, e.g., the National Academy of Sciences, has also been left to historians (Cochrane 1978; True 1913), journalists (Boffey 1978), and sociologists (Hilgartner 1991).

\textsuperscript{26} Foreman also provides valuable insight into the investigations subcommittee of the House Energy and Commerce Committee, of which John Dingell is chairman. For more on the subcommittee, see Price (1985) and see III.D.1.
Another shortcoming is that, of the studies that can roughly be grouped as congressional-bureaucratic relations (Cohen and Noll 1991; Foreman 1988; Jahnige 1968; and Green and Rosenthal 1963), the principle output of the bureaucracies studied is economic rather than scientific activity. As Lowi (1992) might suggest, political science has studied these bureaucracies because, in their economic character, they are capable of being described by the discipline. NASA performs a great deal of research but, as Jahnige points out, the agency’s goals are to produce large and visible technological programs that create jobs in particular districts. Similarly, a great deal of research and development is associated with the projects described by Cohen and Noll, but the central role of Pork Barrel Technology is distributive economics. The agencies that Foreman studies perform and use scientific research, but they produce regulations with an expected economic impact.27 The same is largely true of AEC in Green and Rosenthal. Political science has not attended to bureaucracies whose output is measured, in Representative Brown’s words, “by number of publications or citations or research grants.”28 It has thereby missed the transition of agencies like NIH from producing these outputs, to being asked to produce outputs that are, like these other bureaus, more economic and more measurable.29

The point of this brief review is not to suggest that science should be studied more by political scientists because it is special, that is, necessarily different from

27 Indeed, since the Carter Administration, regulations from these and other agencies have been evaluated explicitly for their economic impact by the Office of Management and Budget. The studies of risk analysis, risk management, and research for regulation involving agencies such as FDA, EPA, and OSHA are too numerous to list, but see Jasanoff (1990), Landrigan and Selikoff (1989), Crandall and Lave (1981), Fischhoff, et al. (1981).
28 Derek J. de Solla Price was the inventor of the “science of science,” also known as bibliometrics or scientometrics, of which citation analysis is one field. Although first presented in 1950 and first published in 1961, the method and its results—most prominently the geometric increase in scientific activity over time—did not become popular until 1963, and then only among scientists. Price, of course, was a physicist and historian. See Price (1986; 1975). The technique matured in the mid-1970s (OTA 1986). See Elkana, et al., (1978), for an overview of this maturation. OTA (1986:42) describes citation analysis as a useful “complement” to peer evaluation of research programs.
29 Ironically, in this example, although political science has missed the change in the state, it is the state that is lagging behind political science. This seems to be an indicator of the privilege of science.
economics, education, or other domains of public policy. Such claims to the specialness or exceptionalism of science have been made; indeed, the social contract for science is but one example. But such claims are both intellectually flawed—as I hope to suggest in this dissertation—and in the process of being demonstrated to be flawed by political processes themselves (Bimber and Guston forthcoming; Jasanoff 1992; 1990). Rather, the point is that political scientists have not studied science as they have studied other policy domains because they have assumed it to be special, that is, devoid of political content and uninteresting in a very literal way.

This dissertation seeks to fill the lacunae not just in the literature but, ambitiously, in the discipline as well, by addressing science as an explicitly political phenomenon. By focusing on Congress and NIH, I hope to expand the scope of the discipline’s understanding of congressional-bureaucratic relations to a new domain. By showing how politicians have also begun to see and manipulate the political in science, I also hope to expand the scope of the discipline’s understanding of its subject matter. This task has actually become easier because, as the events that I am studying have unfolded, politicians themselves have been performing much of the labor of linking science to society, creating measures of science, and in general rendering science more open to political scrutiny.31

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30 The intellectual flaws of the idea can be identified through the lens of the anthropology and sociology of science cited in I.B.2.c. This literature rarely deals with science as object of public policy, however. Bimber and Guston (forthcoming) and Jasanoff (1992; 1990) show how, like academic studies “deconstruct” claims of a special or unique science, adversarial political processes such as congressional-executive conflict, congressional investigations, and litigation do so as well. Also see III.D.2 for Representative Dingell’s deconstruction of science, based on Guston (forthcoming).

31 “Science under scrutiny” is an idea that I developed along with my involvement in a NATO Advanced Study Institute on “Managing Science in the Steady State,” in October 1989. The idea behind “science under scrutiny” is that contemporary political demands will create an environment in which science will have to fight for any privilege it receives in an explicitly political manner, regardless of whether funding levels are rising or falling. See David H. Guston, “Observations on the NATO ASI: science under scrutiny,” EASST Newsletter (November 1989), pp. 17-18.
C. Congress and NIH in context

1. Institutions, science and politics (reprise)

At times scholars write as if the events they are recounting and explaining had themselves taken place in an ideal laboratory, isolated from the intervention of confounding variables. I hope to give this impression as little as possible, because it is exactly my contention that even the laboratories at NIH are not so isolated or immune. So it is useful to attempt in an overt way to contextualize the more focused work that will follow.

There are a series of contexts that are relevant to the three levels of inquiry I described above. One context consists of the history of the interplay between the principal and the agent in the kernel of the analysis. I will defer my more detailed examination of the history of the relationship between Congress and NIH until the next chapter (II.C.4), but I will instead deal briefly with the Congress and NIH as principal and agent, respectively. The overview then addresses the two substantive issues, the integrity and the productivity of science, and it will also introduce my application of principal-agent theory. I will then expand the inquiry to the next level of analysis, the social contract for science, and then the final level of the boundary between politics and science.

2. Congress, the principal

The most important aspect of the principal-agent dynamic is the information (or lack of it) that the participants have about one another (Pratt and Zeckhauser 1991; Moe 1984; Niskanen 1971). Generally, it is assumed that congressional principals have a

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32 I do not wish to reify the roles of principal and agent in Congress and NIH. My research, in fact, shows that doing so would miss important aspects of the relationship. Nevertheless, starting with this assumption is both standard and useful.

33 Congress is an aggregate of individuals, subcommittees, committees, parties, conferences, and other groups. Usually, specific subcommittees or committees are identified as the principal. In this section and elsewhere, at times, I refer to Congress as the principal when I discuss general influences such as staff changes that affect many of the lower levels of aggregation. The exact identity of the principal differs between my two cases, and probably among any set of issues.
more difficult time gathering information about bureaucracies and costs of bureaucratic production than bureaucracies have in gathering information about Congress and congressional preferences (Niskanen 1971). But a set of innovations in congressional information systems have strengthened the position of congressional principals vis-a-vis the executive in general and the expert executive agencies in particular. These innovations include changes both very internal to Congress, such as the increase of congressional staff and the augmented role of subcommittees and specialized oversight committees; as well as changes at the margins of Congress such as the creation or strengthening of legislative service organizations, and the institution of Inspectors General.

The most relevant set of changes internal to Congress began in the context of the struggle between the legislature and the Nixon Administration, which was not only a partisan feud but which also pit the institutional interests of the first and second branches against each other. The Legislative Reorganization Act of 1970 was a great decentralizing force, limiting the number of committee and subcommittee chairmanships that members could hold and thereby providing wider access to the institutional levers of power. Decentralization increased the opportunity for members to engage in oversight (Aberbach 1990). The subsequent House Committee Reform Amendments of 1974 and the Senate Committee System Reorganization Amendments of 1977 strengthened the capacity for oversight by extending to the investigations and oversight subcommittees of the legislative committees the powers of broad review similar to those

34 For an empirical perspective from the agent's side, see Davis (1983) on congressional liaisons.
35 The Legislative Reorganization Act of 1946 also responded to the perceived institutional ascendancy of the executive. It gave the government operations committees in both chambers broad authority to investigate the efficiency and effectiveness of the conduct of the executive agencies. See Huntington (1965) for the postwar changes leading to increased congressional oversight of the executive.
36 It helped facilitate the transition from the old "textbook" Congress to the new textbook Congress, a transition from an equilibrium of jurisdiction, party and geography to an equilibrium of subcommittees, party leaders, and individual members (Shepsle 1989). Smith (1985) sees a similar change as decisionmaking in both chambers has become more "collegial" and less controlled by a "directorate."
of the House Government Operations and the Senate Government Affairs Committees (Oleszek 1989; Sundquist 1981). Concurrent to decentralization was a sharp increase in the number of committee staff, which had been increasing steadily since the early 1960s but which more than doubled from 1971 to 1977; this staff increase tracks closely with an increase in congressional oversight activities (Aberbach 1990:44). An internal, procedural innovation was the single- or multi-year reauthorization for agencies and programs (Schick 1983) that institutionalized oversight into regular reauthorization hearings and created still more opportunities for congressional intervention.

The Legislative Reorganization Act of 1970 also began to strengthen Congress relative to the executive by reorganizing the Legislative Reference Service into the Congressional Research Service (CRS) and expanding its role and staff, which more than doubled in size from 1970 to 1975 (Sundquist 1981:403-04). The 1970 Act also recognized the changing role of the General Accounting Office (GAO), which had been created as an independent auditing and accounting agency by the Budget and Accounting Act of 1921,37 but had increasingly become involved in responding to congressional requests directed at executive programs (Dodd and Schott 1986:251-56). In 1972, Congress recognized a need for its own specialized information source in science and technology, especially in the wake of conflicts with the Nixon Administration over technically complex projects such as the supersonic transport and the anti-ballistic missile system, by chartering the congressional Office of Technology Assessment (Bimber 1992). In a similar maneuver, Congress created the Congressional Budget Office in 1974 to provide its budget and other money committees with economic advice and information (Dodd and Schott 1986:258-62). In 1976,

37 The Act, "a watershed in the creation of a new American state and the establishment of a new bureaucratic politics" (Skowronek 1982:207) with Progressive ideals of expertise and efficiency, had also created the Bureau of the Budget, later the Office of Management and Budget. Also see Stewart (1989) for the impact of the Act on budget politics.
Congress extended the logic of creating legislative staff agencies one step further by passing the Inspector General Act. The Act established an Office of Inspector General (OIG) in each cabinet-level department and many of the independent agencies to report both to the Secretary (or administrator) and directly to the Congress on the efficiency and effectiveness of programs. These changes in internal congressional organization and in methods to liberate and evaluate information from executive agencies correspond to an increase in congressional oversight activity (Aberbach 1990), that is, congressional attempts to assert its position as principal. Part of the task of the dissertation will be to demonstrate the impact or consequence on the effectiveness of oversight that some of these changes have had.38

Apart from this historical role in creating some of the institutional players, interbranch competition as such or divided government does not seemed to have played a great role in congressional attempts to control the integrity and productivity of science at NIH. There is almost no presidential involvement in the issue of scientific integrity, although there are very clearly signs of competition within the PHS and its parent department, Health and Human Services (DHHS). But this competition is just as easily linked to Congress as well, because it seems to focus on limiting the role of the DHHS Inspector General and maintaining the independence of the research arm of NIH.

Where the President does intervene, by vetoing relevant legislation for unrelated

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38 Aberbach's (1990:195) attention to the consequences of oversight is largely limited to one paragraph in the concluding chapter: "And congressional oversight has a significant effect on agency behavior. Although I cannot assess these effects with any precision, the existing literature...suggest[s] that this is true." Aberbach cites Kaufman (1981), Fenno (1966) and Wildavsky (1984) as evidence for the influence of oversight. Wildavsky deals exclusively with the budgetary process and its specialized committees and hearings, and therefore does not address the effectiveness of oversight performed by legislative subcommittees or the government operations committees. Likewise, Fenno dwells entirely on the relatively direct influence of appropriations. Kaufman (1981:163) studies six bureau chiefs and concludes that the "power of Congress was the predominant factor" in influencing bureaucratic action. Kaufman, however, seems to fall too much on the side of congressional power. The consensus of the pre-reform literature on congressional oversight (e.g., Henderson 1970; Jahng 1968; Bibby 1966; Sharkansky 1965; Scher 1963) is that oversight is unsystematic, fragmented, and largely ineffective. For more detailed review of this literature, see Guston (1990a), Aberbach (1979) and Ogul (1976).
reasons, he acts almost as an instrument of political fortune rather than as another principal.

There is a much greater role for the executive in the case of the productivity of science, as indeed I begin its telling with the domestic agenda of President Carter. Although the Republican administrations that followed were hostile to some aspects of the technology policy that emerged in the 1980s, they actively supported other details and, more importantly, the overarching concept of applying explicit incentives to federally funded scientists. The executive involvement around the issue, however, is not surprising given the explicitly economic character of the productivity of science and the economic questions that are at the heart of the assessment by voters of presidential performance (Kiewiet 1983; Tufte 1978).

3. NIH, the agent

The scientific bureaucracy I have chosen to study is the National Institutes of Health (NIH). NIH is significant for the scale and scope of its activity, as well as for the particular aspects of its connection to issues of scientific integrity and productivity. The primary mission of NIH is to support research that contributes to the mission of improving the public health, a near-sacred mission ranking only behind national defense in its ability to attract the commitment to research funds. NIH is the world’s largest sponsor and performer of biomedical research. For fiscal year 1993, Congress approved a research and development (R&D) budget for NIH at almost $10 billion, or 13.6% of the approximately $75 billion federal investment in R&D. Of this $10 billion, some $5.6 billion was slated for basic research, or 41% of the $13.7 billion for all federal basic research. Excluding defense R&D, biomedical research at NIH constitutes about 29% of all civilian R&D and more than 45% of all civilian basic research. The NIH basic research budget is roughly three times that at either the

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39 NIH R&D accounts for almost 96% of its total budget. All figures in this section are taken from AAAS (1993).
National Science Foundation (NSF),\textsuperscript{40} the Department of Energy (DOE), or the Department of Defense (DOD).

This largesse is distributed over twenty-one institutes, centers and divisions (ICDs), most of which sponsor research both in government-owned and -operated laboratories (intramural research) and in university, non-profit, and for-profit laboratories (extramural research).\textsuperscript{41} Approximately 12\% of all NIH R&D is conducted in its intramural laboratories, although this percentage differs greatly among the ICDs because some, such as the National Institute of General Medical Sciences (NIGMS) which spends almost $1 billion extramurally, have no intramural research program.

NIH provides between 35\% and 40\% of the nation's total health R&D (NSB 1991:100). Because NIH is the major sponsor of biomedical research, I will define the biomedical research community as the group of scientists who are or are potentially supported by NIH research funds, as well as their colleagues who interact with them and may be influenced by NIH policy.\textsuperscript{42} This definition intentionally suggests that the biomedical research community is in part constructed by the politics of research funding. The definition also includes scientists in all sectors of research performance. Because of its large plurality share of research funding, NIH has a somewhat monopolistic role in biomedical research community in the sense that all the players from other sectors must take its actions into account in their own choices. Private foundations often direct their research funding to areas that complement NIH funding.

\textsuperscript{40} Although NSF spends more than 87\% of its R&D funds on basic research.

\textsuperscript{41} Exactly why there are both intramural and extramural programs is an interesting question worthy of formal inquiry. The programs have independent bureaucratic origins, but is there a formal reason why government patrons would want some research performed by government employees and other research performed by grantees, even if the criteria for research and program evaluation were largely the same between the two types?

\textsuperscript{42} “Potentially supported” is appropriate because applicant institutions and not just grantee institutions are subject to NIH policies for safety, integrity, etc. The biomedical research community is also constitutive of NIH, as its members staff study sections to peer-review grant proposals, for example, or as extramural scientists review intramural projects and programs.
rather than supplement it, to ensure that biomedical researchers can find at least some support from some patrons. By setting standards for the use of recombinant DNA material under its grants, for example, NIH also helped bring about the application of these standards to privately funded research as well.\(^{43}\) Similarly, the government ban on the use of fetal tissue in federally funded research had repercussions beyond federally funded scientists (see Childress 1991).

Biomedical research is the most significant portion of the federal government's basic research budget. Within a context of increasing austerity, conflict, and scrutiny, the science budget—and biomedical science in particular—has performed very well.\(^{44}\) NIH obligations for research and development (R&D) increased from $3.182 billion in fiscal year (FY) 1980 to $7.714 billion in FY1991 (NSB 1991:313).\(^{45}\) During this same period, outlays for the domestic discretionary budget increased from approximately $140 billion in FY1980 to $198.1 billion in FY1991.\(^{46}\) NIH R&D thus raised its share of the domestic discretionary budget from 2.3% to 3.9% by accounting for 7.8% of the total domestic discretionary increase in the period. NIH is therefore an important focus of congressional and scholarly scrutiny as the scientific agent in an

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\(^{43}\) At first, acceptance of the standards was voluntary, but then acceptance became a condition of licensing the process from the patent, administered by Stanford University. The standards, though, were those developed by NIH.

\(^{44}\) There is no science budget, per se. Research and development (R&D) spending is distributed over a dozen or so agencies, most of it concentrated in the departments of Defense, Health and Human Services, and Energy, and in the National Aeronautics and Space Administration. The Office of Management and Budget, organized largely in parallel to the executive agencies, has even discontinued publication of its "special analysis" of R&D spending. The Federal Coordinating Council for Science, Engineering, and Technology (FCCSET) has in the past few years established "budgetary cross-cuts" that attempt to provide a unified look at R&D areas of special interest—e.g., materials science, biotechnology, etc.—across the executive agencies.

\(^{45}\) Obligations are in current dollars; the 1991 figure is an estimate. In constant dollars (1982), NIH R&D obligations still increased over the period from $3.755 billion to $5.604 billion (NSB 1991:315). Basic research accounted for most of this increase.

\(^{46}\) In current dollars. The domestic discretionary budget is that part of the budget remaining when defense and foreign aid, interest payments, and entitlements are deducted. The absolute increase in domestic discretionary spending, however, represents a decline of its share of the total budget from about 25% to about 16%. So, as the relative share of domestic discretionary outlays have been decreasing, the relative share of NIH research funding was increasing. Numbers derived from CQ (1991; 1980)
evolving principal-agent dynamic, and biomedical research is also a reasonable platform from which to make consider generalizations about the rest of the scientific community.

This success continued the NIH budgetary tradition in which the Department of Health and Human Services (DHHS) and the Office of Management and Budget (OMB) would each reduce NIH budget requests, and Congress would restore the cuts, with an incremental increase as well (Wildavsky 1984). With its century-old roots and a pre-War charter to pursue the almost sacred goal of protecting the public health, as well as a strong connection to the disease lobbies, and the ideology of a separate and self-regulating biomedical research community (Strickland 1989; 1972), NIH has a deeper and firmer institutional foundation than the newer scientific agencies such as NSF or the regulatory agencies such as EPA. “[A] brilliant jewel in the crown” of the Federal research establishment (Harden 1986:179), NIH seems a likely candidate for an overthrowing bureaucracy.

Nevertheless, the overall budgetary climate, what Walter Oleszek (1989) calls the “politics of fiscal austerity,” had a great deal of influence on congressional-NIH relations. From providing members of Congress with a greater return on their efforts in oversight and programmatic reform than new legislation, to providing an easy lever on any issue that bears on integrity and productivity, tight budgets make for tight politics. Without easy measures of the integrity of science, for example, members of Congress used fiscal integrity as a “metonym” (Turner 1990a) or proxy to help control NIH. But NIH was also retaining some element of privilege compared to other domestic programs in the austere climate. Members of Congress, who also faced other research-related issues such as the AIDS crisis, needed a robust and well-financed biomedical research system, and in order to get it, they had to help it demonstrate its integrity and productivity.
4. Integrity, productivity, and principal-agent theory

The evolution of the principal-agent relationship between Congress and NIH is the concern at the heart of this dissertation. The issues about which this concern will be elaborated are closely associated with the principal-agent logic. These issues are the integrity and the productivity of science. By the integrity of science, I refer to the question of whether scientific research has been performed honestly or whether, in common parlance, the research is fraudulent.47 By the productivity of research, I refer to the question of whether scientific research has contributed to any goal that has previously been articulated, for example, the general pool of knowledge, an agency mission such as the public health, or national technological innovation and economic competitiveness. What follows is a brief paragraph on each issue, and a summary of their association with NIH, government research in general, and principal-agent theory.

Attention to scientific fraud has been a relatively recent phenomenon, although scientific disputes, generally over priority in making discoveries, are as old as science itself (Merton 1973:ch.14).48 There had been well-known frauds such as Piltdown man (Spencer 1990; Weiner 1955) and the midwife toad (Koestler 1971), but popular interest was piqued by the infamous case of the “patchwork mouse” (Hixson 1976) at Sloan-Kettering Institute for Cancer Research in New York in 1973 and by Nobel laureate Peter Medawar’s recollection of the case (Medawar 1979).49 Revelations about the late Sir Cyril Burt, the psychologist, and a spate of cases in the late 1970s and

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47 For now, I will use the common term. Much of chapter IV is dedicated to describing how NIH attempted to arrive on acceptable terminologies and definitions.
48 Some historical research has shown that major figures in the history of science have failed to live up to even ethical standards for research expressed in their own times. Studies of the laboratory notebooks of Robert Millikan, Hans Krebs, Michael Faraday, and Louis Pasteur all show that none of these scientists conformed to a “sterile” description of scientific method and that there were discrepancies between the records of experimentation in private notebooks and public statements (Geison 1993).
49 Piltdown man was a fraudulent anthropological artifact that combined a human skull and an ape jawbone. The midwife toad fraud involved an injection of ink into the heels of toads in an attempt to demonstrate a theory of inheritance. In the patchwork mouse fraud, a researcher painted a white mouse with black ink, initially fooling a review committee into believing that the researcher had conducted a successful skin graft.
the early 1980s involving federally funded scientists triggered both a small publishing industry\(^{50}\) and congressional interest. Although there had been hearings by the House Government Operations Committee on NIH administrative practices in 1960,\(^{51}\) and investigations into forged research data in environmental regulation by the oversight subcommittee of the House Energy and Commerce Committee (U.S. Congress 1984), the first attention to scientific fraud in government grants did not occur until 1981 (U.S. Congress 1981a; 1981b) and it was independent of direct concerns for the integrity of science-based regulation. That is when my story of congressional interest in scientific misconduct begins.

The productivity of science has long been an explicit tension, both within science and between science and its patrons. In the early history of the Royal Society, for example, there was strife between what would now be called basic research and applied or industrial research. In the American context, Tocqueville (1969 [1848]) remarked and historians (e.g., Richard Shryock and I.B. Cohen) have written about an indifference to basic research but an abundance of applied research and invention, although Nathan Reingold (1991:ch.3; 1964) has directly challenged this thesis. Shortly after the early scientists refined their professional rhetoric of "science for science's sake" in the 1870s (Reingold 1991:ch.2; Daniels 1967), members of a special congressional commission on the organization of government science (the Allison Commission) critically suggested that if science was economically productive, then it should be funded by industry; but if it was not, then there was little reason for the government to fund it (Cohen 1980 [1886]).\(^{52}\) Although scientists tried to make themselves useful during World War One—the creation of the National Research

\(^{50}\) The output included: Kohn (1986); Bechtel and Pearson (1985); Ben-Yehuda (1985); Chubin (1985); Klotz (1985); List (1985); Schmaus (1983); Broad and Wade (1982); Bridgstock (1982); Woolf (1981); and Weinstein (1979).

\(^{51}\) See Rettig (1977), Strickland (1972), and II.C.4.c.

\(^{52}\) The Allison Commission is treated in passing in many works, e.g., Manning (1988; 1967); Kevles (1978); Dupree (1957); and even in Bush (1980 [1945]), from the perspective of the scientists. For the congressional perspective, see Guston (1990b).
Council was part of this effort—the military was skeptical of civilian scientists. As the scientific and technological triumphs of World War Two wiped away the Depression-era fears of technological unemployment, enthusiasm for the technological products of science-based innovation became something of a national religion.⁵³ Since World War Two, however, there have been many attempts to discover exactly what the contribution of basic research actually is or has been. Two significant attempts were DOD’s Project Hindsight and NSF’s counter-project, TRACES, but other attempts included the institution of bibliometric analysis at NIH in 1970 (OTA 1986; see II.C.4.d.). None of these earlier attempts seemed to provide policies that averted the economic malaise of the late 1970s, and by 1980, where I begin my account, members of Congress and President Carter were contemplating ways to augment and harness the productivity of research.

The cases of integrity and productivity of science are appropriate for both a focus on NIH and generalization to other aspects of the research enterprise. The cases that attracted congressional attention in 1981 and later in the decade generally involved research that had been funded by NIH. This situation is not unexpected given the share of NIH funding and the proximity of its sponsored research to applications that can affect human well-being. Much of the controversy has also swirled around the particular personalities involved: Representative John Dingell, a powerful committee chairman; David Baltimore, a Nobel laureate; and Bernadine Healy, an outspoken new NIH director. But there have also been publicized cases involving federal funds from many sponsoring or performing agencies including NSF, the Geological Survey, and EPA.⁵⁴ Similarly, although the greatest conflict arose from cases handled by the office established at NIH to handle misconduct, all research agencies developed policies and

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⁵³ Although the atomic bomb raised great apprehensions about the role of science-based technology, the promise of the peaceful uses of atomic energy—whether realistic or not—tempered this apprehension and further demonstrated the enthusiasm.

⁵⁴ NAS (1992:ch.4) provides some information on the distribution of misconduct, including some episodes in other nations such as Great Britain, Germany, India and Australia.
procedures to handle allegations and the White House Office of Science and Technology Policy has been working on a government-wide definition of scientific misconduct. Likewise, the legislative changes directed at increasing scientific productivity were broad-gauge in their approach, applying to universities and to federal laboratories regardless of their disciplinary association. But NIH has been at the forefront of activity in part because the biotechnology industry has been growing in parallel with the increasing demands on federally funded research. It has also been at the forefront of controversy because the public health mission of NIH is remarkably sensitive to issues such as the expectation of technological benefits, the pricing of new technologies, conflicts of interest, and the association to issues of scientific misconduct as noted above.

Because of these changes in the congressional information systems and in the politics of fiscal austerity, there is no necessity to assume that, in the case of scientific integrity for example, there have been any changes in biomedical research that have led to an increase in scientific fraud and misconduct. There are plausible causal accounts to explain any perceived increase in misconduct. Interestingly, these accounts often rely on the idea of an increased demand for scientific productivity, whether measured by the number of publications ("publish or perish") or by increased commercial incentives ("patent or perish"). And these accounts may in fact be correct. But particularly given the discovery of significant irregularities among important historical figures (Geison 1993), the claim that the contemporary environment presents special opportunities for fraud is, I think, difficult to establish. Nevertheless, it is similarly clear that the biomedical research community has become much more oriented toward commercial products for reasons that include, but also for reasons well beyond, the technology policies discussed here.

In a similar vein, the problem of the connection between science and industrial innovation may not reside in a lack of scientific productivity, but may instead reside in
macroeconomic conditions or microeconomic choices. The role of this dissertation is not to assess where, in fact, the fault lies; nor is it to assess whether Congress correctly diagnosed the malady. It is instead to explain how Congress came to understand what it did understand, how it set about to change what it thought it could change, and how far it could effect and control this change.

Despite their rather separate histories, questions of the integrity and productivity of science are related in a very useful way: they are both problems that can be defined by principal-agent or “ideal contracting” theory as “moral hazards.” The problem of moral hazard is the problem that principals face in recognizing that they cannot easily observe the behavior of their agents after the contract takes place, and these agents may actually be using the principal’s resources for rascality or for pursuing goals that are not congruent with the principal’s own.\footnote{The term “moral hazard” is derived from the insurance industry where, for example, insurance against certain hazards like fires actually creates an incentive for wrong-doing such as arson (Arrow 1991).} For the federal funding of scientific research, moral hazard can have at least two meanings: one, that members of Congress cannot tell, for example, whether the experiments that funded scientists report actually occurred and gave the results the scientists claim they did (the integrity of science); and two, that members of Congress cannot tell if the experiments, even when performed with integrity, will produce results that will advance an agency mission, the national economy, and so forth (the productivity of science). Principal-agent theory suggests that to avert these moral hazards, principals will establish various mechanisms to increase the observability of their agents’ actions and assure the congruence of goals.

I argue that as the funding relationships for scientists were established in the postwar period, no moral hazard problem was recognized. Although legislators, bureaucrats and scientists held differing opinions about many aspects of the organization of the federal support for science, belief in the integrity and productivity of science was not a point of conflict. The reward system for science, which is often
thought to operate like an economic free market (Tullock 1966; Polanyi 1962), would regulate the conduct of scientists and produce the best new knowledge to feed innovation. Simply, they all shared a belief that science was a certain way, and that science policy should therefore be a certain way. This shared belief, the social contract for science, precluded participants from envisioning a formal, principal-agent relationship because they shared goals and they shared the means thought important to achieve those goals. In short, what was good for science was deemed good for the country. Such perceived alignment of interests precludes the need for explicit monitoring or incentives (Pratt and Zeckhauser 1991:14-15).

Part of the evolution of the principal-agent dynamic as I describe it, is the substitution of "information" for "ideology." That is, members of Congress discovered, through the information systems at their disposal, that the previously shared idea about science and science policy did not correspond with the reality they understood. I set the words "information" and "ideology" off in quotation marks here because I do not wish to attach evaluative connotations to the two terms. Given a technical definition of information as something that reduces uncertainty, then ideology may be informational as well as data, perhaps even more so. I also set them off because I do not wish to imply that the process is necessarily a "rational" one, although it is a rationalization in the sense of a bureaucratization or institutionalization. But the emphasis on information also suggests something that the oversight literature has not addressed well: the principal-agent dynamic has an intellectual or cognitive underpinning; that is, problems must be see-able in order to be seen. The new

57 McCubbins and Schwartz (1984) provide something like an intellectual or cognitive underpinning, the fire alarm, in which an interested constituency or some other unsystematic mechanism alerts a congressional principal. This mechanism is active in the cases I study, particularly in the misconduct case. But the fire alarm is a very thin intellectual underpinning. It says that something is important to a congressional principal because it is important to somebody else; the criterion of evaluation would then be whether that somebody else is important to the principal. It does not specify how to sort among similarly important claimants, for example: scientists who claim that extant fraud damages science (whistleblowers) or scientists who claim that political interference and irresponsible claims of
intellectual framework allows congressional principals not only to recognize shortcomings in scientific performance, but also to believe that monitoring, incentives, and other mechanisms can help them control the scientific community. Nevertheless, because of the continued flexibility of the boundary negotiations, this control is by no means complete.

5. The social contract for science

This shared belief that I call the social contract for science is not merely an imaginative metaphor created for the purposes of this dissertation. It has an independent existence in the language of both science policymaking and science policy scholarship (see II.B). The standard account of the social contract for science is that it was negotiated in the 1940s, outlined by Vannevar Bush in *Science: The Endless Frontier*, and exemplified by the federal agencies such as NIH that support basic research. As the empirical and historical material in chapter II shows, the social contract for science holds that the federal government will provide resources to the scientific community and allow the scientific community to retain its decisionmaking mechanisms, and in return expects forthcoming but unspecified technological, economic, and other benefits.

This social contract for science has at least two premises: that the scientific community can be allowed its own decisionmaking mechanisms; and that this organization of science can provide the expected benefits. The two premises correspond to the two issues, the integrity and the productivity of science, whose scrutiny by Congress I present.\(^{58}\) The social contract for science is a shorthand or argot for the postwar relationship in which goals and important means to those goals were held in common. That is, it is a baseline against which to measure the evolution

\(^{58}\) These are premises in that a claim for a social contract for science is not sustainable if the integrity and productivity of science are not sustainable.
of the principal-agent dynamic. Recurrent crises in science policy have brought the
premises of the social contract for science under occasional scrutiny by Congress. But
it was not until it was clear to members of Congress that these premises were
untenable, that they began to institutionalize the monitoring of and inducements for
scientific performance. It is this process of clarification and institutionalization—the
creation of the principal-agent dynamic that is also the end of the social contract for
science—that I explain here. To do so, I examine the creation of two offices at NIH, the
Office of Scientific Integrity and the Office of Technology Transfer, which monitor
scientific integrity and apply incentives to increase scientific productivity, respectively.

As described above, NIH is not the only research agency affected by issues of
integrity and productivity, although it is the only one I study. Nevertheless, the social
contract for science purports to address other research communities—indeed the entire
scientific community—in addition to the biomedical one. I do not want to provide more
than an operational definition of “the scientific community” because, as I describe in
chapter IV, defining the scientific community, its boundaries, and its interests is part of
the intellectual problem that needs to be answered by observation rather than by
definition. Generally, by scientific community, I mean that group of persons who
actually practice (whatever gets defined as) science. In parallel to the biomedical
definition, the scientific community could be that group of scientists who are or could
be supported by government agencies in any discipline, or their colleagues who interact

59 In the U.S. in 1989, there were about 900,000 employed Ph.D. scientists and engineers (NSB
1991:294). Not all of this number are engaged in R&D activities, but there are more non-Ph.D.s
involved in R&D than Ph.D.s not involved (see NSB 1991:301). Of the employed Ph.D.s, fully one-
half are employed by industry, one quarter by the federal government, and one sixth by educational
institutions. For the almost 116,000 Ph.D. life scientists, however, roughly two-thirds are employed
by educational institutions, one-fifth by industry, and less than one-twelfth by the federal government.
I should also spell out that when I use the word “science” I mean both science as a process and science
as a body of knowledge. I include both aspects because I believe them to be interrelated to a great
degree. Some, perhaps, would go so far as to say that the latter aspect is entirely dependent on the
former aspect. That is, something can be part of the body of scientific knowledge if and only if it is
produced by the scientific method. This is, in fact, the perspective of those who defend science as an
autonomous, market-like phenomenon (e.g., Polanyi 1962, and II.C.3.c) As with the scientific
community, the interesting aspect of science is in the ways it gets defined in practice.
with them and may be influenced by federal research policy. I try to use the phrase only when I make what I consider to be a reasonable generalization from the biomedical community.

The definitions of both the disciplinary "biomedical community" and the encompassing "scientific community" are centered on the identification of scientists performing research because the demography of the community seems both the most important and least problematic way of operationalizing a definition. This emphasis on demography would, for example, correspond to the emphasis on peer review as a scientific mechanism (see IV.B.2). It also would seem to be practical in the sense that it would at least specify who the stake-holders are in defining the communities in practice. No one who falls outside the definition of the biomedical research community as stated above would have a great stake in its definition. Some might argue that the definitions are too broad, including not only the actual scientists performing the research but also "their colleagues who interact with them and may be influenced by federal research policy." A broad vision of collegial relations and the influence of federal policy could include a vast number of people, from patients at clinical trials, to AIDS activists, to independent citizens sitting on local ethics boards. This expansiveness is intentional. My analysis does not depend on it, but my conclusions about the increasing linkages between politics and science and the politics internal to science, I hope, will help demonstrate that this expansiveness is legitimate. And as suggested above, the definition highlights the fact that politics has some role in defining the community. It is not only an expansive definition of the scientific community, but it is an expansive definition of politics.\footnote{Latour (1988) describes how scientists such as Louis Pasteur had to expand their laboratories in order pursue their goals, and Price (1986) describes the exponential growth of the modern scientific enterprise. Tocqueville (1969 [1848]) describes how democratic politics (equality) expands to all segments of society. Dahl (1985) provides a normative argument for the expansion of democratic politics to any sphere where political power is exercised--in his example, economics.}
6. The boundary of politics and science

The social contract for science also provides an ideology or, in Geertz's (1973) sense, a map of social reality, according to which participants navigate through science policy. This ideological map has been a substitute or proxy for other information (Moe 1984; Downs 1957) that could have enhanced congressional control of scientific research. As members of Congress gathered new information about the political world of science—information pertaining to the integrity and productivity of science—they began to redraft the map and, consequently, rewrite the social contract for science.

Redrafting the map and rewriting the contract also means renegotiating the sharp boundary between politics and science that had been established. It is no longer sufficient for scholars or politicians to assume that science is analytically distinct from nonscience: that scientific processes are separate from legal processes; or that scientific tasks are distinguished from administrative or economic tasks. It is also no longer sufficient to assume that the scientific community is composed solely of people in white laboratory coats. Scholars should rather investigate empirically whether it displays some of the expansiveness suggested above. In this more expansive community, and in this reciprocallly expansive politics, scientists and nonscientists negotiate over boundaries that involve matters as traditionally "scientific" as the definition and adjudication of cases of scientific misconduct, and the drafting of research plans and the selecting of research partners. It is in the context of these renegotiations at the newly created Office of Scientific Integrity and Office of Technology Transfer that further limits of congressional control can be found. Although the offices were inspired by congressional action, the details of administration and implementation were, in these cases, beyond congressional influence, although they need not have been.

In order to satisfy the demands of explaining the boundary work that occurs in NIH, as well as the demands of explaining the creation of the new offices which provide the institutional context for this boundary work, I try to provide something like
a "principal-agent ethnography" of these new institutions and their work in monitoring and applying incentives. To this end, I conducted sixty-nine semi-structured interviews with current and former congressional staff members and bureaucrats involved in the creation and implementation of the institutions, monitoring, and incentives. These interviews provide a wealth of accounts and perceptions of the new boundary work and a biomedical enterprise increasingly embedded in social relations.61

As described above (I.A.2.d), Karl Polanyi's historical account of the embeddedness of economics in society—and the dual political maneuver of constraining and extending the economic market—can be seen as a model for explaining the embeddedness of science in society. The boundary work performed in the cases of integrity and productivity in science show this same dual maneuver of constraining and extending science as an integral and productive enterprise. This maneuver with regard to the integrity and productivity of science is particularly important because of the role played by science in legitimating liberal-democratic values. As described by Ezrahi (1990), science plays two such roles: combating the threat of extreme individualism and disorder by providing a model for the peaceful, coherent aggregation of individual viewpoints into a cumulative, progressive enterprise; and combating the threat of unconstrained public action by clarifying certain relationships—between cause and effect, between governor and governed—that serve public values of instrumentality and accountability. The scientific community is, in the first sense, a model polity for the liberal-democracy; and in the second sense, science allows governors to serve the governed productively and the governed to witness, and change if necessary, the governors.62

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61 Although there may be problems in relying on interviews with participants, because of their interest in placing their own interpretation on events, I found a remarkable consistency of interpretation among my interviewees across institutional boundaries, and among current and former participants.
62 Ezrahi (1990) argues that science, facts, and the scientists who represent them become even more in the American (as compared to the English and Continental) system because American law recognizes a plural and not a unitary sovereign. The plurality of sovereigns translates into a flexibility of rules.
If its integrity and productivity are called into question, science has a harder time filling these roles. A scientific community rife with misconduct or conflicts of interest cannot serve as a model polity. It cannot provide an example for liberal-democratic politics of credibility, objectivity, and impartiality. It cannot demonstrate that authoritative public action really can assert itself above individual "ambition, profit, or lust." Neither can bad science serve its instrumental ends, nor can self-indulgent, unproductive scientific research assist the governors in serving the governed. When the governed expect instrumental action in the management of the economy, in providing for the public health, or in performing any other public realm, and do not find their expectations realized, not only does science lose credibility, but so do the governors administering these actions.

Ezrahi argues that trends in the natural and social sciences--e.g., quantum uncertainty and Arrovian instability--have undermined the ability of science to perform these roles, leading to a more volatile relationship between "postmodern science and postmodern politics" (Ezrahi 1990:ch.12). Although Ezrahi is sanguine about the future of American democracy under such conditions, he acknowledges the possibility that postmodern politics, without the ideological model or clarifying lens of science, could slide into unbridled Fascism or stultifying authoritarianism. Gerald Holton (1993), physicist and historian of science, feels the possibility of Fascism in the demise of objective science more palpably than Ezrahi. Holton even goes so far as to link thematically pre-Nazi Romantic philosophers, Vaclav Havel, and Representative Brown as misguided threats to the stability of the nexus of science and democracy.

Bruno Latour, a constructivist sociologist of science, is also one of Holton's targets. Latour (1991) has a more optimistic vision than Holton or even Ezrahi of what he calls the "nonmodern world" where science and politics intermingle freely. Latour

Such flexible rules cannot themselves compel agreement or constrain disorder, but they provide an arena in which neutral authorities, experts, and facts can become the seeds of consensus.
(1991:7) views the separation of science and politics that had prevailed since Hobbes as a “shameful Yalta Pact” that divided the world inappropriately into science and politics, facts and opinions, persons and things. Without this highly artificial division, Latour is hopeful that science and politics can actually collaborate to solve problems like crime and environmental degradation that neither science nor politics seemed capable of solving alone, and that perhaps the very separation of science and politics had contributed to causing. Latour (1991:18) defends this perspective, rather than the separatist attitude of Holton, as the true extension of the Enlightenment, as he moves from the political construction of “no taxation without representation” to the political/scientific mix of “[n]o innovation without representation.”

With the congressional recognition of the politics internal to science, and its subsequent intrusion into monitoring scientific integrity and providing incentives for scientific productivity, the possibility of the adjudication of scientific misconduct and the innovation of biomedical technologies with representation now exists. This dissertation will explain how this possibility came about, by explaining the development of the principal-agent relationship between Congress and NIH. It will explain what kind of representation is involved in the new models of adjudication and innovation being put in practice, by explaining the negotiation of a new social contract for science. And it will speculate on the meaning of representation in these areas, by further considering the pliable boundary between politics and science, and the character of the relationship between democracy and science.

D. Conclusion: plan of the dissertation

The dissertation is organized into a historical chapter, two parallel case studies, and a conclusion. The historical chapter (II) is primarily concerned with the social contract for science, and its main purpose is to establish a baseline from which to understand the changes I describe in the case studies. As shown by my interviews, by
a synthesis of the pertinent literature, and by the history of the creation of the modern
U.S. research enterprise in the immediate postwar period, this baseline is the social
contract for science. It relied on a then-unproblematic assumption of the integrity and
the productivity of science. The chapter not only provides a historical rationale for the
contract, but it also outlines its intellectual rationale. The chapter further tests the social
contract for science by examining various episodes in the history of congressional
oversight of or intervention into biomedical research at NIH.

Chapters III and IV comprise the pair of the parallel case studies that addresses
the integrity of scientific research, and chapters V and VI comprise the pair addressing
the productivity of research. In each pair, the first chapter describes the discovery of
the issue by Congress, and the instruments used in the subsequent attempts to achieve
congressional goals. Both cases describe how the information acquired by Congress
seemed to indicate that the premises of the social contract for science were not, in fact,
being upheld. The second chapter of each pair describes the negotiations that occur in
the bureaucracy in the wake of congressional action, as scientists and nonscientists
renegotiated the boundary between politics that had been transgressed by the
congressional action described in the first chapter of each pair. The fact of these
boundary negotiations and their outcomes help determine the extent of congressional
control of the bureaucratic function involved.

Chapter III explains how congressional oversight without legislation can lead to
bureaucratic action. Beginning with a fuller description of principal-agent theory, the
chapter examines how the issue of scientific misconduct in NIH-funded research came
to the attention of congressional committees first in 1981 and again several years later.
The chapter shows Representative John Dingell, in particular, making an incisive,
coherent critique of scientific claims to special privilege and, finding these claims
wanting, proposed threatening legislation. In an attempt to “pre-empt” congressional
action and to preserve the social contract for science against congressional intervention,
the director of NIH created a new office to monitor scientific integrity. This pre-
emption is distinguishable from other types of more dependent responses by
bureaucratic agents.

Chapter IV demonstrates how the boundary between politics and science is
constructed by competing interests in an unpredictable way. The chapter provides an
elaboration of this concept of boundary work and describes four “explanatory
repertoires” or rationales for scientific misconduct upon which a definition of scientific
misconduct could be based. The chapter also elaborates two procedural models by
which scientific misconduct cases could be adjudicated. In both cases, I argue, the
biomedical community drove decisionmaking in the direction that, I think surprisingly,
restricted the dominance of scientists and their exercise of scientific judgment in the
identification and resolution of misconduct allegations. Nevertheless, in both cases the
ability of the biomedical community to drive decisionmaking was permitted by
instances of political fortune, i.e., constraints on congressional intervention due to
unrelated issues.

In chapter V, I show how Congress and, in this case, the President, perceived
that the productivity of science was questionable given the state of the economy in the
late 1970s and attempted to intervene. The resulting legislation established a principal-
agent relationship, with scientists who could demonstrate the productivity of their
research through patents, for example, being rewarded by allowing them to share in
exclusive licenses based on those patents. But because the application of such formal
incentives intervened in the reward system of science, the legislation undermined the
social contract for science and blurred the boundary between politics and science.

As the second of the pair, chapter VI describes the boundary work that occurred
at NIH to implement the legislation and increase the productivity of research. An Office
of Technology Transfer at NIH coordinates the collaboration between scientists and
nonscientists to produce indicators of productivity such as patents and licensing
income. In the course of this collaboration, nonscientists participate in decisions and judgments that had previously been made exclusively by scientists, for example, deciding what a discovery is, in order to patent it, or deciding with which scientists to cooperate, in order to assure fair access to government-sponsored opportunities.

In the final chapter (VII), I draw conclusions on the three nested levels of analysis I described earlier: the central question of the particular principal-agent dynamic between Congress and NIH and its implications for congressional control and bureaucratic independence; the broader question of the social contract for science and the change in the relationship between government and science in the U.S. over the last two generations; and the broadest question of the nature of the interaction between politics, particularly democratic politics, and science.
Chapter II. The Social Contract for Science

A. Introduction

Standards of scholarship suggest that a citation is not necessary for an item found in five or more sources, because that level of representation constitutes "general knowledge." By this standard, the social contract for science is part of general knowledge, as any amount of time in the science policy circles of Washington, D.C. confirms. But just because a phrase is used commonly does not mean that it is used correctly or with full cognizance of its historical and other connotations. I do not propose a "Shandean postscript" to centuries of metaphor in literature and art (e.g., Merton 1965). However, since the analysis of contemporary science policy--both in this dissertation and elsewhere--relies on it, I think that teasing out as full and subtle a sense of the social contract as possible is an appropriate task. This chapter will therefore be dedicated to explicating the social contract for science, and in that way providing a framework for the analysis to follow.

The social contract for science is not only a framework, but it is also a baseline against which to compare the cases of integrity and productivity in science that I discuss in later chapters. Understanding the social contract for science as a tacit agreement among science policymakers and scientists about the nature of science and therefore the nature of science policy is crucial to understanding the significance of the changes in the 1980s. These changes occur at each of the three levels of analysis I described in the previous chapter (I.A.2): in the specific relationship between Congress and NIH; in the

\[1\] In On the Shoulders of Giants, Merton romps through the ages tracing Sir Isaac Newton's famous phrase, from user to user, in various media. Merton's interest in the history of the phrase is in part related to his scholarly interest in plagiarism, both intentional and unintentional ("cryptomnesia"). See the discussion of the Merton repertoire in chapter IV.C.1.b.

\[2\] Although I will not focus on this issue, the language used by policy participants often plays a significant role in how policy problems are framed, argued, and resolved. See, for example, Nelson (1984) and Edelman (1977).
terms of the social contract for science; and in the broader relationship between politics and science.

The first section of this chapter shows how the usage of the social contract for science may have evolved and how I found it used by my interview subjects on congressional staffs and in the scientific bureaucracy. It also delineates the descriptive and analytical power of the social contract for science language that helps make it such a common and useful construction. The second section offers a history of the formative period of modern American science policy, the few years from when policymakers began to plan for demobilization from World War Two until the creation of the National Science Foundation in 1950. In this period, the baseline consensus on the social contract for science was drawn. The final section of the chapter briefly examines episodes in the history of science policy since 1950 that have challenged the social contract for science. These episodes show a continuing attempt by politicians and particularly by Congress to engage the scientific community more closely, but they also show that politicians discovered neither the appropriate issues nor the appropriate instruments for departing from the social contract for science.

B. Why a social contract?

1. Whence the social contract for science?

The origins of the usage, the "social contract for science," are still obscure. I have developed two plausible hypotheses, neither or both of which may be correct. The first is that the social contract for science is derived from the idea of a social contract for scientists. This idea, concerning a tacit agreement among scientists rather than one between scientists and society, was propounded by the English chemist and philosopher Michael Polanyi (1964 [1946]) in the immediate postwar period and later adopted by American sociologist Harriet Zuckerman (1984; 1977). The idea of such an agreement or fellowship among scientists is about as old as the idea of the social
contract itself, offered by the 17th historian Thomas Sprat to describe the Royal Society as “a union of eyes” (see Ezrahi 1980).

It is possible that either through conscious or unconscious extrapolation, the one social contract evolved into the other, presumably through the idea of science as a profession that can have a social contract with society, as other professions like medicine, law, or the clergy. June Goodfield (1977:209), for example, provides an early example of the social contract language in just this sense in her examination of scientific responsibility and the recombinant DNA debates, which “could be the starting point for a new social contract both within the professions and in its external relations.” Goodfield (1977:79) does differentiate between science as a profession and other professions with respect to the social contract, however. Other professions have formal external accountability; whereas, “an external accountability is missing from the social contract between the scientific profession and society.” That lack, she explained, was central to the recombinant DNA controversy (see II.C.4.e).

An exchange between Zuckerman and philosopher Warren Schmaus on the question of norms and deviance in science elucidates the idea of a social contract between society and the profession of science. Zuckerman (1977:113) asserts that the “institution of science involves an implicit social contract between scientists so that each can depend upon the trustworthiness of the rest....[T]he entire cognitive system of science is rooted in the moral integrity of individual scientists.” Schmaus (1983:15) replies that if “the norms of science [are to be considered] as contractual obligations that result from accepting a position as a scientist...[and] are to be considered special moral rules,...it is helpful to think of scientists as participants in a larger social contract among all members of society.” But Schmaus (1983:16) does not complete the transformation of the social contract for scientists into a “larger social contract” because he believes that the potential contractors “would immediately confront two [intractable] problems: (1) defining precisely the role of the scientist, and (2) agreeing upon the
importance for society of carrying out the functions attached to that role, whatever they
may be.” Although I am in sympathy with Schmaus’s critique, and indeed believe that
it ultimately may be correct, it is nevertheless the case that his critique did not prevent
others from transforming a social contract for scientists into a social contract for
science.3

The second hypothesis also relies on an ambiguous extrapolation or evolution.

between political processes and institutions and scientific ones. Price describes his
view in counterpoint to two extremes:

The lawyers and accountants and reformers who fear the predatory
private interests argue that private institutions must not be allowed to
profit on government grants. Suspicious capitalists and scientists
who are jealous of their academic freedom say that he who pays the
piper calls the tune—that the government, whenever it puts funds into
research, is bound to destroy the liberty and initiative of those who
receive the money.

These two points of view, while superficially at opposite poles, are
fundamentally in agreement. They both hold that the public interest is
necessarily opposed to private interests, and that government cannot
ever provide funds without destroying the independence of the
institutions that receive them. But this is only another version of the
idea that power is indivisible—the old idea that was the core of the theory
of sovereignty as Austin explained it; the idea that is the basis of the

Price sees both perspectives as mistaken because of his belief that the United States is
not a nation based on this “unitary” vision of power and organization. Instead of
revolving around a central and unitary sovereign, the American system developed a
layered form of sovereignty called federalism, in which a federal government shared
political power with its constituent states. By inventing a grant and contract system that
“gives support to scientific institutions that yet retain their basic independence,” “the
United States has improvised a new kind of federalism for the conduct of research”
(Price 1954:67-68). At the heart of this new system, wrote Price, were the grants let

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3 One could attempt to address Schmaus’s claims head-on by attempting to derive a social contract for
by the Public Health Service (PHS) and the National Science Foundation (NSF) to universities and other research laboratories to perform science "for its own sake," and contracts from such military agencies as the Office of Naval Research (ONR) that amounted to the same thing. Military contracts, for example, might not even be let for each project; rather, one "master contract" would be negotiated with each institution. "The 'master contract' is the basic charter of the new federal relationship" (Price 1954:70). Price's focus on the mechanism of the contract and on the freedom retained by institutions despite the contract system may have been extrapolated into a social contract for science.

The first references to the overall relationship--rather than to its characteristic parts--as a contract that I have come across are related to the period in the early and middle 1970s when advances in molecular biology attracted great public concern about safety and ethical issues. Goodfield cast these concerns directly in the language of a social contract with the profession of science. More general references were contained in the essays collected by the American Academy of Arts and Sciences in the *Limits of Scientific Inquiry* volume (Holton and Morison 1979). In the letter sent to solicit participation in the original American Academy Advisory Group, organizers Gerald Holton and Robert S. Morison quoted the words of Walter Rosenblith that "scientists and scholars have long had a bargain with society by which they have produced ideas and devices with few constraints, but that now this bargain is in danger of breaking

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4 For a history of the NSF and its grants programs, see Wilson (1983) and England (1982), among others. For a history of the PHS research and grants programs, particularly those at the National Institutes of Health (NIH), see Strickland (1989) for the postwar period, Harden (1986) for the prewar period, and Fox (1987) and Fredrickson (1993 forthcoming) for the transition. Sapolsky (1990) provides a detailed history of the creation of ONR and its scientific programs. Also see II.C below.

5 See also: Chubin (1990:160), "These investments [in research] are contractual agreements; they specify forms of accountability and ground rules for negotiating the contract that binds science to society;" and Hackett (1990:275), "[U]nderstanding the roots of the problem in resource relations and the quest for legitimation also suggests a remedial strategy [to the problem of funding independent institutions]: change the pattern of university support and intervene, directly and consciously, to modify the 'contract' between the university and its patrons."

6 Price (1961) has also referred to the relationship as a "concordat." And of course, Price (1965:20n.23) popularized and systematized the notion of science as an "estate"--skin to the estates of the nobility, the clergy, and the burgesses--although he recognizes this nomenclature as an older one, coined by Arthur D. Little in the 1920s.
down or in need of revision” (quoted by Graham 1979:1). Contributors to the *Limits* volume variously refer to the relationship as: “an implicit bargain” (Price 1979:25) or a “tacit bargain” (Price 1979:79-80); an “implicit contract” (Culliton 1979:152); “this postwar contract” (Nelkin 1979:193); an “old bargain between science and society” (Holton 1979:236); and, quoting Price (1961), a “concordat” (several authors). The referent for these descriptions was invariably the postwar relationship, and for many the famous report written by Vannevar Bush, *Science: The Endless Frontier*.

Harvey Brooks (1985), responding in part to the impulses that caused the observers of the recombinant DNA debate to speculate on the demise of the “bargain,” helped to fix the social contract language:

Despite much debate, and much apprehension and cries of alarm from the scientific community, it appears to me that the basis [sic] outlines of the ‘social contract’ between science and government proposed in Bush’s famous report to President Roosevelt, *Science the Endless Frontier*, have remained more or less intact, and are still broadly accepted by public and politicians. Reality has changed much less than rhetoric. True, there has been some intrusion of government into the management of the extramural scientific enterprise, but after many threatening draft regulations and bursts of threatening rhetoric in the Congress, the end result has not been much real change, certainly not as great as the alarmists feared.

Brooks (1990b:12) would later polish his definition of the social contract:

Science was to be supported largely through grants and contracts to private institutions, leaving “internal control of policy, personnel, and the method and scope of research largely to the institutions themselves” [citation omitted]. Overall, this suggested a kind of social contract between the scientific community and the American people as represented by the federal government (including the Congress). The social contract promised widely diffused benefits to society and the economy in return for according an unusual degree of intellectual autonomy and internal self-governance to the recipients of federal support.

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7 Nelkin cites Smith (1965) and Kevles (1977) as describing the creation of “this postwar contract,” but I have not found any such language in either source.
These representations do not exhaust references to the social contract for science in science policy literature, but they do provide a basic sense of the shared attitude among scholars of science policy—a shared attitude indicating that some more intersubjective phenomenon is at stake.

2. Social contract for science among policy participants

The attitude is also shared by policymakers and not merely the academic analysts of science policy, although particularly in the generations of Price and Brooks, there was a substantial overlap of these populations. Many bureaucrats and congressional staff I interviewed had heard of or even use the phrase, the social contract for science, although few were willing to hazard a guess as to its precise meaning. For those who would, however, its meaning was plain. A professional staff member with a subcommittee of the House Science Committee said, “The deal was, after the War, ‘we [the government] will give you [the scientists] money and leave it in your capable hands to get the most amount of progress.’” A former deputy director for extramural research at the National Institutes of Health (NIH) had heard of the social contract for science and said that, “in its crudest terms, the government will give scientists money to do what they want to do; in return, scientists will try to work on things that are going to be good for...the people whose money they’re spending. It is a contract which is relatively new [and] certainly did not exist before the Second World War.” A former House Science staffer said pithily, “science gets the pleasure of serving society by doing what it wants with other people’s money.” Former NIH director James Wyngaarden explained his view in more detail:

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8 This section relies on my interpretation of interviews of sixty-nine current and former bureaucrats and congressional staff during the fall and winter of 1991-1992 and during January 1993.
9 Interview with professional staff member at the House Science, Space and Technology Committee, Washington, DC, January 9, 1992.
10 Interview with Katherine Bick, former Deputy Director for Extramural Research at the National Institutes of Health, Washington, DC, December 27, 1991.
11 Interview with Greg Simon, former professional staff member at the House Science, Space and Technology Committee, Washington, DC, December 20, 1991.
I think it runs something like this: that there are many views as to why the government supports science. I...would like to think that [the government supports] science for science’s sake, and that they’re doing the Lord’s work, and that it’s of such great interest to society that society should give them the money and move them along, and never tell them what to do. On the other hand, in Washington, it’s clear that with some exceptions, that the government--Congress [and the] White House--looks upon the support of scientists [and believes] that science gets done for [its] utility. Now they all have to recognize that you have to give fundamental scientists a lot of freedom because there’s a long time-frame with what they do. Nevertheless, it’s supported with the firm belief that it will be useful to society in time.\(^\text{12}\)

Several of my interview subjects who had not heard the phrase readily adopted it into their narratives. A former counsel for the Department of Health and Human Services (DHHS) had not heard the term but, when asked for his \textit{a priori} theory of what it might mean, speculated that it contained some element of \textit{quid pro quo}, probably built around the idea of some external body like a government agreeing to foster an environment for science, in exchange for an agreement from the scientific community that it would carry out research in an honest and appropriate manner.\(^\text{13}\)

Another former NIH professional who had not heard of the social contract for science suspected that the idea was not too distant from the concept behind research grants, which are not like procurement contracts.\(^\text{14}\)

3. Descriptive and analytic power

As these brief discussions of its possible evolution and usage may suggest, the terminology of the social contract for science displays some of the descriptively and analytically important themes in the relationship between the federal government and scientific community in the United States. In this section, I want to elaborate briefly on

\(^{12}\) Interview with Dr. James B. Wyngaarden, former director, NIH, Washington, DC, December 2, 1991.

\(^{13}\) Interview with Robert Charrow, former principal deputy general counsel, DHHS, Washington, DC, January 3, 1992.

\(^{14}\) Interview with Janet Newburgh, former institutional liaison officer, NIH, Bethesda, MD, December 12, 1991. As suggested in I.A.2.c., participants are apt to contrast the grant system with both the procurement system and with entitlements.
four of the most important themes. Each will reoccur, like an *obligato*, in the general discussion, because they are integral to the idea of a social contract for science.

a. science as a public good

A basic aspect of the contractarian idea of the government-science relationship involves the provision of the public good of research by the scientific community. Versions of the public good argument—that the private sector will underinvest in scientific research because it is difficult for private sponsors of research to appropriate the return on their investment—have been used in science policy in the United States from the beginning of the Republic. The specification of patents and copyrights in the U.S. Constitution (Article I, section 8) is an attempt to address just this problem by offering protective monopolies to creators and inventors in order for them to realize the returns on their inventiveness more easily.\(^{15}\) By the 1880s, the difficulty of private support for science became part of the increasingly successful rationale for the expanding federal role in funding science in both federal bureaus and the nascent research universities (Cohen 1980 [1886]; Miller 1970). Although corporations and private philanthropies began to invest larger amounts in scientific research by the turn of the century and particularly after World War I, much of these funds dried up during the Great Depression, re-emphasizing the unreliability of private support (Kevles 1977; Weiner 1970).

Throughout the post-World War II era, advocates of federal leadership in basic scientific research have underscored the federal obligation in the provision of a public good. Analysts have distinguished between public productive goods and public consumption goods (e.g., Rottenberg 1968). As a public productive good, science is an investment for future payoff, and because it uses current resources it may or may not

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\(^{15}\) There are two competing rationales for the patent system, the public interest model and the economic model. The former places as the primary goal of the patent system benefit to the public, and relegates as secondary and merely a means to that end the reward of inventors. The latter focuses on the patent system as a provider of incentives for inventors to engage in innovative activity—research and development (Ackiron 1991).
be as beneficial an investment as other uses of those resources. As a public
cconsumption good, science is an investment for culture akin to other public investments
in museums, fountains, and parks. With the growing investment by government in
science, arguments justifying or supporting the continued expansion have tended to rely
on findings that the science as a productive investment is uniquely profitable (Lederman
1991; Mansfield 1991). Current arguments for an expanded federal role in the creation
and dissemination of new technologies use the public good argument to justify the
federal role, but also to constrain it: some investments in technology are difficult for a
single firm to appropriate, so there is a role for the government; but government should
be limited to only those technologies that cannot be appropriated (Alic, et al. 1992).

The science-as-investment argument also involves distributional questions with
generational overtones that resonate with social contract decisions. By entering into a
social contract, even abstract Rawlsian individuals--unsure of their temporal location
behind the veil of ignorance--must consider questions of generational equity (Rawls
1971). That is, they must plan for a well-ordered society over the long term,
understanding that the decisions made affect the quality of life of descendants as well as
contemporaries. Similarly, a social contract for science commits money, people, and
material in the expectation of a return on the investment for a future generation. Future generations will not only reap the benefits from current investments in research,
but they will also bear any social, environmental, and other costs of research
applications as well. Therefore, these questions of generational justice are embedded
within the decisions to fund scientific research in particular ways. Especially in an era
of severe budgetary strain and extreme concern for the positive and negative

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16 For example, does the potential for commercial products derived from the space effort justify investment in rockets rather than in cities?
17 Is the scale of the public investment in science as a consumptive activity appropriate compared to other public investments?
18 "Generation" is probably the right time-frame. The average time to market for a new pharmaceutical, for example, is on the order of twelve years. There have been reports, however, that the time to market for various new technologies is shrinking.
consequences of technological change, generational equity in science funding and technological development is a vital question.

b. accountability and autonomy

In addition to the provision of public goods, the social contract concerns the balance of responsibilities and values between the government and the governed. With respect to science, this balance coordinates the values of accountability and autonomy between an elected government on one hand and a professional community and independent institutions, such as universities, on the other. It is not only the case that the federal government provides a public good, but the government does not itself conduct the bulk of research. It instead delegates the conduct of research to the professional community in universities and other research institutions. Such delegations are neither gifts nor procurements, but are grants or contracts with specific terms and conditions. As Price (1954) argues, through research grants and contracts, the government (with the cooperation of the research community) instituted a new type of federalism that attempted to perpetuate both government-sponsored research and independent institutions. The social contract for science is synectochal for these literal contractual relationships.

Furthermore, as an analytical framework, the social contract corresponds to a more detailed tool for the empirical analysis of such delegatory relationships. This concept has already been partially introduced to science studies by philosopher Stephen Turner, who writes of the continuing patronage relationship between the federal government and the scientific community: “The general ‘contract’ between the

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19 The federal government funded about 61% of all basic research in 1991, but conducted only about 12%. Similarly, the federal government funded about 35% of all applied research in 1991, but conducted only about 12% (NSB 1991:93). About 12% of all NIH research and development funds were spent on the intramural program in 1991 (AAAS 1993:163-64).

20 In a recent court case in which a federally funded researcher's property interest in his grant was at issue (for due process rights), the judge held that “The grant awards made to the Board of Regents are not made for [the researcher’s] benefit, but for the benefit of the public that may enjoy the fruits of his research.” See Abbs v. Sullivan, U.S. District Court for the Western District of Wisconsin, 90-C-470-C, December 20, 1990.
scientific community and its primary patron, the state, and the individual relations of patronage embodied, for example, in particular grants or particular bureaucrats’ personal relations to scientists are both uncheckable” (Turner 1990a:189). Political science has adopted a more formal way of expressing the patronage relationship, principal-agent theory, which uses a contractual perspective on organizational relationships—like that between a congressional committee and a bureau—to model the behavior of the patron (principal) and the patronized (agent) (Moe 1984). Turner’s “uncheckable” relationship is the problem of moral hazard in the principal-agent dynamic. To the extent that the social contract for science reflects the delegation of the research task from Congress to public and private research agencies, the social contract and principal-agent theory are useful and related models.

In my analysis of misconduct in science and technology transfer, I will use principal-agent theory to clarify the relationships involved. The theory predicts that principals will use explicit mechanisms or techniques to control their agents. In contrast, the social contract for science maintains that the political community tacitly accepts the independent, self-regulating nature of the scientific community and the unspecified nature of the future payoff from research. Changes in the relationship that apply explicit techniques of control would therefore indicate an erosion of the social contract for science.

c. the social contract for scientists

Not only does the social contract describe the relationship between the scientific community and the polity, but it may also be useful in describing relationships within the scientific community—a social contract for scientists. As noted above, the work of Michael Polanyi (1964 [1946]; 1962) and Harriet Zuckerman (1984; 1977) describe the

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21 Also see Tullock (1966:26) about the “very difficult problems of supervision” that arise under conditions in which researchers are hired by an interested party. Tullock believes that these problems of supervision are not “particularly distinctive to research,” but he does not consider the possibility that the ideology of autonomous science can restrain supervision.

22 This point is important and I will revisit in the final section of this chapter and in the following chapter.
relationship among scientists as a social contract under which scientists agree to obey
the tacit rules of knowledge production. Polanyi (1964 [1946]:16) interprets this
agreement as a Rousseauan type of social contract on which the freedom and ultimate
success of science hinge: "The Republic of Science realises the ideal of Rousseau, of a
community in which each is a equal partner in a General Will." As Rousseauan
citizens, scientists submit to a General Will represented by scientific opinion. "This
absolute submission leaves each free" but each is also obliged and devoted to the
"ideals of scientific work" (Polanyi 1964 [1946]:64).

Zuckerman's (1977:113) view of "an implicit social contract [among] scientists"
reflects a strain of thought in the sociology and philosophy of professions that
generalizes a "tacit social contract among professionals" to justify norms of
professional conduct (e.g., Bayles 1983).23 Bayles (1983) does not believe that the
social contract model can sustain professional norms because "[f]irst, it presupposes a
nonrelativist norm, namely, that contracts should be adhered to. Second, it would not
justify norms concerning conduct towards clients and other persons who are not
members of the profession and thus not parties to the contract." Schmaus (1983)
provides a similar criticism (see II.B.1 above). However, Zuckerman's (1977)
argument that the "profession" of science lacks a clientele would render moot Bayles'
second criticism.24 Zuckerman (1984:12) also argues, counter to the first criticism,

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23 In this sense, the social contract for science may not be a unique phenomenon; there could be social
contracts between society and any professional specialty that could display a social contract among its
members. But the relationship between society and other professional groups is at once more formal
and less formal. For example, physicians are licensed, attorneys examined, and accountants certified by
government boards in cooperation with professional advisors. There is no similar certifying of
scientists. Yet some scientists are chosen by their own peers, who are themselves selected by the
government and in concert with professional advisors, to be supported by the state in their professional
activities. The claim by scientists for a social contract with the polity seems made in a more absolute
way: whereas, other professional groups can be self-regulating through an internal social contract; the
scientific community claims that it must be, in order to satisfy the rigors of truth production.

24 It may, however, be the case--and I believe the social contract for science makes this case apparent--
is that the polity is in fact the client for science, or at least for basic research. In the U.S., the
government funds the greatest share of basic research and subsidizes through tax credits and non-profit
status the basic research in the other sectors of the economy. As Ezrahi (1990) explains, a liberal-
democratic polity gets a good deal on this investment even beyond the instrumental returns in the form
of technologies and medicines. As described in the introduction (I.C.6), science provides ideological
support for liberal-democratic politics by providing a model of consensus-building and integration
that "it is the pragmatic element, rather than its moral quality, that I would emphasize in the notion of the social contract in science." In other words, no prior, nonrelativist norm of contractualism is required because scientists will enlist out of pragmatism or self-interest. Organs of the scientific community voice this pragmatic aspect of the contract for scientists, for example, in a style manual for publishing in biomedical research:

Scientists build their concepts and theories with individual bricks of scientifically ascertained facts, found by themselves and their predecessors. Scientists can proceed with confidence only if they can assume that the previously reported facts on which their work is based are indeed correct. Thus all scientists have an unwritten contract with their contemporaries and those whose work will follow to provide observations honestly obtained, recorded, and published (Huth 1988).

As I suggested above, the social contract for science may have been extrapolated from the social contract for scientists. I also believe that the former social contract relies on the latter's existence—such a relationship among scientists is a predicate or premise of a social contract between science and society. I will explore this reliance in greater depth below (II.C.3.d.).

d. consensus and change

Finally, the social contract for science—in addition to being analytically useful and sociologically interesting—can express an original consensus against which change can be measured and evaluated. If anything approaching a social contract for science can be identified, delineated, or rationalized, then the premises on which it rests can serve as a theory-rich base from which to measure change over time, rather than relying (science as model policy) and an account of instrumental action, cause and effect, and the possibility of accountability (science as clarifier). See also the concluding discussion about science and clientelism (II.C.3.b).

25 Zuckerman then may have to face the question of whether pragmatic or self-interested action is sufficient justification for identifying a norm or set of norms. Indeed, this critique is part of Mulkay's (1975:653-54) attack on the norms of science as an ideology or "vocabulary of justification."

26 Also note the generational aspect of the social contract for scientists as well.
on ad hoc empirical or chronological divisions. Brooks (1990a:33), for example, believes that:

Many times in the last forty-five years commentators have predicted the imminent dissolution of the "social contract" between the scientific community and the polity which was so cogently formulated in the Bush report. The year 1971 was a particularly acute time of "doom and gloom" in this respect, and yet the relation has not deteriorated to the extent anticipated. Today many of the same negative signals that existed in 1971 are again evident. Will science recover to experience a new era of prosperity as it did beginning in the late seventies, or has the day of reckoning that so many predicted finally arrived? Only time will tell.

Richard Barke (1990:1), however, recognizes that "allegations of reneging on the 'compact' between science, technology, and government often are based on rather selective interpretations of Bush's message...; yet some fundamental changes do appear to be emerging in the relationship between the scientific enterprise and its public patron." And in times of trouble, spokespersons for the scientific community such as Frank Press (1988:2), president of the National Academy of Sciences, assert that "[s]cience has been faithful to that compact--that the American people for their support of science could in time expect a better life and a stronger nation. And we continue to honor that compact." Those who herald changes in the contract do so with apprehension for the continued success of the old system; those who trumpet its durability do so with the apprehension of future challenges. Part of my research agenda for the social contract for science is to settle this issue of change and apprehension.

C. A history of the social contract for science

1. The mythic origin

Uncovering the historical baseline against which to measure a change in the social contract is a difficult task. Like other social contracts, the social contract for science need not be an actual historical document. *Science, The Endless Frontier* looms
large in the mythos of American science policy. Representative George Brown (D-CA), chairman of the House Science Committee since 1990, describes the report—as viewed by the scientific community—as "an almost biblical command for robust, no-strings-attached federal support of scientific research" (Brown 1991:26). As former NIH director Donald S. Fredrickson said of a recent, major science policy report that invoked the Bush report, "I wanted to say [that Bush was] the ritual reference, but I'd better say [that it was] the traditional reference....It is a hopeless ritual." In this mythos, if the highest positions on the Olympus of science are reserved for laureate scientists, then Vannevar Bush is an artificer like Hephaestus, who provided the more powerful Olympians with the chariots, thunderbolts, and other tools of their trades; in this view, Bush invented and engineered the institutional machinery of the golden age of American science.

The actual history is not as clear as the myth, but it is hard to say that the polemical force of the myth has suffered for its lack of historicity. What words historians speak about the politics and accidents of the establishment of the federal research system have but poor power to tarnish these already consecrated institutions. Nevertheless, a brief recounting of the history, cast in terms of the social contract for science, might help us understand both the past and the present in a more thorough manner. I will recount this history in a somewhat episodic fashion, beginning with the "original position" of the social contract for science and following with brief visitations of periods of stress or challenges to it.

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27 Interview with Donald S. Fredrickson, former director, NIH, Bethesda, MD, January 5, 1993.
28 Historian of science Nathan Reingold (1991:ch.13) calls Bush's report a "triump of the old order," in contrast with the New Deal that governed the rest of politics and policy in the 1940s. John R. Steelman (1947) provided the Truman Administration with another major science policy report containing more New Deal-ish views of science. Although it contributed to the compromise legislation authorizing the National Science Foundation (NSF), the Steelman report remains almost apocryphal. Whereas, the Bush report, re-issued by NSF several times (at least in 1960, 1980 and 1990), is canonical.
2. The original position
   a. a "magna carta of science"

   The social compact of Thomas Hobbes would relieve humanity of the war of all against all by creating a commonwealth that would foster commerce, leisure, and science. The Federal Constitution defended by Publius would counteract the impending chaos by implementing a new science of politics and a new "constitutional technology" (Wolin 1989:100) to promote tranquility and the general welfare. Vannevar Bush's report, written toward the end of the Second World War and implemented during the Cold War, promised military superiority and domestic prosperity through science-based technology.\(^{29}\) The original position for social contract for science was a dangerous and threatening place, but beyond the chaos lay a secure, prosperous and enterprising order.\(^{30}\)

   In addition to the wars, there was an uncertain domestic political context that must be included to understand the creation of the contract and its fulfillment. Historian of science Daniel Kevles (1977) recreates this political context in much the same way that Don Price (1954) recalls it, as a struggle between liberal reformers who want to organize and plan science for the purposes of the state, and conservative scientific and business interests who want to curtail the public domain and keep science private and free.\(^{31}\) During the Second World War, Bush directed the Office of Scientific Research and Development (OSRD), which managed through its several committees the federal sponsorship of science and technology for the war effort. Among OSRD's administrative inventions was the full cost research contract with universities that did not require advertising (Sapolsky 1990:43). But criticized for neglecting the ideas of independent inventors and small colleges and universities, as well as for allowing its

\(^{29}\) For a discussion of what impact the end of the Cold War has and will have on the social contract for science, also see Sapolsky (1992).

\(^{30}\) The parallel between Bush and Publius is too tempting to ignore, but too complex to engage here.

\(^{31}\) This conflict, of course, parallels in the U.S. the debate in the U.K. between Michael Polanyi and J.D. Bernal. See, for example, Turner (1990b). But in this debate, as in politics generally, the U.S. displayed a relatively constrained spectrum of options compared to its European counterparts.
contract recipients to retain patent rights from war research, OSRD fell squarely within the academic-industry-military alliance that opposed the liberal reformers (Kevles 1977).32

Leading the liberal reformers in Congress was Senator Harley M. Kilgore (D-WV). Dissatisfied with the war mobilization policies--in part based on his experience on the Senator Harry S Truman's commission--Kilgore proposed an Office of Technological Mobilization in 1942. The next year he expanded his vision to include science and postwar considerations. His Science Mobilization Act would have established an Office of Scientific and Technological Mobilization to finance education and research and compel the disclosure and dissemination of technical information from the recipients of federal research assistance. Kilgore's bill was heralded by Assistant Attorney General Thurman Arnold, in a felicitous metaphor, as "the magna carta of science" (Kevles 1977:10-11). But the bill was greeted less pleasantly by trade associations, by the Departments of the Army and Navy, and by the scientific leadership, who were "unalterably opposed 'to being made the intellectual slaves of the State'" (Kevles 1977:11). Bush, however, was ambivalent about the bill: he found some aspects of the peacetime provisions to his liking; but as director of OSRD he did not like its wartime provisions.

Bush also seemed to think that Kilgore did not understand the threat that politics and political control posed for science:

'Politics' meant the intrusion of the extraneous--in Bush's worldview violating the intrinsic nature of specific programs or institutions by favoritism to individuals, by pandering to economic, regional, and other interest groups, and simply by concern for irrelevant issues. Bush's ministry decided what was relevant and, by a system of checks and balances, kept 'politics' out of their deliberations (Reingold 1991:304-05).

32 Parsons (1946:9) reports that the liberals felt that OSRD "should not serve as a precedent for peacetime conditions." England (1982:11) finds that even OSRD staff believed that it "is not democratic enough in organization for peacetime--the director has too much autocratic power."
Kilgore took some of the criticisms from Bush and the others to heart, even receiving some mild praise from Bush and his allies as he continued to refine his postwar science plans (England 1982:10-11). But his activity pushed his political opponents at OSRD to attempt to seize the initiative by other means (England 1982; Kevles 1977).

b. the dance of legislation

Although President Franklin D. Roosevelt's letter to Vannevar Bush is almost as exalted as Bush's response, the letter was the result of a flurry of entrepreneurial activity by White House and OSRD staff and not of presidential divination (England 1982; Kevles 1977). In November 1944, Roosevelt issued a four-point letter that had gone through successive drafts by OSRD and White House staff. Bush established committees to correspond to each of the points in the letter. The reports of these committees, together with Bush's synthesis, constituted the June 1945 response in *Science: The Endless Frontier*.

Bush planned for the introduction of legislation supporting the recommendations in *Science: The Endless Frontier* on the day of the report's release in July 1945 (England 1982:23). Wilbur D. Mills (D-AK) arranged to have a bill introduced in the House and Warren G. Magnuson (D-WA) introduced the Senate companion (S. 1285). Kilgore, angered at having been abandoned by Bush, introduced his own bill a few days later (S. 1297). A long fall of hearings highlighted the issue of responsiveness to or insulation from the President as the primary difference between the two schemes. Kilgore's bill would have had the foundation administered by a presidential appointee; Magnuson's bill would have had it governed by a board of

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33 It is not clear from the accounts given by England or Kevles whether Bush actually wrote the first draft of the letter or not, although he clearly had a role in its preparation. Fredrickson (1993 forthcoming) suggests that Bush did not draft the latter and that the original impetus came from individuals such as Mary Lasker, the leader of the disease lobby, who were interested in medical research.

34 The scientific community showed initial interest in the Bush report, but it became preoccupied with the question of atomic energy and the organization of atomic research, which "constituted an emergency so great that even the related problem of a national framework for research should be subordinated to it" (Smith 1965:441).
scientists not subject to presidential authority. At loggerheads, the two Senators compromised and Kilgore introduced a new bill (S. 1850) in February 1946. The compromise passed the Senate, but died in the House largely because Mills reintroduced a bill akin to the original Bush proposal, splitting the scientific community (England 1982). Bush felt no regrets and looked forward to the 1946 elections in which Republican prospects were high (Kevles 1977:24-25). The 80th Congress, the first Republican Congress since before the Depression, delivered a new Bush bill by the summer of 1947. But Truman, on the advice of Bureau of the Budget director James E. Webb and based on a memo by Don K. Price, pocket-vetoed the bill because it failed to provide for Presidential direction. The Senate soon passed a new version that would pass Presidential muster, but it died in the House. The Senate tried again in 1949, and in February 1950 a parliamentary maneuver freed the bill from the House Rules Committee. Truman signed the bill, which provided for Presidential appointment of the director of the new National Science Foundation, in March 1950.

In addition to the conflict over the governance of the new foundation, other issues that kept the members of Congress, the scientists, and the lobbyists engaged in the debate for five years included the question of geographic distribution of research funds, the role of the social sciences, and the status of patents in federally funded research. Generally, the liberals associated with Senator Kilgore approved of the application of some geographic formulae, approved of the inclusion of the social sciences in the purview of the new foundation, and disapproved of the granting of title to patents to the recipients of federal grants and contracts. The conservatives associated with Bush and Senator Magnuson disapproved of geographic formulae, disapproved of the social sciences, and approved of granting title to contractors and grantees.

Although the other issues are revealing of the concept of science-government relations that existed in the late 1940s--and how that concept may or may not have changed in the decades since--the most important of the three issues for my purposes is
the status of patent rights. Magnuson’s S. 1285 left patent policy to be decided by the foundation’s governing board which, under his plan for an independent board, would most likely have resulted in contractors’ and grantees’ being able to claim title. Kilgore’s S. 1297 provided government ownership and nonexclusive licenses (England 1982:26). According to Talcott Parsons’ (1946:7) commentary on the original two bills, “[t]he Magnuson Bill involved no innovations in patent policy, following the practice of OSRD....The Kilgore Bill, on the other hand, provided for rather radical innovations in this field.” Despite this stark comparison, S. 1297 had been a revision of even more radical proposals by Kilgore, and in 1946 Kilgore and Magnuson managed a compromise on the patent issue. The compromise bill, S.1850, kept patents public under normal conditions, but the administrator of the foundation could vest rights in the contractor or grantee under certain circumstances (Kevles 1977:24). There was general support in the scientific community for the compromise bill and, in passing it, the Senate fought off several amendments—including one that attempted to return to Magnuson’s original patent provision. But Mills’ maneuver in the House, for which Bush had some responsibility (England 1982:58-59), killed the compromise and delayed the progress of the foundation. Had S. 1850 been passed in 1946, the new foundation would have been in a stronger position to lay claim to a larger share of research and organizational jurisdiction vis-a-vis the Office of Naval Research, which was created in mid-1946, and the Public Health Service, which had just received most of the OSRD contracts in medical research at the beginning of the year.36

In the fall of 1946, President Truman tried to regain the momentum for the reformers by appointing his own Scientific Research Board, with John R. Steelman as

35 The geographic distribution of research funds was, as it is today, also a conflict between private colleges and universities which generally oppose formula distribution, and the public universities which often favor it. Since the 1940s, the issue has evolved into not one of formula approaches, but one of research earmarked by members of Congress for research in their districts by public or private institutions. Such “pork-barrel science” has increased rapidly in the past few years, and Rep. George Brown has recently begun an anti-pork initiative. See, for example, CRS (1992a; 1992b).
36 It also might have avoided some of the troubles over the loyalty and security issues, first raised by the House Un-American Activities Committee in 1947 (see II.C.4.b).
the chairman. This maneuver was, in some ways, akin to that by which Bush and his allies had attempted to swing momentum from Kilgore with *Science: The Endless Frontier* (England 1982:88). Steelman reported in August 1947 in five volumes, including ones on “administration,” “manpower” and “medical research.” He avoided addressing the patent issue because the Attorney General had recently reported on government policy (Steelman 1947a.ix). He did, however, produce a report that surpassed Bush’s own in scope, detail, and even in optimism about the possible role of scientific research on economic and social progress. Steelman also surpassed Bush in his advocacy for a government role in scientific research, emphasizing the “functional part of [science in] the framework of the community” (Steelman 1947c:27) and the role of public administration in assisting the productivity of research. Yet Steelman (1947c:99) seemed the equal of Bush in making “the wise decision to maintain a relatively high degree of segregation organizationally for basic research” and in recognizing that “[c]ertain exceptions and dispensations from [administrative] ‘regimentations’ may be essential to the efficient conduct of research and development” (Steelman 1947c:130).

The report, however, had less legislative impact than might have been hoped for by its patron and its author. Two bills were introduced in the Republican Congress in 1947, one a duplicate of the compromise S. 1850 and another akin to the original Magnuson and the diversionary Mills bill, containing no patent compromise. The latter bill cleared both chambers rapidly and was the one vetoed by President Truman. When the Senate responded with a bill that would satisfy Truman, the House killed it in part because it also allowed the foundation to own the patents.

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37 Elsewhere, Steelman quoted J.D. Bernal’s *The Social Function of Science* (1939) directly.
38 Steelman (1947c:130) added, as if in a calculated jab at the conservatives, “but scientists will find it harder to obtain these as long as they dismiss the whole framework of public administration with contempt.” Bush and his associates exchanged “scathing comments” in private about the Steelman report (England 1982:87).
The Democrats regained control of both chambers in the 81st Congress, but the conservative Democrats maintained a working majority with the Republicans, especially as anti-communist fervor flared. The National Patent Council still “ranted” about the possibility of the foundation’s owning patents (England 1982:101), but the patent issue loomed less large for two reasons: one, it became clear to legislators that the stakes of the patent question were lower because it became clearer that the foundation would only be supporting basic research, which was of less immediate commercial potential (England 1982:6); and two, the issue of loyalty overran other issues (England 1982:93-105). The Democratic Senate passed its bill easily. The House bill had to be forced out of the Rules Committee, only to be amended to include an affidavit and a Federal Bureau of Investigations (FBI) check on potential grantees. The conference committee deleted the FBI check. In his signing message in 1950, Truman cited Science: The Endless Frontier.

c. the plural system

But while Bush, Magnuson, Mills, Kilgore and Truman were taking one step forward and two steps back, the bureaucrats were not merely spectators to the dance of legislation. As early as 1943, about when Kilgore started planning for postwar science, the Navy Department trouble-shooters known as the Bird Dogs began to plan for a Navy research office after the war (Sapolsky 1990:9). Although this planning did not in itself lead to the result, Truman signed legislation creating the Office of Naval Research (ONR) in August 1946. ONR was intended to be an organizational base from which Vice-Admiral Howard G. Bowen could build a nuclear Navy. But Bowen lost jurisdiction over the nuclear propulsion program, and he took terminal leave shortly after ONR was created. Lacking this planned mission, ONR turned to funding a wide array of scientific research because of “nothing more than a bureaucratic accident” (Sapolsky 1990:118). ONR’s approach went beyond the flexibility developed by OSRD for letting research contracts to universities, to the point where “the formal
distinction between contracts and grants [was] inconsequential" (Sapolsky 1990:43). From its establishment in 1946 until NSF was finally established in 1950, ONR served more as the “Office of National Research” (Sapolsky 1990:35).

Similarly, the bureaucrats in the Public Health Service (PHS) were scrambling to protect their stake in the postwar research system. PHS had established an enviable wartime record with its role in the development of penicillin, gamma globulin, adrenal steroids, cortisone, and blood plasma (Strickland 1989). Planning for the postwar world began in August 1944, shortly after the passage of the Public Health Service Act, which gave PHS “broader authority for investigations of all types in the ‘causes, diagnosis, treatment, control, and prevention of physical and mental diseases and impairments of man’” (Steelman 1947d:17-18). Although Bush had established a Committee on Medical Research, PHS and NIH “simply did not loom large in Bush’s view of the structures of federal power” (Reingold 1991:298). The committee requested a separate institution to fund medical research--distinct from both the NSF proposal and the extant PHS--but Bush opposed the idea.39 Nevertheless, PHS leapt into the breach much as ONR had done during the legislative delay of NSF; when OSRD was terminated on January 1, 1946, PHS received jurisdiction over many of its outstanding contracts in medical research (Fredrickson 1993 forthcoming).

Several months later, NIH established its Office of Research Grants. Finding it had money left over from its penicillin contracts because the price of the drug had fallen under mass production, PHS advised medical school deans of the availability of money for grants. Newly decommissioned physicians and scientists overwhelmed the two professional grant officers with more than one thousand proposals in the first year (Strickland 1989:23-25). By October 1946 the NIH grants program had authorized 264 projects for $3.9 million (Steelman 1947c:190-91). “The decision to channel most

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39 Senator Claude Pepper favored the proposal by the committee; only the Surgeon General Thomas Parran and the NIH director Rolla E. (Gene) Dyer argued that PHS and NIH should assume responsibility for all postwar medical research (Strickland 1989:18).
of the federal dollars for biomedical research to the universities, medical schools, and non-profit research institutions at the end of World War II was made virtually by default" (Ginzberg and Dutka 1989:3). The National Mental Health Act created the National Mental Health Institute in 1946, and the National Heart Institute Act created another of the categorical institutes in 1948. Shortly after Truman signed the NSF legislation including a division of biological and medical science, NIH received an essentially unlimited authority to expand in the Omnibus Medical Research Act (Strickland 1989). But by then, NIH had already established a track record of providing flexible basic research grants to universities and NSF was essentially out of the running to fund medical research on a large scale.40

The few years between the end of the war and the creation of the National Science Foundation saw the passage of the Atomic Energy Act as well as well as the construction of the ONR and PHS research programs described above. These agencies, “firmly entrenched” by 1950, assured that the nation’s support of basic research would occur in a pluralistic manner (U.S. Congress 1986a:35).

3. Framing the social contract for science
   a. technological enthusiasts

If postwar legislation for the federal sponsorship of scientific research was to be a “magna carta” or a social contract or a constitution, Vannevar Bush and others—John Steelman, Harvey Kilgore—were competing to be its Framer. There was no clear choice between one constitution and the status quo: the status quo was constantly evolving in the early postwar years and the provisions of the postwar legislation were flexible from one session of Congress to the next. There was a consensus that laissez faire for science would end (Kevles 1977): the only scientist who testified in opposition to the federal role was Frank Jewett, president of the National Academy of

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40 Steelman (1947d:42) recognized NIH and ONR as particular examples of government funding of research "not aimed at defined objectives," and NIH and the Department of Agriculture were “excellent examples of agencies with organizations reflecting the impact of research as a primary or end function” (Steelman 1947c:40).
Sciences as well as Bell Laboratories. But there was no agreement on the new order and not even an comfortable ideological line-up. Pro-business conservatives advocated a state role in funding research and a “liberal” patent policy akin to the generous wartime measures that mixed public and private interests by granting title to contractors and grantees. New Deal liberals advocated government administration of scientific research but a more separate and limited state role in the economy through the retention of title on sponsored inventions by the government. Bush and Steelman wrote forceful and hopeful rhetoric about the promise of technology. Bush, a founder of Raytheon Company, had a Hamiltonian vision of the vitality of small manufactures. Yet Kilgore, untrained in science and a practicing populist, was not merely an advocate of the yeoman farmer. He instead did yeoman’s duty drafting and compromising and redrafting legislation for seven years before his efforts helped lead to the federal role in basic research.

What yoked Bush and Kilgore together was their opportunity, at the end of what historian Thomas P. Hughes (1989) calls “a century of invention and technological enthusiasm,” to create another special American century based on the continuing promise of science-based technology. Bush wanted the new order to look more like the old Republican order of the 1920s (Reingold 1991:ch.13). Kilgore wanted a national research foundation with the new view of the nation that had been introduced by the New Deal--a state whose idea of general welfare was not limited to bettering business. But they both wanted to harness their concept of the new order to the morally and instrumentally powerful shoulders of science.

Who won? Is it reasonable to say that Bush wrote the social contract for science, as many people seem to think? Reingold (1991:321) says, indisputably, that “Science: The Endless Frontier was more honored than literally heeded.” But I do not think that Bush was quite the loser that Reingold (1991:285) portrays, a “Moses” who pointed the way but stayed behind. Reingold is of course right, that on many important
points Bush was in fact forced to yield, by legislative necessity or by political circumstance. The scientific board did not govern the foundation. The gate was left open for the social sciences to be included. Military research was conducted by ONR and the other services, and medical research was largely appropriated by NIH. No changes in the patent law were made, but the plurality of funding agencies created a plurality of patent policies, ranging from the liberal military agencies following wartime patterns, to the NIH and the Atomic Energy Commission, which did not generally allow recipients of funds to patent research products for quite different reasons. But neither did Kilgore or Steelman win these points.

Reingold (1991:304-05) writes that “[w]hat Bush did not propose (nor countenance afterward) was what Don K. Price later dubbed ‘government by contract.’ Those arrangements could and did violate Bush’s sense of the proper, differing roles of industry, government, the universities, and the like.” But here I think Reingold confuses what was intended and what resulted. Bush and OSRD invented the full cost contract without advertising, and implemented a liberal patent policy. Bush wanted to continue these relationships after the war. Price praised the system Bush and OSRD invented because it granted the full measure of independence possible to the funded institutions thrown into a relationship that was novel for both partners.41 It was only later that Frank Jewett’s fears about the interference that followed government funds would appear. Reingold is correct that Bush would not have countenanced such interference, but he did not anticipate it. Rather, he invented the mechanism for it. Bush was in this sense less a “creative anachronism” (Reingold 1991:321) than a reasonable incrementalist.

41 Price at least seemed to believe this in 1954 when he wrote Government and Science. His The Scientific Estate (1965) is a little more sanguine about the actual degree of separation that had been maintained in practice, but he believed that a reasonable degree could be maintained by using professional and administrative buffers between science and government along the “spectrum from truth to power.” See also Price (1981).
The irony is that just as scholars and policymakers are disputing whether or not Bush's social contract for science is coming to an end--mourning its passing or questioning how it should be renegotiated--Bush's ideas are in one sense finally come to their most complete fulfillment. The Patent and Trademark Amendment Act of 1980 crossed the organic acts of the funding agencies to unify patent policy and allow all contract and grant recipients to more easily obtain title to the fruits of sponsored research. Bush sought just this policy across all federal research and development. Bush would no doubt be pleased with the impulse behind, if not exactly with the formulaic set-asides for, the Small Business Innovation Act, which guarantees a share of research grants to small businesses. The Federal Technology Transfer Act, authorizing the cooperative research and development agreements between federally employed scientists in government laboratories and researchers in private firms, might have been too much for Bush. But neither is this relationship so different from the wartime relationships that Bush himself had fostered among industry, universities, and government; nor is it much more than an incremental step beyond the Patent and Trademark Amendments.

The irony can be explained away, however, by realizing what I have argued above: that the patent provisions were not part of the social contract for science because Bush lost this part of the conceptual and legislative battle. The social contract for science was constituted by the noncontroversial views about science that he shared with Steelman. If the social contract for science had been Bush's alone, then the patent policy would have been like that at OSRD for all agencies from the end of the war, and there would have been no need for the changes in the 1980s. Bush was not the Framr of the social contract for science; nor was Science, The Endless Frontier its complete or sole articulation. Bush was one of several apostles of science, along with Steelman and Kilgore, and his report was but one part of the good news of the federal role in science funding.
b. a reciprocal boundary

Looking back through the lens of history and seeing such a fractured reality, does it make sense to draw a sharp line differentiating the present from the past? After all, ""Vannevar Bush and John Steelman and Senator Kilgore didn’t sit down in a room over at the Capitol to negotiate this contract. In fact if that’s what happened, then it really isn’t a social contract"" (Hamlett 1990:31). I believe that a social contract for science has existed, and this is why. Although all the parties (but Jewett) agreed that laissez faire for science should end, there was little agreement on the outlines of the intervention. But with the end of the negative commandment of laissez faire, some new boundary was needed. The idea as instituted by Bush and OSRD and interpreted by Price (1954) was to make the new relationship merely one of financial support, to construct a boundary between politics and science through which only money could pass in one direction and technology in the other. What transpired on the science side of this semipermeable boundary was quite immaterial to the politics side of the boundary. When and precisely what technologies would pass were unspecified, but it was an article of faith that they would in fact pass.

In designing and erecting this boundary, Bush and Steelman were in substantial agreement, and this agreement prevailed. With respect to the concern of the government about what transpired on the science side of the boundary, Steelman (1947a:50), referring to both NIH and ONR, wrote that "grants—a gift made to individuals or institutions whose competence has been demonstrated for the purpose of an investigation whose outcome cannot be known precisely in advance" are the appropriate mechanism for the support of basic research. Some administration of

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42 The speaker is Christopher Hill, then of the Congressional Research Service and currently at the Critical Technologies Institute at RAND. The context is a discussion, "Dialogue on science and Congress," among political scientists and science policy participants that focused in part on the social contract for science. See section II.D below.

43 Of course, there was no, and cannot be, complete laissez faire. From the patent section in the Constitution to distribution of scientific patronage to university scientists by John Wesley Powell (Turner 1977) to the National Advisory Committee on Aeronautics (once headed by Bush), the government has "interfered" with science.
science is necessary in Steelman's conception, but lay-administrators suffer from two common maladies when confronted by science: awe and ignorance. Neither have positive administrative results and therefore "[t]he inevitable conclusion is that a great reliance must be placed upon the intelligence, initiative, and integrity of the scientific worker; and that he must, by the nature of things, be much less subject to the usual controls and regulations imposed in governmental administration than most other government employees" (Steelman 1947b:129). Similarly, for Bush (1980 [1945]:9), the new foundation "should recognize that freedom of inquiry must be preserved and should leave internal control of policy, personnel, and the method and scope of research to the institutions in which it is carried on." The "prime obligation [for government support of research is] to provide the individual worker with an opportunity for free, untrammeled study of nature, in the directions and by the methods suggested by his interests, curiosity, and imagination" (Bush 1980[1945]:15).

With respect to the creation of technologies, Steelman and Bush were equally enthusiastic about the productivity of basic research. Steelman fit the federal support of science into a constitutional frame, because "[s]cientific inquiry is necessary to the effective performance of these functions" of defense, standards, and commerce, not to mention the "sweeping general welfare clause" (Steelman 1947a:48-49). "It is difficult to think of any other national activity which more directly benefits all the people or which makes a larger contribution to the national welfare and security" (Steelman 1947a:26). Not only did science serve this broad national purpose, but it "is equally the basis for our progress against poverty and disease" and "the basis for an expanding economy, and continued high levels of employment" (Steelman 1947a:3-4). These results could only be expected if the government recognized that the pressure for results is counterproductive, because on this recognition "rests the wise decision to maintain a relatively high degree of segregation organizationally for basic research" (Steelman 1947c:99). Particularly with respect to basic medical research, "[t]he intent is to
support research of general value to the whole of medical science, not alone to the
operating programs of the agency, on the assumption that in the long run--no one can predict how long--there will be cumulative, beneficial results” (Steelman 1947d:42-43).

These results were serendipitous and important implications could come from even unlikely sources. Steelman (1947b:3-4) provided an example of “biological experimentation to develop a fungus-resistant tomato [that] led to the discovery of ‘tomatin’ which was analyzed and found to contain the same ingredient as substances used to counteract ringworm, immediately resulting in the development of an impregnating agent in military boots and footwear.” These implications were “immediate” once the research happened. In his extended discussions of administration for science, Steelman (1947c:12) acknowledges the need for “liaison between operating units...for interchange of ideas and experience between both the scientific and administrative personnel.” Such liaisons were particularly important between scientific fields and for project initiation within a particular field. Steelman does not identify, however, the need for liaison work, for example, in disseminating scientific work, in linking the different sectors in which research is performed, or in linking the different types of research (e.g., in linking plant biologists in government laboratories who discover tomatin with industrial chemists who fumigate army boots).44

Bush’s attitude was quite similar. “Basic research is performed without the thought of practical ends,” and “is necessarily speculative” (Bush 1980 [1945]:18;32). “But it is important to emphasize that there is a perverse law governing research: Under pressure for immediate results, and unless deliberate policies are set up to guard against this, applied research invariably drives out pure. The moral is clear: It is pure research which deserves and requires special protection and specially assured support” (Bush 1980 [1945]:83). Science must also therefore be, to use Steelman’s word, “segregated” or “free from the influence of pressure groups, free from the necessity of

44 This kind of liaison work, unanticipated by Steelman, is the work performed by the technology transfer professionals studied in chapter VI.
producing immediate practical results" (Bush 1980 [1945]:79). Nevertheless, despite its detachment from practical ends and its inherently speculative and therefore vulnerable character, the basic research creates "the stream of new scientific knowledge to turn wheels of private and public enterprise" (Bush 1980 [1945]:18). Therefore, "[t]he simplest and most effective way in which the Government can strengthen industrial research is to support basic research and to develop scientific talent" (Bush 1980 [1945]:21). Without basic scientific research supplied by the government, industrial development would "stagnate" (Bush 1980 [1945]:18).

Both Bush and Steelman understood an interrelation between the privileged position of science and scientists that they advocated and the productivity of science. For Bush (1980 [1945]:12), "[a]s long as...scientists are free to pursue the truth wherever it may lead, there will be a flow of new scientific knowledge to those who can apply it to practical problems in Government, in industry, or elsewhere." But even the "free play of free intellects" "must be responsible to the President and Congress [because o]nly through such responsibility can we maintain the proper relationship between science and other aspects of a democratic system" (Bush 1980 [1945]:12;33). Steelman (1947c:27) is even more explicit about a privileged yet responsible science:

However, in organizing scientific effort, the administrator must recognize the special characteristics of research and development work and the special qualities of the scientific mind. Otherwise, he cannot function effectively. For their part, scientists must accept the organization which is essential to all large scale operations, and--in the Federal Government--the necessity for rules and regulations which accompany public operations of great scope. Neither public administration nor modern science can exist or function in a vacuum.

....After conceding the special status and vital role of the scientists in our society, it is important to note also that society and its organization are important to the scientist. Science is a functional part of the framework of the community. Scientists are members of society living within limits set by social responsibility, economic reality, and democratic government. The democratic ideals of our society have fostered science and helped to give it its present place in the world at

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45 This is but one example of the fluid metaphor Bush used to describe the rather automatic relationship between science and technological innovation; see V.B.2.
large. It is the obligation of scientists, in turn, to give willing aid to the
tasks involved in making our system work well.

Both Bush and Steelman therefore had some kind of idea about a reciprocal relationship
between science and politics: one that involved the “free play” of scientists in an
environment “segregated” from political and market pressure; and also one that, if given
this freedom, would be productive for the political community because its investment in
science would provide welfare and security.

The relationship they conceived, the social contract for science, can be
described as follows: the political community agrees to provide resources to the
scientific community and to allow the scientific community to retain its decision-making
mechanisms and in return expects forthcoming but unspecified technological benefits.
This social contract for science was used by both Bush and Steelman to campaign for
the federal role in science, and it has been used, implicitly or explicitly, to legitimate
this role. As evidenced by the descriptions given by the science policymakers above
(II.B.2), these very ideas of the social contract for science continue to have currency.

c. two premises of the social contract for science

Such a social contract for science has at least two implicit premises. One is that
the scientific community is actually capable of being “allowed to retain its decision-
making mechanisms.” That is, the social contract for science is premised in part on
what is often referred to as an “autonomous” or “self-regulating” scientific community
(I prefer the latter term). The second premise is that under this arrangement, the
scientific community can produce the technological and other benefits expected by the
political community. Both premises—I call them the premise of self-regulation and the
premise of technology transfer, respectively—are important because it is conceivable
that a self-regulating community could exist, but could also fail to be technologically
productive. These two premises are the basis of the social contract for science and, as I
shall argue further below, when the political community begins to question and even to
tamper with the premises, the social contract for science is eroding and the boundary between science and politics is becoming less clear and less stable.

Is there a coherent conception of science underlying these premises, in addition to the rhetoric of Bush and Steelman? There are attempts, for example, to defend the premise of self-regulation by referring to the social contract for scientists, discussed above.\textsuperscript{46} Another variety of this defense is also given by Michael Polanyi in “The republic of science” (1962), in which he provides a political economic model of the scientific community.\textsuperscript{47} Simply put, Polanyi (1962:54-56) argues that the “free cooperation of independent scientists”\textsuperscript{48} is, like an economic market, a “special case of coordination by mutual adjustment.” The rules of Polanyi’s republic of science keep order, as in a market, despite competing interests. Polanyi’s model specifies a science that corrects its mistakes and regulates the conduct of its members.\textsuperscript{49}

Latour and Woolgar (1979) extend the economic model by providing a more complete account of transactions among scientists. They describe a “credibility cycle,” which traces the flow of scientific “capital” through a heterogeneous circuit of domains: recognition; grants; money; equipment; data; arguments; articles (which are also products); and recognition.\textsuperscript{50} In an autonomous scientific community, the free flow of such capital is permitted and encouraged, and successful scientists efficiently convert

\textsuperscript{46} I take Zuckerman’s social contract for scientists to be derived from Merton’s four social norms of science--communism, universalism, disinterestedness, and organized skepticism (CUDOS) (Merton 1973:ch.13)--which have been used as a defense for scientific autonomy or self-regulation. Indeed, as Jasnoff (1987:196) has written, “Much of the authority of science in the twentieth century rests as well on its success in persuading decision-makers and the public that the Mertonian norms present an accurate picture of the way science ‘really works.’”

\textsuperscript{47} See also Tullock (1966) and Turner (1990b).

\textsuperscript{48} Recall Bush’s “free play of free intellects.”

\textsuperscript{49} This view of the scientific community, shared by American Pragmatist philosophers such as John Dewey, is the one Ezraji (1990) points to as the model polity that undergirds liberal-democratic action. See also 1.B.4.

\textsuperscript{50} Latour and Woolgar (1979:204) “had the distinct impression that the constant investment and transformation of credibility taking place in the laboratory mirrored economic operations typical of modern capitalism.” Arie Rip (1988) has also expanded the notion of the credibility cycle to account for the entrepreneurial behavior of research councils and institutions (corporations) in addition to individual scientists. This not-so-casual metaphor also evokes the more contemporary relations of scientific research to economic development spurred by the technology policy legislation of the 1980s, e.g., Entrepreneurial Science (Johnston and Edwards 1987).
their capital into products such as scientific papers which, if credible, are demanded by
other scientists. A market is thus created, with a supply and demand of credible
information.\textsuperscript{51}

Under the social contract for science, the role of the government in this market
is limited to the provision of money for grants—a monetarist macroeconomic policy if
you will. Thus there is no strict \textit{laissez faire}, as Bush and Steelman agreed. But the
microeconomic aspects exclude government action because they require the exercise of
scientific judgment. For example, the conversion of recognition into grants, and
articles into recognition, requires the scientific judgment of peer review. Similarly, the
conversion of money into equipment involves the scientific judgment of choosing
experimental problems. As the application of judgment by scientists themselves is said
to moderate economic exchange and transformation, and as credibility is cyclical, the
system could be said to be self-regulating.\textsuperscript{52}

The premise of self-regulation is inextricably bound to the premise of
technology transfer in Polanyi’s republic of science. Polanyi argues that only by
allowing science this free association can the rest of society expect the benefits that
science can provide. First, relying on the “invisible hand” metaphor, Polanyi
(1962:56) claims that this organization of science is “the most efficient possible.” This
organization also drives the “maximum advancement of science,” just as the Smithian

\textsuperscript{51} This market-like perspective on the scientific community is largely similar to system of
competition for (the scarce resource of) recognition from their colleagues describe by Hagstrom (1975).
Hagstrom (1975:78) even makes an analogy between science and the economy involving oligopolistic
cooperation and high-energy physics research facilities. Bourdieu’s (1991:pt.1) vision of an “economy
of linguistic exchanges” in which individuals “struggle for linguistic authority” and “recognition” of
their “symbolic capital” provides another similar model which is especially applicable when one
considers that the primary product in the credibility cycle is the scientific paper.
\textsuperscript{52} Focusing inside the laboratory rather than on the larger scientific community and its orientation
toward society, Latour and Woolgar do not draw the obvious conclusion from their credibility market:
any policy regime that interfered with the microeconomic transactions would, from the perspective of
the scientific community, threaten the economic structure of science by intruding upon the exercise of
scientific judgment that converts one form of capital into products and other forms of capital. Latour
and Woolgar (1979:70) cross this policy question in yet another crucial way, but still manage to avoid
it; when the participant-observer in their study is tempted with “going native,” the question that returns
the anthropologist to his critical equilibrium is, “How can we account for the fact that in any one year,
approximately one and a half million dollars is spent to enable twenty-five people to produce forty
papers?”
market produces the maximum satisfaction possible. Second, “that research would no longer be conducted for itself as an end in itself” but would instead be guided “into socially beneficent channels” is an “impossible and nonsensical” aim (Polanyi 1962:62). One could not guide, direct or plan science because one cannot predict the uses to which scientific discovery might be put. Polanyi provides a self-effacing example of his and Bertrand Russell’s inability, in January 1945, to predict any “possible technical uses” of the special theory of relativity. Some forty years after the formulation of special relativity and yet merely seven months before its manifestation at Hiroshima, Polanyi and Lord Russell did not predict such a technical use as an atomic bomb.53 Third, because of this unpredictability, Polanyi (1962:62-63) concludes:

Any attempt at guiding scientific research towards a purpose other than its own is an attempt to deflect it from the advancement of science. You can kill or mutilate the advance of science, you cannot shape it. For it can advance only by essentially unpredictable steps, pursuing problems of its own, and the practical benefits of these advances will be incidental and hence doubly unpredictable. ....In describing here the autonomous growth of science, I have taken the relation of science and technology fully into account.

The “relation of science and technology” is summarized by Polanyi here as “incidental.”54 But the technological product is directly derived from the advancement of science, and one cannot, according to Polanyi, increase the former by attempting to manipulate the latter.55 In this political economic model, both the maintenance of the

53 Although this claim is rhetorically powerful, if it is not disingenuous it may be taken as just as damaging to the reputations of Polanyi and Lord Russell as to the theory that science can be channeled. Obviously, the scientists who proposed the atomic bomb project predicted a technical use of special relativity. It might be that Polanyi has missed a step in his argument, one that says something like, “only scientists very close to the facts and theories can predict with certainty the next, small incremental step of technical uses.” But the experimentalist Frederick Soddy speculated on the possible destructive forces derived from the energy in atomic nuclei (Sclose 1989). And of course, H.G. Wells (1934) speculated about a powerful weapon based on the atom in The Shape of Things to Come. Polanyi might have dismissed these as mere speculations and not predictions.

54 Polanyi cites his own work on the relationship between academic and industrial research on this regard, a 1961 article in J. Inst. Met., which I have been unable to locate.

55 Bernard Barber (1962:239) makes a similar point: “However much pure science may eventually be applied to some other purpose than the construction of conceptual schemes for their own sake, its autonomy is whatever run of time is required for this latter purpose, is the essential condition of any long run applied effects it may have.” My thanks to Harvey Brooks for bringing this example to my attention.
integrity of science and the contribution of scientific advance to technological innovation are unproblematic. They are managed by the normal, automatic functions of science.

These two premises also correspond to my case selection. If the scientific community is self-regulating, then misconduct in science should be managed well by the application of scientific judgment within the credibility cycle. If the premise of technology transfer is correct, then as long as the political community provides funds and does not otherwise interfere, the scientific community should provide the scientific raw material for technological innovation and industrial productivity.56

In this intensively technological age, politicians must rely on science and scientists for material and ideological support.57 The material contributions of science to politics depend in part on sound judgments and conclusions; a hypothetical AIDS vaccine is useless for easing human suffering or for settling political conflicts if clinical data supporting its safety and efficacy have been forged. Ideological support for politics rests on the integrity of scientists; a community of white-coated rascals and knaves cannot be a model polity of nonviolent consensus-formation. If the scientific community cannot perform the self-regulatory task, the support it can provide to politics evaporates, and with it, the support politics provides for science.

Similarly, research investments may be difficult to sustain if technological benefits are not continually and visibly forthcoming. If science promises that the

56 Actually, there may be relevant consequences for "tight-money" and "loose-money" policies with regard to science funding. The former may reduce the rewards expected by members of the community, increasing competition, and perhaps in the extreme leading to anomie and deviance (see Weinstein 1979; Merton 1973:ch.18). The latter may attract to many unqualified persons to research, reducing the marginal return on investment in science and at the same time removing persons from roles as engineers, technologists, and science translators--roles that may be vital for innovation and productivity (see Dresch and Janson 1990; 1987). A "loose-money" policy may also attract persons uncommitted to the "ideals of scientific work" and reduce the integrity of science as well as its productivity. Or it may allow scientists to pay more attention to the results of other scientists and improve the integrity of the system (see IV.C.2).

57 Many authors discuss the reliance of politicians and politics on the instrumental use of science. Of great relevance here are the ideas of Beard and Beard (1930), who claim that the modern government needs science and technology to perform its required tasks, but also needs broad power to manage the demands of science and technology. And again see Ezrahi (1990; 1980).
knowledge it generates will lead to technological innovation and economic productivity, and innovation and productivity sag while Nobel production remains high, then investment in research becomes ever more difficult. This is especially the case if funding has been increasing throughout the pre-payoff period. Politicians will be tempted to emphasize projects with more visible returns than basic research.

But the logic embedded in the social contract for science and its two premises also suggests that politicians, because they have a material and ideological stake in the integrity and productivity of science, will not let it fail. Under the social contract for science, the politicians relied on the scientists themselves as the most efficient and effective way to produce a responsible, productive science. As this social contract for science ends, politicians will attempt to assist scientists in maintaining scientific integrity and productivity, in order to construct the boundaries of the new social contract for science.

4. Challenges to the social contract for science
   a. introduction

If a social contract for science was established in the years immediately after the Second World War, then it has a history. Although I argue that it is only recently that the social contract for science has been altered, it may be the case that at other points in its history, it has been challenged or changed. The sections that follow provide a somewhat episodic history of potential challenges to the social contract for science. Beginning with McCarthyism, as the history of the NSF legislation makes clear (II.C.2.b), the challenge of the loyalty of scientists began even before the ink on the social contract for science was dry. Within a decade, as NIH funding doubled and redoubled, Congress began to investigate grant practices and accountability at NIH, and as funding for all R&D crested through 1967, the productivity of this investment came under scrutiny by studies by the Department of Defense and the NSF. The skepticism toward basic research and military research of this period culminated in the oft-
maligned Mansfield Amendment and programs for more practically oriented work like RANN and the War on Cancer. Finally, in the 1970s, just as the language of the social contract for science was fully developing, scholars were questioning its durability under the challenge of risky DNA research and the “limits of scientific inquiry.” Although these cases may show that the social contract for science has been embattled since its origin, I also believe that they show its continued durability until the 1980s. They also show a variety of congressional strategies to control and direct scientific research within the framework of the social contract for science.

b. loyalty

Beginning in June 1952, about two years after Truman signed the National Science Foundation Act that included an affidavit of loyalty for grant applicants, the Department of Health, Education, and Welfare (DHEW) inaugurated a policy to deny or suspend grants “where it is established to the satisfaction of this Department that the individual has engaged or is engaging in subversive activities or that there is serious question of his loyalty to the United States” (DHEW Secretary Oveta Culp Hobby, quoted in Edsall 1955). DHEW inaugurated this policy well into the Cold War attack on the loyalty of scientists. The House Un-American Activities Committee (HUAC) had begun its campaign against Communists in 1947, abetted by an environment of paranoia fostered by the Truman Administration which meant to advance the Truman Doctrine of continued foreign engagement and the postwar reconstruction of Europe (Freeland 1971; Theoharis 1971).58 But although the prominent cases of scientists pursued by HUAC generally involved security clearances for classified research, the DHEW action did not involve the protection of classified research.59 And unlike the

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58 There were, of course, external events such as the Soviet atomic bomb and the success of Mao Tse-tung in China in 1949, the trials of the Rosenbergs and Alger Hiss in 1950, and the war in Korea throughout the period that contributed to the anti-Communist fervor.

NSF affidavit, there was no forewarning to action taken by DHEW; a grant could be
denied or suspended without knowledge of the cause (Edsall 1955).

The DHEW policy did not surface publicly until the spring of 1954, when it
was rumored at the annual meeting of the National Academy of Sciences (NAS) that
several scientists had lost or been denied grants from NIH because of loyalty questions
(Wilson 1983). After press coverage of the NAS meeting and an official NAS
request for information, Secretary Hobby issued the statement quoted above on 28
April 1954. Members of the scientific community, most prominently John Edsall of
Harvard, attacked the action because “grants for open, unclassified research were being
revoked or denied, on grounds apparently political and unconnected with the
competence or integrity of the investigators involved” (Edsall 1955:24). Indeed, Edsall
(1955:25) argued that the “action taken against [these scientists] has often involved the
breaking of a moral agreement, if not a legal contract, by the supporting agency.”

Edsall acknowledged, however, that security checks seemed reasonable for
classified research, and that technically the grants were discretionary actions by the
Secretary. Furthermore, the approach he took in his rhetoric against the DHEW action
was oriented more toward the maltreatment of scientists as citizens of a democratic
polity than the maltreatment of scientists involved in a special relationship with the
federal government: “The policies attacked here violate a long tradition—a tradition
deeply rooted in English and American law” (Edsall 1955:26). So Edsall invoked a
three-tiered rhetoric: he accepted the demands of classified research; he pointed out that
the scientific community has mechanisms to determine if “a man is trusted and
respected by his colleagues and neighbors” (Edsall 1955:26); but he contextualized both
the security demand and the internal governance aspect of the social contract for science
within a broader idea of the responsibility of a democratic government to its citizens.

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60 Secretary Hobby reported that “fewer than 30 persons have been denied support” among the 14,000
persons supported on more than 2000 PHS grants (in Edsall 1955:24).
61 It will be important to recognize this strategy in the case of adjudicating allegations of scientific
misconduct. See IV.D.4.
Although the action by DHEW may have been in violation of a social contract for science, it was abhorrent not necessarily because it crossed the boundaries specified by the contract, but rather because it crossed the boundaries specified by Anglo-American legal traditions and liberal government in general.

The revelation of DHEW policy prodded NSF into action because, although NSF had implemented the loyalty oath, it remained possible that scientists whom DHEW found unacceptable could have obtained NSF funding, which would have put NSF in an awkward position. NSF reconsidered security checks, but decided not to implement them for two reasons: its sponsored research was unclassified; and institutions and not individuals were the legal grantees, so the institutions (and not NSF) were responsible for faculty and staff (Wilson 1983). The American Association for the Advancement of Science endorsed the NSF policy, as did NAS in a 1956 report that had been requested by the White House (Cochrane 1978). The Eisenhower Administration opted to implement the Academy’s recommendation across the federal agencies, achieving a general resolution of the issue. If the security checks conducted by DHEW were contrary to the social contract for science, about which there is some ambiguity, then the contract was still restored in relatively short order and largely through action by the scientific community.

c. NIH accountability

Questions about NIH grant practices and financial accountability were a source of some conflict between Congress and the burgeoning research agency during the 1960s. NIH’s budget had increased by an order of magnitude over the decade of the

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62 NSF still, however, conditioned grants on an individual’s not being an avowed Communist or having been determined to be a Communist, or not advocating extra-constitutional means of changing the government (including persons convicted of sabotage).

63 More research on this issue could focus on whether the response of Edsall, and NSF, and the Academy fell into the pattern identified by Wang (1992:240) in which scientists made strong individual statements, “but as a group they did not establish effective policies to defend individual scientists or challenge the assumptions of the national security state.” The policy made a clear distinction between classified and nonclassified research in terms of process, but it certainly did not have an expansive sense of political eligibility for grants.
1950s, headed by Mary Lasker, and the chairmen of the health appropriations subcommittees, Representative John Fogarty (D-RJ) and Senator Lister Hill (D-AL). Representative Lawrence H. Fountain (D-NC), chairman of the Intergovernmental Affairs Subcommittee of the House Government Operations Committee, suspected that these rapid budget increases were not being put to good use. Fountain conducted several inquiries in a partially successful attempt to demonstrate that NIH financial management was too loose and that congressional appropriations had come too easy. He used his subcommittee to challenge the appropriations subcommittees on NIH funding, a position that Government Operations could often find itself in (Henderson 1970).

The initial inquiry began in 1959, and in April 1961 the Fountain Committee recommended thirteen actions to NIH for tightening its financial management. The next year, Fountain held hearings to determine what NIH director James A. Shannon had done to respond to the recommendations; the answer he received was that NIH had not done much (Strickland 1972; Henderson 1970; Greenberg 1967). Fountain issued a second report in June 1962 that took Shannon to task for his position that financial oversight was a concern secondary to choosing the right researchers (Strickland 1972).

The impact of the Fountain Committee’s inquiry as two-fold. One, NIH tightened up its information requirements on its grants, requiring prior approval for grantees to change research plans, the itemization of equipment expenditures and the specification of travel budgets, and mandating that salaries paid by grants reflect only

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64 NIH’s $213 million 1957 budget (Greenberg 1967:272) was not very far from the $200 million level suggested by Steelman (1947a:29) for that year.
65 George Kistiakowsky, President Eisenhower’s second special assistant for science and technology, said of the NIH director James A. Shannon that he “had a perfect understanding with Lister Hill and John Fogarty” (quoted in Greenberg 1967:279).
66 The Government Operations Committee had been created by the Legislative Reorganization Act of 1946. For an analysis of its history and its activity, see Henderson (1970). Ted Weiss (D-NY) took over this subcommittee from Fountain in 1983 (Foreman 1988). In 1988 Weiss became involved in investigating allegations of misconduct and conflicts of interest in research sponsored by NIH.
67 Strickland (1972) reports that other members of Congress came to Fountain to challenge the appropriations for NIH, because they feared going up against the Appropriations Committee.
time spent on research and not be higher than the grantee’s university pay rate. “By nonscientific standards, none of these requirements was onerous (Greenberg 1967:277),” but the biomedical community complained loudly nevertheless. Two, Greenberg attributes the relatively weak increase in NIH funding, from $930 million in FY 1963 to $974 million in FY 1964 to the scrutiny brought by Fountain. Thus, according to Greenberg (1967:271-72), the Fountain Committee was a significant part of the change from the “old politics of science,” which was “predicated on government and scientific sovereignty,” to the “new politics of science,” which was “characterized by the diminution of the de facto sovereignty that pure science had nurtured though the postwar period.” It would seem that the Fountain Committee had struck deep at the heart of the social contract for science.68

Strickland (1972:173) disagrees, suggesting that Fountain’s inquiry “was not a lethal blow to the Cause [of independent biomedical research], by any means, but for a time it was staggering.” I have to agree with Strickland, particularly when it comes to evaluating the impact of Fountain’s inquiries on the social contract for science, which specifies neither a specific nor relative level of funding, nor does it free scientists from the obligations of good financial practice. Although both Steelman and Bush recognized in their reports that some administrative practices might need to be tailored to accommodate scientific practice, neither suggested that even scientists should be free from financial accountability. Indeed, Bush (1980 [1945]:33) wrote into one of his five principles that “[t]he usual controls of audits, reports, budgeting, and the like, should, of course, apply to the administrative and fiscal operations of the [proposed National Research] Foundation, subject, however, to such adjustments in procedure as are necessary to meet the special requirements of research.” Creating the new paperwork requirements did not provide a handle directly on scientific integrity, but

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68 Greenberg (1967:273) continues to say that in his inquiries, “Fountain was chopping at the ventricles of the economic system that pure science had laboriously assembled in the postwar period, for, in effect, he was contesting the traditional scientific view that the internal value system of science guaranteed an ethical standard that required no outside surveillance or reinforcement.”
merely allowed Turner’s (1990a) financial “metonym” to take on a more substantial form. Loose administrative practice was at odds with the social contract for science, not Fountain’s inquiries.

But Fountain stirred up hope as well as trouble for the biomedical research community. In what may have been a direct response to Fountain’s inquiries (Strickland 1972), President Kennedy directed in the summer 1963 that a study be made of NIH operations. The study, led by Dr. Dean E. Wooldridge, saw its primary purpose as investigating the quality of NIH research, but it also saw as cardinal points the examination of “[a]dministrative procedures used to disperse [sic] and control NIH funds” and the “[a]dministrative relations between and recipient or contractee institutions” (Wooldridge 1965:xv).69 After meeting with more than 600 NIH funded scientists and 150 administrators, the Wooldridge Committee concluded sunnily “that the activities of the National Institutes of Health are essentially sound and that its budget of approximately one billion dollars a year is, on the whole, being spent wisely and well in the public interest” (Wooldridge 1965:1). The report found that “[d]espite the tenfold increase in NIH support of research during the last eight years, there is no evidence of over-all degradation in quality of the work supported. On the contrary, there is good evidence that the average quality is steadily improving” (Wooldridge 1965:3). It also assured that “NIH funds and equipment allocated to a project are ordinarily well related to the task to be accomplished, and the funds are expended with care and probity by the investigator” (Wooldridge 1965:4). Having reviewed a program that has grown even to this extent, the report further concluded that “continued growth appears to be imperative” (Wooldridge 1965:6).

Two years after the Wooldridge Report seemed to vindicate NIH, Fountain investigated NIH anew. He not only questioned NIH financial management, but also

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69 Given the question of the quality of NIH financial controls, the substitution of the word “disperse” (from the Latin, “to scatter”) presumably for the word “disburse” (from Latin for “out of the bag”) is a humorous one.
the quality of some of its grant awards. His October 1967 report charged NIH with “weak central management,” geographically elitist funding, “inept handling” of indirect cost reimbursement, and a general “laxity” in administration. He also attacked the quality of scientists and research projects, saying that NIH spent too much money on too many projects. Fountain concluded that Congress was still over-appropriating for NIH.

Despite Fountain’s harsh conclusions, Congress as a whole took comparatively gentle action given the larger context of a “crisis” in government-science relations spurred by conflicts over military R&D, the role of basic research, and the increasingly apparent guns-and-butter problem wrought by deepening involvement in Vietnam (U.S. Congress 1986a). It was also the case that appropriations subcommittee chairman John Fogarty had passed away in 1967, depriving NIH of its man in the House. In the appropriations battle for FY 1969, President Johnson and the Bureau of the Budget cut less from the NIH request than they had in previous years. The House trimmed the request by $40 million, but Lister Hill, still active in the Senate, replaced the $40 million and added $10 million more. The final appropriation, however, fell between the Senate and House figures, a $20 million cut (Strickland 1972:222). Some of this cut, however, was undoubtedly attributable to the $8 billion budget cut required by Congress in exchange for Johnson’s Vietnam War tax increase rather than to Fountain’s attacks. The three largest government R&D programs suffered cuts in FY 1969: defense R&D down 3.2% in current dollars; space down 10%; and health down the smallest increment of the three, 2.9% (NSB 1973).71

70 In particular, he pointed to a five-year “moral commitment” to a block grant to Sloan-Kettering Cancer Institute (Strickland 1972:219). In addition to the problem of promising funds that had not yet been appropriated, Fountain pointed out that the quality of research at Sloan-Kettering did not appear to be “universally high” to warrant the block grant, as 20 of 34 individual grant applications submitted over the previous two and one-half years had been rejected (Strickland 1972:220).
71 There is some evidence that real DHEW basic extramural research was the site of larger relative cuts, however. DHEW basic research lost 11% in real terms, the largest percentage of major programs, with NSF second at 6.5%. But this decrease in current dollars was $26 million, or $6 million more than the NIH cut reported by Strickland which is attributable to other DHEW research agencies that constitute less than one quarter of the DHEW basic research program. Furthermore, DHEW basic intramural
Strickland declares 1968 the "end of an era." Fogarty was a year gone, and Fountain was seemingly having an impact on the budget. The year was not only Johnson's last, with a Republican on his way to the White House, but Senator Hill decided not seek reelection after 45 years in Congress. NIH director Shannon also retired with the change in administrations. Strickland's assessment is more apt than Greenberg's, although given the budgetary context and its impact on other R&D, I think he overestimates the impact of a $20 million cut from a wartime budget. The era Strickland sees ending was the pre-reform, pre-Watergate Congress dominated by appropriations. This era would completely pass with the election of the Watergate class of 1974. Yet even with these demographic changes, NIH funding would recover and continue to grow, if only in small real terms, through most of the 1970s; and NIH would have a budgetary renaissance in the 1980s.

The turnabouts, from Fountain to Wooldridge to Fountain to later renewal, suggest that the technique used by Greenberg and Strickland of assessing the overall relationship between Congress and NIH, or for that matter between Congress and science, by appropriations is not a precise enough measure. Appropriations is a blunt instrument to achieve qualitative control for Congress (and for qualitative analysis for scholars) because it is so dependent upon other exogenous factors and because it can be so easily manipulated by other contemporary and future actors. For Fountain, however, there was no legislative action that could be taken against NIH from the Government Operations Committee other than stirring up trouble and hoping for a budgetary impact (Henderson 1970).

The challenges to NIH accountability in the 1960s threatened but did not disrupt the social contract for science because the only members of Congress who might have wanted to institute a more controlled relationship lacked the institutional means to do

research increased in real terms $12 million or 21%, so there was some reprogramming occurring as well. Again, some of this increase was undoubtedly for intramural research other than NIH. Nevertheless, given the larger budgetary climate, this evidence still seems weak to conclude that Fountain signalled a major change.
so. Fountain’s attention perhaps encouraged NIH to rise to the expectations of financial integrity of any agency—expected even by Bush. But without the ability to intervene in any more permanent way than by spooking the House into making incremental cuts during times that were tough anyway, Fountain did not damage the social contract for science.

d. congressional organization and scrutiny

The mid- to late-1960s, however, did begin some changes in the relationship between science and government most broadly conceived. The Kennedy and the Johnson Administrations often embodied a heady instrumentalism in their programs and approaches. Broad-based cultural critiques, some trenchant and some trivial, tempered the technological enthusiasm in American culture that had driven Bush and Steelman. But focusing on the cultural trends misses what was happening at the organizational and programmatic level, as members of Congress tried both to reassert their policy prerogatives in areas of science policy that were increasingly falling under executive sway, and to rein in and redirect an expanding scientific bureaucracy.

In an effort to “strengthen their body’s role in formulating scientific research and development policy” (U.S. Congress 1986a:48), members of House of Representatives created a Select Committee on Government Research. Carl Y. Elliott (D-AL) led the effort and became the chairman of the Select Committee, which was established in September 1963 and held a number of hearings and published ten reports during the 88th Congress. The Select Committee was disbanded after the moderate Elliott suffered defeat in his 1964 primary, but it did leave a jurisdictional impression. It had recommended that the Congress establish a Joint Committee on Research Policy

72 Smith (1990:ch.4) calls it the beginning of “policy disarray.” For U.S. Congress (1986a:57), it is a “crisis.” Kevles (1978:chs.24 and 25) describes a “new revolt against science” that led to “a degree of disestablishment.” As noted above, for Strickland (1972:ch.10) it is the “end of era.” For views closer to the events, also see Brooks (1971).

73 John Fogarty, the health appropriations subcommittee chairman, was a member of the Select Committee as well. Kevles (1978:413) compares the Elliott Committee, as a “full-scale investigation into federally funded science” to the Allison Commission of the 1880s.
as a counterweight to executive expertise in the Office of Science and Technology (OST) and the rest of the President’s science advisory apparatus.\textsuperscript{74} The thought of such centralization aroused the territorial instincts of the legislative committees responsible for the fragments of science policy. The House Science and Astronautics Committee had established a new Subcommittee on Science, Research, and Development in response to the plans for the Select Committee, and the House Armed Services Committee also created its own science subcommittee (U.S. Congress 1986a; U.S. Congress 1980d).\textsuperscript{75} In parallel to the new committee structures and at the urging of the new Science subcommittee, the Legislative Research Service created its Science Policy Research Division in 1964 (U.S. Congress 1980d).

The Subcommittee on Science, Research, and Development, headed by Emilio Daddario (D-CT), became active in a number of reviews and innovations in science policy. Most prominent were several reports requested by Daddario of the National Academy of Science’s Committee on Science and Public Policy (COSPUP),\textsuperscript{76} which began to address in a more contemplative manner the issues of science and government.\textsuperscript{77} Daddario also worked with Senator Edward Kennedy (D-MA) to revise the NSF charter in 1968. The revision led to increased efforts in the social sciences and in applied research (Wilson 1983; U.S. Congress 1980d) as well as to an annual reauthorization cycle at NSF (Smith 1990:79).

It was not just Congress that was trying to come to terms with the federal role in science organizationally and informationally. The agencies supporting research, in

\textsuperscript{74} The proposed Joint Committee on Research Policy was explicitly compared to the Joint Economic Committee, which was viewed as a counterweight to the Council of Economic Advisors. Compare this institutionally driven innovation with Bimber’s (1992) account of the creation of the congressional Office of Technology Assessment (OTA).

\textsuperscript{75} The Senate created a new Government Research Subcommittee of the Government Operations Committee to help scrutinize federal programs.

\textsuperscript{76} COSPUP had been created by the Academy in 1962 as the standing Committee on Government Relations and renamed COSPUP in 1963. Its creation was intimately associated with OST and the activities of George Kistiakowsky, who became the first chairman of COSPUP (Cochrane 1978).

\textsuperscript{77} The reports were Basic Research and National Goals (1965), Applied Science and Technological Progress (1967) and Technology: Process of Assessment and Choice (1969). The last of these three reports was influential in the creation of the congressional Office of Technology Assessment (OTA).
particular the Department of Defense (DOD) and NSF, began to examine the connection between the support of basic research and technological products. DOD instituted Project Hindsight, which attempted to discover, from current critical weapons systems, the character of the innovative "events" that had led to them.\(^78\) Hindsight concluded that only nine percent of the 700 events identified could be classified as scientific rather than technological, and that only two events in total could be classified as basic research.\(^79\) The conclusions of Hindsight were punctuated by the infamous Mansfield Amendment to the 1970 military authorization bill, forbidding the use of military funds to support research unless it has "a direct or apparent relationship to a specific military function or operation" (quoted in U.S. Congress 1986a:62).\(^80\) A successful coalition of the ends--the liberal Mike Mansfield (D-MT) who was trying to reduce military-university tensions, with conservatives who were skeptical of the military impact of basic research in any event--the coalition fractured and the amendment was watered down in the next year's authorization.\(^81\)

NSF also engaged in a retrospective of the relationship between science and technology, entitled *Technology in Retrospect and Critical Events in Science*, or TRACES.\(^82\) Its conclusion was more "supportive of the assembly-line view" of the

\(^78\) There are many brief secondary accounts of Project Hindsight, including U.S. Congress (1986a:59-60) and Mansfield (1968:177-78). The project issued a interim report in 1966, published an article based on that material in 1967 (Sherman and Isenson 1967), and a final report in 1969.

\(^79\) These conclusions engendered a great deal of controversy both over the methodology of the study, the twenty year time horizon of which was deemed too limited to uncover the real contribution of basic research, and over its findings, which seemed contrary to the premise of the social contract for science that basic research would in fact lead to technological payoff.

\(^80\) According to a DOD review, four percent of its 6600 projects would be affected (U.S. Congress 1986a:62). See also Smith (1990:81-82).

\(^81\) Robert Ketcham helped draft the Mansfield Amendment. He recalls that scientists were up in arms about it, and that their reaction might have indicated that it was breaking the social contract for science. Ketcham was also involved in drafting some of the key technology policy legislation of the 1980s, including the Bayh-Dole Act of 1980 and the Federal Technology Transfer Act of 1986. Interview with Robert Ketcham, former general counsel, House Science Committee, Chevy Chase, MD, January 17, 1992. Former presidential science advisor D. Allan Bromley (1988:20) speaks of a "Ghost Mansfield Amendment" that, after the original amendment had been watered down, percolated down to DOD program officers and caused them to refrain from funding irrelevant basic research should some future Mansfield Amendment be reinstated.

\(^82\) TRACES employed a methodology different from Hindsight, tracing the key innovations from a variety of new civilian technologies fifty years back. These included magnetic ferrites, birth control pills, video recorders, and electron microscopes (U.S. Congress 1986a:60).
role of basic research that had been articulated by Bush (Wise 1986:233). But NSF also suffered a short-term programmatic change in the wake of congressional change and the scrutiny of TRACES. Following the charter newly amended by the Daddario Subcommittee to include applied research, NSF began a program called Interdisciplinary Research on Problems Relevant to Our Society (IRPOS), which was expanded and reestablished in 1971 as Research Applied to National Needs (RANN). Funds under RANN constituted 8.6% of NSF money in 1972 and rose to 13% in 1974; their share then fell to 10% in 1976 (Smith and Karlesky 1977:34) and in 1978 the program was discontinued (U.S. Congress 1986a:68). Although the academic research community was initially uncomfortable with RANN as an applied research program, it gradually became accustomed to both the new money involved and the fact that it was distributed in much the same way as other NSF money. 

NIH also received programmatic impetus toward supporting more applied work, and this also followed an attempt to increase information about science and scientific productivity for policy analysis. In 1970 Congress amended the Public Health Service Act to set aside up to one percent of the funds to a program authorized under the Act for program evaluation. NIH has since maintained several computerized databases to assist in such evaluation (OTA 1986). Although not causally related to the move toward evaluation, NIH was also moving toward more socially relevant work with the advent of the Nixon Administration’s War on Cancer. The Johnson Administration had tried to reorient NIH in a more applied direction (Lambright 1985), but the Nixon Administration succeeded by allying with congressional liberals like Senator Kennedy and with the disease lobby in mobilizing the biomedical resources at NIH for a crusade against cancer (Rettig 1977). Like RANN, the War on Cancer

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83 The assembly-line view, also known as the linear model, is a view of the connection between basic research and technological innovation as an unproblematic, direct and automatic flow. See II.C.3 and V.B.2.

84 The newly-organized Office of Management and Budget had a role in the expansion of RANN, as NSF basically exchanged some programmatic control for larger budgets (Wilson 1983:34-38). Also see Smith (1990:79); U.S. Congress (1980d:500-06).
consisted of a large amount of money for an applied goal and was opposed by the relevant scientific community. In this case, the funding mechanisms would favor contracts over grants and clinical research over basic research; but this was only a shift in emphasis and not a rejection of grants or basic research. As with RANN, although the community initially protested, scientists took the money, performed the research, and then came to accept the program as it grew familiar.\textsuperscript{85}

Hindsight, TRACES and program evaluation at NIH were symptomatic of a discomfort--general in society and specific in Congress--about the connection between scientific research and beneficial technologies. In an effort to ease the symptoms, Congress attempted to make some programmatic changes that emphasized mission-oriented and socially relevant research. My claim is not that the programmatic changes were directly and causally related,\textsuperscript{86} and in fact they are probably both dependent on the larger cultural context and the organizational changes in Congress. Rather, my claim is simply that new ideas and new tools were being tried. The tactics used were different from the attacks by Fountain. Instead of simply targeting the size of the research budget, the Mansfield Amendment, RANN, and the War on Cancer tried to direct scientists programmatically to produce relevant research. By directing scientists from above, these mechanisms did not invade the internal reward system of the scientific community, but they did presage the more complete set of instructions and incentives provided by the technology policy of the 1980s (see chapter V). These changes were temporary--very temporary in the case of the Mansfield Amendment. The scientific community eventually took to both RANN and the War on Cancer. These changes did show how the new organization of Congress was responding to the challenges of governing scientific programs in more direct ways. They also show that science itself

\footnotesize{\textsuperscript{85} A similar pattern seems be emerging with funding for the human genome initiative. For a history of the development of the initiative and its funding mechanisms, see Cook-Deegan (1991). \textsuperscript{86} Indeed, none of the secondary literature makes any causal claims here either. Pursuing an explicit connection between the episodes of analysis and policy change--as I attempt to do to some extent for the 1980s--could be an enlightening exercise.}
was subject to study and that attempts could be made to scrutinize the technological promise of the social contract for science. But they did not constitute a break in the social contract for science.

e. research risks and the limits of scientific inquiry

Another challenge to the social contract for science was presented by the concern over research risks, particularly those from new recombinant DNA techniques, in the 1970s. This concern was expressed in a set of essays published by the American Academy of Arts and Sciences in 1978 and later as a separate volume, *Limits of Scientific Inquiry* (Holton and Morison 1979).87 Self-consciously titled in counterpoint to *Science: The Endless Frontier*, the *Limits* volume may have done as much to disseminate the language of the social contract for science as it did to describe its possible renegotiation.88 Likewise, June Goodfield's (1977) *Playing God* frames the issue of identifying the risks of recombinant DNA and beginning to regulate them in terms of a social contract between society and science.

The primary concern of the *Limits* volume and Goodfield's *Playing God* is the nature of risks from research and the reciprocal impact that understanding these risks has on science and on society. In this sense, the concern has expanded somewhat from how I have described the interest of political science in science: expanded from the impact or bearing of science on politics to include the impact of the response of politics to science. This concern is also more nuanced than budgetary levels. It was not new in the 1970s, having begun for physicists in the dual concerns for atomic weapons secrecy, and for life scientists and physicians as early as the war with concerns over the conduct of Nazi doctors.89 The most contentious of these concerns, however, erupted

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87 Other important views of the period include Weiner (1993 forthcoming); Wright (1993); Fredrickson (1991); Krimsky (1982); Goodfield (1977); and Rogers (1977).
88 As, in fact, I fear I may be doing.
89 In 1947, the war crimes tribunal at Nuremberg established a list of ten precepts governing the use of human subjects. This Nuremberg Code served as the basis for the Declaration of Helsinki of the World Medical Association, published in 1964 (Swazey 1979). In the U.S., Senator Walter F. Mondale (D-MN) had been calling for a commission to investigate the use of human subjects in research as early as 1969 (Culliton 1979). These concerns were crystallized in the U.S. by revelations of federally funded
over new techniques in molecular biology pioneered in the early 1970s that led to the ability to recombine genetic material from different organisms in one organism. These recombinant DNA techniques, or genetic engineering, offered prospects of the molecular control of disease, the mass-production of pharmaceuticals through biological means, and the specific control of heredity. Along with such possible boons, it was recognized, came the specter of accidental epidemics, novel workplace hazards, and eugenics. Biologists began to talk like physicists about having known sin.

Although in retrospect their actions often appear like an effort to circle the wagons, to their credit, some prominent scientists were instrumental in forcing both their colleagues and the public to recognize the dangers as well as the promise of the new techniques. Scientists first raised questions about hazardous experiments in private as early as 1971, and at a small meeting at the Asilomar Conference Center California in early 1973. At the Gordon Conference later that year, researcher Herbert Boyer first described the techniques that he and Stanley Cohen had been developing—the process that would in 1980 be acknowledged in the Cohen-Boyer patent that entrenched the biotechnology industry (see VI.C.1). But the social implications of such research could hardly be contained at the meeting. The attendees at the conference agreed to write a letter describing the techniques and concerns about them to the NAS and the Institute of Medicine (IOM), and they also agreed to "go public" by sending the letter to the journal, Science. Responding to the Gordon Conference letter, NAS decided to establish a study panel and invited Paul Berg, a Stanford researcher intimately involved in developing recombinant techniques and a principal in the 1973 Asilomar meeting, to lead it. Berg gathered a group of scientists, many of whom had

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90 Many of these concerns were visible a decade earlier (see Weiner 1993 forthcoming:6-7), and some, like eugenics, had been of concern for nearly a century (Kevles 1985).

91 The following account relies mainly on Krimsky (1982).
track records in issues of social concern, and the group met at MIT in the spring of 1974. The panel published a set of recommendations (the "Berg letter"), including a moratorium on certain applications of the new techniques thought to display the greatest potential hazardous, and the establishment by NIH of an advisory committee to perform oversight, procedural, and regulatory tasks.

Following the Berg letter, the NAS charged a second meeting to be convened at the Asilomar Conference Center. The meeting, in February 1975, is known generically as "Asilomar" and it stands as something like a benchmark of scientific responsibility, although like all history its interpretation is contested. What is agreed upon is that, as Krimsky (1982:99) writes, "No single event has had more impact on the public-policy outcome of rDNA research than this four-day conference. It was a key point in the transition...from informal scientific to formal governmental channels." The details of Asilomar are far too numerous to relate here, but it is clear from Krimsky's (1982:99) account that scientists at Asilomar were engaged in complex and public "boundary work."

"The objective was not necessarily to close off debate and discussion on other aspects of the research--the social, ethical, and political considerations, for example--but only to separate those issues from the task of establishing the conditions under which the research would be conducted." But in the recommendations that the conferees agreed upon:

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92 Krimsky (1982:84) reports that it was not until after the panel drafted its recommendation that NAS officially sanctioned its activities.
93 See III.D.1 for use of Asilomar as just such a benchmark by congressional staff judging the response of the scientific community to scientific misconduct.
94 Boundary work is the distinction by a community of itself and the issues over which it wants jurisdiction, from other communities and other issues (Gieryn forthcoming; 1983; Jasanoff 1992; 1990; 1987). See IV.B. for a more complete discussion of boundary work.
95 Indeed, one of the most impressive presentations for the scientists at Asilomar was the presentation of Indiana University legal scholar Roger Dworkin, who painted an image of jurisdiction over laboratory procedures being exercised by the Department of Labor, to protect lab workers, rather than NIH (Krimsky 1982:142).
96 Krimsky describes several times how the scientists present at the meeting voted to decide several issues, but he does not mention if voting rules (e.g., quorum, majority, supermajority, unanimity) were ever a problem for them. Fredrickson (1991:281-82) mentions, however, that one scientist--who must have recognized the influence of agenda control--asked if the group would get to vote on each paragraph of the statement. Fredrickson also hints at some use of power by Berg, who was the chairman calling for the vote. For the nonobviousness of voting rules, see Dahl (1989).
the organizers of Asilomar had accomplished two objectives: (1) they
defined the issues in such a way that the expertise remained the
monopoly of those who gain the most from the technique, and (2) they
chose to place authority for regulating the use of the technique in the
agency that is the major supporter of biomedical research in the United
States (Krinsky 1982:153).97

In October 1974, a few months after the Berg letter and still several months before
Asilomar, DHHS Secretary Caspar Weinberger implemented the scientists’
recommendations and created the Recombinant DNA Molecule Program Advisory
Committee (later the Recombinant DNA Advisory Committee or RAC) in NIH. The
first meeting of the RAC was held on the tail of Asilomar in February 1975.

What might be apparent here is the wealth of scientific activity and the dearth of
political activity up until this point. The first congressional hearings on the recombinant
DNA issues were held by Edward Kennedy, chairman of the Health Subcommittee of
the Senate Committee on Labor and Public Welfare, late in April 1975. RAC meetings
continued through 1975 and in July 1976 DHSS published the Guidelines for
Recombinant DNA Research in the Federal Register. City council hearings in
Cambridge, Massachusetts in the summer of 1976 vented the issues of the containment
of hazardous experiments and the role of public participation in decisions about siting
hazardous research.98 In September 1976, Kennedy held a second set of hearings
jointly with the Judiciary Committee. President Ford also established a Federal
Interagency Committee on Recombinant DNA Research that met for the first time in the
fall of 1976.

Scientific meetings continued through this period, and congressional activity
accelerated through 1977 as NIH revised its guidelines and an environmental impact

97 Susan Wright (1993) supports this interpretation, suggesting that the problem was constructed as a
public health problem rather than a moral or ethical problem, and therefore a problem of scientific and
not political solution.
98 The City Council imposed a moratorium on the research, causing worries that patchwork local
regulations would complicate research and increasing pressure for some national legislation.
Cambridge overturned the moratorium, however, and in doing so relieved some of this pressure.
statement that it had also published on the impact of the guidelines. But counter to expectations, and although some dozen bills were introduced to regulated recombinant DNA activities, there was no legislation (Krimsky 1982:312-13). Lack of support from the outgoing Ford Administration and even the incoming Carter Administration complicated congressional efforts,\textsuperscript{99} and the breadth of the issue, involving not just health jurisdictions but environment, judiciary, and science in general, caused damaging congressional fragmentation. But the primary reason seems to be that, as illustrated by the great deal of scientific activity prior to the initiation of congressional involvement, the opponents of legislation were better organized and better mobilized than the supporters of legislation.\textsuperscript{100} Despite initial expectations, Congress passed no legislation to regulate scientific activities on recombinant DNA because, whether accurate or not, the biomedical community made the case that it could regulate itself.\textsuperscript{101}

D. Conclusion

In the midst of the changes of the 1980s that I will argue changed the social contract for science, the House Science and Technology Committee, the under the chairmanship of Don Fuqua (D-FL), organized the Task Force on Science Policy. Fuqua himself directed the Task Force, which drew its membership from a large subset of the full committee. Over the course of the 99th Congress, the Task Force produced twenty-four volumes of hearings and twelve background reports in an attempt to

\textsuperscript{99} For example, in January 1973 the outgoing Ford Administration’s Assistant Secretary of Commerce for Science and Technology published a notice in the \textit{Federal Register} announcing the acceleration of patent applications for recombinant DNA techniques if the applicants adhered to the NIH guidelines. Pushed by Senator Dale Bumpers, the new Carter Administration Secretaries of HEW and Commerce agreed that the acceleration would be terminated (Krimsky 1982:317). But many of the Carter Administration officials opposed at least the more stringent bills, if not preferring rulemaking to legislation outright.

\textsuperscript{100} A well-organized effort by the American Society for Microbiology was especially important (Krimsky 1982).

\textsuperscript{101} Congressional inaction, however, was only a penultimate event in the history of recombinant DNA regulation. Under the pressure of a “research sector” represented by increasingly allied academic and industrial scientists, the regulations promulgated by NIH became interpreted as a “handicap” in a technological “race” against technological competitors (Wright 1993:95). After using its power to turn back Congress, the research sector was able to redeploy and succeeding in causing major deregulation in 1978 and 1980 and still further deregulation since then (Wright 1993).
generate a comprehensive view of the research enterprise and help the Committee participate in setting national goals in science policy.\textsuperscript{102} The Task Force was thus somewhat similar to the efforts of the Elliot Committee of 1963 (II.C.4.d).

According to a group of political scientists and science policymakers who gathered in 1988 to discuss it, the Task Force was a congressional activity involved in the renegotiation of the social contract for science, primarily because it examined scientific programs across the research establishment with an eye for their results (Hamlett 1990). Another factor identified by this group, and also briefly engaged by the Task Force, was the question of scientific integrity.\textsuperscript{103} Indeed, in questioning NIH director James Wyngaarden in writing after the hearing, the Task Force asked to what extent the social contract applied to biomedical science. Although his response focused on the productivity of research and did not refer to scientific integrity, Wyngaarden averred that “[t]he reciprocal obligations between biomedical scientists and the society that provides their support can indeed be viewed as social contract” (U.S. Congress 1985a:92).

The discussion group did not come to any definitive conclusions about the social contract for science, and it was largely ineffectual in establishing a research agenda around the idea.\textsuperscript{104} Nevertheless, the group and the Task Force further clarify what the other episodes of challenges to the social contract for science demonstrate, that there is a relationship between congressional organization, information-gathering and

\textsuperscript{102} Of the twelve background reports, seven were prepared by CRS or members of it staff, two were prepared by OTA, one by GAO, and one by a staff member who was at the time a congressional fellow sponsored by the American Historical Association.

\textsuperscript{103} Walter Stewart and Ned Feder, two NIH scientists who investigated some episodes of scientific misconduct on their own, appeared before the Task Force hearing on publication practices in science because they were having difficulty publishing a manuscript that analyzed the ability of the journal peer review system and of existing authorship practices to deter or detect fraudulent practices (U.S. Congress 1986c). At the group discussion, House Science Committee staffer and Task Force study director John Holmfield said of the early 1980s that “[t]here were vague, often unarticulated feelings that the system was not working as it was supposed to. The trigger was concern that come from the emergence of fraud....So some members began raising questions about the scientific community’s ability to self-regulate, to work things out on its own” (Hamlett 1990:38).

\textsuperscript{104} Personal communication with Patrick Hamlett (one of the organizers of the group).
oversight, and the bond of the social contract for science. At various times, Congress has attempted to improve its access to and control over the scientific community by making committee- or subcommittee-level changes, either permanent or ad hoc. Congress has also experimented with various kinds of oversight and control mechanisms, exemplified by the Fountain Committee's gadflying and the programmatic direction of the War on Cancer. Although these episodes verged on the issues of scientific integrity and productivity that are so crucial for the social contract for science, they brought neither a recognition of the politics internal to the workings of science nor a mechanism for monitoring or inducing appropriate scientific behavior. That is, even if the interests of the congressional committees and the scientific community appeared to be diverging at times from the postwar consensus, putting strain on the relationship, the committees had not yet invoked the firmest mechanisms of control described by principal-agent theory.

Without the invocation of these mechanisms, the social contract for science that had been articulated by Vannevar Bush and John Steelman—the belief in and reliance on the integrity and productivity of science—remained unaltered. The consensus that they described was rooted in an articulation of a self-regulating republic of science whose ultimate provision of technological benefits was predicated on segregation from political and commercial pressure, and was immediate from the production of good science. The social contract for science, that the political community agrees to provide resources to the scientific community and to allow the scientific community to retain its decision-making mechanisms and in return expects forthcoming but unspecified technological benefits, together with its premises of self-regulation and unproblematic technology transfer, endured from its postwar articulation to be included in the vocabularies of science policy analysts and science policymakers, as well as to be enabled in the institutional relations between Congress and the scientific bureaucracies such as NIH.
But the boundaries established by the social contract for science, necessarily established in the absence of a true laissez-faire policy, have been recurrently pushed and tested. Sometimes these episodes have taken on crisis proportions, like the Fountain Committee whose reputation for causing problems still remains part of the collective memory at NIH, and like challenges posed to the limits of inquiry by new recombinant DNA research in the mid-1970s. The reason these crises are recurrent—that boundaries of the social contract for science are somewhat plastic and that the old politics of science turns into the new politics of science—is that politics and science are similarly expanding and interpenetrating enterprises. Tocqueville (1969 [1848]) described the march of democratic equality through the ages and into the American polity; like Bush and Steelman after him, he prescribed insulating basic research lest it be overcome by a levelling democratic culture.\textsuperscript{105} One step in this march is the pursuit by elected representatives in Congress of their goals and their creation of both an expansive set of executive agencies to increase the instrumental success of government, as well as an expansive set of internal and marginal institutions to control the executive agencies. Latour (1988) described the expanding laboratory of French biologist Louis Pasteur; Pasteur in effect expanded the boundary of his laboratory in an increasing set of contexts to enroll allies in his scientific enterprise. A domestic example is the creation by biologists of new techniques that encounter preexisting political conditions, precipitating political change in a wide array of contexts from the regulation of scientific inquiry to medical care to ethics.

Each expanding enterprise inevitably runs up against the other. But the meetings will not be cataclysmic, as both are expansive precisely because they are mutually dependent on instrumental and ideological resources from the other (Ezrahi 1990). Some democratic politics will infiltrate into science, and some science will creep into politics. Some scientists will be hired or coopted by politicians; some

\textsuperscript{105} For details of Tocqueville’s view, see Guston (1993).
politicians will be enrolled by scientists. During the course of this reciprocal process, as I will describe in the following chapters, members of Congress have noticed the ambitions, profits, and lusts—the politics—that is internal to science. They have questioned the premise of self-regulation and instigated the creation of formal mechanisms to manage the integrity of science. They have questioned the premise of immediate technology transfer and have mandated formal incentives to encourage it. In each instance, the response of the biomedical community has limited or moderated the influence of congressional action. But each case also demonstrates that Congress has obtained the information and created the policy instruments to take full advantage of their position as principal to their scientific agents. Each case also demonstrates that the social contract for science has been renegotiated, and that the boundary between politics and science—marked and remarked by new groups of scientists and nonscientists—is even more plastic than before.
Chapter III. The integrity of science, one: creating OSI

A. Introduction

The contemporary institutional relationships of science policy were established in the immediate postwar period as a plural system in which a number of research agencies funded and conducted research and a number of congressional committees oversaw the enterprise. A common rationale yoked the elements of the plural system together, however. Articulated by such diverse spokespersons as Vannevar Bush and John Steelman, the integrity and productivity of science were central to this overarching rationale. The shared belief that a self-regulating scientific community would maintain the integrity of its members and would provide the scientific inputs necessary and sufficient for technological innovation and economic progress indicated aligned objectives toward policies for science that maintained a separation between politics and science, despite new and large amounts of federal funding. Such aligned objectives precluded the need for formal monitoring or incentives to mold the relationship and assure the integrity and productivity of science.\(^1\)

But in the 1980s, this situation changed. Congressional hearings at the beginning of the decade highlighted some cases of data falsification by scientists receiving federal support and pointed out some modest changes that could be made to reinforce the self-regulating system. But as cases of misconduct by scientists, and incidents of scientists and universities handling such cases with extreme self-interest and apparent disregard for professional norms, accumulated, some members of Congress became more aggressive in their application of pressure on the scientific community. The pressure bore greatest on the National Institutes of Health (NIH), which not only encountered one of the most vexing cases of alleged scientific

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\(^1\) Pratt and Zeckhauser (1991:14-15) describe how the understanding of the "alignment of objectives" can mitigate the need to invoke formal mechanisms for controlling agents. Examples they provide include family businesses, religious cults, and student-athletes seeking physically challenging summer jobs.
misconduct, but was also subject to the exacting oversight of Representative John Dingell, perhaps the most probing investigator in Congress. This chapter explains the progress of the issue of scientific integrity from one in which, despite some obvious problems, members of Congress seemed satisfied to continue to rely on the professional norms of the scientific community, to one in which members felt that they had to intervene more substantially.

The primary concern of this chapter, however, is explaining one of these more substantial interventions: the creation of a formal institution to monitor and assure the integrity of biomedical science. Explaining the creation of this institution—the Office of Scientific Integrity (OSI)—poses an interesting challenge to the study of congressional-bureaucratic relations in general and principal-agent theory in particular because it involves what seems to be a clear-cut case of congressional influence without legislation. This chapter explains why NIH created OSI despite the fact that Congress failed in its attempt to legislate a similar office. This explanation reveals a bureaucratic strategy that is a "pre-emptive" response rather than the "anticipated reaction" described in the standard principal-agent accounts (e.g., Calvert, Moran and Weingast 1987). In providing this explanation, I also point to the impact that previous changes in congressional-bureaucratic relations have had on the case of scientific integrity, and I make a modest methodological critique of some applications of principal-agent theory.

The creation of OSI also points to the possibility of the demise of the social contract for science because of the failure of scientific self-regulation. Recall that according to the market model of scientific self-regulation, scientific integrity is efficiently managed by scientists alone, largely through the micro-level application of scientific judgment to transactions in the credibility cycle (see II.C.3.c). Instances of scientific misconduct that are not efficiently managed undermine this implicit model and thereby challenge the social contract for science. If the scientific community is not self-regulating, then there is no basis for confidence in the delegation of authority made by
the political community, and there is a threat to both the material and ideological support provided to the political community by science (Ezrahi 1990). Since the authority is delegated by political actors who can be held accountable (electorally and otherwise) for their actions and the actions of their agents, the definition of "efficiently managed" can be a political and not a scientific question.

Thus, the discovery of failed self-regulation and the creation of a formal monitoring institution threatens the existence of the social contract for science. But if one takes a perspective on the relationship between politics and science that Karl Polanyi (1957 [1944]) encourages through his description of the relationship between politics and the economic market, the newly created institution need not be only a threat to the social contract for science. It could also be an attempt to reconstruct and enhance the integrity of science. Indeed, this was part of the motivation of members of Congress in instigating and NIH in creating OSI. Even though it is contrary to the free market idea of science, the creation of OSI is not enough to assess the status of the social contract for science. Neither is it enough to assess the extent of congressional influence. As I show in the subsequent chapter on the inner workings of OSI and questions of the definition and adjudication of scientific misconduct, both are influenced by the details of implementation.

This chapter begins with an overview of how I apply principal-agent theory to the cases of scientific integrity and productivity, including the problem of congressional control without legislation, the methodological innovation, and the new description of bureaucratic pre-emption. The second section of the chapter describes initial congressional attention to the problem of scientific integrity and how the involved members of Congress tried to keep NIH and scientists responsive to their professional norms, and thus preserve the social contract for science. The third section explains the ultimate failure of this approach, based largely on the discovery by congressional investigations of episodes of the failure of professional norms, and the existence of
politics and power relations within science. It subsequently explains the preemptive response by NIH in its creation of OSI.

B. Congressional oversight and the social contract for science

1. Principal-agent analysis

In describing the general delegation from the political community to the scientific community, the social contract for science corresponds to a tool for the empirical analysis of specific delegatory relationships (II.B.3.b). This tool, principal-agent theory, is also known as “ideal contracting,” because it uses a contractual perspective on organizational relationships—like that between a congressional committee (principal) and a bureau (agent)—to model behaviors and outcomes. I apply the principal-agent model to the cases of scientific integrity (chapters three and four) and scientific productivity (chapters five and six) to explain congressional and bureaucratic activity and institutional innovation.

Principal-agent studies can be traced back at least to William Niskanen (1971), whose *Bureaucracy and Representative Government* stands as a neoclassical translation of the Weberian warning against the “overtowering” bureaucracy (Weber 1946:232).\(^2\) Niskanen motivates the political economic assumptions of methodological individualism and utility-maximizing behavior to reduce the congressional-bureaucratic relationship to a “bilateral monopoly,” in which a single bureau promises a set of expected outputs in exchange for a budget granted by a single sponsor committee.\(^3\) The crucial characteristic of Niskanen’s bilateral monopoly is an asymmetry of information between the agent and principal: the bureaucrat needs little information, most of which can be garnered from the revealed preferences of legislators; the sponsor, however, needs information to set a budget, but has little access to such

\(^2\) Niskanen (1971:4) distinguishes himself from Weber by his own “instrumentalist” view of the state, as opposed to Weber’s “organic” view.

\(^3\) But as Pratt and Zeckhauser (1991) and Moe (1984) make clear, principal-agent theory does stand in contrast to political economic methods that make assumptions of perfect information.
information because there is no reference market price for bureaucratic services. Bureaus therefore command budgets larger than those that, under complete information, would provide the greatest net benefit. That is, bureaus overconsume and overproduce.

Because the principal needs the agent to produce things generally underproduced by markets—e.g., the public good of scientific knowledge upon which technological growth is supposed to be predicated—the principal is assumed to be relatively ignorant of how and at what cost such tasks are performed. Further, the agent may not share the goal espoused by the principal, but might instead prefer to conduct research on anything interesting or personally lucrative, regardless of its technological potential. Or the agent—because of the principal’s ignorance and the agent’s desire for reward—may in fact be the wrong agent to accomplish the goal. In the argot of principal-agent theory, the problem of the integrity and productivity of research performance is a problem of “moral hazard” or “hidden action.” The problem of identifying scientists to perform research with integrity and productivity is “adverse selection” or “hidden information.” Both problems are involved in the cases of misconduct and technology transfer, but for the most part I will focus on the moral hazard aspect of research performance.

Principals can manipulate institutions and incentives to align the agent’s goals with the principal’s own (Niskanen 1971:ch.18). For example, structural changes can increase competition among bureaucracies in producing similar outputs. Multiple bureaucracies fracture the bilateral monopoly on one side, increasing the availability of

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4 See also Turner (1990a) and Tullock (1966).
5 Arrow (1971) describes how both problems are derived from problems in the insurance industry, in which, for example, the provision of fire insurance creates a “moral hazard” or “hidden action” problem by providing an incentive for the commission of arson, and the provision of health insurance creates an “adverse selection” or “hidden information” problem by attracting exactly those people who are most likely to require the insurance benefits provided.
6 In Turner’s description of the patronage relationship, the patron controls the scientific recipients through personal relationships, public attestations by the scientific community, and by “certain metonyms” such as financial accountability that are symbolic of the relationship and comprehensible to the patron (Turner 1990a). See also the discussion of the Fountain Committee’s inquiries in II.C.4.c.
information about bureaucratic production costs and putting budgets at risk if one bureau lags in comparison to others. Although Niskanen argues that such competition does not directly reduce bureaucratic overconsumption, it does increase the chance that one bureaucracy will “end-run” around the committee principal to other committees or the legislature at large, thus breaking the bilateral monopoly at the legislative end. Changes in incentives can encourage bureaucrats “to maximize, not the total budget, but the difference between the obtainable budget and the minimum total costs of the service” (Niskanen 1971:201). Niskanen therefore recommends reward systems to encourage agents to cut costs while maintaining output, such as rewarding thrifty bureaucrats with a portion of budgetary cuts (residuals) they make. Such bureaucrats would hunt for waste, fraud and abuse, and they would share in any amount of money they saved their principal. This residual-sharing scenario is the logic behind the institution of Inspectors General in all the cabinet departments and many of the independent agencies.

The relationship described by principal-agent theory is similar to that described by the social contract for science, but on a lower level of abstraction. As applied here, principal-agent theory generally pertains to congressional-NIH relations and the creation and implementation of explicit mechanisms to shape those relations. This is the first level of analysis as described in the introduction (I.A.2.b). The social contract for science pertains to a broader relationship between society and science, exemplified in particular congressional-bureaucratic relations. It is the second level of analysis (I.A.2.c). Nevertheless, the reciprocal and contractual nature of both is important.

I use the existence of explicit mechanisms predicted by principal-agent theory to determine the status of the social contract for science. Within the social contract for science, the role of institutional changes and inducements is minimal. Instead, the ideologies of the self-correcting scientific community and the automatic flow of basic research to technological progress substitutes for a lack of information about scientific
work. The social contract for science is "social" or tacit because it is based on this shared ideology. However, attempts at control through institutional changes and the direct application of incentives are increasingly visible in the scientific misconduct and technology transfer cases. As we shall see, OSI, as well as more general bodies like the Inspector General (IG) are institutional innovations designed to increase the competitive supply of information about federally funded programs. They are also monitoring regimes that reify by counting and reporting their objects of study, such as scientific misconduct. Similarly, changes in the patent laws induce federally funded scientists to direct their research toward marketable products. When we see such institutions and inducement mechanisms, we can ask if the tacit understanding of the social contract has become explicit and the contract no longer social.

2. "Bureaucratic dominance"

Since Max Weber’s description of the "overtowering bureaucracy," the control of unelected bureaucrats with specialized knowledge by elected politicians without such knowledge has been a problem of continued interest. Niskanen (1975) reviews empirical studies sparked by his theory that support the central claim of bureaucratic overproduction and other more strictly economic conclusions. Although confirming the predicted result of information asymmetry, these studies do not confirm the asymmetry directly. This "dominance" of the bureaucracy is consistent with legislative preference for overproducing bureaucracies and with legislative indifference or incapacity, as well as with lack of information. Thus, the status of information asymmetry is uncertain; other assumptions, e.g., legislator preference (Fiorina 1981), can lead to overproduction. So empirical investigations of bureaucratic dominance must focus on the congruence of assumptions as well as conclusions to reality--what do principals and

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7 Downs (1957) writes of ideology as a substitute for information. Also see the discussion of Geertz in IV.B.1.
8 Both characteristics are important and the tensions between unelected/elected and specialized/unspecialized tend to run in opposite direction. The problem of control is not at all limited to congressional-bureaucratic relations, but also includes what is often known as "responsive competence" of the bureaucracy to the President's control (Moe 1985).
agents know, and what do they want?9 And if information rather than indifference or incompetence is the problem, then empirical investigations should find that the tools for increasing information flow—e.g., the redistribution of residuals from the reduced budgets of bureaus and specialized information services for sponsors—that Niskanen suggests will increase political control do in fact play a crucial role.

3. "Congressional dominance"

The "bureaucratic dominance" of Niskanen is often contrasted with the "congressional dominance" of more recent principal-agent work, which claims that "[c]ongressional influence over the administration of policy is probably just as extensive as Congress scholars have tended to assume" (Calvert, Moran and Weingast 1987:517).10 This literature elaborates the influence of congressional committees three ways: 1) the formidable reward and sanction power of committees, through budget, threat of new legislation, Senate confirmation, etc.; 2) the primacy of the study of Congress, its committees, and theories of legislative choice for understanding the bureaucracy; and 3) the effectiveness of routine casework to produce anticipated reactions from agencies and fire alarm oversight to keep a stubborn bureaucracy in line with legislative goals (Moe 1987).

Moe dispels the force of these claims, arguing for example that: the budget is not a dependable instrument of control because it is too widely shared among committees and between Congress and the President; that threats of new legislation are largely empty because of the difficulty in passing it; and that the confirmation power is secondary at best. He admonishes congressional dominance theorists for presenting not a case for congressional dominance, but instead one for potential influence. The language of the observational equivalence among indirect congressional influence, strict

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9 It is possible, for example, for some bureaucratic agents to prefer programmatic autonomy to increased budgets, e.g., the War on Cancer in its initial phases. See II.C.4.d.

10 Central pieces of this literature include McCubbins (1985), McCubbins and Schwartz (1987), McCubbins and Page (1987), and related work by Weingast and coauthors extending their empirical analysis of the Federal Trade Commission (FTC).
congressional control and bureaucratic dominance reveals this point: "the process of policy administration by autonomous agencies is observationally equivalent to that under strict congressional control" (Calvert, Moran and Weingast 1987:501). Calvert, et al., therefore suggest that studies of the process of influence are misguided, and instead scholars should focus on episodes of change. In their FTC case, they build a quantitative model of a change in bureaucratic policy being driven by a change in congressional preferences, as observed by the bureaucrats.

For Moe (1987:481), the central shortcoming of this work is that it fails to apply principal-agent theory rigorously, making "assertions about congressional control [that] are neither based on a systematic investigation of these analytical dimensions nor logically derived from a principal-agent model." Moe maintains that the principal-agent model describes control as "a two-way street" and that the congressional dominance theorists have only elaborated one direction, that originating in Congress.\textsuperscript{11} Although they are concerned with the consequences of congressional oversight, they have not examined the relationship with the viewpoint and resources of the bureaucracy in mind, treating the bureaucracy merely as a dependent variable.

Moe's criticism seems a little over-wrought, especially of the rather circumspect language in the FTC case. But Moe--and others (e.g., Kelman 1987)--also dispute the empirical evidence that Calvert, Moran and Weingast (1987) present in support of congressional dominance. Controversy has also arisen over the empirical evidence for the congressional dominance of Environmental Protection Agency (EPA) (Cook and Wood 1989). The two controversies over the empirical evidence are revealingly anti-parallel. About FTC, Moe argues that the regression model is underspecified, and it therefore misses elements of presidential control and FTC independence. About FTC, Cook also argues that Wood's model of EPA independence is underspecified, but in

\textsuperscript{11} Applied to Calvert, Moran and Weingast (1987), this criticism is somewhat unfair. Their work is self-conscious in its exclusive focus on Congress as it attempts to respond with a plausible case for congressional influence. They elaborate the congressional direction because others have elaborated the bureaucratic direction.
this case the model omits variables that would indicate congressional influence. Wood acknowledges the criticism, but only to the extent that his model avoids the damaging collinearity he says would fatally flaw a more specified model.

These scholars cannot agree on the variables that should be used to represent the principal-agent model. Therefore, they can neither agree on what constitutes relevant empirical evidence. Such controversies suggest that other methodological approaches are warranted, especially if Moe’s sense of principal-agent theory as a “two-way street” means reciprocal causation or mutual influence, as for example Dodd and Schott (1986) portray it. A method more sensitive to the ebb and flow of influence between Congress and the bureaucracy could help resolve the controversy.\textsuperscript{12} After all, scholars are interested in a congressional-bureaucratic relationship and no relationship monopole exists. Scholars must therefore not limit their exploration to interest group ratings or a tally of the number of agency decisions for, as Wood suggests, these proxy indicators of congressional intent and bureaucratic response measure neither enduring relationships nor causally linked events (Cook and Wood 1989). Such proxies and their statistical aggregation also obscure the actors in congressional committees and agencies--actors understood by principal-agent as intentional and who are therefore worth attending to as individuals.\textsuperscript{13}

4. Principal-agent ethnography

This discussion suggests the following prescriptions for research: case studies of oversight can be guided fruitfully by principal-agent theory by examining assumptions like information asymmetry and strategies like residual redistribution; they must have dual foci in the congressional committee and the bureau; and they must be

\textsuperscript{12} Ogul and Rockman (1990) concur, concluding that an institutional focus beyond the legislature is necessary to understanding oversight.

\textsuperscript{13} "The logic of the principal-agent relationship...is its great advantage; its stylization of facts is its vulnerability" (Ogul and Rockman 1990:21).
wary of methods insensitive to the "two-way street" between the committee and the bureau.¹⁴

My goal, framed in an ambitious way, is to combine political economic theory with ethnographic data.¹⁵ I have interviewed key actors on both ends of the principal-agent dipole and supplemented these with documentary evidence, hearing transcripts, and press coverage.¹⁶ I interrogate the actors and the documents about issues derived from principal-agent theory: questions about information, incentives, and institutional innovation. Such an approach permits an informed reconstruction of the particular episodes of oversight I examine, particularly the administrative impact of the oversight—that is, of oversight as an independent variable.¹⁷ But it does not neglect the possibility of independent action on the part of bureaucrats, either. Indeed, it allows the bureaucrats to speak for themselves about their actions or reactions.

¹⁴ In addition to the methodological argument presented here, one could not provide a statistical analysis of the case of scientific misconduct because the data—bureaucratic output in terms of cases of scientific misconduct adjudicated—did not exist in any coherent fashion before the creation of the office under investigation. More generally, statistical models would have difficulties measuring across the discontinuity represented by institutional change.
¹⁵ The anthropological approach has been taken before with the bureaucracy (Kaufman 1981) and with Congress (Weatherford 1985; Fenn 1990). Although the interview is a research tool shared by most social scientists, I use the interviews I conducted in a more anthropological way, to discover how the interview subjects interpret and order their environments. Fenn (1990:65) makes the distinction between perceptual questions such as these and behavioral questions more often of concern to political scientists. This strategy becomes clearer in the following chapter on boundaries. Prior to and during the conduct of the interviews, I was employed by the National Academy of Sciences from August 1990 to January 1992 as a research assistant to a project on scientific misconduct. In this role, I was a participant-observer to many episodes of negotiating policy recommendations for misconduct in science and I interacted professionally with many key policymakers.
¹⁶ Reporting the most frequently used oversight techniques (for the 95th Congress), Aberbach (1990:132) ranks staff communication with bureaucrats first (with a mean score of 1.274 on a 5-point scale where 1 is frequently and 5 is never). Oversight hearings are the third most frequently used technique (2.561). The second most frequently used technique is congressional support agency reports (2.382). The House Science, Space and Technology Committee requested one report from the Congressional Research Service in the spring of 1989. And although General Accounting Office (GAO) personnel were called in to assist House Energy and Commerce Committee investigators, no GAO or Office of Technology Assessment studies were requested.
¹⁷ Ogul and Rockman (1990:21) write that "when we look at oversight as an independent variable intended to influence the performance of administration, we are still operating in highly uncertain terrain. It is not entirely clear what oversight should achieve. It also is not always clear what it does achieve."
A compelling aspect of principal-agent theory is that it provides a strategic blueprint for intentional actors to follow.\(^{18}\) That is, if principals are dissatisfied with the level of control they exert over their agents, then it is logical for them to make the kinds of structural changes and apply the kind of inducements laid out by the theory. In attempting to map actors onto this blueprint, I ask very simple questions: Who is the principal? Who is the agent? How are they communicating, acting and reacting? Do they invoke the techniques that principal-agent theory predicts? Are those technique effective?

The standard for “effectiveness” I use is Moe’s question of whether Congress can cause a change in bureaucratic behavior without passing a law (Moe 1987). This question is a more complicated measure of congressional or bureaucratic dominance than it may appear. Legislation is a poor measure of congressional influence because even if a law is passed, change is limited, moderated, and mediated. The change caused by the legislation itself is difficult to distinguish from the change caused by the executive in implementing (or failing to implement) the law,\(^{19}\) and from the change caused by the judiciary in interpreting the law and in mediating between the executive and the legislature. What laws signify is not decided by the letter of the law as passed, but by subsequent interpretations by courts, or other elected or unelected officials (e.g., Ely 1980; Lowi 1979).

This lack of reliability of legislation as an indicator of change is precisely the reason Congress engages in oversight. The question of whether Congress can cause change even with a law is complex, and thus the question of change without a law is more complex still: change without law eliminates one complexity of the law itself, but admits many complexities in vetoed legislation, failed legislation, and obstructed

\(^{18}\) “If something is important to them, it is important to you [as a researcher]. Their view of the world is as important as your view of the world” (Fenno 1990:113).

\(^{19}\) Executive implementation runs all the way down to street-level bureaucrats, who are often distant from rules, regulations, and oversight but who are also a vital step in programmatic success or failure (Lipsky 1980).
legislation. What information about congressional preferences and abilities do such events sent to bureaucrats? The question about change without law may also be more important because the processes it relies upon operate in the presence as well as in the absence of legislation.

This complexity means, perhaps perversely, that the passage of a bill (or its signing into law) is not so crucial an event. Bills are usually omnibus rather than unidimensional. One bill is potentially many changes, and a bill may pass or fail for a variety of substantive reasons related to any of its provisions. Further, bills pass or fail for a variety of nonsubstantive, i.e., procedural or structural, reasons. For example, complicated amendments may fall by the wayside, or pork may be successfully hidden, in the chaotic, sleepless hours at the end of a legislative session. In the case I describe, the fortuna of omnibus legislation and end-of-session workload rushes against the efforts of both legislative and bureaucratic actors. If, in such circumstances, intentions do not easily translate into results and information is in short supply at both ends of the relationship, it may be difficult to discern which actors are the principals and which are the agents.

In such fluid circumstances, the information that bureaucrats receive about congressional preferences and the capacity of Congress to act on those preferences is not as complete as Niskanen lets on. In fact, the information asymmetry may not be empirically correct; bureaucratic agents may be as uninformed about congressional intent and capacity as congressional principals are of theirs. The battle between principal and agent may not be for the dominance of one over the other, but more simply to acquire enough information to act in any capacity. The job of the analyst is also more difficult because the objective reality of legislation passed or not passed can be of marginal consequence to the bureaucratic response. The inquiry, therefore, should also focus on how principals and agents attach significance to each other’s actions.
Another consequence of the complexity of nonlegislative influence is the possibility of “anticipated reactions” from bureaucrats. According to Calvert, et al. (1987:514), “[b]ureaucrats know what types of policies they need to pursue in order to survive and advance.” In their study of FTC, Calvert, et al. (1987:513) demonstrate a statistical relationship between ADA scores as predictors of congressional preferences on FTC-related issues and FTC decisions such “that the preferences of congressmen have considerable effect on agency policy choices and that this influence need not take the form of new legislation.” Yet the authors neither make it absolutely clear through what mechanisms the bureaucrats come to this knowledge (do they perform multiple regressions on congressional ADA scores, or do they rely on liaison or personal contacts for oversight cues?), nor do they provide a logical account of why bureaucrats react to the extent that they react (how much alignment between the reaction and perceived congressional preferences is necessary to prevent congressional retribution? how much is possible?).

In other words, Calvert, et al. do not take into full account the possibility of the lack of bureaucratic information. Consequently, their category of “anticipated reaction” is a broad one that can include a wide variety of bureaucratic behavior from kowtowing to expected congressional preferences to minimal (and perhaps strategic) concessions. This latter response is close to the response of NIH that I discover in this case of scientific integrity, what I call after the language of the NIH director who took this action a “pre-emptive response.” It differs from the largely undefined category of anticipated reaction in that it is clearly a satisfying response on the part of the bureaucrat that gives more credence to the bureaucrat’s lack of information, to motives other than budgetary maintenance such as programmatic autonomy, and to the role of implementation in limiting congressional influence.

Despite these complications, the heuristic of seeking bureaucratic change without legislation is a good starting point. After a discussion of the first congressional
activity on the misconduct issue in the early 1980s and the subsequent efforts to preserve the social contract for science, I describe the intense scrutiny of the special status of science unleashed by John Dingell, chairman of the Energy and Commerce Committee, on NIH, that led to the preemptive creation of OSI.

C. Maintaining the contract: responsiveness to professional norms

1. Beginning to oversee

The oversight hearings I examine in this chapter occurred many years into a resurgence of oversight activity. As described in the introduction (I.C.2), this increase in oversight is tied to the congressional challenge of executive leadership and the decreasing ability and incentive for members of Congress to engage in legislative, rather than oversight, activity. The Legislative Reorganization Act of 1970 and House and Senate reforms in the mid-1970s strengthened oversight by legislative committees (Oleszek 1989; Sundquist 1981). The increase in staff—some fifty percent in the House from 1972 to 1987—increased capacity for oversight. Internal reform, resulting in the decentralized subcommittee system, and external changes such as the growing complexity and level of intervention of government, increased the opportunity and the demand for oversight (Aberbach 1990). Further, the national focus on budgetary politics since 1980 and the “politics of fiscal austerity” (Oleszek 1989)—at least in the domestic arena—created a zero-sum environment even before the 1990 budget agreement, in which members had relatively greater incentives to review and revise old programs, or point out their contribution to waste in government, than to initiate new programs. Partisanship added to the structural and fiscal fuel for increased oversight, with partisan votes steadily rising in the House through the 1980s as the Democratic

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20 For Huntington (1965) the resurgence is post-World War II in origin; for Aberbach (1990; 1979) and Sundquist (1981) it is post-Watergate.
Party became more homogenized and as the Reagan Administration provided a willing foil.\textsuperscript{21}

Congressional oversight with respect to scientific integrity and misconduct began in 1981, and for several years both the interested congressional committees and NIH attempted to handle the problem without resorting to formal monitoring arrangements and within the social contract for science. Hearings by the Investigations and Oversight Subcommittee of the House Science and Technology Committee, chaired by Albert Gore, Jr. (D-TN), and by the Senate Labor and Human Resources Committee, chaired by Orrin Hatch (R-UT), first raised the issue. The two-day Gore hearing focused on the adequacy of misconduct detection and investigation with respect to cases involving scientists Marc Straus and John Long (U.S. Congress 1981a). The Hatch hearing also focused on the institutional response to these cases, but was oriented more toward financial waste, fraud and abuse (U.S. Congress 1981b). The conventional story of the two hearings holds that the committees raised the issue, but nothing happened until the dam broke with the so-called Baltimore case seven years later.\textsuperscript{22} However, this story misses results from each hearing that are important for the account of the impact of congressional oversight and the fate of the social contract for science.

2. Gore hearing: out to lunch?

Gore’s hearing on scientific fraud was the first in a series on ethical issues in biomedical science—including biotechnology, human subjects research, university-industry relations, etc. Although combating waste, fraud and abuse in government was a general motive, the particular context was overseeing an area that Gore thought had

\textsuperscript{21} Congressional Quarterly (CQ 1990:32B) reports that party voting in the House rose from 38% in 1980 to 55% in 1989 and achieved a maximum for the data set (post-1954) in 1987 at 64%. Mayhew (1991:ch.2) disputes claims from the oversight literature that oversight increases with partisan conflict between the legislature and the executive, but his empirical evidence focuses only on high-profile investigations and not common oversight. Aberbach (1990) finds some influence of divided government and partisan influence on oversight, however.

\textsuperscript{22} For accounts of early congressional attention, see Broad and Wade (1982) and Gold (1993).
been noticeably lacking in oversight.\textsuperscript{23} As one staffer at the time explained, Gore understood scientists better than most members of Congress did. He was not “bedazzled” by scientists and neither did he share the “naive reverence” that other members have for them. Instead, he thought that they should be held to the same standards as defense contractors.\textsuperscript{24}

The hearing solicited testimony from spokespersons from the scientific community, the NIH, and independent experts on ethical issues in science. Prominent witnesses played down the threat of scientific misconduct. Philip Handler, president of the National Academy of Sciences (NAS), called the problem “grossly exaggerated” and expressed total confidence in the current “system that operates in an effective, democratic and self-correcting mode” (U.S. Congress 1981:11). Staff took Handler’s testimony to be an expression of the social contract for science as “a one-way street” in which “government is supposed to cough up the money and not attach any strings.” NIH director Donald S. Fredrickson testified that misconduct was not and would never be a problem because of scientific self-regulation. House Science staffers recall that the witnesses from the scientific community thought Gore was “out to lunch” in pursuing the issue.\textsuperscript{25}

Staff were “dissatisfied, disillusioned” by the defensiveness of the scientific community. They were also upset with the difficulty that some in NIH had convincing their superiors in the offices of the director, the general counsel, and the Assistant

\textsuperscript{23} Interview with former professional staff member, House Science and Technology Committee, Washington DC, December 4, 1991. The Gore Committee was examining whether or not changes in the research environment, like those involved in university-industry relations and conflicts of interest, could influence scientists to engage in misconduct. See the following chapter for a description of the various repertoires used to explain misconduct aired at the Gore hearing (IV.C.1). The concerns about university-industry relations and the conflicts of interest were also in part derived from the new legislation in 1980 on technology transfer and patents that I discuss in chapters five and six.

\textsuperscript{24} Interview with former professional staff member, House Science and Technology Committee, Washington, DC, December 4, 1991.

\textsuperscript{25} Interview with former professional staff member, House Science and Technology Committee, Washington, DC, December 4, 1991.
Secretary for Health to take the issue seriously.\textsuperscript{26} Prior to 1981, NIH handled reports of fraud and misconduct among its grantees in an \textit{ad hoc} manner. The hearing helped make it clear that a more organized approach was necessary, and the Institutional Liaison Office (ILO) in the NIH Office of Extramural Research began to play a larger role coordinating investigations.\textsuperscript{27}

The hearing led to legislation as well. Gore, who at the time also sat on the House Energy and Commerce Committee, worked with his colleague from that committee Henry Waxman to add a scientific misconduct provision to the triennial NIH reauthorization bill in 1983 (§485 of H.R. 2350, the Health Research Extension Act of 1983).\textsuperscript{28} Congress passed the bill over two Reagan vetoes in 1985.\textsuperscript{29} The misconduct provision underscored the responsibility of universities to investigate and report misconduct allegations (P.L. 99-158) by directing DHHS to require all NIH applicant institutions to establish misconduct procedures and to report investigations of substantial misconduct. The misconduct provision, reinforcing the idea of the social contract for science by affirming the primary role of the universities, was clearly not such an instance of micromanagement.\textsuperscript{30} But because of Reagan's other political objections, this congressional directive was delayed for two years.

\textsuperscript{26} Interview with former professional staff member, House Science and Technology Committee, Washington, DC, December 4, 1991.
\textsuperscript{27} Interview with Mary L. Miers, former Institutional Liaison Officer, Office of Extramural Research, NIH, Bethesda, MD, December 6, 1991.
\textsuperscript{28} A similar provision contained in the Alcohol and Drug Abuse Amendments of 1983 (P.L. 98-24) instructed the Administrator of the Alcohol, Drug Abuse, and Mental Health Administration (ADAMHA) to "establish a process for the prompt and appropriate response to information provided the Administrator respecting scientific fraud in connection with projects for which funds have been made available under this Act." By failing to specify how the "information" was to be "provided" to the Administrator, P.S. 98-24 implies that the existing route via universities' own procedures is acceptable.
\textsuperscript{29} President Reagan vetoed the reauthorization because he thought it "manifest[ed] an effort to exert undue political control over decisions regarding scientific research" (Reagan 1988:1361) by making inefficient administrative and program requirements and unnecessary new organizations such as a nursing research center.
\textsuperscript{30} NSF took a similar attitude toward the primary responsibility of universities with its posture on loyalty tests during the McCarthy era. See II.C.4.b.
Gore's 1981 hearing influenced the NIH reauthorization, re-emphasizing the social contract for science and scientific self-regulation. He sought responsiveness from NIH and the scientific community, but responsiveness to professional norms rather than to political demands.\textsuperscript{31} Gore was saying, in effect, that if scientists follow their own rules, that it would be okay with him. There were no proposals for constraints, new structures or organizations, or even incentives offered from the committee--merely a formal reminder. To this end, NIH began working on a proposed rule for applicant universities to follow when conducting scientific misconduct investigations. The Gore Committee and NIH intended such activity to maintain the self-regulatory system by having the scientists at NIH provide it with some formal guidelines. Gore's subcommittee performed little follow-up. Because of the scope of the hearings, the subcommittee had "four or five irons in the fire"\textsuperscript{32} and demurred on misconduct, expecting the scientific community to handle this problem. The subcommittee requested that NIH make periodic progress reports, which the agency never did.\textsuperscript{33}

3. "The cancer doctors descended on Hatch"

The Hatch hearing began as a simple exercise in financial oversight and resulted in a fracas over fraud. The Senate Labor and Human Resources Committee first became interested in the oversight of scientific work when the HHS OIG produced a report on procurement practices at the National Cancer Institute (NCI) showing sloppiness and favoritism. Staff investigators, working through OIG and General Accounting Office (GAO) financial audits, discovered that Marc Straus, who had a few

\textsuperscript{31} Ogul and Rockman (1990) distinguish among various types of agency responsiveness, including responsiveness to professional norms. So long as Gore or other members advocated this type of responsiveness, the social contract for science could be intact.

\textsuperscript{32} Interview with former professional staff member, House Science and Technology Committee, December 4, 1991.

\textsuperscript{33} Interview with professional staff member, House Science, Space and Technology Committee, Washington, DC, January 9, 1992.
years previously been accused of scientific fraud, received a new grant from NCI.\textsuperscript{34} Hatch would have held a hearing on NCI procurement in any event, but decided to focus on the Straus case because it was within the committee’s purview and it seemed so egregious.\textsuperscript{35}

Hatch opened the hearing with a paean for cancer research carefully yoked to a justification for oversight:

The National Cancer Institute, with a budget exceeding $1 billion annually, is responsible for dispensing more than $800 million annually to fund many worthwhile projects to research known and suspected causes of cancer. We share everyone’s ultimate hope that there will be findings of cures or relief for this most dreaded and elusive of diseases.

We all share the anxieties and heartaches of families, friends, and colleagues who must undergo intensive, painful, and many times futile cancer treatments....

The assault on cancer at a national level in 1971 was stepped up to a war on cancer. Congress has willingly raised the budget for this endeavor from an appropriation of $230 million in fiscal year 1971 to about a billion dollars a year at present....

As with all Government efforts and organizations, there have been disappointments and shortcomings. Those are to be expected. Yet they must not remain immune from public scrutiny. At NCI, as with all Government-funded programs, there have been discoveries of abuses and mismanagement resulting in poor procurement and contract rules, lack of monitoring, and, as is often the case, an institutional reluctance to address such deficiencies. (U.S. Congress 1981b:1-2).

Hatch continued to detail the charges against NCI, including the mishandling of the Straus case, based on OIG and GAO reports. Senator Paula Hawkins (R-FL) painted a starker picture: “It is shocking for me to read in the Gore report that falsification involving cancer patient treatment cannot only be ignored, but is, in fact, rewarded. If the relationship between Agency and researcher is so fraternal and cozy that coverups of this magnitude could occur at all, then it is time that the Congress stops winking at wrongdoing” (U.S. Congress 1981b:29). Hawkins further intimated that

\textsuperscript{34} NCI’s failure is typical of informal, decentralized decisionmaking. It highlights the fact that self-regulatory systems require good information systems to function—perfect markets require perfect information.

\textsuperscript{35} Interview with James G. Phillips, former investigator, Senate Labor and Human Resources Committee, Bethesda, MD, November 20, 1991.
such episodes only tended to confirm her “latent suspicions and doubts about peer review” (U.S. Congress 1981b:242). 36

NCI director Vincent T. DeVita testified that despite Boston University’s severance of Straus, the Institute did not conduct an investigation because then-director Upton took a position that “NCI ‘cannot intervene in the internal affairs of institutions’” (U.S. Congress 1981b:55). DeVita’s decision not to inform the peer review board considering Straus’s application about the case “was based on the fact that the grant was scientifically sound and Dr. Straus was innocent until proven guilty [by NCI]” (U.S. Congress 1981b:36). NCI’s conduct reminded Senator Metzenbaum (D-OH) of the “old saying of if you fool me once, you are a fool; if you fool me twice, I am a fool. I think you are putting yourself in a position to be fooled twice with $900,000 of the Federal Government’s money.” Under such scrutiny, DeVita admitted that, if he “had to go back and do it again, there is no question that at the time...I would have advised the board that we were initiating an investigation and deferred the payment of the grant” (U.S. Congress 1981b:122).

Although DeVita conceded this ground, the biomedical community reacted particularly harshly to Hatch’s inquiry, in part because there was “some paranoia” about what a “heavy-handed conservative” like Hatch, who was not well-known at the time, would do with the new political emphasis on waste, fraud and abuse in government. One Senate Labor investigator recalls “the cancer doctors descend[ing] on Hatch”--including two members of the President’s National Cancer Board--in a “full court press” lobbying effort. They attacked the integrity of the staff and sought extensive media coverage. NCI had also tried to “squelch the hearing,” complaining that there were too many documents to photocopy for the committee and that the inquiry was interfering with the War on Cancer. The committee had never before been “back-doored” to this extent. 37

36 The witnesses included OIG and General Accounting Office (GAO) auditors.
37 Interview with James G. Phillips.
The hearing’s attention to Straus’s continued funding had an impact at NIH. It “somewhat strengthened the faction--faction is a loaded word, I don’t really mean faction--strengthened the thinking here that you know, perhaps, we needed to be a little aggressive and remember the rights of the government and the rights of the public.”38 After the Hatch inquiry, NIH responded by creating what they called the ALERT system—a system of confidential information sharing between grantee institutions and the various national institutes to monitor misconduct cases. Institute directors could then use this information to suspend or delay grants, grant extensions, or renewals for scientists like Straus whose conduct was gravely suspect.

The Senate Labor Committee was a bit more aggressive following up than House Science had been. The staff investigator kept the OIG on watch, and would call NCI every two years and make the bureaucrats think the committee was conducting another “sweep” of its files. But staff did not want to be “distracted” with micromanagement, not having the expertise and believing that the scientists should get “maximum flexibility until they do something dishonest.”39

4. Contract in tact

As a result of the Gore and Hatch hearings, Congress and NIH attempted to contain the issue of scientific misconduct within the social contract model. The abuses identified by the committees were uncovered through general information sources and staff work. Particularly in the Hatch hearing, the role of OIG and GAO suggests that Niskanen’s information hypothesis and recommendations for increasing information to political principals are very accurate.40 Yet neither OIG nor GAO is a mechanism particular to the oversight of research misconduct. Nor did the monitoring by Congress through these general mechanisms lead to changes in structure or incentives. The changes—the Health Research Extension Act and the ALERT system—were techniques

38 Interview with Mary L. Miers. This comment also raises the issue of multiple agents within a single bureau, and the question of the role oversight in dividing and conquering the bureaucracy.
39 Interview with James G. Phillips.
40 More on the HHS OIG will follow (III.D.3).
to facilitate what the self-regulating scientific community was supposed to be doing anyway. The legislation prescribed merely that each university should have procedures to deal with misconduct—a forum for self-regulation—and that the funding agency should be kept abreast of the events in these fora. The ALERT system was akin to codifying the reputational damage that scientific fraud is supposed to cause.\footnote{\footnote{That is, it would help provide information for the more complete application of scientific judgment in the credibility cycle. See II.C.3.c.}} In the self-regulating model, information about scientists’ conduct is relevant for funding decisions, and the ALERT system helped connect the various funding subcommunities to spread this information in a need-to-know basis.

Responsiveness thus can mean more than responsiveness to Congress. By reiterating the self-regulating role of universities and by pushing for increased information sharing with the ALERT system, the first response of the Gore and Hatch committees was to keep NIH responsive to professional scientific norms. As Pratt and Zeckhauser (1991:6) write, both principal and agent have an interest in constructing a relationship that is closest to the one in which information about bureaucratic behavior is costless and therefore available. If the professional community of scientists and their norms within the university system could maintain their own monitoring, as encouraged to do by Gore and Hatch, monitoring could continue to be very low-cost for Congress.

The Health Research Extension Act also provides a view of the slight difference between passing and not passing a law. The bill was introduced in 1983 and passed in 1984, so the bureaucracy was cued to congressional interest, intentions and capacity to act then, rather than when the bill was passed over Reagan’s veto and became law late in November 1985. Indeed, PHS had printed draft guidelines dated October 10, 1985, before their obligation under the law to provide such guidelines. This draft resembled in most details the interim guidelines published in 1986 (NIH 1986), ostensibly in response to what had then become the agency’s obligations under the Health Research
Extension Act. So in this instance, turning a bill into a law was not crucial, but
signifying intent and some capacity to act on that intent was crucial to instigating
bureaucratic action.

According to staff, the committees were reasonably satisfied with the NIH
response, particularly since the reaction from the scientific community ranged from
denial to hostility. Committee staff took minor steps to continue overseeing the matter
for three interrelated reasons: they trusted NIH and the scientific community to take
care of a problem once it was pointed out to them; they remained committed to the
social contract for science; and they simply had better things to do. As Mary L. Miers,
the person at NIH with primary responsibility for misconduct at the time, recalls:

We actually had remarkably little direction from congressional staff. It was my impression, particularly on the House side, that the Gore
hearings produced some sense that yes, there was a lack of policy, but
that the government's intentions were honorable and that things would
improve....There was a sense that they knew that it wasn't a simple
matter and they were willing to give the NIH and the research
community a chance to try to develop some better procedures....

Well, by and large the scientific community over the years has
enjoyed a great deal of respect and support from Congress. So they
were not as likely to come down in that case as if it were defense
contractors or now we say bankers, or whatever.\textsuperscript{42}

Without belief in honorable intentions, there could be no reliance on low-cost devices
like reliance on professional norms and a commitment to the social contract for science.
Without such commitment, there is little separating scientists from defense contractors.

D. Reaction or pre-emption?: creating OSI

1. New congressional actors

Despite congressional emphasis on these low-cost solutions such as the
continuation of the social contract for science, and some progress by NIH in
reinforcing professional awareness and information systems, cases of scientific

\textsuperscript{42} Interview with Mary L. Miers.
misconduct and evidence of universities’ handling cases poorly continued to mount.

Although there was no formal response from the scientific community, some organizations and individuals acted in a way that gratified staff, especially in contrast with the tone of Handler’s testimony.\textsuperscript{43} Nevertheless, the response was “not Asilomar by any means,”\textsuperscript{44} and there was even some “backsliding” by NIH and the scientific community: ILO did not get increased staffing or expertise; cases continued to crop up\textsuperscript{45} and universities performed poorly in how they handled them;\textsuperscript{46} and as important

\textsuperscript{43} These included the development of guidelines by the Association of American Medical Colleges (AAMC), the decrease in the number of “scientific doyens” taking Handler’s perspective, some private encouragement to the committee by scientists, and public statements such as those by Yale president A. Bartlett Giamatti (1982). Interview with former professional staff member, House Science and Technology Committee, December 4, 1991. The report from the OIG (DHHS 1989) released in draft at the second Weiss Committee hearing cited the AAMC report, a similar report by the Association of American Universities (AAU), and a joint workshop by the American Association for the Advancement of Science (AAAS) and the American Bar Association (ABA) as the only relevant responses by the scientific community. The CRS report (1989) cited the AAU, AAAS/ABA, and AAMC activities, as well as the reports by the Institute of Medicine (IOM 1989) and Sigma Xi (1986). The CRS report also summarized the OIG report.

\textsuperscript{44} Asilomar is the name of a conference center in California where an international group of scientists met in 1975 to discuss the new techniques in genetic engineering and to devise a scheme for their safe laboratory use. Krimsky (1992:100) writes that the scientists “who organized the Asilomar meeting, surmised that if the NIH did not act with dispatch in responding to the potential risks of gene splicing, Congress might pass restrictive legislation.” As evidenced by the quotation, Asilomar became something of a benchmark for the concerned response of the scientific community. See II.C.4.e.

\textsuperscript{45} Between the spring of 1981 and the spring of 1985, the House Science and Technology Committee received some forty or fifty allegations of scientific misconduct. Some seemed legitimate, some were being made “to nail competitors,” and others may have been simple mistakes. There was extensive contact between the committee and Mary Miers of ILO regarding these cases. Interview with former professional staff member, House Science and Technology Committee, Washington, DC, December 4, 1991.

\textsuperscript{46} One case in particular involved John Darsee, who worked in the laboratory of eminent Harvard cardiologist Eugene Braunwald. Darsee had authored over one hundred papers or abstracts in two years. One evening in the spring of 1981, co-workers witnessed Darsee fabricating data. Darsee confessed to the single act, and was allowed to remain in Braunwald’s lab. But in the fall of that year, NIH officials who were sponsoring the work informed Harvard authorities that there were problems with data submitted by Darsee (Broad and Wade 1982). Eventually, thorough examinations of Darsee’s corpus found scores of tainted publications. Walter Stewart and Ned Feder, scientists employed at NIH, investigated on their own the Darsee case, particularly aspects of the authorship practices with Darsee’s coauthors. Their paper on the Darsee affair (Stewart and Feder 1987) had great difficulty getting published, presumably because it was contrary, rather than because it was of poor quality. Their findings, and their difficulties in publishing, were the subject of a 1986 House Science Policy Task Force hearing (U.S. Congress 1986c). For their whistleblowing activities, Stewart and Feder were criticized for arrogating to themselves a task no one had set for them, an interesting criticism in a supposedly self-correcting community, of mistaking error for fraud, and for practicing “scientific McCarthyism.” See, for example Peter Gwynne, “Have the fraudbusters gone to far?” The Scientist (July 11, 1988), p. 1. On the other hand, Stewart and Feder were being paid as government employees to perform research at NIH. Their role was itself a microcosm of the larger issue, described more in detail in the next chapter, of who if anyone is privileged to define who a scientist is and what science is.
cases mounted, the scientific community returned to a defensive posture, even when some positions were indefensible. The response of the universities to the 1985 legislation was "slow," "convoluted" and "characterized by denial." Whereas, in 1981 the congressional committees had called NIH up on the carpet and told them to get cracking; by 1988, the committees were pointing out major deficiencies to NIH.

Attention to scientific misconduct had spread to other congressional committees. The new actors were Ted Weiss's Intergovernmental Affairs Subcommittee of the House Government Operations Committee and John Dingell's Oversight and Investigations Subcommittee of the House Energy and Commerce Committee.

Dingell's subcommittee is famed for its investigative prowess. Serving since 1955, Dingell (D-MI) was socialized during the ascendancy of committee chairmen and his power is reminiscent of the pre-reform House (Dodd and Oppenheimer 1985:47). Weiss (D-NY), an "unreconstructed liberal" and "the conscience of the House," was known for using his subcommittee as his "personal soapbox" on health care issues (Duncan 1989). The former committee has legislative jurisdiction over the research programs at NIH and had the third largest staff in House in 1987; the latter committee has a broad mandate to oversee federal programs and had the eighth largest

47 Namely, Braunwald's defense of his protegee Dasee. Also see Braunwald (1987).
48 Interview with former professional staff member, House Science, Space, and Technology Committee, Washington, DC, December 20, 1991.
49 Interview with Mary L. Miers.
51 Energy and Commerce acquired unquestioned jurisdiction over health research in the mid-1970s by cutting a deal with then-chairman of the House Science and Technology Committee Olin E. Teague, who, because of his own interests, traded health to Commerce in exchange for aviation research (which more naturally went with House Science's NASA jurisdiction. Interview with professional staff member, House Science, Space, and Technology Committee, Washington, DC, January 9, 1992.
52 It may also be the fastest growing staff, increasing from 42 to 153 over the same period (AEI 1990).
staff (AEI 1990). Weiss first became interested in the subject in the fall of 1987, and requested information pertaining to several cases from ILO.\textsuperscript{53} Dingell did not come on board until early in 1988, when he became aware of the Baltimore case through the efforts of NIH scientists Ned Feder and Walter Stewart.\textsuperscript{54}

In their inquiries, Weiss and Dingell went beyond the administrative and information matters with which Gore and Hatch had dealt. Weiss (U.S. Congress 1988a; 1988c; 1989c) focused on a number of cases that included conflicts of interest and university-industry relations, while Dingell (U.S. Congress 1988b; 1989a; 1990a)\textsuperscript{55} scrutinized the case involving Nobel laureate David Baltimore and the related issue of whistleblower protection.\textsuperscript{56} The hearings accomplished two things of great significance for the development of the principal-agent relationship and the fate of the social contract for science: they made public the traditionally unseen aspect of science, an aspect that involves politics and power in the place of scientific judgment and therefore does not correspond to the self-regulating market model underlying the contract; and by doing so they shook congressional confidence in the social contract for science and the low-cost and informal solutions to the principal-agent problem.\textsuperscript{57}

\textsuperscript{53} Interview with Mary L. Miers.

\textsuperscript{54} Interview with a former congressional professional staff member, March 13, 1992. There is some uncertainty as to whether Feder and Stewart approached the Dingell subcommittee or vice versa. The details of the case involving the actions of MIT and Tufts researcher Thereza Imanishi-Kari and her co-authors, including David Baltimore, are too many to describe here. See, among others, Imanishi-Kari (1991), O'Toole (1991); Hackett and Chubin (1990) and Baltimore (1989).

\textsuperscript{55} Weiss's witnesses were individual experts and agency officials. Dingell's witnesses included whistleblowers and investigators from GAO and the Secret Service.

\textsuperscript{56} Perhaps by parsing the issue in this way, they avoided damaging turf conflicts, while at the same time liberating more information from NIH by offering two principals to which the bureau needed to respond.

\textsuperscript{57} The bureaucratic calculations differ with regard to the potential of Weiss and Dingell, however. A former principal deputy general counsel with HHS explains that "Weiss was viewed merely as an annoyance" because he had no legislative authority on Government Operations, had a slight legislative record, and had problems with House Judiciary Committee chair Jack Brooks (D-TX), who sometimes would refuse to issue Weiss subpoenas. Dingell, on the other hand, demanded "a very different calculus," "so you pay attention." Interview with Robert Charrow, former principal deputy general counsel, HHS, Washington, DC, January 3, 1992.
2. Congress deconstructs science: the coherence of congressional scrutiny

The Energy and Commerce Committee approached scientific misconduct with the same aggressive demeanor as they approached investigations into Wall Street and defense contractors. But staff also expected that after one short hearing in which the plight of whistleblowers was highlighted, that the solutions would be obvious, and that the biomedical community could handle the problem itself.\(^{59}\) Perhaps because of the aggressive attitude of the Dingell and Weiss Committees, and perhaps because of the defensive posture of the biomedical community, the expectations of staff were not fulfilled. Dingell staff called the experience with the biomedical community both one of the most challenging and one of the most disappointing experiences in many years on the Hill. They had high expectations that the scientific community could handle the problem, and they were disappointed by the community's lack of a measured, disinterested, "scientific" response.\(^{60}\) As self-serving as these comments by staff may sound, they had every reason to hold these beliefs based on the testimony of such witnesses as NAS president Handler and NIH director Fredrickson at the earlier Gore hearing. They also would be expected to hold these beliefs if they preferred a lower-cost solution such as adherence to the social contract for science.\(^{61}\)

At Dingell's first hearing, Representative Wyden (D-OR) presaged a more active role in monitoring by Congress by saying that the original NIH reauthorization provision, inspired by Gore and reaffirming self-regulation, did not go "anywhere near

\(^{58}\) The following section is based largely on research presented by the author in February 1991 at the White Burkett Miller Center for Public Affairs at the University of Virginia; it may also be found in Guston (forthcoming) and is excerpted in Bimber and Guston (forthcoming).

\(^{59}\) Interview with professional staff members, House Energy and Commerce Committee, Washington, DC, November 5, 1991.

\(^{60}\) Interview with professional staff members, House Energy and Commerce Committee, Washington, DC, November 5, 1991. Many of my interview subjects commented negatively on the style of Dingell's investigators, but no did not at least begrudge them the legitimacy of their pursuit of the integrity of research funds and the process for investigating misconduct at NIH and the universities.

\(^{61}\) Indeed, at times the public debate became a series of charges and counter-charges about which side, the Congress or the scientists, broke the social contract first. I think this is the wrong way of looking at; each side was attempting to preserve what it understood to be its interests and prerogatives under the contract.
far enough.” Wyden said that in the next reauthorization, he would pursue the creation of “a new, independent program for quality assurance in research...making sure that it can’t be controlled by the Director of NIH or by any of the Institutes” (U.S. Congress 1988b:4). As one staffer said about this reaction, “Congress does not take kindly to an abuse of delegated authority.”

NIH testimony emphasized its responsiveness in an attempt to maintain the status quo. At Weiss’s second hearing in 1988, then NIH director James Wyngaarden asserted that the “world has not remained still since the hearing you held in April...[T]he Department, the Public Health Service, and the NIH have taken a number of concrete steps to...combat scientific misconduct” (U.S. Congress 1988c:165). Wyngaarden catalogued the responses, including: the establishment of a task force by Assistant Secretary Windom;63 a workshop and study planned with the Institute of Medicine;64 and increased staffing, communication, and training for relevant offices—steps designed to contain the problem within the low-cost framework of professional norms.

But Weiss put high expectations on the quality of scientific self-regulation. Weiss claimed that “the American public depends on science to solve human problems and to help us find ways to make the world a better place....Because we depend on science so much...many people expect science to be infallible and scientists incorruptible” (U.S. Congress 1988a:1). In fact, Weiss and Dingell had selected cases in which the fallibility of science and the corruptibility of scientists seemed evident.65 Dingell identified a pattern: “Over the past several years, the number of cases of fraud

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62 Interview with professional staff member at the House Science, Space and Technology Committee, Washington, DC, January 9, 1992.
63 The task force was chaired by DHHS principal deputy counsel Robert Charrow and comprised largely of political appointees.
64 The workshop resulted in the publication of IOM (1989), which discussed the advisability of a new office at NIH to manage scientific integrity.
65 The Dingell subcommittee chose the particular case because, although they believed there to be a number of likely cases, Nobel Laureate and MIT researcher David Baltimore was “a big fish” to fry. Interview with former congressional professional staff member, March 13, 1992.
in scientific research and misconduct that have come to NIH's attention have increased dramatically and are now averaging better than two a month" (U.S. Congress 1988b:1). Not only were the norms of science not deterring misconduct, 66 but it appeared that the norms were systematically being flouted:

In this system, the university gets the first crack at the data or lack thereof, and at the witnesses. It also gets to frame the issues....In most of the gross and celebrated cases of scientific fraud and misconduct, like that of Dr. Darsee at Harvard and Dr. Felig at Yale, the institutions engaged in what appears to be a cover up effort to protect the institutions and some of their senior scientists. In some cases, the investigations of scientific fraud and misconduct are carried out by the very targets of the investigations, and in other cases, by only the interested parties themselves (U.S. Congress 1988b:2-3).

Misconduct challenges the peer review system (Chubin 1990; Chubin and Hackett 1990; Ben-Yehuda 1985), especially as Congress discovers that peer reviews and departmental committees—the mechanisms of self-regulation to which the earlier legislation had delegated responsibility--led not to universalistic decisions based on a skeptical analysis of the empirical evidence, but rather to conflicts of interest and conspiracies. 67 Experience reinforced this perception; within twenty-four hours after its first hearing, the subcommittee received more than a dozen calls about new cases of scientific misconduct. 68

Dingell also attacked the idea of self-regulation through the experiences of whistleblower Margot O'Toole. O'Toole had brought to light problems in the paper published by Thereza Imanishi-Kari, David Baltimore, and others. In testimony before Dingell, O'Toole explained her confusion over Baltimore's lack of response to her

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66 The "norms of science" are generally regarded to be institutionalized commitments to the communal nature of scientific knowledge, the universal criteria of scientific judgment, the disinterestedness of scientific analysis, and the organized skepticism of scientific evaluation (Merton 1973:ch.13). See II.C.3.d.
67 Recall Senator Hawkins remark about latent skepticism of peer review.
queries about errors in the published paper. She was surprised and dismayed at his lack of interest. She was forced to conclude that believing in self-regulating science is counterproductive, because adherence to such belief allows scientists like Baltimore and Imanishi-Kari to disregard their own errors and leave them for others to correct. She identified what could be called a free-rider problem: if self-regulation is a public good in science, no individual has great incentive to contribute to it. O’Toole’s attests to the existence of a self-regulating system only to the extent that individual scientists are devoted, in Polanyi’s (1962) words, “to the ideals of scientific work.” Unless a scientist questions possible errors in the literature, then any self-regulating mechanism of science cannot be engaged. Since the errors were presumably attributable to Imanishi-Kari as the principal investigator and to Baltimore as the senior author, their lack of response appeared as an act of self-interest and ambition rather than the expected disinterested search for truth.

A second whistleblower argument that Dingell deployed suggests that the market of science has big players who can use reputational power to squeeze out little players, regardless of the empirical merit of their claims. Margot O’Toole complained in testimony to the chairman that after she had approached Baltimore with her concerns about Imanishi-Kari’s research, that Baltimore challenged her to publish them in a letter to the journal. O’Toole testified that Baltimore threatened her by promising that he, too, would write a letter to the journal. With both letters published, the scientific peers in the marketplace of ideas would adjudicate their competing claims (U.S. Congress 1988b). Who would the peers believe? Dr. David Baltimore, Nobel laureate and director of the large and prestigious Whitehead Institute for Biomedical Research? Or Margot O’Toole, postdoc? In other words, the hearing showed that the republic of science is not very egalitarian, and the distribution of resources among scientists is

69 In another manner, the demonstration of scientific error or scientific fraud requires the individual scientist to construct the same types of alliances as does the demonstration of scientific fact. These alliances—"heterogeneous networks" (Latour 1987)—extend from the contents of test tubes to members of Congress.
highly uneven, leading to an unfair outcome even under what may be fair rules of exchange.

Dingell also challenged the construction of privileged scientific truth. In the May 1989 hearing, Dingell interrogated the authors of the research paper in question about an image of a published autoradiograph. As was conventional practice for contributors to the journal *Cell*, the authors had published a composite image. Separate columns of the autoradiograph had been photographed and exposed separately, and subsequently reconstructed in a way that best displayed the authors’ scientific claims. The journal did not require that composites be labeled as such. Dingell became curious when the Secret Service forensics experts he had detailed to the investigation described in testimony how each column had been photographed and exposed separately and then reconstructed into a single image representing a scientific claim. Dingell wondered, how can the scientists know which photographs and which exposures to select? Since scientists often assert that research, by definition, is an investigation into the unknown, and that so little is known about the results of research that it cannot be planned or managed, how can it be known which exposures to select? If exposures are selected based on the hypothesis, then is the so-called evidence not tautological? Or fraudulent? The question of data selection gets to the heart of science as a constructive endeavor.\(^{70}\)

Through these and other episodes in the hearings and from staff work, Dingell and other members and staff came to believe that politics—ambitions, profits and lusts—were present within the application of what was supposed to be exclusively scientific

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\(^{70}\) The so-called “new sociology of science” maintains that science is not founded on any necessary first principles, scientific method, or immediate correspondence with reality, but is instead an interpretive endeavor or social construct little different from other scholarly disciplines or even other trade practices (i.e., it is socially constructed). Also known as “the sociology of scientific knowledge” (SSK), it developed in counterpoint to Mannheim (1936) and was heavily influenced by Marxist thought (the idea of technical change as epiphenomenal to economic relations). Seminal work includes Berger and Luckman (1967); Latour and Woolgar (1979); Gilbert and Mulkay (1984); and others. Such scholars do not deny the application of “scientific judgment” to such questions as data selection, but they emphasize that judgment is just that—an interpretation of data involving *ad hoc*, arbitrary or intuitive decisions rather than observing the revelation of nature. See II.A.2.d. Also see Knorr-Cetina and Amman (1990) about the interpretation of autoradiographs.
judgment. In other words, they discovered that self-regulation was not good regulation.

Dingell refused to accept the premise that self-regulation makes science especially deserving of autonomy from congressional oversight and investigation. As Hatch had suggested in 1981, financial accountability motivates the oversight of science. After taking a good deal of criticism in the wake of his first hearing in 1988, Dingell retaliated in 1989 with an attack on the science’s scrutiny-exempt status:

...Other critics have claimed that the Congress is not capable of understanding science, or even raising questions about science. However, no one questions the ability of the Congress to deal with these issues when the Utah scientists demand $25 million for a cold fusion experiment, or the Air Force needs $70 billion for the Stealth Bomber Program, or when hundreds of billions of dollars are requested for Star Wars, or when enormous sums of money are requested for enormous particle accelerator programs in budgets which we are now contemplating (U.S. Congress 1989a:2).

According to Dingell, congressional scrutiny is symmetric; if members must know enough to appropriate for science, and if scientists come to Congress for appropriations, then members must know enough to oversee and question the activities these appropriations support, and scientists must bear the scrutiny. Staffers added, “What would happen to tax revenues if the IRS worked like NIH? It’s the same tax dollars.”71

The subcommittee was reaching these conclusions as the scientific community continued to pressure Congress to rely on the system of professional norms or the social contract for science. One former congressional staffer relates:

[I]t’s fair to say that [Dingell’s investigators] are the type of guys who are really fired into action if someone did sort of voice an argument against them, which was certainly what the scientific community was doing: “oh, we’re special, leave us alone. We’re self-correcting. You can’t possibly...we’re an art, you can’t possibly understand it....The community brought this stuff right down on their heads, fairly....They

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71 Interview with professional staff members, House Energy and Commerce Committee, Washington, DC, November 5, 1991.
should have said, “Oh, we hear you. Yes, yes, things are not great. Give us a chance to sort of clean things up ourselves and we’ll come back to you and tell you what we’ve done....”

And Dingell would have, I know, because...that’s exactly what he did to the accountants. Now he had a hearing and the accountants said, “oh, yeh, okay, mea culpa. We screwed up”....[And so, you know, we were fighting like crazy to give these guys more money, and they were saying, “but don’t tell us how to spend it.” Well, remember that our philosophy was, that we were well and truly looking over the shoulders of Medicare doctors. You know we were well and truly looking over the shoulders of industry, in terms of what they do with their pollutants. We were certainly looking over the shoulders of the food and drug and device industries, and the defense industry, and, you know, a whole lot of other things. So what suddenly made the academics so special that we should leave them alone? I mean, they weren’t able to make that argument very effectively....[The only argument would have been, “leave us alone because here are our demonstrated efforts that we are doing our best to internally fix up these problems.” But you know, it’s a bit like, on this issue, every stone you turn over, has got a toad underneath it.

....One of the things that’s interesting...is that I don’t think scientists think enough about science. And you know the theory of science is that you work out a theory, you work out a hypothesis, and then you run experiments to refute that hypothesis. Hardly any scientists I know actually operate like that....They say, this is what I think the result is going to be, and I’m going to set up an experiment to get me that result. And you know that was sort of what Thereza Imanishi-Kari did. And you know, it sort of goes to, what is the philosophy of science, what are the mechanisms of science, and you know, truth--what is truth in science? Well, truth in science is a very pragmatic thing; it’s something that exists only until you do the next experiment. So some of it comes as a result of sort of scientists’ naiveté about the art form they’re dealing with.

....I think that if anyone was disillusioned, it was the Congress. I think a lot of congressmen now think that science is pretty seamy. 72

Another staffer said simply, “Members are less awed with the thunderbolts from the Olympus of science.” In this sense, “[we are] starting to see a change in the terms of the compact between politics and science.” 73

Yet seeing this change does not mean that Dingell and his committee have become “anti-science.” Staff, in fact, believe that they are defending the scientific community. If there are scandals to be had, then the real enemies of research and of peer review will have ammunition, and the funding of research is too important to be

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72 Interview with former professional congressional staffer, March 13, 1992.
73 Interview with professional staff member, House Science, Space, and Technology Committee, Washington, DC, January 9, 1992.
threatened by the lack of integrity of a few scientists or a few universities. Without this kind of oversight, staff believed, the scientific community itself runs the real risks. Representative Roy Rowland (D-GA), a member of Dingell's subcommittee and a physician, expressed a similar sentiment at a hearing in January 1991 on financial misconduct in the NIH laboratory of AIDS researcher Dr. Robert Gallo. In his opening statement, Rowland explicitly stated that if a researcher were dishonest in his or her personal life, then the researcher might be dishonest in his or her research life; that if the public suspected the financial dealings of a researcher, like those of Dr. Prem Sarin under investigation, then the public might also suspect the scientific validity of the research itself (U.S. Congress 1991a). Given that Gallo’s AIDS lab is the largest and arguably the most important lab in the NIH intramural system, and that AIDS research funding had increased rapidly from 1987 to 1991 despite great contention, Rowland’s point is on the mark. Rowland’s remarks also demonstrate the instrumental and ideological support that science provides politics (Ezrahi 1990) by pointing not only to the need for government to be taking effective action, but also by being sensitive to the need for government to be seen as taking effective action.

In discovering the faulty mechanisms, self-interested behavior and power relations of science, members of Congress did not stumble onto the academic literature of the new sociology of science. But they did become “disillusioned.” They discovered by and for themselves that “the science made, the science bought, the science known, bears little resemblance to science in the making, science in the searching, science uncertain and unknown” (Latour 1991:7). Like Jasanoﬀ (1990) discovers about the litigious nature of science advice in U.S. regulatory agencies, the normal processes of government reproduce aspects of the agenda of the social studies.

74 Interview with professional staff members, House Energy and Commerce Committee, Washington, DC, November 5, 1991.
75 Dingell’s investigation of Sarin came on the heals of the criminal conviction of Dr. Zaki Salahuddin, on similar charges of financial misconduct, also from Gallo’s lab. Also see the discussion of conﬂicts of interest in VI.C.1.
of science. In this sense, although the oversight subcommittees may not be acting like a “textbook scientist (or even a textbook social scientist)” (Aberbach 1990:199), their hearings produced a cogent and coherent public critique of the scientific community that parallels current academic work.

Latour and Woolgar (1979) offer the metaphor of a wall of literature being the barrier between scientists and the chaos of science in the making. In overseeing federally funded science, members of Congress are privileged to draw books off the shelf and peak through the space, witnessing the chaos. Once members of Congress discovered for themselves that in the process of correcting falsehoods and regulating conduct, science crosses political interests by crossing ambitions, profits, and lusts, then the separation between science and politics implicit in the social contract for science cannot be maintained. Members of Congress, reliant on science for their own goals and held accountable for the fiscal integrity of programs, cannot simply replace the books and turn their heads.

3. Information and the Marquis of Queensberry Rules

Hearings were not the only source of information for members of Congress in their oversight of NIH. The Dingell subcommittee, in particular, took advantage of other information-gathering devices such as the Inspector General that recall Niskanen’s argument about information, side payments to bureaucrats, and bureaucratic competition.

In 1972, having faced a series of confrontations on technical matters with the Nixon Administration, the Congress strengthened its ability to gather and analyze specialized information by passing the Technology Assessment Act, which created the Office of Technology Assessment (OTA). By 1987, OTA had grown to 143 full time employees (AEI 1990) with numerous contractors and other contacts with the academic community. OTA has a bipartisan, bicameral governing body, the Technology
Assessment Board, of which Dingell is a member. Dingell’s Energy and Commerce Committee is a primary consumer of OTA reports (Bimber 1992).

Dingell’s Oversight and Investigations Subcommittee has also taken advantage of other staff-related information gathering devices. One key subcommittee staffer was a congressional fellow from the American Society for Microbiology in 1983 and remained with the subcommittee until 1991. Another member of Dingell’s investigative staff is a Ph.D. microbiologist, who conducted the questioning of scientists at one hearing. During the course of the investigations into the Baltimore case, Dingell had NIH researchers Walter Stewart and Ned Feder detailed to the subcommittee. He also made prominent use of Secret Service investigators.

The most pertinent congressional instrument for information gathering with respect to the misconduct case has been the Inspector General (IG). The IG closely corresponds to the control device Niskanen describes in which one bureaucrat is allowed to keep a fraction of the waste or slack the bureaucrat can uncover. The IG’s mission is to “promote the efficiency, effectiveness, and integrity of the programs in the United States Department of Health and Human Services....Created by statute in 1976, the Inspector General keeps both the Secretary and the Congress fully and currently informed about programs or management problems and recommends corrective action” (U.S. Congress 1988c:124). The IG competes internally with bureaucrats for patronage and can reduce bureaucratic independence by decreasing the information asymmetry. Bureaucrats are not usually happy to have them around: “I don’t think anybody likes, no matter what agency you’re with, likes the Inspector General of your agency. When they are coming, it’s not good news. And even when you would, and you would prefer to punish your own, rather than let somebody come in from the

76 The congressional fellows program, coordinated by the American Association for the Advancement of Science, is intended both to assist technical decisionmaking in Congress by supplying members and committees with expert staff free of charge, and to expose scientists to the way congressional decisions on technical matters are made. Fellows, like academic researchers and others new to Washington, are often known to “catch Potomac fever” or “go native,” sometimes to the chagrin of the professional societies that sponsor them. See Casper (1981).
outside and deal with it."\textsuperscript{77} The role of the IG as an interested information conduit, however, raises the problem of agency between Congress and the IG itself: who will watch this watcher? The conflict between the IG and NIH described below exemplifies this problem.

As ILO was struggling through the few years after the initial congressional attention, the IG made preliminary inquiries into how NIH was managing misconduct. "[A]round 1984 or so, the Inspector General got quite interested about this, came out with some of his staff, talked to all the people at NIH that were involved with investigations....He made it very clear that he did not want the Department of his office to be embarrassed by anything that we might do, or fail to do."\textsuperscript{78} Later, after the Weiss and Dingell inquiries began, "the Office of the Inspector General became much more proactive." There was "active consideration" of handing the misconduct function to the IG.

I think they call it ‘evaluation,’ which is a benign term for a rather in-depth scrutiny of the agency's operations. And I not only heard from people I knew in the Inspector General's office, I heard from various people that I had never heard from before in the central office, and even from the Philadelphia region....Here everyone is on my case because I'm not moving the work along, and all I'm doing is talking to the IG. It got a little comical. I'm sure, you know, trying to look at it from the IG’s perspective, two congressional committees breathing down your neck, he didn’t want to look like he wasn’t doing his job.\textsuperscript{79}

In this case, HHS Inspector General Kusserow presented oral testimony and a written report (DHHS 1989) on misconduct on science damaging to NIH:

I'd like to go over some of the basic findings in our study. One is that within the Department, we found that there really is no central locus for responsibility or accountability for scientific misconduct....

A second finding was that NIH has been slow in formulating policies and procedures in dealing with scientific misconduct cases — in

\textsuperscript{77} Interview with Katherine Bick.
\textsuperscript{78} Interview with Mary L. Miers.
\textsuperscript{79} Interview with Mary L. Miers.
handling allegations and investigations on an ad hoc basis which resulted in some inconsistencies.

Third, only 22 percent of the NIH grantee institutions overall have policies and procedures in place to deal with scientific misconduct....

Fourth, scientific misconduct procedures that are in place are generally not comprehensive and are fairly limited.... (U.S. Congress 1988c:101)

NIH director Wyngaarden, also testifying at the hearing, reacted with hostility to the IG's "grandstanding" and "confrontational" manner (U.S. Congress 1988c:165):

Jim Wyngaarden changed his testimony on the spot. Being a good person, he let his higher-ups know that he was going to do it, but he did it. And I certainly felt it was justified. I think the problem was that the report was inaccurate. It was full of, shall I say, half-truths, or truths presented with only one half of it--half truths. And that was what I think was the real difficulty. But it was an interesting political ploy.80

Criticism of the report focused on its statistical presentations, particularly Kusserow's third point above. Although in fact only 22% of NIH grantees in total had misconduct procedures in place, the institutions that had procedures in place represented 86% of NIH funding. The majority of grantees that did not have procedures in place were small businesses and small colleges with a single grant. As Katherine Bick, who reviewed an early draft of the report and had her comments go unaddressed, said, "It wasn't the Marquis of Queensberry Rules."81

In addition to demonstrating the role of side payments to bureaucrats like the IG for information about bureaucratic function, the dispute over the fairness of the IG report raises another interesting question with respect to principal-agent theory: if the principal is incompetent to judge the agent, how competent is the principal to judge the Inspector General? The bureaucratic competition encouraged by the Inspector General model may not be fair competition. Neither may the side payments to inspectors general be refined enough to assure valid rather than invalid information. Principals

80 Interview with Katherine Bick.
81 In 1867, the Marquis of Queensberry Rules codified the rules of modern boxing and mandated the use of gloves rather than bare knuckles, among other reforms.
may therefore need to rely on surrogate measures for inspectors general’s performance, such as reputation,\textsuperscript{82} or on secondary information, such as GAO reports.

The HHS OIG not only competed for resources with NIH, but also competed for jurisdiction over the investigation of scientific misconduct. There was widespread belief in the PHS that the Office of Inspector General “wanted that office” that would investigate misconduct.\textsuperscript{83} Uncertainty about the IG was a crucial part of the strategic environment for the subsequent bureaucratic actions.

4. “Pre-emptive strike”

Shortly after the first Weiss and Dingell hearings, Assistant Secretary for Health Robert Windom established a high-level task force to deal with the misconduct issue. The task force was chaired by HHS principal deputy general counsel Robert Charrow and included representatives of OIG, the Office of the Assistant Secretary of Health (OASH), and the various PHS agencies. The task force met three times, twice before May 1988, to consider the process at NIH for managing misconduct allegations. It concluded that the prevailing organization was not tenable, even in theory.\textsuperscript{84}

To test the waters in the scientific community, PHS issued an advanced notice for proposed rulemaking (ANPRM; PHS 1988:36344-47). One question posed for public comment was, should DHHS create “within the Department...an office of scientific integrity, consisting of an investigative branch and an adjudicative branch...responsible for receiving all allegations of scientific misconduct...,” monitoring

\textsuperscript{82} A number of my interview subjects mentioned the high reputation of the HHS OIG after the successful conduct of high profile investigations into Medicare fraud. They also attempted to distinguish between the expertise of physicians on the IG staff in the Medicare investigation, and the IG’s lack of expertise in scientific matters related to misconduct.

\textsuperscript{83} Interview with former senior policy analyst, Office of Assistant Secretary of Health (OASH), Rockville, MD, January 8, 1992. Several other interviewees suggested that, in their opinion, the OIG wanted jurisdiction of the misconduct cases. Interview with former professional staff member, House Science and Technology Committee, Washington, DC, December 20, 1991; interview with former congressional staff member, March 13, 1992. Other interviewees provided more explicit but off the record information with respect to the OIG.

\textsuperscript{84} The task force did not generate notes or documents because it was concerned about congressional investigations following a paper trail. Interview with Robert Charrow; Interview with a former senior policy analyst, Office of the Assistant Secretary for Health, Rockville, MD, January 8, 1992.
awardee institutions, conducting investigations where necessary..., and determining which allegations appear potentially to have merit.” Another question was whether that function should be vested exclusively in the OIG.

The agency report of the responses to the ANPRM summarize the public comments on the first question as: “Creation of an Office of Scientific Integrity would be intrusive, unnecessary, premature, wasteful, inappropriate, impractical, duplicative, costly, ineffective, inefficient, and contrary to the principle that it is the responsibility of the institutions to investigate allegations of misconduct” (Rhoades 1989:10; emphasis in the original). The report continued to summarize the responses, which said that the “government should encourage and require the scientific community to police itself” and that an “oversight” or “advisory” office like the Office for Protection from Research Risks at NIH could be acceptable, as long as it was “divorced from any PHS investigative or adjudicative function.” Respondents found an Office of Scientific Integrity “preferable to placing such matters with [OIG]” because OIG is not “qualified to perform the kind of investigation necessary...which requires substantial expertise in the most current research practices and the ability to understand the details of the research in questions [sic]” (Rhoades 1989:10-11).

The strategic situation for the bureaucrats who recognized the instability of the prevailing arrangements was complicated. ILO thought that the Inspector General wanted jurisdiction, and OIG inspectors paid frequent visits to examine ILO files and ask questions.85 Those at the Department level and especially in OASH and NIH, however, saw OIG as lacking the necessary scientific expertise. Wyngaarden “just didn’t think the science would get enough credit” at OIG.86 There also seemed to be a general expectation that Dingell would prefer OIG jurisdiction. That preference may

85 Interview with M. Janet Newburgh, former Institutional Liaison Officer, Bethesda, MD, December 6, 1991.
86 Interview with James Wyngaarden, former director of the National Institutes of Health, Washington, DC, December 3, 1991.
have been revealed by Dingell’s investigators in their interviews of bureaucrats, but it was not revealed by other staffers nor by Dingell in public.\textsuperscript{87} NIH thought that it possessed the scientific expertise, and that it merely needed to staff up ILO, which to this point had been managing the misconduct case-load with one and one-half full time equivalents. NIH had recently placed a scientist in charge of ILO after the previous institutional liaison officer—a political scientist—resigned.\textsuperscript{88} NIH had also added three staff members, including a financial investigator, to supplement ILO. Higher-ups in OASH and the HHS were not convinced, however, that NIH was the right locus because it lacked investigative expertise and was, from the perspective of the Department, too close to the scientists. Another problem with NIH was that there was, in addition to extramural research at universities, nearly a billion dollars of intramural research to oversee as well. An NIH office would therefore be in the same vulnerable position of investigating its own scientists that universities had been in.

However, NIH and PHS thought that the Office of the Secretary did not have the appropriate people for the job, either. NIH was giving out “mixed vibes” on the matter: they were wary of taking on a regulatory task, but they did not want to see jurisdiction reside anywhere else.\textsuperscript{89} The task force was not responsive because at the hearings, staffing up did not seem like a sufficient answer, and everybody knew that “decision-making is driven by hearings.”\textsuperscript{90} Eventually, the Secretary and the Under-Secretary came to the conclusion that any office would not be housed in NIH. “One of the things driving the whole process was the concern that if the Department didn’t act, Congress would,” and that “if you need to be controlled by rules, it’s better if they’re your own rules.”\textsuperscript{91}

\textsuperscript{87} Interview with former congressional staff member, March 13, 1992.
\textsuperscript{88} Although there were rumors of a new office when the scientist was put in charge of ILO. Janet Newburgh, that scientist, was told when she interviewed for the job (which she began October 10) that a new office was a long way off. Interview with Janet Newburgh.
\textsuperscript{89} Interview with Janet Newburgh.
\textsuperscript{90} Interview with former senior policy analyst, OASH, Rockville, MD, January 8, 1992.
\textsuperscript{91} Interview with Robert Charrow.
By the fall of 1988, Congress gave notice that it would act. After uncovering the failures of scientific self-regulation, Dingell and Energy and Commerce colleague Waxman proposed an amendment to the 1988 NIH reauthorization bill to create what would have been called an Office of Scientific Investigations.\textsuperscript{92} The amendment would have placed this office, responsible for investigating scientific misconduct, at the level of the Office of the Secretary of Health and Human Services, the parent department to PHS and NIH. The reauthorization passed without the amendment, but PHS responded by creating the Office of Scientific Integrity (OSI) and the Office of Scientific Integrity Review (OSIR).

Details in the scientific press suggest one story of the amendment; my interviews suggest another. One headline touted, “A threat to monitor science is quashed,” and led by announcing that the “research establishment has beaten back Congress’s most serious attempt yet to grab control of investigations and punishment of scientific misconduct.”\textsuperscript{93} The article describes how scientists “reacted quickly,...pooling their resources in a concentrated effort to derail the plan. Such organizations as the American Association of Universities [sic], the Federation of American Societies for Experimental Biology, and the American Association of Medical Colleges fired off mailgrams and telegrams that urged members of both the House and the Senate to delay action until next year.” The article quotes Carol Sheeman of AAU, saying “[t]his language is an abomination....The issues are complicated, and...aren’t amenable at this point to legislation.” It describes a situation in which the five-page amendment--providing for an office whose director would be appointed by OIG and whose investigators could make on-site inspections and recommend debarment for violators--failed on the merits as argued by the scientific community.

A former staff member described the events differently:

\textsuperscript{92} I have yet to locate a copy of the amendment, which was never printed by the House.

\textsuperscript{93} Jeffrey Mervis, “A threat to monitor science is quashed,” \textit{The Scientist} (October 31, 1988), p. 1.
And at ...the end of that year...these things were all hotting up, and we still hadn’t done the NIH reauthorization bill. I think the Senate had done their version and we hadn’t done ours. And so there was a last minute rush to put a whole lot of things in that bill, and as always, everyone--every man and his dog--has an issue that they want at NIH. I mean, they wanted to move the National Institutes of Mental Health to NIH, they wanted the sleep research committee, they wanted half a dozen, more than that, twenty different things. And one of them was, was this the opportunity to do something about this issue, to sort of formalize the guidelines that were either out there or actually were being developed. And we decided that we would at least have a go, and with, I mean, the staff with our bosses’ permission and encouragement, I suppose, decided we would have a go at drafting this up.

We consulted with extraordinary numbers of people, and there was a draft piece of legislation floating around. Now, and that was at one point incorporated in the draft of legislation that would have become part of the NIH bill. I’m vague on exactly what the procedures were. I don’t think we ever marked that bill up in subcommittee or committee. [There was no House version of the NIH reauthorization]...But somehow or other, we ended up being able to conference an NIH bill with the Senate, but I think it was a bill that had never passed committee, and that piece of legislation got dropped. The legislation itself was extremely contentious within the scientific community, fueled more by the professional societies, than by individual scientists. Because if you sent it to individual scientists for comment, they’d write back and say, “well, I don’t like this, and I’m not sure why you’re doing that, but you know, that’s okay.” But if you gave it to, like, AAU, they would have apoplexy. And, you know, discretion being the better part of valor, we dropped the whole thing.

By the staff account, the amendment was circulated for comment and the disapproval of the professional societies was noted. The amendment was also circulated to or discussed with people at NIH, OMB, the HHS General Counsel’s office, and various individuals from the IOM Committee for the Study of Responsible Conduct of Research. Whatever responses were given, the amendment was offered in conference:

I mean [we] were down to doing the NIH bill in the last two days of the session. The Kennedy staff were quite happy, I think, to have a provision in the bill that would have dealt with misconduct, but they wanted it their way, and that went for a whole lot of other things as well. And in the end, what we did was a really stripped down, pared down, bare bones version of the NIH reauthorization, where anything that wasn’t in agreement, anything that was additional over and above straight reauthorization basically got taken out. So there was a series of discussions to and fro with different drafts of legislation—even if it was in fairly rough form—exchanged, and you know it may well have been that if we’d had two months instead of not much more than two days—I mean, we worked, you know, through the night for two or three days in
a row to do this—then it may well, some provision related to scientific misconduct may well have ended up in the bill. But that's fairly standard sort of, what happens at the end of the session....It was really an issue of time rather than what outside forces were saying.  

A former principal deputy general counsel at HHS explicitly indicated that without congressional action, there would have been no Department action. “Congress serves a function of lighting a fire under agency butts.” The task force created by Assistant Secretary for Health Windom met several times and was working toward a solution somewhat similar to the Dingell proposal. Late in 1988, probably mid-December after the failure of Energy and Commerce staff to attach the amendment to the reauthorization bill, NIH Director James Wyngaarden decided to launch what he called a “pre-emptive strike.” Apparently, Wyngaarden had been in contact with a key Dingell staff member who indicated that Dingell was flexible with regard to the institutional solution. So Wyngaarden proposed to the task force a “two-office solution.” An investigatory office would be established in the Office of the Director of NIH and an oversight office would be established at the PHS-level in the Office of the Assistant Secretary for Health. The task force agreed to Wyngaarden’s proposal on the condition that it be cleared through Dingell.

Wyngaarden said his proposal was based on his previous success with moving the AIDS office to the Office of the Director. That move “had quieted the critics, so I thought, ‘this has worked once, let’s try it again.’ And so rather than doing nothing, and let the Inspector General walk off with it, I decided we need some kind of new organization with visibility and a move that would be interpreted as a reorganization internally.”

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94 Interview with former congressional staff member, March 13, 1992.
95 Interview with Robert Charrow.
96 Interview with James Wyngaarden.
97 Interview with James Wyngaarden.
It was, by this time, early January 1989. As a rule, nothing of substance happens in Washington in the January following a year divisible by four. The meeting with Dingell never occurred, but the proposal went through. On January 17, 1989 Wyngaarden sent a memo to Assistant Secretary Windom, including a draft *Federal Register* notice. On Friday, February 3, Windom, Wyngaarden and other PHS officials met to discuss the proposal. Drafts continued to circulate, and on February 22 the Assistant Secretary received from the Deputy Assistant Secretary for Health Operations a longer memo and a recommendation, including staffing and resources for the new offices. Acting Assistant Secretary Ralph Reed signed off on the Statement of Organization, Functions and Delegations of Authority for the *Federal Register* on March 3. "Dingell staff read about the two offices in the March 17 *Federal Register* just like everybody else."\(^{98}\)

Weiss commended NIH on its responsiveness at the opening of his third hearing: "The National Institutes of Health responded constructively to our last two hearings, by increasing resources available for investigating allegations of misconduct and by initiating an Office of Scientific Integrity" (U.S. Congress 1989c:1). The creation of OSI went largely unnoticed in Dingell's hearing (U.S. Congress 1989a). But for Energy and Commerce staff, the congressional inspiration for OSI was not lost: "The universities failed to respond, and we need to turn the screws a little, and OSI is our screwdriver."\(^{99}\)

E. Conclusion

To recapitulate this account, the Gore and Hatch committees found flaws at NIH, and they clarified and re-emphasized their expectations that their objectives and those of the biomedical community were aligned, thus allowing for a low-cost solution

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\(^{98}\) Interview with former senior policy analyst, Office of the Assistant Secretary for Health (OASH), Rockville, MD, January 8, 1992.

\(^{99}\) Interview with professional staff members, Energy and Commerce Committee, Washington, DC, November 5, 1991.
like the social contract for science to the problem of scientific integrity. They did not offer any specific structural or incentives changes. NIH responded, helping to improve information for the market of science. But the premise of self-regulation of the social contract for science proved inadequate because members of Congress were able to identify the politics within the operation of science, and because the response from the biomedical community proved insufficient to prevent other cases of misconduct and institutional mistakes from crossing political interests. Other committees joined in, pointing out systematic failures in self-regulation and incongruities in the market of science.

Dingell revealed a preference with the failed amendment, and bureaucrats at HHS and PHS were ready to accept a similar solution. However, NIH Director Wyngaarden, not wanting any other agency to claim jurisdiction, made a counter-proposal. Although Wyngaarden may have had back-channel information from Dingell staff, the proposal was never formally cleared through Dingell. Dingell’s solution, with OSI at the Department level and directed by the IG’s designee, would have spelled the end of the social contract for science. Such an office, as the professional societies sensed, would have marked a substantial change in the delegation of authority to the scientific community. Its monitoring and investigating represented a new institutional solution to the previously tacit agreement regarding scientific integrity—the kind of structural change predicted by Niskanen that principals use when attempting to gain greater control over the behavior of their agents.

OSI resulted from the recognition by political actors that science could not keep up the self-regulation premise of the social contract. That any office is needed to engage professional self-regulation admits the weakness of that regulation. An invisible hand does not need a glove. But Wyngaarden thought that the separation of science and politics implicit in the social contract for science could be protected in deed if not in fact if OSI could be controlled by scientists, namely those at NIH.
Wyngaarden’s “pre-emptive strike” pre-empted not just Congress--which had shown willingness but the probably temporary inability to act--but also pre-empted other DHHS and PHS bureaucrats in internal negotiations to solve the problem of scientific misconduct.

This chapter also reveals some difficulties with attempting to apply principal-agent theory. When we ask the simple questions that the theory begs of us to ask--who are the principals and agents? how do they interact? what techniques for control do they use?--the answers are not so simple. When a bureaucrat like Wyngaarden “pre-empts” an anticipated congressional action, it is difficult to distinguish the (presumed) principal from the (presumed) agent. Wyngaarden’s action could not be clearly described as an “anticipated reaction” for two reasons: first, because Dingell had tried and failed, the creation of OSI was not clearly done in anticipation; second, because Wyngaarden’s OSI was not in the same bureaucratic location as Dingell’s, its creation was not done to satisfy but to satisifice the congressional principal. Wyngaarden substituted information from his experience about the administration of the AIDS program for direct knowledge about congressional preferences.

The information asymmetry also seems to exist in both directions. Dingell’s intentions were crucial data for Wyngaarden, but so was the actual inability of Dingell to work through a short legislative calendar to authorize an office of scientific investigation. Members of Congress do make use of Inspectors General, as Niskanen suggests, but their quality is questionable: if members of Congress delegate oversight responsibilities to the IGs, who is competent enough to watch the watchers? One could imagine the entire principal-agent framework reversed and operating in similarly revealing way. Such a reversal would lead us to examine what agencies get out hearings (“lighting a fire under agency butts;” that is, they provide information for setting the agenda) and how agencies’ other “surveillance systems” are constituted (e.g., offices of legislative liaison). That the principal-agent theory maps onto a
congressional-bureaucratic relationship, respectively, is itself an assumption—one that becomes increasingly problematic as the ethnographic details of congressional-bureaucratic relations make clear. This is what the "two-way street" means.

The following chapter will continue to use these details to examine whether Wyngaarden's hopes of the continued separation of science and politics occurred at OSI. In tracing the parallel trajectory of changes in the definition of scientific misconduct and the policies and procedures for managing misconduct allegations, and finally the demise of OSI, we see that Wyngaarden's hopes were dashed. Surprisingly, they were dashed not by Dingell or Weiss, but seemingly by scientists themselves.
Chapter IV: The integrity of science, two: the boundary between science and politics

A. Introduction

The social contract for science has two premises: one, that science is self-regulatory; and two, that the production of technological and economic benefits is automatic. The former premise attempts to isolate science through the construction of a normative edifice maintained solely by scientists. The latter attempts to isolate science by delimiting the scope and variables of public intervention to the support of basic research (see V.B.2). The founding myth of the social contract for science is thus that science is separable from other activities, particularly politics.

Separability actually involves both the social contract for science and the social contract for scientists (II.B.1 and II.C.3.c). The latter is the agreement among scientists to abide by the norms of truth-telling and other social and technical norms (Zuckerman 1984; 1977). The former is the agreement between this so-constituted community of scientists and the political community. The social contract for scientists is a professional community model that has been applied by other professions to justify separate status and self-regulation. Some philosophers (Schmaus 1983) and ethicists (Bayles 1983) suggest that the social contract model is inappropiate for professional communities because it leads to problems in demarcating the boundaries of the community and defining responsibilities across these boundaries. This boundary problem is at the heart of my discussion about the development of definitions of scientific misconduct and policies and procedures for handling misconduct allegations in this chapter.

The previous chapter showed how politicians tried at first to rely on the low-cost solution of professional norms and preserve the social contract for science. Then, as congressional scrutiny revealed the politics of science in the making, National Institutes of Health (NIH) director James Wyngaarden pre-empted action by
Representative Dingell and Inspector General Kusserow by creating the Office for Scientific Integrity (OSI). Wyngaarden created a new institution at the frontier of science and politics that he hoped would preserve the social contract for science.

In this chapter, I will examine whether Wyngaarden's outpost succeeded or not. I focus on definitions, policies and procedures—the literary masonry of institutions—to see if they support the social contract for science. The success or failure of Wyngaarden's creature has implications for all three levels of analysis of the dissertation. From the principal-agent perspective on congressional-NIH relations, the details of OSI's pursuit of scientific integrity help delineate the extent of congressional influence. Indeed, the conflict between political and scientific solutions to scientific misconduct is not settled by the creation of OSI; instead OSI becomes the arena for its continued negotiation. From the perspective of the social contract for science, OSI could conceivably preserve a next approximation, as Wyngaarden hoped, if the details of its policy implementation provided for the kind of scientific dominance that the NIH director hoped to provide in his office. Yet as the chapter shows, the biomedical community rejected OSI, its plans, and what remained of the social contract for science. From the perspective of the relationship between democratic politics and science, it would seem that, in the strategies pursued by the biomedical scientists, that they did not view science as a model polity. Yet, oddly perhaps, even as more formal procedures, recognized as more akin to those of the broader society and in that sense more fair, are applied to questions of scientific integrity, the scientific community does in fact become more like a model.

The chapter is divided into three main parts. In the first, I review in greater detail the problems at the boundary of science and politics. In the second, I exemplify the boundary problem with respect to the contested evolution of the definition of scientific misconduct. In the third, I provide a second example of the boundary problem with the conflict over policies and procedures at OSI for investigating scientific
misconduct. I emphasize the work of individuals who attempt to mark these boundaries because the separability of science from politics is neither something that can be taken for granted, nor that can be defined and set aside. Rather it is subject to ongoing negotiations and only temporary settlements. Both the political community and the scientific community have a role in settling their shared frontier, but neither has a complete understanding of the interests of either community.

B. The boundary problem

1. Watching it get done

The analytical problem of demarcating science from other activities ("non-science") has long been a problem in the philosophy and the sociology of science (Gieryn 1983). The new sociology of science (the sociology of scientific knowledge--SSK) rejects the assumption that any clear or consistent demarcating line or principle can be found. In response to the "essentialism" of Karl Popper, Robert Merton, and Thomas Kuhn—which sought to identify unique demarcations for science—the "constructivist" approach views the demarcation of science as the product of contextually sensitive and interested activities on the part of scientists and others (Gieryn forthcoming). Moving beyond a critique of "essentialism," SSK has begun to focus on episodes in which "the boundaries of science are drawn and defended in natural settings often distant from laboratories and professional journals—a process known as 'boundary work.' Essentialists do boundary work; constructivists watch it get done" (Gieryn forthcoming:1; emphasis in the original). Constructivists interested in boundary work therefore observe the rhetoric and ideology of essentialists, because it is by the success or failure of their arguments and actions—and not by some inherent

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1 The critique focuses on: the difficulty of reproducibility with respect to Popper’s criterion of falsifiability; the flexibility of rules and rule-based behavior in response to Merton’s normative structure; and the contextually contingent nature of consensus within Kuhn’s paradigm.
or essential quality of science—that science and non-science are in practice
distinguished.

This chapter is an exercise in such constructivism. It is of political importance
and not just sociological interest because boundary work is itself political activity, and
because the result of this boundary work will be to demarcate politics from science,
thus establishing at least temporarily the extent of political influence. In other words,
the reach of congressional influence is constrained by the boundary negotiations that go
on at NIH during implementation. A problem defined during implementation as a
scientific problem may then lay beyond political scrutiny.

In my case study of misconduct in science, scientists, bureaucrats, and
congressional staff and members are all involved in doing boundary work. That is,
they are all attempting to determine the location, and thereby to enforce the integrity, of
the border shared by the polity and the republic of science. The influence of political
institutions and processes in the demarcation of boundaries between science and non-
science in risk and regulatory science has been noted by Jasanoff (1987:224), who
finds that “the effort to make such distinctions is politically charged. How one
characterizes an issue on the spectrum between science and policy bears on the way it is
ultimately decided, both institutionally and procedurally.” Not only does issue
characterization affect institutional and procedural decisions, but the converse is true as
well: the institutional and procedural context helps determine the boundary between
politics and science.

2. Separatism and cartography

“Separatism” in science policy is the practical manifestation of “essentialism” in
the sociology of science. “Separatists” believe in an analytically distinct science that
can be removed from policy decisions or, complementarily, in a distinct politics that can
be removed from science.² Jasanoff (1987) identifies three schemes of separatist

² Could one be a “separatist” without being an “essentialist”? I do not think that one can believe in an
analytically distinct science without being an essentialist; that is axiomatic. One could still desire and
thought in the regulatory arena: the “trans-science” or science policy scheme; the risk assessment and risk management scheme; and the peer review scheme. The first is derived from Alvin Weinberg’s (1972) attempt to distinguish scientific questions from trans-scientific ones, the latter having the form of scientific questions but which, for practical, ethical, or other reasons, could not be answered empirically.\(^3\) Scientific questions were, under Weinberg’s scheme, to be left to scientists, and trans-scientific ones to informed policymakers. The risk assessment and risk management scheme attempts to separate the “scientific” process of determining the magnitude of a potential hazard from the “policy” process of deciding what to do about it.\(^4\) This scheme, in protecting scientific sovereignty over the estimates of hazards, can circumscribe policy discretion by deriving a single risk that is directly comparable to other risks. The third separatist scheme is peer review, in which independent scientific panels are called upon to evaluate the scientific basis of regulatory decisions; this scheme is likewise consistent with circumscribing bureaucratic discretion by defining a distinct scientific component to be judged by outside scientists.

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\(^3\) The essentialism in Weinberg’s scheme is the Popperian idea that a scientific question is falsifiable. Questions that are falsifiable in practice are actually scientific; questions that are falsifiable in theory, as well as questions that are not falsifiable, are trans-scientific. For example, the question, “Alar is not a carcinogen at a normal level of ingestion” is falsifiable in theory, but not in practice. Actually determining its carcinogenic potential would require an impractically large number of mice to detect a one-in-a-million risk, which would be of policy interest. Likewise, such a question in humans would be trans-scientific for ethical reasons.

\(^4\) The risk assessment scheme believes that there are objective, empirically identifiable, and quantitative inputs to policy. Lowrance (1976:75-76) distinguishes between “measuringrisk,” which is “an empirical, scientific activity,” and “judging safety,” which is “a normative, political activity.” The National Academy of Sciences (NAS 1983:151) has likewise recommended that “regulatory agencies should take steps to establish and maintain a clear conceptual distinction between assessment of risks and the consideration of risk management alternatives.”
Peer review is the most general of the separatist schemes Jasanoff describes. It has also, of course, been applied in one form or another by both federal and private research sponsors to identify projects meriting support. As such, it is considered the sine qua non of self-regulation, "a symbol and guarantor of the autonomy of science (Chubin and Hackett 1990:2). It is this aspect of peer review tapped by the patrons of science to solve the problem of adverse selection: only peers are thought to know the best science, and the best science is thought to produce the best opportunity for technological advance. It is also the most important model for my purposes because, as I will explain in the third section below, it is the model upon which the original policies and procedures for adjudicating scientific misconduct were predicated. For the purposes of this chapter, I will also refer to peer review as "demographic dominance," since it asserts the authority of a group of people--scientists--over a process. Indeed, the other two schemes could also conceivably be reduced to demographic dominance; i.e., the trans-science and the risk assessment schemes can be read as heuristics to identify which issues should be dominated demographically by scientists and which need not be.\(^5\)

The social contract for science is also a separatist scheme. As such, it contemplates distinct domains of politics and science and the extent of their respective sovereignties. If science is conceived as a republic unto itself, then the social contract for science is a treaty, or perhaps more appropriately, a cease-fire line. In a similar but more confrontational metaphor, Latour (1991:4) has called the conceptual division between politics and science since the English Enlightenment a "shameful Yalta Pact" that resulted in an unproductively and dangerously bifurcated relationship.

This "cartographic metaphor" is a powerful one in constructivist studies of boundary work (Gieryn forthcoming). The metaphor is intuitive, and it complements the idea of some kind of contract or treaty between politics and science. The

\(^5\) This emphasis on demographic dominance corresponds to the definitions of the biomedical community and the scientific community in I.C.3 and I.C.5, respectively.
cartographic metaphor is also tied to ideology, for “[w]hatever else ideologies may be--
projections of unacknowledged fears, disguises for ulterior motives, emphatic
expressions of group solidarity--they are, most distinctively, maps of problematic
social reality” (Geertz 1973:220). After the Second World War, the close new
interaction between scientists and politicians was a “problematic social reality.”
Politicians desired to associate with and appropriate the successes of science, and
scientists reciprocated, desiring to do more science at the expense of the public purse.
The social contract for science was the ideological “good fence” for these two new
neighbors. The problem we observe with ideology is not that it is emotive or
nonscientific; but rather, like the map, it is often used for availability rather than
accuracy, and courses of policy plotted on the basis of ideology will tend to run
aground against newly emergent or previously uncharted social realities.7

3. Strains and interests

Geertz (1973) classifies the two prominent social determinants of ideology from
the sociological literature: interest theory and strain theory. In the former, ideology is a
“mask” and a “weapon” in a group’s struggle for advantage against other groups--a
battle plan for group sovereignty or autonomy. Thus the social contract for science is
deployed to protect the republic of science from external enemies like meddlesome

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6 Although Geertz (1973:198) explicitly rejects the Parsonian definition of ideology as “a discrepancy
between what is believed and what can be [established as] scientifically correct,” I believe that Geertz’s
cartographic ideology can be reconciled with, for example, the Downsian view that ideology is a low
cost substitute for information (Downs 1957). What are maps, after all, but selectively reduced-
information representations of the social, political, and geographical world? If a map were not a reduced
information representation, it would be (indistinguishable from) reality. This is not to say that Geertz
was no essentialist, for he distinguishes science and ideology by “the attitude contained toward them.”
For science, it “is one of disinterestedness...restrained, spare, resolutely analytic: by shunning the
semantic devices that most effectively formulate moral sentiment, it seeks to maximize intellectual
clarity.” For ideology, it “is one of commitment...ornate, vivid, deliberately suggestive: by
objectifying moral sentiment through the same devices that science shuns, it seeks to motivate action”
(Geertz 1973:230). It seems that the functionalist and essentialist Merton, for example, would have
less problem with this distinction than would constructivists, who would point to the non-empty
emotive or sentimental content of the very idea of science.

7 Whether such obstacles are either newly emergent or previously uncharted is largely uninteresting to
the constructivist. See I.C.4 about whether scientific misconduct is a newly increasing phenomenon.
bureaucrats and prying members of Congress. In the latter, ideology is a “symptom” and a “remedy” in a group’s search for equilibrium—a course of treatment for community health. The social contract for science is thus applied to meliorate the effects of upstarts or deviants. Geertz maintains that interest and strain are not mutually exclusive explanations of ideology, and that individually they have weaknesses. Interest theory’s “psychology is too anemic” and its “sociology too muscular” (Geertz 1973:202). Strain theory shares the explanatory problems that plague functionalism generally (Geertz 1973:206). Geertz dissolves the two explanations in their mutual neglect of rhetoric, or the style of ideological discourse.

Gieryn (1983:792) elaborates Geertz’s solution of strain and interest with the following formulation: strains “enable” alternative repertoires of discourse in boundary work; and interests guide their selection. In other words, scientists recognize the integral needs and interests of their community and they act to preserve them. Gieryn (forthcoming) identifies four types of boundary work—monopolization, expansion, expulsion, and protection—all of which defend and preserve the cartographic space and cognitive authority of science. Similarly, Jasanoff (1987:224) writes:

For scientists, the primary interest in these boundary disputes is to draw the lines between science and policy in ways that best preserve the

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8 Although I do not have the exact quotation, indeed one member of the National Academy of Sciences (NAS) Panel on Scientific Responsibility and the Conduct of Research—to which I served as staff—used the social contract for science phrase spontaneously in just this way. Similarly, Frank Press’s (1988:2) assertion that “[s]cience has been faithful to that compact...[a]nd we continue to honor that compact” expresses the same sentiment (II.B.3.d).

9 The NIH Guidelines for the Conduct of Research (NIH 1990:5), which begs of its target audience---intramural researchers at NIH—that “only by adherence to the highest standards of intellectual honesty in formulating, conducting and presenting research does science advance and do scientists fulfill their contract with the community at large.” By citing the social contract for science in this way, NIH hopes to inoculate its researchers with an added sense of responsibility in order to improve their behavior.

10 Gieryn (forthcoming) exemplifies monopolization boundary work with Shapin and Schaffer’s (1985) account of the battle between Boyle and Hobbes over the groundrules for establishing certain knowledge. Expansion is exemplified by the work of the Encyclopedists, who colonized other intellectual fields with scientific thought. The case of Sir Cyril Burt, in which the famed psychologist was degraded for his alleged misconduct but psychology was preserved, exemplifies expulsion boundary work. And Jasanoff’s work on regulatory science and science advisory committees shows the protective type. In each of these cases, the scientists maintained or enhanced their cognitive authority. Under what conditions might they not?
authority and integrity of science. Scientists have an institutional stake in reducing public interactions between science and the administrative process, since these interactions emphasize the indeterminacy and lack of consensus within science, thereby weakening science's (and the scientist's) claim to cognitive authority.

These statements about the interests of scientists must be tempered by the recognition that interests are probably as pliable as boundaries. It would be absurd to say that scientists act to destroy, rather than to preserve, the interests of the scientific community. But the long-term interests of the scientific community are not always clear, even when it comes to such central issues as preserving cognitive authority. There might even be a conflict between short-term and long-term interests. Except for a possible common stake in the demographic dominance of science, it is likewise absurd to maintain that all scientists have the same interests. Individual scientists may be forced into circumstances in which they must choose between the collective good of the cognitive authority of science and what they view as their private property in their reputations, grants, faculty positions, and so forth. There also might exist choices among the different strategies (e.g., protection or expulsion) that need to be explained.

Therefore, instead of focusing on cases where the interests guiding boundary work are monolithic, or the aggregate and individual interests are consonant, or where strategic choices are clear, I examine a case in which the cognitive authority of the community is at stake, but interests are at odds and the strategies multiple.\footnote{It strikes me that this approach may not actually be case-specific, but rather it depends on the level or stage of examination. Just as constructivists discount "Nature" as an actor in the settlement of scientific questions, they should also discount a preconceived notion of "cognitive authority," because cognitive authority is itself what is being constructed by the boundary work. Cognitive authority may alternatively be procedural dominance, demographic dominance, conceptual dominance, etc.} The negotiation strategies and subsequent boundaries in the misconduct case could not have been deduced \textit{a priori} from hypothesized interests of the scientific community: scientists were not satisfied with what seemed to be the most obvious route to demographic dominance. My analysis of the controversy surrounding the definition of misconduct and the application of due process finds that a variety of alternative
repertoires were aired, more than one of which might have advanced a conception of
the cognitive authority of science. The choice of repertoires and its consequent effect
on the cognitive authority of science also bears on the ability of the scientific
community to exclude political control, to maintain the social contract for science, and
to serve the instrumental and ideological needs of democratic government.

C. Defining fraud and misconduct in science

1. Alternative repertoires

There are a variety of ways to define scientific misconduct, in part because there
are a variety of social contexts in which misconduct can occur and in part because there
are a variety of conceptions of science. The contexts for scientific misconduct range
from individual laboratories, through immediate institutional settings such as an
academic department, to broader institutional settings such as universities and
professional societies, to where the buck stops with the sponsor or patron of the
research. Each contextual level can have a different standard of conduct, and each
member of a single contextual level can have a different standard as well. In setting a
standard, each level may also invoke a different conception of science. This Protean
flexibility of science--the huge number of available repertoires--makes defining
scientific misconduct (and other boundary tasks) a Herculean task. Indeed, rational
argument and analytical distinctions may have less to do with such outcomes than
power, structural jurisdiction, or political fortune--although tracking such rhetoric as I
do in this chapter helps indicate where the power enters the equation. Therefore, the
stakes in selecting a repertoire and defining scientific misconduct reach beyond the
merely operational ability to adjudicate cases to the relative balance of political power
between scientific and political institutions. This political power is used to define
membership in one community or the other and to regulate the conduct of members.
Negotiating the boundary between science and politics does not involve merely the
control of a small corner of the federal bureaucratic apparatus, but it implicates broader questions of the cultural and cognitive authorities of science and politics. 12 Without much hyperbole, at stake are the questions of who is a scientist, what is science, and what is the extent of politics.

Since I am interested in the social contract for science and the public consequences of its underlying model of science, the repertoires dealing with various conceptions of science are the most interesting. Although one could develop a story about how different laboratories with idiosyncratic cultures or traditions define conduct, that question has lesser public consequence than questions about defining conduct at higher contextual levels. The search for public consequence draws attention up the contextual pyramid because dollars—those indestructible atoms of accountability—emanate from the top. But in the social contract for science, accountability has been delegated from the sponsoring congressional committees back down the contextual pyramid to the funding agencies and to the universities, 13 as for example, the Health Research Extension Act of 1985 did for scientific fraud investigations.

The story of movement among contextual levels for the definition of scientific fraud parallels the story about the locus of responsibility for fraud (III.C and D). The first hearings by Representative Gore and by Senator Hatch in 1981 adopted the vernacular of "scientific fraud," and there was little indication of problems. For Gore, "[t]he ethics of fraud, per se, [were] clear" (U.S. Congress 1981a:2). Congress adopted this popular nomenclature in the Alcohol and Drug Abuse Amendments Act of 1983 and the Health Research Extension Act of 1985. There was no self-conscious effort to define the behaviors, consistent with the maintenance of the social contract for science. Congress delegated the problem to universities and agencies, which attempted to work together but who ultimately failed in the eyes of members of Congress, who

12 This is largely the theme of Ezrahi (1990), who argues that the erosion of the authority of science undermines liberal-democratic politics.

13 Which are, rather than individual investigators or laboratories, the legal recipients of federal research grants.
subsequently attempted to reappropriate the issue. However, within the biomedical community, the most important variable for discussions of definitions was the conception of science.

Below I describe four alternative repertoires by which fraud or misconduct may be identified. They are “repertoires” because they are each a bundle of elements: a particular conception of science, an embedded causal theory of fraud, and a set of prescriptive actions. Each repertoire has elements that, when emphasized, could protect the cognitive authority of the scientific community. Each repertoire also has elements that could degrade that same authority. I have named them after their original describers: Charles Babbage, Robert Merton, Harriet Zuckerman, and Warren Schmaus. After I present these repertoires, I will discuss how the scientific community struggled to choose among them.

a. Babbage

The brief writing on scientific integrity by Charles Babbage—mathematician, inventor, and prophet of information technology—is widely cited in discussions of scientific misconduct, and his nomenclature is widely integrated into common parlance. In *Reflections on the Decline of Science in England*, Babbage critiques the Royal Society and, in passing, describes four forms of scientific dishonesty: “hoaxing,” “forging,” “trimming,” and “cooking.” Babbage defines these four deviations as “the frauds of observers” and provides real and hypothetical examples (Babbage 1989 [1830]:90-93). Both “hoaxing” and “forging” are intentional deceit of a grand scale, differing “inasmuch as in the [hoaxer] the deceit is intended to last for a time, and then to be discovered, to the ridicule of those who have credited it; whereas the forger is one who, wishing to acquire a reputation for science, records observations

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14 One of the most widely circulated professional publications on misconduct (Signa Xi 1986) cites Babbage and his nomenclature is incorporated into another (NAS 1989). Both Merton and Zuckerman cite Babbage as well.
15 Babbage does not mention plagiarism, perhaps because that form of dishonesty is not limited to what Babbage considers science, which seems to relate to observers rather than writers or readers.
which he has never made." "Trimming consists in clipping off little bits here and there from those observations which differ most in excess from the mean....This fraud is not perhaps so injurious (except to the character of the trimmer) as cooking...[because] from respect for truth, or from prudent foresight, he does not distort the position of the fact he gets from nature" (emphasis in the original). Cooking "is an art of various forms, the object of which is to give to ordinary observations the appearance and character of those of the highest degree of accuracy," including selective reporting of only the best observations, the repeated statistical modification of observations, or the selection of the value for constants or other catalogued numbers that provides the best result.

For Babbage, intent is the crux of "the fraud of observers." Each fraud is an intentional act with known consequences. As intentional acts, frauds are distinguished from errors caused by the improper use of instruments, discussed by Babbage (1989 [1830]:86-88) in his immediately prior subsection, "On the art of observing." Fraudulent observers do not "really value truth" and a "fraud of observers" is the action of a person contrary to that central tenet of science. "In fact, the character of an observer, as of a woman, if doubted is destroyed" (Babbage 1989 [1830]:92). Fraud directly contradicts the reward system of science, damaging that system and the fraudulent individual. Although he does not use such terms explicitly, Babbage describes a situation where only an irrational, psychopathic, or sociopathic observer would commit a fraud. Such an explanation implies that fraudulent observers can be cut off from the rest of the scientific community in a relatively cost-free way. However, by focusing on individual intent, this repertoire can risk excluding scientists from the adjudication of fraud, because scientists do not have a privileged claim on discerning the intent of their comppeers.\textsuperscript{16} Nevertheless, this explanation also denies any meliorative action toward the scientific community.

\textsuperscript{16} It also risks overlooking fraud committed by more than one individual, but presumably "they" would be "psychopaths" as easily as a sole individual could be.
b. Merton

Sociologist Robert K. Merton argues that the effective institutionalization of the norm of disinterestedness protects science from fraud. Fraud, as a violation of disinterestedness, is marked by intent. Merton demonstrates this connection by going through great pains to distinguish between intentional plagiary and various unintentional duplications such as multiple discoveries and "cryptomnesia" or "unintentional plagiary" (Merton 1973:ch.18).

But Merton describes how such deviations from disinterestedness are driven by the conflicts within the normative structure of science itself, which can induce anomie in scientists by impressing them into ambivalence (be humble, but fight for priority) and by failing to provide sufficient rewards for approved behavior (only the first gets recognized, regardless of how good or how close the second is) (Merton 1973:chs.14 and 18). The "social pathologies of science" are therefore the "results of specific discontinuities between the normative structure of science and reward systems....[E]xtensive institutional emphasis upon recognized achievement, greatly encouraged by the reward system, can be as dysfunctional for human purposes and social institutions as the systematic flouting of norms" (Merton 1973:ch.13, 282-83). "The culture of science is, in this measure, pathogenic" (Merton 1973:ch.14, 323).17

Merton's normative structure of science is generally regarded as a defense of scientific autonomy. But anomie theory, the embedded theory of causation in this

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17 Merton has some ambivalence himself on the relative roles of institutionalized social norms and scientists' own personal integrity. Compare: "The virtual absence of fraud in the annals of science...has at times been attributed to the personal qualities of scientists...who exhibit an unusual degree of moral integrity. There is, in fact, no satisfactory evidence that such is the case; a more plausible explanation may be found in certain distinctive characteristics of science itself" (Merton 1973:ch.13, 276) with "Apart from the moral integrity of scientists themselves--and this is, of course, the major basis for honesty in science--there is much in the social organization of science that provides a further compelling basis for honest work" (Merton 1973:ch.18, 311).

18 Merton formulated this view in opposition to a common thesis, articulated in James Watson's The Double Helix (1963), that the modern research environment is to blame for deviance (which resembles conflict theory; see Weinstein (1979)). In all generations, Merton argues, the inability of the reward system to distribute benefits to all within the community who aspire to them fosters deviance. Others split the difference between Merton and Watson by suggesting that conflicting norms may be found in different research settings (Mitroff 1974) and as parts of evolving institutional contexts (Ziman 1990).
repertoire, condemns the ambiguous nature of scientific norms and the "pathogenic" nature of the reward system. In fact, "[t]he great cultural emphasis upon recognition for original discovery can lead by gradations from these rare practices [of overt falsification] just beyond the edge of acceptability, sometimes without the scientists' being aware that he has exceeded allowable limits" (Merton 1973:ch.14, 311). Individual scientists cannot then be easily distinguished from the group because they draw motivations from group norms and institutionalized rewards, and may in fact be acting without explicit, deviant intentions. Further, the policy prescriptions embedded within anomie allow the possibility of political manipulation of norms and rewards to control fraud.

Merton, however, plays down this pathogenic risk of fraud to the body scientific--and by implication to the body politic--by reference to its extreme infrequency, the "moral integrity of scientists themselves," and the "public and testable character of science" (Merton 1973:ch.18, 311). Merton (1973:ch.13) argues against the political manipulation of the reward system or the normative structure of science because it can only be counterproductive for science and for society. The bad comes with the good of the normative structure of science. At best, perhaps, government policies might protect and reinforce those norms in specific ways, but productive science always has these modest flaws.

c. Zuckerman

Harriet Zuckerman (1977) expands upon Merton's norm-based account to provide a greater array of definitions of fraud. Zuckerman motivates Merton's distinction between social norms (CUDOS--see II.C.3.c) and technical (or cognitive) norms (empiricism and logical consistency) to distinguish between fraud and error, respectively (Zuckerman 1988; 1977). Fraud is a deviation from social norms, error from technical norms. Zuckerman further subdivides error into two species: reputable

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19 Zuckerman does admit the distinction between technical and social norms is impossible in reality, but maintains that it is useful in analysis.
errors, those beyond a scientist’s technical control or cognitive grasp; and disreputable errors, those contrary to accepted practice, such as inadequate use of controls.20 Zuckerman divides the genus fraud into species by the degree of their impact on science as a body of knowledge. In this context, plagiarism might be viewed as a lesser crime than fabrication of data because it affects the reward system rather than the content of scientific knowledge directly. In this manner, Zuckerman (1977:120-22) also distinguishes less significant violations of social norms such as seeking publicity and failing to acknowledge colleagues appropriately.

Zuckerman thus attempts an analytic distinction between error and fraud, but not one based on intent. Similarly, she argues, law does not distinguish between between “actual [willful] fraud” and “constructive [negligent] fraud.” “In other words, fraud is fraud whether intended or not. The same is true in science” (Zuckerman 1977:115).21 Science developed no explicit mechanisms to manage fraud because, unlike other professions, it lacks a relationship with a clientele who must be protected (Zuckerman 1977:97). Although Zuckerman (1988; 1977) explicitly rejects a number of standard sociological explanations for deviant behavior in science, including anomie theory, as lacking sufficient empirical evidence, she does recapitulate the Mertonian hypothesis that the prevention and detection of deviance is dually constructed by the individual integrity of scientists and the institutionalization of norms.22 More than Merton, Zuckerman emphasizes the process of socialization for supporting or internalizing norms.

20 Zuckerman (1977:110) maintains that “Scientists implicitly discriminate ‘reputable’ from ‘disreputable’ errors,” thereby suggesting that her conceptual distinctions have a practical life.

21 Zuckerman’s legal authority on fraud is Black’s Law Dictionary. Her assertion that the law does not distinguish frauds by intent is troubling given other arguments about legal fraud (IV.C.4). It is not clear whether Zuckerman wants scientific fraud to follow legal fraud logically, normatively, both or neither.

22 Zuckerman (1977:131) does not judge which is more significant, although she does hypothesize that this combination makes for the unusual situation in which the pressure for deviance and the chance of detection rise together.
The identification of fraud in Zuckerman's scheme is therefore dependent on a
three-fold process: the identification of norms in science; the distinction between
technical and social norms; and the identification of serious deviation from social
norms. This process of identification would likely be dominated demographically by
scientists. In judging allegations of historical cases of scientific fraud, Zuckerman
(1977:106) shows a marked deference for context: "since deviant behavior is socially
defined, the question of its occurrence must be judged in the context of its time." One
can reasonably infer that she would extend this contextual sensitivity to disciplinary
variations. Zuckerman also implies the limiting of adjudication to scientists by arguing
that there are "phases" of scientific research during which some violations from social
norms are not deviant, so adjudicators must be sensitive to the phase context of
research as well as historical and disciplinary contexts. This focus on context is
important because it shows that only those cognizant of social definitions—that is, those
socialized into those definitions--are context-sensitive enough to judge allegations
fairly. Thus, only scientists could adjudicate allegations of misconduct. Such an
explicit reliance on peer review would allow scientists to retain demographic dominance
over an adjudicatory process, as they might not over an intent-based process derived
from Babbage's repertoire. Meliorative policy action is possible in this repertoire, and
would likely center on those few reinforcements of the normative structure a la Merton,
as well as possible regulatory reinforcement of socialization, for example training and
mentoring.

d. Schmaus

Philosopher Warren Schmaus (1983), responding to Zuckerman, suggests that
the standard of judging deviance in science should be general moral precepts related to
being honest and doing one's duty, rather than any technical or social norms thought
distinctive to science. Schmaus first argues a general point that the definition of

23 For an example of the application of general moral theory to specific problems in scientific practice,
see Gert (1989).
special moral norms cannot guide the conduct of members of a professional community. Then Schmaus (1983:13) argues specifically that the norm of disinterestedness is not sufficient to the task of protecting against fraud because in Merton’s formulation disinterestedness is “wholly derivative from the other norms of science,” particularly organized skepticism. The systematic criticism and testing of one scientist’s results by the community should therefore “provide a sufficient guarantee of honest work” (Schmaus 1983:13-14) and thus self-interest, rather than moral compulsion, deters fraud. Schmaus (1983:14) rejects Zuckerman’s analytic distinction between fraud and negligence because both “result from the scientist’s failure to subject his or her own beliefs or practices to critical scrutiny.” That is, in both cases, scientists fail to do their job well. Schmaus also suspects that the reason why science has failed to develop internal procedures to detect fraud is not related to its lack of a clientele. Rather it is precisely because the verity of scientific findings—meant to correspond with reality—has little to do with whether findings were made honestly, sloppily, or fraudulently. In other words, a claim can correspond to reality even through accident or deviance as well as through good science, and scientists in practice are more concerned with correspondence than with how that correspondence was achieved.

Of the various repertoires, only Schmaus’s is explicitly concerned with science policy and acknowledges the public clientele of science. The approach “has the virtue of making negligent, careless, sloppy, and reckless work just as much a violation of a moral duty as fraud” (Schmaus 1983:17), an important virtue in a society in which science is both publicly financed and the basis of expected technologies, as negligence or fraud could cause serious harm. Both error and fraud pose serious problems to publicly supported science, “but to distinguish fraudulent misrepresentation of scientific results from errors resulting from honest mistakes often requires access to the intentions of the scientist in question” (Schmaus 1983:18). A standard of honesty and

24 In a similar way, Mulkay (1980) argues that norms or rules cannot unambiguously be connected with social action.
duty rather than one of norms or deviance makes such difficult questions of intent irrelevant.25

Like the other repertoires, however, Schmaus’s also fails to address the more political question of who will perform the tasks of defining sanctionable conduct. But again, we can infer the “who” from Schmaus’s description of “what.” “In the case of scientists, this general moral rule [governing their conduct] entails that they fulfill their obligations as scientists. The responsibilities of scientists, in turn, consist largely in maintaining the highest standards of intellectual rigor” (Schmaus 1983:20). Who would determine the standards? Arguably, departments, faculties, disciplinary and professional societies, and peer review boards who already perform such tasks. So even with the potentially harsh consequences for both negligence and fraud, the Schmaus repertoire also provides for demographic dominance.

2. Airing the repertoires

Representative Gore’s 1981 hearing was his subcommittee’s first of that year, and the chairman outlined an ambitious and comprehensive set of questions to investigate with respect to scientific fraud. Gore hoped to discover whether the recent spate of cases were merely “episodes that will drift into the history of science as footnotes” or whether they were more closely coupled to the environment and organization of science (U.S. Congress 1981a:2). Clearly if the latter, there would be an active role for the House Science Committee. He was presented with three of the four repertoires by the witnesses before the subcommittee.

National Academy of Sciences (NAS) President Philip Handler, who took “little pleasure and satisfaction in testifying” (U.S. Congress 1981a:10), performed the Babbage repertoire.26 Handler dismissed fraud as “an aberration that is difficult for the

25 But Schmaus only takes into consideration the instrumental consequences of bad science and not the ideological effect bad science would have by failing to provide a good model for politics (Ezrahi 1990).
26 Another witness, Ronald Lamont-Havers, the director of research at Massachusetts General Hospital where one of the scrutinized cases occurred, supported Handler’s use of the Babbage repertoire: “the reasons why investigators would disseminate fraudulent data are not due to the environment in which they work, but in themselves as human beings.” Lamont-Havers also went so far as to say that reduced
rest of us to understand” and as the result of “psychopathic behavior” (U.S. Congress 1981a:12). He refused to admit any connection between the serious cases of fraud under scrutiny and the nature of the research enterprise, and he rejected congressional interest by denying that scientific fraud was of larger social concern because the management of fraud “occurs in a system that operates in an effective, democratic, and self-correcting mode” (U.S. Congress 1981a:11).27

NIH director Donald S. Fredrickson spoke from a different repertoire.28 Fredrickson referred directly to the authority of Merton, testifying that violations of the scientific ethic are part of the natural fabric of science and as such have been around for a long time (U.S. Congress 1981a:32). Fredrickson toed the Mertonian line that science has always been competitive, but that the correction of fraud and errors is both “feasible and inevitable” because of the “rational nature of the process” (U.S. Congress 1981a:38). The bad and the good are derived from the same structure, and as a natural but disturbing part of the scientific process, fraud needed to be managed only through the most flexible kinds of administration. Fredrickson described the progress NIH had made in “develop[ing] some new administrative procedures” including “the promulgation last year of debarment regulations” (that had previously applied only to contracts but would now apply to grants as well) and in establishing an internal system to track violators.29

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27 Note Handler’s invocation of political values by his use of the word “democratic.” The implication is that the scientific community is, in the instance of misconduct, like a separate polity whose values are comparable to those of the larger society. Although scientific decision-making in no way resembles a “one person, one vote” system, some (e.g., Ezrahi 1990; 1980) have suggested that scientific decision-making does provide a peaceful, instrumental, and meliorative model for liberal democracy.

28 Fredrickson confirms this distinction between Handler’s and his own testimony. Interview with Donald S. Fredrickson, former director, NIH, Bethesda, MD, January 5, 1993.

29 Not surprisingly, William F. Raub, then associate director for extramural research at NIH, maintained the same repertoire in his testimony. He explained that the same research environment that may pressure scientists to misdeeds also “provides powerful sanctions against deceptive practice” (U.S. Congress 1981a:167). Raub’s testimony, on April 1, 1981, was in large part directly drawn from a memorandum from Fredrickson to the DHHS Secretary, written on August 7, 1980. This fact, and the lack of progress at NIH it implied, was not lost on the subcommittee. Among other things,
A third witness, Princeton sociologist Patricia Woolf, cited Harriet Zuckerman’s work in presented a third alternative repertoire to the subcommittee. Woolf spoke of the two pillars against fraud: control mechanisms such as replication; and, “most vital,” the socialization of individual scientists (U.S. Congress 1981a:355; 351-52). Woolf also used the language of scientific “malpractice,” which parallels Zuckerman’s conflation of “actual” and “constructive fraud.” Akin to Zuckerman’s emphasis on the role of socialization, Woolf advocated possible educative and mentoring efforts for scientists to combat misconduct.

The real-life case that the subcommittee scrutinized could not assist in deciding among the repertoires. Scott Long, the scientist whose misconduct was at issue, told the subcommittee that environmental pressures created the situation, but that he failed in his responsibility to understand and mitigate those pressures (U.S. Congress 1981a:59). There was also conflicting testimony from witnesses with respect to the details of the case. Gore complained at the outset of the hearing’s second day that the testimony had been “interesting and perplexing” and that “[i]n one short hour, the subcommittee went from the Olympian heights of a nonproblem to the depths of a potential perjury.” Gore said that nobody wanted to believe that the problem was “with the fiber of science” but maintained that it was best to examine its “dark corners” of its fabric nevertheless (U.S. Congress 1981a:111).

As the Gore and Hatch committees did not pursue the scientific fraud question beyond the mild measures described in the previous chapter (III.C), and as the two brief amendments in the ADAMHA and NIH reauthorizations used the vernacular of “scientific fraud,” NIH was left to sort out the various repertoires. In doing so, it also helped determine the reach of congressional control.

Fredrickson wrote in the memo that the debarment regulations to which he himself referred in testimony were designed for “business management activities” and were inappropriate for scientific fraud. Fredrickson maintained, “Where the substance of science is involved, however, a more strongly held bias against administrative intervention comes into play.” The memo is reprinted in U.S. Congress (1981a:150-54).
3. Early uncertainty in regulatory repertoires

Even before explicit congressional attention to scientific misconduct, NIH had begun moving from extreme informality in managing cases (see D.1 below) to define misconduct as part of producing their debarment regulations. The final rule for debarment (DHHS 1980:67266) described a “record of serious unsatisfactory performance (or failure to perform)” as a cause for debarment, among other causes including criminal convictions, statutory or regulatory violations, and other government debarment. DHHS explained that the “use of the term ‘serious’ is intended to limit debarment to causes involving matters of importance, or which have important or dangerous consequences [including] intentional falsification of research reports in order to meet deadlines, comply with the terms of awards, or secure personal or professional advantage” (DHHS 1980:67263).

Two years later, after the Gore and Hatch inquiries, NIH included under the rubric misconduct (1) mismanagement of funds, (2) fraudulent or markedly irregular practices in carrying out research procedures on [or?] handling research results, (3) serious failures to comply with requirements governing the protection of human subjects and the welfare of laboratory animals, and (4) serious failures to comply with other conditions of an award such as the guidelines for research with recombinant DNA molecules (as quoted in Olswang and Lee 1984; emphasis added).

NIH thus treated fraud or “markedly irregular practices” as one among many kinds of actionable misconduct specific to their grantees, despite the fact that as early as August 1980 NIH director Fredrickson had decided that such treatment was insufficient for scientific rather than business administration matters.30 To some extent, this definition paralleled the Schmaus repertoire, because it did not distinguish among various ways in which federally funded scientists could fail to perform their jobs honestly. The unity of

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30 Fredrickson left NIH several months after Ronald Reagan took office, although he had been asked to remain; James Wyngaarden became the new director in February, 1982.

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the definition was precisely why, Fredrickson thought, the definition needed to be changed. In the 1980 memo, Fredrickson wrote:

Fraudulent research practices present a different set of problems. In the first place, the process by which such violations are discovered in quite different [than auditing financial records]...And while the scientific method is subject to a general body of ethics, including generally accepted medical practice, consensus as to the existence and seriousness of specific violations may not be easily attained.

That is, scientific fraud differs from financial fraud because scientists, rather than auditors, discover the former (they dominated its discovery demographically), and because there is little consensus on the details of what constitutes scientific fraud in any event.

By early 1983, NIH had still made little progress in separating the business aspects of misconduct from the scientific aspects, defining misconduct as “breaches of professional ethics that raise serious questions about an investigator’s or institution’s scientific or fiscal integrity” along with material failure, human or animal subjects violations, and “serious failures to comply with other specific terms or conditions of an award” (Brandt 1983). Shortly thereafter, PHS revised the definition:

“Misconduct” is defined as (1) fraudulent or highly irregular practices in carrying out research or in reporting the results of such research; (2) material failure to comply with Federal requirements affecting specific aspects of conduct of research, e.g., the protection of human subjects and the welfare of laboratory animals; and (3) serious misappropriation of Federal research funds, e.g., diversion of such funds to personal use (as reprinted in U.S. Congress 1989d:701).

Neither definition succeeded in separating the scientific matters from the business matters—in moving from the Schmaus repertoire according to Fredrickson’s script. The character of scientific misconduct fluctuated radically, as the first 1983 definition is expansive and even perceptual (“raise serious questions”) in nature, but the second returns to what was largely the 1982 definition of “fraudulent or highly irregular
practices.” Even this definition, however, retained the legal and vernacular ambiguities of the word “fraudulent” in conjunction with the professional connotation of “practices.” Although yoking scientific and business practices together to a common definition of misconduct, the definition did not attach an honesty or duty standard that would satisfy the Schmaus repertoire. PHS had not yet managed to invoke a repertoire clearly.31

4. Moving to a norm-based repertoire

By 1985, a working group chaired by deputy director for extramural research William Raub had changed the definition again, dropping the term “fraud” and substituting “misconduct in science” (PHS 1985). In draft policies and procedures (dated 10 October 1985) and in the published form (dated 18 July 1986), PHS defined misconduct in science as: “(1) serious deviation, such as fabrication, falsification, or plagiarism, from accepted practices in carrying out research or in reporting the results of such research; or (2) material failure to comply with Federal requirements affecting specific aspects of the conduct of research—e.g., the protection of human subjects and the welfare of laboratory animals” (NIH 1986; emphasis added). Raub wrote in the preface to the 1986 policies and procedures:

Excluded are deviations from grant or contract management policies....Also excluded are matters that involve possible criminal violations. Most importantly, perhaps, this definition does not include certain types of possibly inappropriate practices that should be of concern to scientists everywhere but do not necessarily call for Federal

31 Meanwhile, the professional groups began to publish documents. The Association of American Medical Colleges (AAMC 1982) used the nomenclature “research fraud” and although the report did not explicitly discuss a definition, it did define fraud parenthetically as “the intentional fabrication, falsification or ‘stealing’ of research data.” Like PHS, AAMC seemed to conceive of fraud as a subdivision of misconduct, referring to “misconduct, including fraud.” A report by the Association of American Universities (AAU 1983) classified “fraud or deviance” into four categories: “scholarly fraud by falsification of data, plagiarism, abuse of confidentiality, and deliberate violations of regulations.” Both reports use what appears to be an intentionally flexible nomenclature. Reviewing these and other professional and university reports, Olsang and Lee (1984) describe the central elements of fraudulent acts as “deliberate action by an individual with a view towards claiming credit for work or thought achieved under improper means. Further, these acts require examination by experts within the field in question to detect, analyze and prove.”
action. These include, for example, coauthorship practices, recognition of collaborators, and multiple publication.

PHS thus carved a niche for scientific misconduct separate from business administration, criminal behavior and unprofessional conduct. The details of this activity are obscure. Then-NIH director Wyngaarden explained it this way:

Initially, we had used the word fraud, and we were advised that fraud had a very different meaning in traditional English law, that fraud is intentionally depriving someone else of something of value. And so you had three features of that: one was the intent....And two was depriving someone of something of value, which was usually financial. Now, there might or might not be intent in something that was fraud; we'll assume that if it’s basic misconduct, there’s an intent to deceive there. But when you now go the next step, whom are you depriving of something of value? And what is its value? You get into such imprecise argument that the definition fails. Furthermore, if it is financial, we already have plenty of laws to deal with that. We didn’t want to put fraud in, if it was just not accurate.

The overt reason for this change involved the legal subtleties of the term fraud, particularly the necessity to link the questionable activity to harming an identifiably individual. Although fraud had been the scientific vernacular since Babbage, this repertoire ran up against the well-codified, legal use of its vocabulary. Agency scientists like Wyngaarden and Raub found that the legalistic connotations of fraud

32 The definition clashed with the fraud language in the Health Research Extension Act of 1985, because the legislation was actually three years old. In an Advanced Notice of Proposed Rulemaking (PHS 1988), PHS later warned its readers:

not [to] infer any intent on the part of the Department to broaden the coverage...beyond that intended by the Congress. Rather, the term 'scientific misconduct' is being used because it has become widely accepted in the scientific community and there is no indication that the Congress meant anything different when it enacted [the 1985 change]. In fact, both the conference report and the bill report accompanying Pub. L. 99-158...use the terms 'scientific fraud' and 'scientific misconduct' interchangeably. In addition,...it is unlikely that Congress intended to equate 'scientific fraud' with common law fraud.

The ANPRM then outlined in greater detail Wyngaarden's argument that "in the case of scientific misconduct, it is frequently difficult, if not impossible, to ascertain who the defendant intended to deceive."

limited their dual goals of protecting scientific research and protecting federal funds (see also Olswang and Lee 1984; and Andersen 1988). Indeed, Raub also used this reasoning to reject adoption of the Schmaus repertoire:

In his written response to earlier draft of [Schmaus’s] article, William Raub...agreed that:

The distinction between fraud and negligence is tenuous if both are viewed as failure to maintain the standards of intellectual rigor that ought to characterize scientists’ work. We [at NIH] have encountered instances of alleged fraud that upon closer examination turned out to represent carelessness or lack of intellectual rigor. In such instances research findings may be worthless, regardless of a investigator’s motive.

Raub states that deliberate fraud alone, however, constitutes clear grounds for debarment or some other serious sanction. With no precise standards for what we can expect from a “reasonable and prudent” scientist, he believes, it remains unclear whether researchers can be debarred for negligence (Schmaus 1983:20).

Legal fraud was too difficult to prove in the context of science—even when scientists had, in their own minds, clearly done something wrong.

Rejecting the standard of harm implicit in fraud also accepts the sense of the Merton and Zuckerman repertoires that science has no identifiable clientele who could be harmed by misconduct. It rejects the perceptual aspect (“raise serious question”) that an earlier definition had acknowledged. The substitute language, “serious deviation...from accepted practices,” professionalizes the definition along the lines of Merton or Zuckerman as well. That is, it assumes the existence of “accepted practices” and suggests the demographic dominance of scientists in determining what a “serious deviation” is.

This initial shift from an ambiguous repertoire, with elements of Babbage and Schmaus, to a Merton or Zuckerman repertoire fits well within the maintenance of the social contract for science model in the mid-1980s. It demarcated what conduct could have been managed by scientific self-regulation. The new professional, practice-
norm-) based repertoire provided for the continued delegation of scientific integrity
issues to the scientists themselves. And it narrowly constrained the attention of
administrators and hopefully the Congress to conduct that was largely defined by what
it was not—not financial, not criminal, not merely unprofessional.

By using norm-based language, the 1986 PHS definition attempts what Gieryn
would call protection boundary work—only scientists could recognize “serious
development...from accepted practices” and distinguish such deviations from the excluded
category of “inappropriate practices.” They would maintain demographic dominance
over misconduct, much like peer review of the quality of research proposals. The
narrow construction of the definition runs counter to the exclusion strategy because
scientists whose practices are “inappropriate” are still identified as not subject to
expulsion or other sanctions, although they could perhaps be subject to less severe
penalties administered informally by their compeers. Expulsion is, in this sense, ad
hoc boundary work—a last resort and clearly directed at other goals like protection.

5. Refining the repertoire re intent

Discarding fraud, however, did not mean that PHS simultaneously discarded
the question of intent. PHS had not yet decided whether intent mattered, whether its
repertoire would be Merton or Zuckerman. After the Dingell and Weiss hearings in
1988, PHS published an advanced notice of proposed rulemaking (ANPRM; PHS
1988:36344-36347) that discussed the definitional issue in depth.34 The ANPRM
asked, “should scientific misconduct be limited to only intentional transgressions [as in
the Merton repertoire], and if so, how should the requisite intent be defined? Or rather,

34 PHS took the ANPRM route at the behest of OMB, which had rejected the application of the 1986
interim guidelines as permanent rules. OMB felt it necessary for PHS to take the formal rulemaking
procedure. See David L. Wheeler, “Proposed NIH rules on science fraud are sent back to drawing
board,” The Chronicle of Higher Education (June 1, 1988). Wheeler also reports that OMB had two
substantive problems with the proposal: that PHS should adopt a system more similar to the Food and
Drug Administration (FDA), which routinely audits research; and that PHS attention should be
restricted to scientific fraud rather than scientific misconduct.
should scientific misconduct encompass negligent conduct [as in the Zuckerman repertoire]?

The PHS proposed rule subtly changed the definition from the 1986 version, defining misconduct in science as "(1) fabrication, falsification, plagiarism, deception or other practices that seriously deviate from those that are commonly accepted within the scientific community for proposing, conducting, or reporting research; or (2) material failure to comply with federal requirements that uniquely relate to the conduct of research" (PHS 1989:2; emphasis added). This approach modestly subordinated the vague nature of "other practices" to the more concrete and consensual "fabrication, falsification, plagiarism, [and] deception."

After receiving 139 letters in response to the proposed rule, PHS altered the definition to exclude the "material failure" section because it duplicated other guidelines or policies. This alteration culminated the separation of scientific practice from business administration, a separation appropriate within the social contract for science.35 It also excluded "'deception' inasmuch as deception can be an acceptable component of specific types of research" (PHS 1989:2-3).36 PHS retained the omnibus "other practices that seriously deviate" category of misconduct "to assure coverage of any serious misconduct that might not technically be considered 'fabrication, falsification, or plagiarism'" (PHS 1989:3). Former NIH director Wyngaarden explains how this "catch-all" category could work:

We [had] one example of a cancer scientist, who probably had manufactured many of his numbers, but that is very difficult to prove. The person did finally admit that his records were so substandard that he would accept a debarment on the basis of not having met the expectations of research on that degree. It wasn't plagiarism, but fabrication and falsification are extremely difficult to prove. Somebody

35 Public respondents to the ANPRM approved of the move from fraud to misconduct, including the acceptance of gross negligence as misconduct. They opposed the catch-all category, however (Rhoades 1989).

36 Deception is used as an experimental technique in some psychological and behavior research. Complications with regard to deception were central in the Federal government's decade-long struggle to issue government-wide regulations on human subjects research.
claimed that he had lost notebooks and things of that sort, and it was a little bit like sending Al Capone to prison for tax evasion, and we knew it at the time, but it worked, and we thought we got a scoundrel out of the business.\textsuperscript{37}

Thus, the catch-all category could contribute to the expulsion boundary work by allowing scientists to get “a scoundrel out of the business,” or it could be a violation of protection by allowing greater and dangerous discretion to bureaucrats administering the rules. But one could not know \textit{a priori} which boundary work scientists would prefer.

When PHS issued its final rule, about six months after the creation of OSI, it defined misconduct in science as “fabrication, falsification, plagiarism, or other practices that seriously deviate from those that are commonly accepted within the scientific community for proposing, conducting, or reporting research. It does not include honest error or honest differences in interpretations or judgments of data” (PHS 1989). Although the “other practices” category was retained in part to please lawyers at PHS and the Office of Management and Budget (OMB),\textsuperscript{38} it would still seem to concede demographic dominance to scientists, who alone could determine what constitutes a serious deviation from accepted practices. It also added the exclusionary final sentence to compensate for opposition to the “other practices” category within the scientific community. No intent standard was adopted; nor was there any discussion of intent in the final rule despite the solicitation in the ANPRM. The issue seemed to be merged with the retention of “other practices” and the compromise exclusion of “honest errors,” which presumably would allow action on “dishonest errors.” Resonating with the rhetoric of negligence, Wyngaarden said, “[H]onest error certainly should not be included, but sloppiness might very well be considered. Sloppiness in automobile

\textsuperscript{37} Interview with James Wyngaarden. Note Wyngaarden’s use of the colloquialism, “out of the business” with respect to science, when the purpose of his and the former director’s work with respect to the misconduct definition had been exactly to distinguish scientific practice from business administration.

\textsuperscript{38} Interview with James Wyngaarden.
driving can get you ticketed, or charged with manslaughter, even if there is no intention. There are standards of driving, and there are standards of science."39 The PHS final rule thus fell into the Zuckerman repertoire.40

6. Response--NAS Panel

The exclusionary sentence did not completely mollify scientific opposition. Early in 1990, a year after the creation of OSI and as Representative Dingell's investigations were continuing, NAS created the Panel on Scientific Responsibility and the Conduct of Research, under the chairmanship of former presidential science advisor Edward E. David. The David panel was charged with examining the advantages and disadvantages of explicit guidelines to strengthen scientific standards and clarifying the roles of public and private institutions in promoting responsible research practices and in managing allegations of misconduct in science.

The question of repertoires and definitions was a difficult one for the David panel, which equivocated on what I have called the embedded theory of causation of scientific misconduct:41

Two alternate, possibly complementary, hypotheses have been advanced for considering the causes of misconduct in science and formulating methods for prevention and treatment. Many observers have explained the problem of misconduct in science as one that results primarily from character or personality flaws, from environmental stimuli in the research system, or from some interaction of both.

....[The former] approach to explaining deviant behavior in science has had strong support within the scientific community....

....The panel believes that speculations about individual pathology or about environmental factors as the primary causes have not been verified; misconduct in science is probably the result of a complicated interaction of psychological and environmental factors (NAS 1992:30-31).

39 Interview with James Wyngaarden.
40 With the institution of OSI and OSIR, PHS was also becoming more involved in questions of research training and mentorship, further indicating the move to the Zuckerman repertoire.
41 The prior Institute of Medicine (IOM 1989:3) report equivocated as well: "serious misconduct in science is rare and is ultimately a manifestation of individual deviance, [but] institutions fail to detect and correct early deviant behavior primarily because of an excessively permissive research environment that tolerates careless practices."
The NAS Panel recommended that this uncertainty surrounding the role of environmental factors should yield a very cautious policy approach.

The definition itself was one of the chief obstacles to consensus for the David panel. In fact, the panel’s report included a rare “Minority Statement” addressing the definition. The majority statement proposed three categories:

(1) misconduct in science, (2) questionable research practices, and (3) other misconduct.

The panel seeks to accomplish several goals by proposing these three categories. Foremost is a precise definition of misconduct in science aimed at identifying behaviors that scientists agree seriously damage the integrity of the research process. For example, although using inadequate training methods or refusing to share research data or reagents are not desirable, such actions are generally regarded as behaviors that are not comparable to the fabrication of research data. In the same manner, sexual harassment and financial mismanagement are illegal behaviors regardless of whether scientists are involved, but these actions are different from misconduct in science because they do not compromise, in a direct manner, the integrity of the research process (NAS 1992:25-26).

The David panel majority thus derived a rationale similar to the one William Raub described in 1986, distinguishing scientific misconduct as the object of attention by Federal agencies from conduct thought improper but not worthy of such attention and from conduct worthy of such attention but not, as such, scientific. Interestingly, the criterion of worthiness for Raub was “affecting the funding or other direct transactions with PHS”; whereas, the criterion of worthiness for the NAS to reach the same result was “how seriously the various behaviors compromise the integrity of the research process” (NAS 1992:26). Different institutional interests developed the same repertoire.

The NAS panel defined scientific misconduct much the way PHS did, with one exception:

Misconduct in science is defined as fabrication, falsification, or plagiarism, in proposing, performing, or reporting research. Misconduct in science does not include errors of judgment; errors in the
recording, selection, or analysis of data; differences in opinions involving the interpretation of data; or misconduct unrelated to the research process.

....The panel unanimously rejects ambiguous language such as the category “other serious deviations from accepted research practices”...Although government officials have often relied on scientific panels to define “other serious deviations,” the vagueness of this category has led to confusion about which actions constitute misconduct in science. In particular, the panel wishes to discourage the possibility that a misconduct complaint could be lodged against scientists based solely on their use of novel or unorthodox research methods. The use of ambiguous terms in regulatory definitions invites exactly such an overexpansive interpretation.

....[T]he panel did not reach final consensus on whether additional flexibility was needed to address as misconduct in science other practices of an egregious character similar to fabrication, falsification, and plagiarism. These issues deserve further consideration by the scientific community to determine whether the panel’s definition of misconduct in science is flexible enough to include all or most actions that directly damage the integrity of the research process and that were undertaken with the intent to deceive (NAS 1992:27-28; emphasis added).

The middle paragraph of the above text is about the type of boundary work Gieryn calls protection: the David panel tries to protect members of the scientific community from an “overexpansive interpretation” of scientific misconduct, fearing that “government officials”--prod by Congress--would inappropriately intrude on science.

Interestingly, the panel “unanimously rejects” the idea that demographic dominance is a sufficient safeguard against intrusion. That is, it rejects the professionally-based criterion of “deviations from accepted research practices” that could be best defined by peer processes as a defense against aggressive bureaucrats with Congress at their heels. Even more interestingly, perhaps, the panel views the threat of intrusion as coming from those who would lodge “a misconduct complaint...against scientists solely on their use of novel or unorthodox research methods.” The panel would thus seemingly be seeking protection not only against an external intrusion but also against an internal threat.42 If there were an unclear

42 The concern about internal threats seems to have two components. One is the desire to protect work that is revolutionary (in the Kuhnian sense), because such work could be described as a deviation from “accepted practice.” Another is the idea that the changing demographics of science and the declining hegemony of white males among its ranks has been associated with the current stress in the
definition of scientific misconduct, then scientists within the community could use it to ally themselves with bureaucrats enforcing the ambiguous standard and with members of Congress seeking to root out fraud. The David panel is trying to enroll the regulatory agencies into helping it settle the integral needs of the scientific community.

The final paragraph of the text also demonstrates this attempt at enrollment: there is only so much exclusion the integral needs of the scientific community can tolerate, such that the definition of misconduct “include all or most actions that directly damage the research process and that were undertaken with the intent to deceive” (emphasis added). The implication is that by extending the attention of the public and bureaucrats beyond actions defined in this way, the integral needs of the scientific community may be threatened. If this reading is correct, then the David panel is admitting to an insufficiency on the part of the scientific community, an insufficiency that points to the vulnerability of the social contract for science.

Responding to perceived inadequacies in the full report, NAS Panel members Howard K. Schachman and Keith R. Yamamoto write in their “Minority Statement”:

[T]he report is equivocal in defining misconduct in science and is inadequate in stating explicitly the problems inherent in alternative definitions. The “other misconduct” category introduces ambiguities into the definition, and blurs the boundaries between misconduct in science and questionable practice. Misconduct in science requires rigorous adjudicatory machinery and governmental oversight, protection of whistleblowers, due process, strong sanctions, and full disclosure. In contrast, questionable practices raise issues about the value system and culture of science, and underscore the need for explicit dialogue and education. Governmental intervention is inappropriate for concerns regarding errors in collecting and interpreting data, incompetence, sloppiness, selection of data, authorship practices, multiple publication, and the like. The absence of consensus on the definition overtly

community, including misconduct. The David panel writes that the research group of the past “was small, closely knit, and composed of individuals who generally shared a common cultural heritage,” but contemporary research teams have become more “diverse” (NAS 1992:70).

43 Schachman is from the Department of Biochemistry and Molecular Biology at the University of California, Berkeley, and is a past president of the Federation of American Societies for Experimental Biology (FASEB). Yamamoto is from the Department of Biochemistry and Biophysics at the University of California, San Francisco and has been head of the NIH study section on molecular biology.
undermines a primary goal of the report to achieve clear boundaries for
the definition of misconduct in science (NAS 1992:180).

Schachman and Yamamoto suggest that expulsion is based on consensus about the
character of acts, rather than an analytically defined act. Consensus is emergent; that is,
scientists claim they recognize it in fabrication, falsification, and plagiarism, but make
little or no attempts to discover the existence of or promote a consensus.44 As NIH
director Wyngaarden speculated, “It’s like jazz...You can’t define it. You either know
it when you hear it, or you don’t.”45

Schachman and Yamamoto also argue that the character of the act determines
which repertoire is appropriate: for those acts about which there is a consensus,
something like the Babbage repertoire that clearly separates the deviant individual from
the community even through legalistic mechanisms is warranted; but for acts “about the
value system and culture of science,” Schachman and Yamamoto believe there is no
appropriate role for intervention. The “clear boundaries” they wish to achieve are thus
that fabrication, falsification, and plagiarism are distinct acts whose origin lies within
individual, deviant personalities who, until their deviance, happen to be scientists.
After their deviance, they may be exiled with impunity. But “interpreting data,
incompetence, sloppiness, selection of data, authorship practices, multiple publication,
and the like” are part of the Merton repertoire and therefore under the domain of
demographic dominance by scientists, who will discuss and educate to ameliorate.

Schachman and Yamamoto’s argument embodies the social contract for science
in one respect, and undercuts it in another. The argument arrogates for the scientific
community the opportunity to choose between misconduct and questionable practices in
particular cases. Is this plagiarism, or is this inappropriate authorship practice? If the
former, then the deviant is not one of us, and you can punish him. If the latter, then the

44 By emerging in this way, the nature of scientific consensus over the character of scientific conduct
is strikingly similar to the nature of the Kuhnian consensus over the dominant paradigm.
45 Interview with James Wyngaarden.
scientist has merely been incompletely socialized or unduly stressed, and we will manage it. This assumption of the decisionmaking assumes the social contract for science.

But what Schachman and Yamamoto did not express is the ability of the scientific community exactly to make these distinctions to the satisfaction of the political community. That is, their rhetoric fails to acknowledge the substantial inroads into deconstructing this ability that Dingell and Weiss had made during their hearings (see III.D). Schachman and Yamamoto present no coherent scheme or mechanism other than reliance on scientists—the automatic aspect of the social contract for science—to distinguish the deviants from the scientists. They are, in effect, saying that there is no consensus among scientists about practices for data selection, for authorship criteria, or for publishing practices, key elements in the “credibility cycle” that undergirds scientific self-regulation (see II.C.3.c). In other words, there is no consensus “about the value system and the culture of science.” This lack of consensus is exactly the failing that Dingell pointed to and which cripples the social contract for science and invites congressional and other political activity.

The reason for this lack is that the essentialist goal of “achiev[ing] clear boundaries for the definition of misconduct in science” is impossible. For example, plagiarism, generally defined, is claiming for one’s own the work of another. One usually plagiarizes by copying words exactly and without attribution, but one could also misappropriate ideas rather than exact words without attribution. What makes plagiarism wrong is not the use of the words, but the lack of attribution. How are authorship practices different? If one contributor to a research publication is not credited for the contribution, how can that use without attribution be distinguished from plagiarism? Or if someone who did not contribute to a research publication is named as an author, how can that person’s claim on the content be any different than a plagiarist’s claim?
Similarly, how can the misconduct of fabrication or falsification of data be clearly demarcated from the unacceptable practice of data manipulation, or from the standard practice of data selection? How different is inventing an experiment from inventing one data point? From shifting one data point? From reporting one data point and excluding another? These are not questions to be defined analytically, but by the boundary work exemplified here.

7. Response--OSIR Advisory Committee

The David panel could exert no direct authority over any decisionmaking body with respect to scientific misconduct, nor even any direct advisory relationship. The study was self-initiated, rather than having been requested by a congressional act or an executive agency (although a number of federal agencies contributed to its funding). But another scientific committee seems to be pushing in the same direction as the David panel, and may in fact spearhead another change in the definition.

In June 1991, the Office of Scientific Integrity Review (OSIR), the supervisory office for OSI, created the PHS Advisory Committee on Scientific Integrity. As of October 1992, the Advisory Committee had met five times and had focused on issues including the definition and due process. Derived from an earlier suggestion, the Advisory Committee was a way of integrating more input from the scientific community: "I think it's a very good group, and I think that it is really important that they have a major voice in the process...they're representing the scientific community. But I want them to have a role in shaping our policies."47

At the first Advisory Committee meeting in July 1991, Assistant Secretary for Health James O. Mason, outlined two possible perspectives toward misconduct in science:

46 The September 1988 ANPRM asked if such a committee should be established. Respondents generally supported the idea, but they "expressed some concerns about membership, function and cost" (Rhoades 1989).
Before you get to work, there is one thing I want to be upfront [sic] about....There are two perspectives that can form the basis for scientific misconduct policies. At the heart of one is protecting the integrity and the well-being of the scientific enterprise and of scientists broadly. At the heart of the other perspective is assuring that the federal tax dollars appropriated for scientific research are properly spent and accounted for. In the long term, these two perspectives are not inconsistent or in conflict (PHS 1991:8).

These perspectives parallel the "strain" and "interest" models of ideology from the discussion above. Mason suggested that as a PHS official, his duty was to see to the "interests" perspective; whereas, the Advisory Committee would more appropriately address the "strain" perspective.

The issue of the definition took the fore in the presentation by the recently appointed NIH director, Bernadine Healy. Healy first made an "essentialist" definition of science:

Science is about the pursuit of knowledge and unlike other disciplines,... science is about challenging present knowledge. The knowledge base in science...grows by trial and error, by disproving present beliefs, by replacing old dogma by new and by scouring current facts for errors, inaccuracies and misunderstandings.

The scientific knowledge base is subject to experimental testing, and that which cannot be proven by experiment is left as theory, folly, or falsehood. The scientific method is the framework for creativity set within bounds of generally accepted procedure. Much of the scientific method, indeed, is tedious, journeyman-like moving very slowly, confirming existing concepts, but there is, also, room and needed room for bold and daring leaps of imagination and for serendipity that can take science into realms of uncertainty, controversy and instability with the challenge ultimately settled by fact derived from scientific experimentation, quantitative analysis and convincing and logical reasoning.

The system of scientific revolution as well described by Kuhn as shifting paradigms is solidly grounded in honor. Although heretical hypotheses may be formulated, experimentation and presented data must be sound, carefully derived and reproducible for new hypotheses to take hold as fact (PHS 1991:53-54).

Healy takes a Kuhnian turn to suggest that although science might appear to some people as predictable, "tedious, journeyman-like" and therefore tempting to encode in rule-like procedures, the progress of science is dependent less on this normal science
than on the “bold and daring leaps of imagination” that spark scientific revolutions.

Healy implies that the overseers of scientific misconduct need to be wary of dousing the flames of scientific creativity and progress.48

Moving from this essentialism, Healy attempts to separate error and fraud:

Error is inherent in science, but it is inevitably corrected by the process. Integrity and honor are essential in a system based upon a community of knowledge expanding one paradigm linked upon the other. In this context, scientific dishonesty is the ultimate abuse of scientific method and a betrayal of the system of science. But we must be careful to distinguish error from fraud and mistakes from misconduct. We must be careful to distinguish bold leaps of imagination, clever tinkering and daringly innovative method from “serious deviation from practices that are commonly accepted within the scientific community for conducting research”....

The original PHS guidelines published back in 1986, defined scientific fraud as serious deviations from commonly accepted practices of science, such as falsification, fabrication, and plagiarism, but in the 1989 and 1991 Federal Register notices that definition was changed to [include] other practices that seriously deviate from those that are commonly accepted within the scientific community for proposing, conducting or reporting research.

Although the change may seem inconsequential,...the latter phrase is a major change and leaves a highly ambiguous state of definition....

Within this category of serious deviation from the accepted norm, tinkering, bold leaps, unthinkable experimental design and even irritating challenges to accepted dogma might fall into the category of misconduct....

When we define scientific misconduct, I suggest that the definition be as concrete and non-ambiguous as possible....

Intent and purposefulness seem to be an important element to distinguish these acts of misconduct [such as fraud, falsification, and fabrication] from human error or sloppiness.

These distinctions, if relevant, are not in our guidelines and are not always considered in practice. Indeed, both human error and sloppiness are deviations from the norm, but as unintentional wanderings or deviation from the truth, I quote from Webster, they perhaps are more appropriately deemed mistakes, inaccuracies or oversights that do not and should not be defined as scientific misconduct.

48 Healy radically misrepresents Kuhn’s perspective on the use of empirical evidence, however. She seems to accept a pre-Popperian verification, rather than a falsification, role for experiment and she claims that controversies are “ultimately settled by fact derived from scientific experimentation, quantitative analysis and convincing logical reasoning.” But the point of Kuhn’s work—one that he has unfortunately backed away from somewhat—is that data cannot account for the scientific revolution, and that social, political, cognitive, and psychological factors must be included for, as Healy says, “new hypotheses to take hold as fact.” Demonstrating these other facts is one of the primary foci of the sociology of scientific knowledge. See, for example, Gilbert and Mulkay (1984). For an explanation of psychological factors related to paradigm choice in scientific revolutions, for example, see Sulloway (1990).
Distinguishing between unintentional wandering and intentional deceit is not always easy....

I do not believe that this dilemma of definitions is an abstract, philosophical dialogue but in fact, is among the most difficult issues faced by investigators and adjudicators handling allegations of scientific misconduct.

Healy acknowledges that the stakes are not merely academic, but have consequences for the adjudication of actual cases. She agrees with the David panel that ambiguities in the definition could threaten the creative or unorthodox practice and progress of science, and thereby attempts to enlist a new kind of definition and enroll the power of the NIH bureaucracy in resolving this internal tension in science.\(^49\) She selectively quotes from the PHS definition, however, leaving aside its last sentence stating that misconduct “does not include honest error or honest differences in interpretations or judgments of data.” It is hard to see how any of the scientific practices she is attempting to protect would fail to be included in this protected category. Healy also contradicts herself, or misuses the word “norm,” when she says that “Error is inherent in science,” and yet “error and sloppiness are deviations from the norm.” If error is inherent, then it is a normal part of the process. To call error a deviation from the norm can then only mean that error is contrary to some moral expectation among scientists. Yet why would such a moral expectation exist if error is inherent in the process? Or why, in some Durkheimian way, if error is contrary to some moral expectation, is it not subject to sanction? Even as Healy tried to return to a Babbage or Merton repertoire, the Zuckerman repertoire seems to arise.

By rejecting the norm-based or professional practice-based aspects of the definition of scientific misconduct and by insisting on the role of intentions, Healy is rejecting the Zuckerman repertoire that had come to dominate the discussion. By

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\(^49\) Both Healy and the David panel seem to subscribe to Kuhn’s view of science as expressed in his essay, “The essential tension: tradition and innovation in scientific research” (1977) that there is a constant and creative dynamic between the old and the new, between authority and subversion, in science. Similarly, Polanyi (1962) describes the traditional authority of science, the purpose of which is to provide the context for the overthrow of scientific traditions.
emphasizing the language of fraud with particular attention to the essential needs of
science, Healy is not really returning to the original definitions (which combined
elements of Babbage and Schmaus), but is closest to performing a pure Babbage
repertoire, which is the only one that does not result in the demographic dominance of
scientists.50

8. H.R. 5207

The success of scientific advisory panels like those at OSIR and NAS in being
able to sort out repertoires within mechanisms normally recognized as scientifically
legitimate, however, has depended on the turbulent flow of legislative fortune. In June
1991, Representative Henry Waxman introduced the National Institutes of Health
Revitalization Amendments of 1991 (H.R. 5207). The bill contained a subtitle (Title I,
part II, subtitle C) on scientific integrity, a section of which (sec. 151) would have
established the Office of Scientific Integrity legislatively and would have, inter alia,
defined scientific misconduct:

(1) The term "scientific misconduct", with respect to the conduct of
biomedical and behavioral research—
(A) means seriously deviating from standards of conduct that are
recognized within the scientific community for proposing, conducting,
or reporting research;
(B) includes fabrication, falsification, and plagiarism; and
(C) does not include unintentional error in the interpretation of data
developed in research or genuine differences of opinion regarding such
interpretation.

This definition resembles the NIH definition, still delegating to scientists based on
"seriously deviating from standards of conduct recognized within the scientific
community," and still excluding "unintentional error...or genuine differences of
opinion."

50 The PHS Advisory Committee has met four times since this meeting, and in each the discussion
has been discussed. The committee has recommended a new definition and a notice of proposed
rulemaking (NPRM) is in the pipeline. I was unable to obtain transcripts of these meetings because of
the expense involved under FOIA.
The fate of the bill, however, was entirely divorced from scientific misconduct. Rather, its fate hinged on language to overturn the controversial fetal tissue research ban made by the Reagan Administration. DHHS had issued the ban, despite support for fetal tissue research from its ethics advisory panel, because abortion foes argued that encouraging research with fetal tissue would encourage abortions (see Childress 1991). After rancorous debate and high-profile hearings, both chambers passed the measure with the Senate achieving a veto-proof majority and the House falling twelve votes short. President Bush vetoed the bill because he found it “unacceptable...on almost every ground: ethical, fiscal, administrative, philosophical, and legal” (Bush 1991:1132). Waxman was adamant about not removing the fetal tissue provisions, and the House failed to override Bush’s veto. Thus, the (pen)ultimate fate of the definition of scientific misconduct rested on the contingencies of political fortune.

9. Denouement

In January 1993, a senior scientist in the Office of the Inspector General at the National Science Foundation (NSF) published a commentary based on the recommendations of the David report (Buzzelli 1993). The commentary defended the definition of scientific misconduct used by NSF that includes the “other serious deviations” category that the David panel condemned. Among other differences, the commentary distinguished between the problem of protecting the integrity of federally funded research and protecting the integrity of science, as Assistant Secretary Mason had. The NSF employee argued that the former duty required the “other serious deviations” standard, even though that standard would be interpreted by scientists; whereas, the latter duty might allow a different standard.

The ongoing attempt to define misconduct in science is exemplary of the constructive nature of boundary work. There is no stable, analytical definition of misconduct in science. Neither was there any immediately apparent strategy for performing boundary work. Schemes advanced by the bureaucrats that performed the
norm-based repertoires could be said to correspond to their interest in protecting federal research funds. But they could also be said to protect the demographic dominance of science. Schemes advanced by the scientific advisory bodies—the David panel of the NAS and the PHS Advisory Board—moved toward an intent-based repertoire with the possible risk of demographic dominance because they seemed to fear these same bureaucrats driven by Congress. Yet the ability of the scientific community to sort through these potential repertoires was provided by political fortune—the coincidence of a controversial issue on an omnibus bill that was vetoed, and which veto was barely upheld.

This is what the boundary between science and politics actually looks like. Like political fortune, the boundary is more like a river than a wall. Because the boundary is flexible and changing, so is the answer to questions like, “who is a scientist?”, “what is science?”, and “what is the effective extent of politics?” This flexibility is not only created by the internal processes of science. When Polanyi (1962) or Kuhn (1977) speak of the dynamism and flexibility of science created by the internal tension between tradition and revolution, they miss a category of dynamism that includes the tension between the scientific community and political institutions. This tension is not limited to the stresses and strains and slippages of funding. Because of the social contract for science and the metonym of financial accountability (Turner 1990a), the tension reaches into what essentialists might call the heart of the scientific community, the definition of its membership and regulation of their good conduct.

At any time in the process of this boundary negotiation over the definition, Congress could conceivably have stepped in and legislated a definition. Indeed, H.R. 5207 contained a definition and had NIH wanted to make an “anticipated response” to its authorizing committee, it knew what definition to use. But as Moe described, even the threat of new legislation is a blunt instrument of control. The omnibus reauthorization bill became bogged down over an unrelated proposal, and NIH could
respond instead to its biomedical constituency. But the reauthorization must be passed eventually, and it remains to be seen if Congress will authorize the boundary work NIH has performed to date. Perhaps Congress will make an “anticipated response” and authorize the direction NIH is headed in; or perhaps it will take “pre-emptive” action and head off the expected changes. Either is possible on the “two-way street.”

D. Adjudicative process

1. Misconduct “in story and song”

The attempts to define scientific misconduct were attempts to negotiate a boundary between what is and what is not science so that the funding agencies have a role in adjudicating and serving sanctions. Likewise, the attempts to determine the adjudicative process were attempts to determine the boundary between processes with scientific criteria for fairness and processes with legal criteria. The relationship between the adjudicative process and the social contract for science is: how does the process attempt, and does it succeed, in separating science and politics? If it does not, one must conclude that the social contract has been terminated because politics had assumed an active role in scientific decisionmaking—decisionmaking that defines the boundaries of science.

The status quo ante with respect to the adjudicative process was highly informal. PHS chief counsel Richard Riseberg relates how scientific misconduct was handled in the golden age before congressional scrutiny:

I am told in story and song, and I don’t know if any of this is true or not because I arrived at NIH...back in 1972, and then at some point a misconduct in science case came to my attention, and I said, “How did we handle these kind of cases?” and I am told that if the Director of NIH at the time or back in history felt that someone had really engaged in it that he made a phone call or two, and that person disappeared from the map of science.

I don’t know whether it actually happened or who he called and what the process was, and clearly that was not, even if it was true, it was not an acceptable way to proceed (PHS 1991:84).
Riseberg’s statement represents the baseline level of informality that NIH began with in managing misconduct in science. He invokes the cartographic metaphor to describe the power of the NIH director, which is strikingly reminiscent of the directors of Soviet scientific institutes. Yet this period is one that scientists often recall rosily, lacking the intrusiveness of the assurances, certifications, rules, and regulations of contemporary science. Yet, as will be shown below, this intrusiveness occurs in significant part to make science operate in a way that is perceived to be a more fair and perhaps, in NAS president Handler’s formulation, a more democratic mode.

2. Formalization

The 1976 Inspectors General Act set in motion rulemaking at DHHS on debarment. Issued in 1980, the final rule treated scientific misconduct as “serious unsatisfactory performance,” which was cause for debarment and subject to the same procedures as other causes including criminal convictions. The rule described debarment actions as “within the discretion of the Secretary and...rendered solely in the best interest of the Government;” the rule also described “serious unsatisfactory performance” as “established by evidence which the Secretary determines to be clear and convincing in nature” (DHHS 1980:67266). The procedures for debarment consist of notice, request for hearing, notice of hearing, and the hearing itself. The respondent is entitled to a hearing and representation by counsel, but the respondent bears the burden of requesting a hearing. A hearing officer appointed by the Secretary from the ranks of DHHS presides over the case, and the General Counsel representing the interests of the department. The hearing is conducted without the formal rules of evidence as in courts of law. After the hearing, the hearing officer presents a determination to the Secretary, whose decision is final and part of the record.

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51 I make this comparison to the organization of Soviet science because it is often used to exemplify the dangers of attempting to plan science or allowing political influence to intrude on scientific decisionmaking, particularly the Lysenko case.
To the best of my knowledge, no case of scientific misconduct was handled under this debarment rule (which is the one NIH director Fredrickson had found inappropriate for managing scientific misconduct) and cases continued to be handled in an ad hoc manner by the various institutes. By 1982, after the Gore and Hatch hearings, it became clear to the person responsible for coordinating investigations of scientific misconduct at NIH that the ad hoc handling of cases was producing an uneven system in need of central policies.52 Within a year, PHS began to establish general policies for handling scientific misconduct that continued to delegate to the institutes, but which also named the associate director for extramural research and training as the designated official for coordination. Under a definition that included material failure and fiscal mismanagement, PHS outlined its expectations that the “scientific community is...to make every effort to prevent misconduct” and that “for every incident of alleged or apparent misconduct which is judged to warrant investigation by an awardee institution, that institution is...to report on the matter to head of the appropriate PHS agency” (Brandt 1983). PHS also delegated to the heads of its agencies and offices the responsibilities to implement policies and procedures to handle allegations, to make decisions regarding sanctions, and to delegate to yet another official the responsibility of coordinating with other offices such as the the Office of the Inspector General and the Office of General Counsel.

Because PHS delegated responsibility for policies and procedures to grantee institutions and their cognizant institutes, cases were handled in a variety of ways, although their coordination allowed for an “almost a case law approach, learning from experience.” Until the Darsee case surfaced in 1981 and 1982 (see III.C.2), NIH or Food and Drug Administration (FDA) staff performed the fact-finding. But with the

52 Interview with Mary L. Miers, former Institutional Liaison Officer, NIH, Bethesda, MD, December 6, 1991.
Darsee case, the National Heart, Lung, and Blood Institute created a scientific advisory panel and that model was applied to other, but not all, investigations.\textsuperscript{53}

The Health Research Extension Act of 1985 instructed DHHS to require assurances from all institutions applying for financial assistance (grants, contracts, or cooperative agreements) that they have in place administrative processes for misconduct allegations, and that such processes conform to guidelines that the department would issue. In partial response to the legislation, PHS issued “interim” policies and procedures. “Issues that are not primarily scientific [were] outside the scope of these procedures” (NIH 1986:8).\textsuperscript{54}

The interim policies and procedures detail the responsibilities of misconduct policy officers (MPOs) in each agency and one MPO who coordinates their activities at the PHS level.\textsuperscript{55} The document directs agencies to consider three elements in all cases: the confidential treatment of allegations to protect the rights of the affected parties; the privacy of informants; and the interests of the government. PHS must give notice of investigations to the affected parties and possible interim administrative actions can be taken to safeguard the investigation, human or animal subjects, or the government interest.\textsuperscript{56} The policies specify the extent of confidentiality of allegations: certain PHS officials may be made aware of current cases, but peer review committees would not normally be told of allegations or ongoing investigations.

The MPO in charge of an investigation should give the affected parties every opportunity to respond not only after completed investigations but also during ongoing

\textsuperscript{53} Interview with Mary L. Miers.
\textsuperscript{54} The policies relegate cases “involving the possible misuse of federal funds” to department audits, the OIG, or the General Accounting Office (GAO), and cases involving human subjects regulations and animal welfare policy to the Office for Protection from Research Risks (OPRR).
\textsuperscript{55} The policies and procedures consisted of three documents in addition to internal policies: “general policies and principles” little different but for the definition from the 1983 statement; “summary of procedures affecting regulated research,” largely for FDA research, which operates under a strict monitoring program; and “policies and procedures for agencies authorized to conduct research,” for grantee institutions to use in conjunction with fulfilling their assurance obligations.
\textsuperscript{56} Recall how the debarment rule required notice only of a hearing and not of an investigation.
investigation. As described, investigations actually have two stages: an inquiry to
determine if a full-scale investigation is warranted; and an actual investigation. The
agency MPO (in consultation with the cognizant agency director and the PHS MPO)
determines the warrant of an investigation, based on the accuracy and reliability of
sources, the seriousness, scope, and context of the alleged misconduct, and any
response provided by the affected parties.

Investigations begin with notice to individuals and institutions under
investigation and the entering of the individual into the ALERT system (see III.C.3).
"Outside consultants" may be invited to participate in an investigation (and they are
given legal protections available to federal employees). The agency-level MPO and the
director of the cognizant agency review the findings of an investigation. If no
misconduct is found, there are various provisions for notice and undoing what actions
might have been done. If misconduct is found, there are provisions for notice and
sanction. Sanctioning requires the participation of administrative personnel beyond
those conducting the investigation. The agency head has the authority to determine
whether debarment, or other lesser sanctions, will be sought. These interim policies
and procedures held sway at PHS until OSI was created in the spring of 1989 and the
final rule was published in August 1989.

3. Separating science and scientists

As I argued in the previous chapter, NIH director Wyngaarden conceived of
OSI as a means of limiting congressional intervention and preserving the social contract
for science, by pre-emptively establishing it as an outpost on the science-politics
border. For such an effort to succeed, OSI could not become a bureaucratic or
administrative body, but would instead need to be scientific in its personnel and
operation. It would have to maintain the demographic dominance characteristic of the
social contract for science. OSI was, in Wyngaarden's mind, part of the "self-

57 Again, recall the contrast with the debarment rule in which response was relegated to the final stage.
regulatory [mechanism of science] in the sense that we put the primary responsibility for investigating allegations of scientific misconduct on people with scientific knowledge."  

That is, Wyngaarden attempted to assure the demographic dominance of scientists. By crude credentialism, OSI achieved this separate identity. OSI professionals all had Ph.D.s or M.D.s., and their vitae showed service at NIH and at universities. In the initial staffing decisions for OSI, Wyngaarden was "looking for people who had a good understanding of science generally, and the processes of science, [and] a high sense of personal integrity."  

Brian Kimes, the first acting director of OSI made every attempt to rotate scientists from elsewhere in NIH into the office.  

More important than the credentials of the personnel, however, was how these scientifically trained professionals viewed their jobs and what model of operation was implemented at the office. With respect to their view of their jobs, there was some ambivalence among the OSI professionals. Kimes was under no illusion that he was doing science at OSI. He was "doing his job," and it was not science because "science is creating....Science is the creation of new information." Kimes believed that science is for the most part self-regulating, but that OSI was not "self."  

Jules Hallum, on the other hand, "would like to consider [him]self a scientist" and he joked that his friends are the ones who still do consider him a scientist.  

OSI senior scientist Barbara Williams felt that she is a scientific professional because she relies heavily on scientific networks to acquire information and the services of experts. She looks through notebooks like scientists do, and engages in fact-finding like scientists do.  

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58 Interview with James Wyngaarden.  
59 Interview with James Wyngaarden.  
60 Interview with Brian Kimes, former acting director of the Office of Scientific Integrity, Bethesda, MD, November 19, 1991.  
61 Interview with Brian Kimes.  
63 Interview with Barbara Williams, senior scientist, Office of Scientific Integrity, Bethesda, MD, November 19, 1991.
scientist Alan Price also emphasized his use of scientific skills and vocabulary, as well as the fraternal understanding that OSI has of scientists, universities, and NIH: "We don't want to see ourselves as different." But Price does consider himself a scientist who became an administrator.64

Suzanne Hadley was most assured about her role as a scientist at OSI:

Oh, yes, absolutely [I felt that I was doing science]. I mean, one of the things that I loved about doing that work, and one of the things that was most fascinating to me in my OSI work, was that extraordinary, mind-bending scientific challenge. I had to, and everybody at the OSI had to...be able to go in to a field entirely foreign [and] understand the data, understand the project, and if I was really going to be effective, I had to know it as well or better than the scientists themselves....I went into research because I loved the challenge of posing a problem and finding out if my [hypothesized] answer was right or not....I believe [that the scientists I interacted with thought I was a scientist], including the people I investigated.65

Perhaps the most interesting view of work at OSI, however, comes from a professional staff member who is engaged in original research while at OSI. This staff member has an extensive background in statistical and computational research, and is attempting to bring that background to bear on the problem of identifying fabricated or improperly manipulated scientific data. The staff member is blazing new ground in "statistical forensics" and prepares data about data. "If we’re talking science--what we do here--the scientist is supposed to present data."66 The work of this staff member is proudly acknowledged by other OSI professionals as an example of the scientific work done in the office. However, the difficulty faced by this scientist is that the work may never be published because it could alert devious scientists to the methods used by OSI to investigate data, giving them opportunities to evade detection.

64 Interview with Alan Price, senior scientist, Office of Scientific Integrity, Bethesda, MD, November 19, 1991.
65 Interview with Suzanne Hadley, former deputy director, Office of Scientific Integrity, Bethesda, MD, December 17, 1991.
66 Interview with professional staff member, Office of Scientific Integrity, Bethesda, MD, November 19, 1991.
4. Separating science and the law

Perhaps because of the ambivalence about the demographic dominance of science at OSI, the office developed a finely tuned rhetoric about its procedures and its model of operation. The model initiated at ILO and implemented at OSI attempted to reinforce the separatist scheme of peer review for misconduct in science. M. Janet Newburgh, the last institutional liaison officer before OSI took over, describes how ILO investigations were constrained by the model of practice that had been initiated. Patterned after the study sections and executive directors in the peer review of grant applications, the NIH approach to misconduct investigations solicited the opinions of a scientific review panel. It was difficult for staffers to have any impact on the panel’s decisions unless they were absolutely wrong.\(^{67}\) Suzanne Hadley, former deputy director of OSI, described the approach similarly: “It’s an extension of the peer review model. It’s an extension of when a study section reviews a grant application, sometimes they’ll go back to the scientists and say, we would like to see your data.”\(^{68}\)

When OSI was created, acting director Brian Kimes was responsible for implementing its policies and procedures. In describing the crux of misconduct cases, Kimes maintained that “data should be able to answer the question” of whether or not misconduct had occurred. By his description, OSI’s procedures were not oriented toward legalistic interpretations of due process but instead trying to “arrive at the scientific truth as quickly as possible.”\(^{69}\)

Kimes’ successor, Jules Hallum, had a similar view. Hallum believed that “OSI was formed within the NIH in a manner similar to the establishment of the Office

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\(^{67}\) Interview with M. Janet Newburgh.

\(^{68}\) Interview with Suzanne Hadley. Brian Kimes, the acting director of OSI for its first eight months, also acknowledges that the peer advisory system of grant application review was the main interpretive model and tool for OSI. OSI senior scientist Barbara Williams (who had previously been a program officer) also feels that the inquiry and investigation process is an extension of the peer review process, and that the misconduct work is the “other end of accountability” from grant peer review (interview with Barbara Williams, senior scientist, Office of Scientific Integrity, Bethesda, MD, November 19, 1991). James Wyngaarden, however, says that he does not recognize that description of OSI.

\(^{69}\) Interview with Brian Kimes.
for Protection from Research Risks or the Office for Recombinant DNA Technology. These offices have succeeded in their mission because they are part of the scientific community, and therefore, they include an emphasis on review of scientific issues” (PHS 1991:136). When Hallum became director of OSI in the spring of 1990, people at OSI started speaking of the “scientific dialogue method” of investigating scientific misconduct. Procedures did not change with Hallum’s directorship, but this new name for their operations was an attempt to persuade the scientific community that OSI was part of that community and had its best interests at heart. It may have also been an attempt to insulate itself from the scrutiny of the Dingell Subcommittee, which was threatening to reduce its perceived independence vis-a-vis the biomedical community by conducting parallel investigations.70 A “scientific dialogue method” of investigation and adjudication might keep not only Dingell at bay, but also might keep at bay the biomedical community, which felt that a Dingell-influenced OSI was the worst option.

The “scientific dialogue method” is an “interpretation” of normal scientific decisionmaking in which, Hallum says, the burden of proof rests on the claimant to produce data and sway the audience. This model functions in much the same spirit as does an editor of a scientific journal in dealing with problems in a submitted manuscript. Theoretically, if an author makes a claim unsubstantiated by presentation of data, the editor can demand that those data be adduced or the paper will not be published. It is obvious to scientists, though perhaps not to laymen, that in this scientific dialogue, that the burden of proof must always fall on the person who makes claims about his or her data....This does not mean that the burden of proving there is no misconduct falls on the accused. The absence of supporting data would not, standing alone, support a finding of misconduct (Hallum and Hadley 1990:650).

Thus, the scientific dialogue is interested in “the reliability of data, not the reliability of witnesses.”71 It is, in the Mertonian sense, universalist rather than ad hominem. And

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70 One of the sensitive issues was the confidentiality of ongoing investigations. Scientists accused OSI of breaching confidentiality by leaking pre-final case information. OSI generally denied being the source of such leaks, and responded that the congressional subcommittees to which it was responsible and to which it gave case documents were responsible for the leaks.

71 Interview with Jules Hallum.
since OSI would rely on scientific advisory committees, it could be disinterested and skeptical as well. OSI "wants to convert these [accusations] into the issues of science....We will investigate this on the basis of data, not the [basis of] personality" (PHS 1991:174). Or, as one member of the OSIR advisory board described, "In short, it is like somebody coming in and saying, 'It is raining outside.' It is irrelevant who the informant is. You go out, and you look and see if it is raining, and it doesn't matter that somebody came and told you....You have got to find that it is, in fact, raining out there, apart from what the informant said" (PHS 1991:175).72

This attitude—from "the data should be able to answer the question" to the reliability of data being of greater interest than the reliability of witnesses—about the active nature of data has consequences for policies and procedures. As Director Hallum explains, if a misconduct investigation is framed in terms of scientific claims, then you do not need the person accused of scientific misconduct to ask questions and witnesses do not need to be challenged by the accused. That is, elements of criminal due process such as confronting and cross-examining witnesses are unnecessary. The data are the witnesses.

Hallum contrasts the scientific dialogue model of adjudicating scientific misconduct with what he calls the "legal-adversarial model," which "clearly offers the most visible and obvious due process protections. In this courtlike process, with its origins in Anglo-Saxon jurisprudence, the accused receives specific 'charges' in writing, can be represented by counsel, can face and cross-examine all witnesses, and can introduce evidence on his or her own behalf" (Hallum and Hadley 1990:650). The primary procedural difference between the two models is that, in the scientific dialogue model, the accused does not have the opportunity to cross-examine witnesses or

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72 This analogy was later disputed by one committee member who replied that if "somebody tells you it is raining, but that is not an accusation. That is a statement of fact. These things [misconduct allegations] rarely come as statements of fact to us; they come as accusations" (PHS 1991:239). This reply attempts to differentiate people who make accusations, and therefore are not scientists, from people who make statements of fact, who therefore are. It is an excellent example of expulsion boundary work.
confront the accuser, largely because of concerns for the protection of whistleblowers and others who offer evidence against someone who may be in a position to take revenge. Also, the legal-adversarial model is neither universalist, disinterested, nor skeptical, because of its reliance on the technique of advocacy.

For Hallum, the choice between the two models involves enormous stakes: “the role of scientists in such [legal-adversarial] proceedings is likely to be minimized to that of ‘expert witnesses’ [rather than scientific peers]. This would represent a serious loss to the interests of science, because the issues are resolved on the basis of civil law or administrative law and not principally on scientific evidence” (Hallum and Hadley 1990:650; emphasis added). Hallum believes, like Wyngaarden, in preemption: if the scientific community cannot write rules that it can live by, then the Inspector General or the Department of Justice will do it. “If we want to govern ourselves,” he warns, “we had better use this scientific dialogue.” Further, Hallum believes that “the use of normal scientific dialogue in dealing with our very occasional instances of misconduct will best serve the public interest” (PHS 1991:140).

OSI attempted to recreate the separation of science and politics in its work. One might have expected the scientific community to support OSI in its attempt to implement a “scientific dialogue method,” to maintain the separation between science and politics implicit in the social contract for science and to distinguish its investigations from congressional hearings, in order to preserve the “interests of science.” However, many members of the scientific community have instead attacked OSI for its “scientific dialogue method” and for not allowing complete due process. Why would members of a scientific community that operates as a free market of scientific claims find the scientific dialogue method unacceptable?

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73 Interview with Jules Hallum.
74 I describe some of these attacks below, but I also base this statement my participation in NAS (1992), my interviews in general, and particularly my observations at many professional meetings on the topic of scientific misconduct.
5. Challenges to OSI

A formal challenge to OSI's "scientific dialogue model" came from a suit filed by James Abbs, a University of Wisconsin neurologist and the subject of an OSI investigation, for failing to provide due process and for failing to follow the Administrative Procedures Act (APA) in promulgating its policies and procedures.75 As described above, PHS attempted to implement the interim rules as regulations, but OMB blocked this effort. After PHS created OSI in the spring of 1989, it issued the final rule derived from the mandate expressed in the Health Research Extension Act of 1985 in the Federal Register (this final rule culminated the rulemaking process started by the September 19, 1988 ANPRM). The rule specified the responsibilities of PHS applicant institutions, but it did not specify the internal policies and procedures OSI would use to review university investigations or to conduct its own. Instead of taking the formal Federal Register route for OSI internal policies and procedures, PHS developed them without public comment and published them in NIH Guide for Grants and Contracts.

Abbs charged that in failing to follow the formal rulemaking procedure specified by APA, DHHS had investigated him with invalid policies and procedures. Abbs further contended that those procedures were substantively inadequate. He argued that he had a property stake in his grant, his academic position, and his reputation, and that OSI had deprived him of these rights without due process of law under the Fifth and Fourteenth Amendments to the Constitution. DHHS argued that no such property rights exist, and even if they did, that OSI provided due process. DHHS also argued that OSI was not required to fulfill any public notice requirements for its internal procedures, but had published them in NIH Guide to demonstrate its openness to the biomedical community.

The judge decided the APA claim in Abbs’ favor, thus invalidating OSI’s internal policies and procedures, but only in the jurisdiction of the Western District of Wisconsin. The judge decided the due process portion of the case, however, in favor of PHS, declaring that the invalid procedures did, in fact, provide sufficient due process. Both parties appealed the decision. Despite the fact that the Abbs case validated the “scientific dialogue” approach to due process, it focused a great deal of scrutiny on OSI policies and procedures, and the judge’s decision was widely misinterpreted as striking down OSI policies for substantive rather than procedural reasons. Other suits were filed against OSI, citing the Abbs decision, to invalidate OSI procedures in other federal court districts where researchers had been found to have committed misconduct. In striking OSI policies down for APA violations, the court removed an obstacle to reforming OSI, regardless of the strict legality of their procedures.

Coincident with the Abbs decision, OSI also came under scrutiny by the new NIH director, Bernadine Healy. Although Healy stated in an April press conference that she did not expect to make any significant changes at OSI, by August she was embroiled in controversy and testifying before Dingell about her handling of OSI. In part because of a “scientific backlash” (Hamilton 1991b:1084) and criticism that OSI was too “zealous” and its investigators were reminiscent of the “Keystone Cops,” Dingell’s subcommittee saw their efforts at instigating OSI and settling the issue coming apart. Since OSI was the subcommittee’s tool to “turn the screws” on scientific misconduct, there was great consternation that “they’re fucking with our

76 Wyngaarden left the post in the spring of 1989. William Raub became the acting director, but no replacement was found until Healy was formally nominated in January 1991 and sworn in in June 1991.
screwdriver.”

Dingell himself stated that Healy “has made a mockery of the OSI’s alleged independence in dealing with misconduct allegations.”

At the hearing, Healy expressed her doubts about “due process, confidentiality, fairness and objectivity” at OSI. Elsewhere, she tried to deflect the “either science or law” rhetoric of OSI by describing:

[t]he process we have [now as] clearly adversarial, whether one admits it or not. But there are other models of quasi-judicial procedures which are derived from the judicial process, but which do not involve a courtroom battle with teams of lawyers on either side. They include some of the things we have in the Grants Appeals Board, some of the administrative law judge activities. There are models which are somewhere in between. When somebody has been accused of some horrible act that is going to deprive them of their livelihood, of their standing in society, you should give them justice. And in our country, the way we give justice is through time-honored judicial proceedings.

The PHS Advisory Committee on Scientific Integrity, which met for the first time a few weeks before Healy’s testimony before Dingell, shared her concern about finding an appropriate model for due process in misconduct investigations. Committee chairman Nicholas Steneck spoke of how:

[t]he pursuit of science and the monitoring of integrity no longer takes [sic] place in contained social settings as they did when science was privately funded and of concern primarily only to other scientists. Today scientists must operate in many spheres, from their own laboratories, to programs, departments, institutes, universities, agencies and in public settings. Unfortunately, from the standpoint of consistent policies, the standards for fair or appropriate behavior, such as due process and burden of proof can change dramatically from sphere to sphere (PHS 1991:35-36).

In terms of the social contract for science, the problems were several. The supposed republic of science has its own property and currency in reputation, and

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80 Interview with professional staff members, House Energy and Commerce Committee, Washington, DC, November 5, 1991.
within its borders, even accusations of misconduct can deprive scientists of this property. Thus, scientists reason, there must be due process before they can be deprived of their reputation. In making positive claims in science—in the making of reputations—there are few complaints about the due process accorded to scientists.\footnote{Although there are complaints about the conservative nature of peer review and so forth.} Yet procedures modeled on those normal processes for positive claims, when used to adjudicate negative claims about misconduct, do not seem to work. Why not?

Because scientists are not completely isolated in a republic of science, and they have daily experience with another set of adjudicatory procedures that provide what seems to be more complete due process. Simply put, citizens of the republic of science are also citizens of the United States. Or, as a former HHS deputy general counsel put it, scientists tend to be Western \textit{Homo sapiens}, and they probably share these views about what makes a successful dispute resolution process: participants need control over the process, which needs to be ritualistic, evenhanded, and meet expectations of efficiency and accuracy.\footnote{Interview with Robert Charrow, former principal deputy general counsel, Department of Health and Human Services, Washington, DC, January 3, 1992. Charrow points out that meeting expectations is more important than objective accuracy; civil suits, for example, can be inaccurate in almost 50\% of the cases because the standard is “preponderance of the evidence.”} So scientist/citizens like Abbs appeal to the process in a different “sphere” when they do not like the outcome in their original sphere. Such a sphere, in which members appeal to authorities outside the sphere when internal processes run counter to their interests, can hardly be called a separate community, yet alone a self-regulating one.\footnote{By Walzer’s (1983) test, then, the sphere of politics does dominate the sphere of science, because criteria of the former are applied in the latter. But this has been done on the invitation of scientists.}

The scientific sphere is embedded in a political one.

With scientific misconduct, however, the relationship is still more complicated. The currency or property scientists are trying to protect is not recognized as such by the larger political community, at least not by the court in Abbs. The Abbs court resoundingly rejected the plaintiff’s claim for a property right, writing that the “grant awards made to the Board of Regents [of the University of Wisconsin for James Abbs]
are not made for [the researcher's benefit], but for the benefit of the public that may enjoy the fruits of his research." So scientists were caught in a middle ground, between what they view as the palpably unfair but scientific process at OSI and the fair but alien legal process in federal district court.

6. The Office of Research Integrity

Despite the favorable court ruling on the due process question, PHS is currently revising the scientific dialogue method and reorganizing the scientific integrity program. In February 1992, Assistant Secretary for Health James O. Mason forwarded to Secretary Louis Sullivan a reorganization plan for scientific integrity. As described in the "Statement of Organization, Functions and Delegations of Authority," OSI and OSIR would be abolished and replaced by a new Office of Research Integrity (ORI) (DHHS 1992:24262-63). The new office consists of three divisions: the Division of Policy and Education, largely what was OSIR; the Division of Research Investigation, largely what was OSI; and the Division of Legal Counsel. ORI is reorganized entirely within the Office of the Assistant Secretary for Health (OASH).

The new policies and procedures for adjudicating scientific misconduct have yet to be published (an interim notice will be published shortly so ORI can proceed with cases until the final is established, perhaps within one or two years). According to one former OSIR and current ORI professional, the new policies and procedures are "quite responsive to what the scientific community wanted."\(^\text{86}\) They are also very similar to the review process used by DHHS in debarment cases for reasons not related to scientific misconduct. The Division of Research Investigation handles allegations, conducts investigations, and notifies the subject of the investigation of the result. If the finding is adverse, the subject has thirty days to appeal the finding and may have a hearing before a hearing officer. At the discretion of the hearing officer, one or two scientists may be appointed to assist the officer in adjudicating the case. ORI is

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\(^{86}\) Telephone interview with Lawrence J. Rhoades, deputy director, ORI, October 18, 1992. The information about ORI policies and procedures that follows is based on this interview.
attempting to establish with professional societies a mechanism for selecting these scientists, who would each have votes equal to the hearing officer. Thus, demographic dominance yielded to a plan in which scientists’ participation is based on the discretion of an administrator. The new terrain will most likely be jointly populated and governed by more legalistic rules.

The impetus for the reorganization came from, among other places, the 2005 public comments received by OSIR in response to a June 1991 Federal Register notice,\(^{87}\) from the OSIR advisory board, and from within the chain of command at PHS—recognizably “scientific” mechanisms. However, the fate of this plan rested on the same contingencies as the definition of scientific misconduct. The same bill (H.R. 5207) that contained a definition of scientific misconduct also contained legislative authorization for OSI under its original organization. The inability of the House to override the President on fetal tissue research provided the scientific community with an opportunity to maintain the semblance of self-regulation of scientific misconduct.\(^{88}\)

E. Conclusion

In struggling over definitions and policies and procedures for scientific misconduct, policymakers and scientists were engaging in boundary work. They were attempting to demarcate the shared boundaries between the polity and the republic of science. If the social contract for science was the tacit agreement that settled those boundaries in the post-War period, then they were renegotiating the social contract for science. This boundary work was also part of implementing NIH’s congressionally

\(^{87}\) More than 1600 of these letters came from members of the Federation of American Societies for Experimental Biology (FASEB), whose former president, Howard K. Schachman, has also lobbied vigorously for the restriction of the definition. Expense prevented me from acquiring copies under FOIA. For a summary, see Rhoades (1991).

\(^{88}\) In the current Congress, a new NIH reauthorization bill has been introduced that contains the same fetal tissue provision, because the new Democratic President Clinton is likely to sign it. But because the fetal tissue provision did not need to be changed, seemingly, nothing else was either. The bill refers to the Office of Scientific Integrity despite the reorganization.
instigated responsibilities, from the Inspectors General Act of 1976 to the Health Research Extension Act of 1985, to the creation of OSI.

The negotiations consisted of movement among a variety of possible repertoires for describing misconduct in science. At the onset of prominent cases in the early 1980s, Gore was uncertain and called witnesses to sort out the repertoires. The result was unclear, but no congressional efforts to sort out the repertoires was forthcoming because—as in the previous chapter—the relevant subcommittees were satisfied with the low-cost alternative of allowing the scientific community to put its house in order.

PHS spent several years sorting out the repertoires. Beginning from one in which scientific misconduct was treated like financial misconduct, PHS isolated scientific misconduct as a category distinct from business administration, research risks, and professional conduct. This category invoked the Zuckerman repertoire, failing to distinguish between intentional and negligent conduct. And although it did distinguish between fraud and error, it included the catch-all, “other practices that seriously deviate....” The adjudicatory regime established by OSI to enforce this repertoire tried to appeal to traditional scientific mechanisms. The “scientific dialogue model” emphasized reliance on data and the role of scientists in regulating their own conduct and in applying the definition.

Throughout this period, Congress was pressing with the kind of oversight, investigations, and coherent and substantive critique described in the previous chapter. But the scientific community rejected both the Zuckerman repertoire and the scientific dialogue model of adjudication, despite their emphasis on demographic dominance. In doing so, it moved for a repertoire more like that of Babbage, and for a “legal-adversarial model” of adjudication. In this move, the biomedical community engaged in protection boundary work at the expense of expulsion boundary work. That is, it protected itself by constraining the definition of scientific misconduct, but it also rendered itself vulnerable by admitting behaviors not subject to expulsion that cannot be
distinguished analytically from behaviors that are reasons for expulsion. Although these behaviors are described as related to the value system and culture of science, they are simultaneously described as not the objects of consensus. Thus, decisions within the community about these cases retain elements of ambition, profit, and lust--of politics. By retaining this contradiction, the biomedical community retains the contradiction in the social contract for science discovered by Dingell and retains the potential for future conflict.

In moving for the “legal-adversarial model,” the biomedical community yielded demographic dominance over the process of adjudicating cases, thus yielding ground in the direction of protection. But the procedure to which it yielded offered more protections to individual scientists than to the scientific community. Because the scientific community is embedded in a larger society that guarantees such procedural rights, individuals elected to appeal to the larger society when their values and property were threatened.

These developments could not have been predicted from any hypothesis of the strains or interests of the scientific community, or cognitive authority of science. As Nature is (at least in part) a product rather than the judge of scientific disputes, the cultural authority of science is the product of these negotiations and it cannot be usefully applied to settle them. It is possible that the cultural authority of science will be decreased, because scientific processes no longer adjudicate the crucial conduct of scientists. Or it is possible that the cultural authority of science will be expanded, because the processes for adjudicating scientific misconduct now look more like the processes of democratic politics in the larger society than they used to.

Nor is the outcome purely the result of a rational negotiation among the scientists and PHS, because both the definition of scientific misconduct and some aspects of the policies and procedures embodied in OSI could have been legislated but for a broader political conflict involving abortion that hinged on a dozen or so votes in
the House of Representatives. The contemplative space for the negotiations of these issues was opened by the contingent incapacity of Congress on a completely different issue. Although, as in the previous chapter, OSI was clearly instigated by the threat of congressional action, the sanction behind the threat may have been overestimated by both the actors involved, and by theorists like Niskanen who see congressional preferences as an easy read.

In the absence of timely congressional action, NIH and the biomedical community had the opportunity to adjust the pre-emptive response taken by Wyngaarden. The definition of scientific misconduct and the description of policies and procedures for adjudicating cases were almost pre-emptive in themselves: NIH-made definitions and policies might forestall explicit legislation. But more than that, the boundary work performed around these definitions and policies indicated that the biomedical community tried to protect itself by constraining the influence of congressional pressure on NIH and OSI. It did this by pursuing an explanatory repertoire that, even while it rejected demographic dominance by scientists, accepted one that would limit the ability of some scientists to enroll ambitious bureaucrats and Congress to press claims of misconduct. It also rejected policies and procedures modeled after peer review and instead accepted those modeled after administrative law.

The events set in motion by the congressional inquiries into scientific misconduct had repercussions at the three levels of analysis important for the dissertation. They revealed the ability of Congress to instigate institutional change in the NIH without passing legislation, and they revealed a new mode of "pre-emptive response" of bureaucrats to congressional pressure. They revealed the attempted preservation of the social contract for science by this pre-emptive response by Wyngaarden, but its failure in the boundary work that followed the implementation of the new institution’s policies and procedures. The boundary work also revealed how congressional influence continues beyond the initial stimulus of creating the institution
in eliciting pre-emptive concerns; but also how the Congress, because of the difficulty in passing legislation, is reduced to responding or reacting to the NIH boundary work if it wants to authorize the new institution. Finally, the boundary work also shows that scientists and the scientific community are firmly embedded within a political context. Not only do political influences from Congress have the effects described on such crucial issues as the definition of scientific misconduct; but also when scientists want to protect the integrity of their community, they rely on appeals to broader processes and values representative of democratic politics rather than of their own demographic dominance.
Chapter V: The productivity of science, one: a tentative technology policy

A. Introduction

The previous two chapters discussed how, through the 1980s, members of Congress and bureaucrats at the National Institutes of Health (NIH) initially attempted to preserve the social contract for science as a low-cost solution to the moral hazard of scientific misconduct, and as an alternative to formal institutional arrangements. But in the face of rising conflict over scientific misconduct, Congress instigated and NIH created new offices to monitor misconduct that have in their operation abrogated the clause of the social contract recognizing a self-regulating scientific community. As established in chapter two, the social contract for science has a second clause in addition to self-regulation. The second clause maintains that the unfettered republic of science can produce the unspecified but ultimately forthcoming technological and economic benefits expected by the political community. This chapter and the subsequent one will show how this technology transfer clause of the social contract for science, too, has been overridden through the application of explicit inducements that correspond to those described by principal-agent theory.

Just as the social contract for science implied a market-like model of the scientific community, the technology transfer clause implies a model of the relationship among science, technology, and economic development. This model, variously named the linear model, the spinoff model, the pipeline model, or the assembly line model, suggests that the conversion or transfer of basic research into commercializable technology is a relatively cheap and automatic process (Alic et al., 1992). Occasionally in the postwar history of science policy, federal agencies have attempted to verify the linkages between basic research and technological advances, for example with Project Hindsight and TRACES. Congress has also tried to provide programmatic direction, with the Mansfield Amendment, RANN, and the War on Cancer, to augment the
applicability of research (see II.C.4.d). But with few exceptions—and these were generally information dissemination and invention reporting programs—the federal government relied on the automatic nature of technology transfer as implied by the social contract for science.

In the 1980s, though, with a set of legislation including the Stevenson-Wydler Act, the Bayh-Dole Act, and the Federal Technology Transfer Act, the federal government specified formal expectations and provided explicit incentives to adduce technological benefits from sponsored research. These incentives provide a politically guided alternative to the reward system of science that is so important to the idea of self-regulation. The incentives are premised on the idea that there exist ambitions and profits within research, and they intervene to augment these interests. The move from tacit understanding to explicit mechanisms to assure technological development mirrors the similar move to assure scientific integrity. It marks the fuller application of the tools of principal-agent theory and the demise of the social contract for science.

Because Congress passed and the President signed technology policy legislation, this chapter does not confront the complications of anticipated reactions or pre-emptive responses seen in the previous case (III.B.4 and III.D.4). Nevertheless, legislation is still mediated by the same boundary work performed in the agency as non-legislation is. The following chapter (VI) will therefore address, among other issues, the limits to legislative change through boundary work. This chapter on the technology policy legislation does, however, offer a remarkably clear example of the application of the kind of contractual incentives suggested by principal-agent theory.

To explain their application, the chapter recasts the problem of technology transfer as a problem of delegated authority. The first section will map the technology transfer problem onto principal-agent theory and show that, like the market model of science, the linear model of scientific and technological development was an ideological substitute for complete information about the process. The second section of the
chapter will describe how the political community became aware, in the late 1970s, of more complete information about the limits of the linear model and began, in 1980, to implement measures to reinforce it. The third section carries the argument to the Federal Technology Transfer Act of 1986, which brought the explicit engagement of basic research to commercial technology development to the government-owned, government-operated laboratories such as those at NIH. The following chapter will examine the implementation of the 1986 Act at NIH and the details of the process of technology transfer from its intramural laboratories.

B. Models of technology transfer

1. A principal-agent model of the technology transfer problem

As Kenneth Arrow (1991:48) makes clear in an overview of principal-agent theory, the costs associated with overcoming the information asymmetry between principal and agent can be very high, creating pressure for simple, real contracts that do not necessarily incorporate the incentives or other features required for a well-structured and efficient relationship. The social contract for science is this simple contract for managing the problems of moral hazard associated with the integrity and productivity of research. With respect to productivity, social contract for science maintains the expectation of forthcoming but unspecified technological benefits from a scientific community that retains its own decision-making mechanisms.

Arrow's simple contract is also a highly schematic vision of the actual contract, in Geertz's sense an ideology or "map of social reality" that substitutes for the lacking information (see IV.B.2). This simple contract can become more complex, as principals uncover information about the performance of the contract with which they can structure the conditions of their delegated authority to assure that the agent performs as instructed. This latter situation is the current state of affairs in which the social
contract for science has been discarded. In the case the integrity of science, the technique of assurance was monitoring and reporting. In the case of the productivity of science, the principal designed positive incentives to align the interests and reward system of the scientists with the goals of the political community.

At the outset of the relationship at the end of World War Two, the political community was persuaded that basic research is a source of useful innovations, and that such innovations are a major source of increases in industrial productivity, advances in public health, and so forth. The major political actors were unified in their "technological enthusiasm" (Hughes 1989; see II.C.3.a). The "strategy" that "basic research deserves public resources because it leads to unpredictable but assured social payoffs has been constant since World War II" (Averch 1985:6). The rhetorical combination of "unpredictable but assured" is a powerful defense against the drive for control expressed through principal agent theory and for the simplicity of the social contract for science. If the scientific community can convince the political community that payoffs are assured by citing such past successes as penicillin, then the political community need not perform costly monitoring to assure future success, nor would the scientific community need to permit the application of external incentives to achieve such successes. Furthermore, if the political community believes in the unpredictable nature of the payoffs, then it is likely to believe that any monitoring regime would be inefficient, and that any incentives would at best be indirect and undirectable.

The politicians and scientists who plotted the postwar map to the social reality of innovation highlighted just these features. In his *Science: The Endless Frontier*, Bush argued for the unique status of basic scientific research, particularly in universities, as a source of innovation in which the federal government could have a legitimate role. The Steelman report took a similar attitude. The exclusive relationship

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1 These are not exclusive options, nor perhaps even analytically distinct. Both are based on "maps of social reality" and in both, the principals structure the conditions of the delegation by relying on these maps. The social contract for science map does not support a strong structure or explicit framework; whereas, the map evolving from the social contract for science does.
between the two exemplifies the "bilateral monopoly" that Niskanen (1971) emphasizes as so important for the principal-agent relationship (see III.B.1). First, basic research was a unique source of innovation. Bush argued that "new products and processes are not born full-grown. They are founded on new principles and new conceptions which in turn result from basic scientific research. Basic scientific research is scientific capital" (Bush 1980 [1945]:6). Private firms would not create enough of this capital because they could not channel its flow once they had created it; private investment in research was not easily appropriable. Thus, even a limited government could play an active role in directly and indirectly supporting basic research, by creating a public resource, akin the government's traditional role to "foster the opening of new frontiers" (Bush 1980 [1945]:8). Similarly, Steelman (1947a:3-4) wrote that "[t]he processes and machines we use...all derive from theoretical discoveries in the various sciences....Only through research and more research can we provide the basis for an expanding economy, and continued high levels of employment." Because belief and circumstance proscribed other federal activity in stimulating industrial productivity, scientific research monopolized government attention to innovation. As historian George Wise notes, it was sound politics for Bush and his colleagues to constrain federal attention to innovation "only" to basic research (Wise 1986:231).2

Second, the federal government was the only appropriate patron because "in order to be fruitful, scientific research must be free—free from the influence of pressure groups, free from the necessity of producing immediate practical results, free from

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2 There is a tension between historians and science policymakers over the impact of Bush and his report. As described in chapter 3, it is hard, as the historians show, to provide direct causal links between Science: The Endless Frontier and many of the important post-War science policy innovations. Before the creation of NSF, the Office of Naval Research pioneered many mechanisms of research administration that were later adopted by NSF (Sapolsky 1990). Similarly, NIH appropriated contracts from the wartime Office of Scientific Research and Development (OSRD) and used that money to create its grant program (Strickland 1989). The previous success of these basic research programs in the mission agencies may have had some impact on the failure of Bush's hopes for NSF to include the full panoply of biomedical and defense science. But the proliferation of the model of unfettered basic research for the ultimate goal of social payoff also demonstrates Bush's success. Although he and his report may have merely captured the spirit of the times rather than caused them, its continued invocation by both scientists and politicians evidences its fertility.
dictation by any central board” (Bush 1980 [1945]:79). The federal government was
the only patron thought capable of promising this kind of freedom. Profit-seeking
industry would misdirect basic research because “there is a perverse law governing
research: Under pressure for immediate results, and unless deliberate policies are set
up to guard against this, applied research invariably drives out pure. The moral is clear:
It is pure research which deserves and requires special protection and specially assured
support” (Bush 1980 [1945]:83; emphasis in the original). For Steelman (1947a:26),
the unique government role was based on the provision of public goods by science: “It
is difficult to think of any other national activity which more directly benefits all the
people or which makes a larger contribution to the national welfare and security” than
research. He also agreed with Bush that the pressure for results requires the “wise
decision to maintain a relatively high degree of segregation organizationally for basic
research” (Steelman 1947c:99). The federal government was the only entity with the
foresight and the ability to provide special protection and assurance for basic research
and its provision of public goods. Thus developed an exclusive and privileged
relationship between the federal government and basic research.

2. The linear model and spinoff

The bilateral monopoly—that the federal government is the only appropriate
principal and that scientists performing basic research are the only appropriate agents—

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3 The history of the development of research institutions could, however, scarcely be described as “free
from the influence of pressure groups,” particularly in biomedical research. See Strickland (1972).
4 Bush’s argument is strikingly reminiscent of Tocqueville’s, who, a century earlier, argued that
industrialism in American democracy would assure the application of science and therefore that “the
whole of organized society should be directed to the support of higher studies and the fostering of a
passion for pure science” (Tocqueville 1969 [1848]:464).
5 I have expanded Bush’s concept in this analysis. He was firm that such research should be located
extramurally in universities rather than intramurally in the proposed foundation. Steelman, however,
was more comfortable with in-house research, and with the growth of basic research in the mission
agencies, the expansion is necessary. NIH is particularly interesting in its fraternal relations with the
university community and its self-described “campus” attitude. The difference between intramural and
extramural research at NIH has long been an issue, and a more complete principal agent discussion of
the relationship between both programs and Congress is probably warranted (but is not appropriate for
this analysis). The first-order response may be that by maintaining scientists performing basic research
under civil service guidelines and under mission agencies, politicians have some degree of standardized
control. However, this control is largely diluted by the fact that advancement even in government-
owned, government-operated (GOGO) laboratories such the NIH intramural labs is determined by
raises Niskanen's question of information and dominance. How does the principal ensure that the agent is not shirking? How does the political community ensure that the scientists' work will produce the expected benefits? After the war, in short, the productivity of science was self-evident. As Averch (1985:43) has argued, "The framers of the post-World War II 'contract' between the federal government and the scientific and technical community felt little concern about an innovation problem."6 The war had been won in MIT's Radiation Laboratory, in Los Alamos and in Oak Ridge, and in the Public Health Service (PHS) laboratories that produced penicillin and blood plasma. The instrumentality of science was patently clear. As long as government maintained the headwaters of the flow of scientific capital, and as long as "scientists are free to pursue the truth wherever it may lead, there will be a flow of new knowledge to those who can apply it to practical problems in Government, in industry, or elsewhere" (Bush 1980 [1945]:12). The primary way to expand the domestic economy and increase foreign trade was "through the constant advancement of scientific knowledge and the consequent steady improvement in our technology" (Steelman 1947a:3-4).

A large number of metaphors, some colorful and others hackneyed, attempt to capture this complacency toward the interaction of science, technology, and innovation. Bush's own metaphor was a fluid one: basic research was a pool of capital and knowledge that flowed across sectors and borders. Wise (1986:229) calls Bush's model "the assembly-line view" which "put money into pure science at the front end of the process. In due time, innovation will come out of the other end." Allen (1967:22), already critiquing the model as a policy tool in 1963, refers to it as the "left to right" or

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6 Averch's task is to provide an analysis of "how we think" about science policy (Averch 1985:5). He examines key science policy documents from the immediate postwar period as well as from later periods and reduces these to formal propositions about the respective roles of government and the scientific community in science policy. My analysis is somewhat parallel, and sympathetic.
"linear sequence" model. These metaphors emphasize the fundamental constraints on policy for innovation conceptualized in this way. "[I]n general," Allen (1967:23) writes, "the flow through the linear sequence may be increased either by increasing the driving pressure of the left hand end [of basic research], or by applying suction to the right hand end. In practical terms, the latter represents the finding or creating of a market for the product."7 But this right hand end of the line was largely taboo for American economic policy, and the narrow map of the innovation process constrained policy to a one-way thoroughfare. Representative George E. Brown, Jr. (D-CA), currently chairman of the House Science, Space, and Technology Committee, has argued that in the linear model as proposed by Bush, "federal support for basic research is the only independent variable in the equation. Increases in research funding should automatically lead to new technologies and economic strength" (Brown 1991:26).

By conceptually constraining political intervention to the single variable of funding, the linear model helped enforce the separation of science and politics inherent in the social contract for science. Although political exigencies may initiate the process of research--politicians may speak to the rock to get the water to flow--there was no other role for policymakers. Scientists would perform the basic research. Scientists would make the discoveries. The scientific literature would disseminate these discoveries. Industrial scientists and engineers would apply them. And industrial engineers and managers would develop and market them. The early part of the process would be mediated only by scientists and technical workers; it would be fired by scientific curiosity and fueled by the reward system internal to science and checked by the credibility cycle (II.C.3.c). "Support of basic research...must leave the internal control of policy, personnel, and the method and scope of the research to the institutions themselves. This is of the utmost importance" (Bush 1980 [1945]:33).

7 As we shall see, finding markets and creating market opportunities for federally-originated technologies is what the technology policy of the 1980s, particularly FTTA, was about.
Policymakers may speak to rock to call forth the water but, like Moses, they may not strike it without ill consequences.

Basic research was to be similarly cloistered from the market and other perverse influences. Colleges and universities were "relatively free from the adverse pressure of convention, prejudice, or commercial necessity. At their best they provide the scientific worker with a strong sense of solidarity and security, as well as a substantial degree of personal intellectual freedom" (Bush 1980 [1945]:19). The model also managed to assuage mainstream political fears of industrial policy, "since in the pipeline view research is safely separated from the free market" (Alic et al. 1992:25). Separated from the realms of politics and the market, science-based innovation "was viewed as more or less automatic--unmanaged and cost-free" (Alic et al. 1992:10). Unmanaged, because innovation occurred in a domain exclusively populated by scientists and technologists and did not require any deliberate, coordinating activities; cost-free because the cost of the research was the only necessary public input, as scientists performed the anticipated tasks with motivations unrelated to any incentives the political community could provide, and as private firms bore the cost of acquisition, development, and marketing of new technologies.

This almost epiphenomenal aspect to innovation is also known as "spinoff." The spinoff model and the linear model are similar in that they share the same

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8 As Kevles (1978) notes, although the 1930s and 1940s saw an increase in scientific personnel from non-traditional groups (particularly Jews but also Catholics), one could hardly say that the colleges and universities were even "relatively free" of prejudice. With respect to commercial necessity, the relative freedom was perhaps more a goal than a reality. Before the war, federal sources ranked third behind industrial sources and private foundations in funding to universities, and some universities had been encouraging commercial ventures since the 1920s with programs like the Wisconsin Alumni Research Foundation (WARF). Although Bush's argument relied on the isolation of research institutions, his map showed how to make them relatively free of commercial and other forces rather than showing them to be free already.

9 Ezrachi (1990) calls the insulation of science from political or economic forces the "autonomy of truth," a modernist concept that scientific truth is and should be acontextual that was adhered to by seminal American thinkers as Franklin and Jefferson. Although he argues that science helps extinguish distinctions between the categories of public and private, Price (1965) maintains that science as the search for truth can be effectively insulated from politics or the market by the appropriate interposition of professions and administration.

10 The usage of "spinoff" for the innovation process is probably derived from the term's usage to describe the "spinoff"of companies from research and development enterprises, companies based on
perspective toward "automatic" technology transfer. But whereas the linear model in its association with Bush seems usually to be associated with university research, the spinoff model is most closely associated with the Department of Defense (DOD) and the National Aeronautics and Space Administration (NASA).\textsuperscript{11} In theory, research and development in pursuit of agency missions would spin products off to the commercial sector. Since intramural research at NIH was not connected directly to the delivery of any health care, the spinoff model may be applied there as well.\textsuperscript{12} The idea of spinoff not only helped scientists justify the grants of resources and autonomy they received from the federal government, but it also helped agencies sell their research programs to skeptical politicians (Doctors 1969) and helped politicians to "sugar-coat" expenditures for wary taxpayers (Alic et al.1992:60).

Spinoff was also, in effect, a surrogate for technology policy (Alic et al. 1992:60).\textsuperscript{13} The intellectual model fit some of the more prominent, contemporaneous examples of science-based innovations that fostered entire industries, e.g., nylon and the transistor (Allen 1967:22). But the model did not incorporate the great deal of innovation that is process-oriented (Alic et al. 1992; Gomory 1990; Allen 1967), a category that became more evident to economists in the early 1960s as they realized "the full importance of competition through new products and processes rather than through direct price competition" (Mansfield 1968:18).

The details of many policy initiatives, however, did not necessarily correspond to the limits implied by the model. Alic et al. (1992:9) show how spin-off from research programs at DOD "typically required substantial additional attention and

\textsuperscript{11} Steelman (1947b:3-4) gives a splendid example of "reverse spin-off" from civilian research to a military application in the passage (II.C.3.c) on the fungicide tomatin.

\textsuperscript{12} Although by all accounts NIH intramural research is modeled most closely on the university/medical school system, from its Bethesda, Maryland "campus" to its "tenure" decisions.

\textsuperscript{13} Sapolsky (1975) also describes how, given the failure of national health insurance, biomedical research and the promise of spinoff was also a surrogate health policy.
investment,” rather than being free and automatic. Nelson (1982:464) emphasizes that the commercially most successful research programs, agriculture and biomedicine, have been concentrated in professional schools, rather than in colleges of arts and sciences, where the connection between research and its application is mediated by other professional roles and consumer demands. Doctors (1969) shows that the Technology Utilization Program (TUP) at NASA was more than merely an information-sharing enterprise. Thus, viewed in hindsight and with a narrow interpretation, the model not only failed to describe accurately the process of innovation, but it also failed to describe accurately some of the tasks actually assumed by government.\footnote{Devine, Thomas, and Adams (1987) divide postwar policy into three stages: the appropriability model (1945 to late 1950s); the dissemination model (late 1950s to late 1970s); and the knowledge utilization model (late 1970s to present). In their framework, appropriability emphasizes the supply of high-quality R&D, dissemination emphasizes the belief that R&D is under-utilized, and knowledge utilization emphasizes an active, interpersonal approach to technology transfer. I argue that the two earlier models both fall under the social contract for science, primarily because dissemination is predicated on the appropriability (single variable) model and because the knowledge utilization model allows a greater array of points of intervention. Dissemination as a mode of technology transfer is still a method of technical communication.} Despite these contradictions, however, the unusual situation of the United States \textit{vis-a-vis} potential technological competitors in the postwar period--an unscathed infrastructure, a primed economy, and a fresh pool of highly trained personnel--meant that even though the model was only partially accurate and partially implemented, it was never consciously challenged as a matter of policy or an article of faith. “The important point about the spinoff paradigm is not that it was a half-truth at best, but that the unusual circumstances of the postwar world did not force Americans to question it” (Alic et al. 1992:10). However, scholars have argued that the persistence of such inappropriate models of technology innovation have resulted in the inappropriate handling of technology transfer policy (Cozzens 1988; Sultan 1988).

3. Beyond the linear model

Although it is correct, given the emphasis in his report, that “Bush propagandized an older idea that only ‘basic’ science led to applied science, future
technology,” and so forth (Kohlstedt and Rossiter 1986:13), the success of Science: The Endless Frontier is more fruitfully sought in its flexibility than in its dogma. Bush (1980 [1945]:7) wrote that for basic science to be successful, “applied research both in Government and industry must be vigorous” and that industry could be assisted by policies to increase the flow of knowledge and the development of scientific talent, to clarify the tax code, and to strengthen the patent system. Indeed, the report maintained that “[p]atents are the life of research” (Bush 1980 [1945]:109). Steelman made a similarly flexible argument, complaining that the United States, “almost alone among major powers, has failed to establish an effective means of determining broad scientific policy either on a national or Government-wide basis” (Steelman 1947c:9). Former House Science Committee staff member John Holmfeld feels that two qualifiers to Bush’s formulation of the productivity of academic research are often neglected: the unpredictability of which basic research project will produce benefits; and the unpredictability of the time to payoff even for projects that do produce benefits.\(^\text{15}\) Holmfeld sees technology transfer as a way to increase the fraction of projects that do produce such benefits, and to accelerate the time to produce them.\(^\text{16}\) Regarding biomedical research, Bush (1980 [1945]:15) recognized, for example, that “[g]overnment initiative and support for the development of newly discovered therapeutic materials and methods can reduce the time required to bring the benefits to the public.” But Bush (1980 [1945]:21) underscored the simplicity of the linear model, and was thus interpreted in that light: “The simplest and most effective way in which Government can strengthen industrial research is to support basic research and to develop scientific talent.”

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\(^{15}\) These qualifiers are apparent in Steelman’s belief, for example, that grants are appropriate for “individuals or institutions whose competence has been demonstrated for the purpose of an investigation whose outcome cannot be known precisely in advance” (Steelman 1947a:50).

\(^{16}\) Interview with John Holmfeld, former research analyst, House Science, Space, and Technology Committee, Washington, DC, January 7, 1992.
Given this interpretation of Bush and the uniquely secure postwar situation of the U.S., Wise's (1986) argument that historians of science and technology outpaced policymakers in dispatching the model is not surprising. Wise bases his claim on the work of historians like Derek de Solla Price, who suggested that science and technology have been so intertwined historically that to place science temporally and causally before technology is at best arbitrary and at worst backward. Although historians had been gathering evidence of the incapacity of the linear model almost since its genesis, the "funeral" for the old model was not held until a 1972 meeting of the Society for the History of Technology (Wise 1986:236).

In addition to the historians cited by Wise, policy analysts have attacked the linear model, and they have also offered alternatives. Allen (1967:27-30) describes a "wheel, hub and axle model" in which investment is the axle, development is the hub, and research, construction, distribution, and production are all parts of the wheel. This model allows Allen to specify several routes of interaction among the variables. For example, friction gears among the various components would mean that a torque applied to one would move all to some extent. But Allen also points out "slip properties" that can disengage some elements, or can promote a controllable and even stable procession of the entire assembly.

Morton (1969:215) recognizes that science-based innovation is neither free nor automatic because it is "a people process." Rather than being linear, the process requires feedback to direct it, but not too strongly as to destroy the flexibility of basic research (he thus recommends, like Bush and Steelman however, an organizational or institutional barrier between basic and applied research). The process is more like a

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17 Significant here is Price's work on the relationship between citations of papers in science and technology, and most interestingly his statements before congressional committees on the matter. See Cozzens (1988) for a review of Price's work.
18 See a collection of papers from the conference in Technology and Culture, introduced by Thomas P. Hughes (1976:428-29), who is concerned with distinguishing among "invention," "innovation" and "development" because "simplistic and ill-defined use of the terminology of technological change obfuscates funding and perhaps even impedes 'development.'"
spiral, a continuous process moving from a practical problem or technical limitation through new knowledge, new devices, more technical limitations, and at each step contract is maintained with elements of the previous cycle.

Other metaphors are even more colorful. Shapley and Roy (1985) replace the linear model “tree” that has basic research roots and technology fruit with a “two-tree” model in which basic science “cross-pollinates” with technology. Dorf and Worthingham (1987) suggest a “basketball game” of technological innovation rather than a “relay race.” Pinch and Bijker (1987) prefer a “seemless web” or systems approach to the science-technology connection, and Gieryn (1990) invokes a “chain-link” model. Gomory (1990:140) contrasts the “ladder paradigm” in which “you put the R&D in here, and the product pops out there,” to the “cycle paradigm” which provides the proper approach to planning the evolution of an existing product. Even the Carnegie Commission on Science, Technology, and Government (1991) admits that the “simple causal model” is flawed.

The ideological attraction of the linear model has remained strong, however, because it specifies the quantity of basic research as the only variable and therefore the only point of legitimate intervention for the federal government. Not only does the model constrain the locus of federal intervention to basic research, but it constrains the mode of intervention merely to adding more money to achieve more output. In times of financial stress, it severely limits the ability of politicians to influence positively scientific productivity. This model also maintains the separation between science and politics, and between science and the market, inherent in the social contract for science.
C. A tentative technology policy

1. Prelude--an errant path to PHS patent policy

The legislation described in this section marks the beginnings of a discrete and broad-based technology policy, even if only "a tentative first step," in the postwar United States. This technology policy is associated with the demise of the social contract for science because, only without the contract, is a discrete and broad-based policy focusing on the elements of the innovation process other than basic science inputs possible. Oddly, this technology policy is largely the legacy of the Carter and Reagan presidencies which, despite their predilection for deregulation and market solutions, opened the flap of the proverbial tent to the camel's nose of technology policy. Although a major theme in the various laws related to technology transfer is the leveraging of federal investments in basic research by coupling them more closely to market incentives--in effect privatizing some aspects of public functions--the incomplete nature of the privatization results in a public-private relationship that is, at times, unseemly to liberals and conservatives alike.

Biomedical research provided an errant but ultimately successful path to the part of this new technology policy that focused on patents as aides in moving inventions from the laboratory to the marketplace. Bush and the conservatives had favored the automatic assigning of title to inventions to grantees, as had been the wartime policy of OSRD. Senator Kilgore had favored public ownership of all inventions created with public funds. Because the dilatory passage of legislation to create the National Science Foundation (NSF) opened the way for a more significantly plural research system with a similarly plural system of patent rights among the funding agencies (II.C.2.c), neither

19 As described by Howard W. Cannon (D-Nev), chairman of the Senate Commerce, Science, and Technology Committee (U.S. Congress 1979:1).
20 "Discrete" because it is not driven by procurement, for example; broad-based because it spans disciplines and technologies rather than focusing on single technology or single system demonstration projects.
21 This is exactly the slippage between public and private that Price (1965) had suggested science could facilitate.
perspective on patents became part of the postwar consensus of the social contract for science.

The Public Health Service (PHS), under the administrative aegis of the Federal Security Agency (FSA), adopted a system of public ownership. FSA, "as a matter of policy, [took] the position that the results of research supported by grants of public moneys should be utilized in the manner which best serves the public interest...[which generally occurs] if inventive advances resulting therefrom are made freely available to the Government, to science, to industry, and to the general public" (FSA 1952:1).

FSA (1952:1) recognized, however, that under some conditions such as joint sponsorship or "in order to foster an adequate commercial development to make a new invention more widely available," that the patent system might have to be engaged and titles granted to grant recipients.

This FSA statement opened the way for what became known as the Institutional Patent Agreement (IPA), which was codified in the reorganization of FSA into the Department of Health, Education, and Welfare (DHEW) in 1953 (U.S. Congress 1978b:58). IPAs were negotiated to allow nonprofit recipients of PHS grants to own the patents on inventions derived from sponsored research if the recipient's patent policies are consistent with DHEW's goals and the public interest (U.S. Congress 1979a:57). Eighteen IPAs were negotiated in the first few years of their availability. But the system then fell into a long period of disuse, despite approving comments in an examination of the federal patent system conducted by the Federal Council for Science and Technology (FCST) of the White House Office of Science and Technology (OST) in 1965 (U.S. Congress 1978b:58).

In 1968, DHEW reinstated its IPA policies in response to a critical report from the General Accounting Office (GAO 1968) about barriers to the commercial testing of potential therapeutics under NIH grants in medicinal chemistry. Grant recipients were

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22 The legal authority for IPAs derives from the discretionary nature of the terms and conditions of grants made by federal agencies. See the testimony of Norman Latker in U.S. Congress (1978b:59).
having problems obtaining testing services without patent policies that enabled the commercial testing services to acquire exclusive licenses. Some researchers were even redirecting their research away from testable products to avoid the dead-end problem of having a product but being unable to find testing services (GAO 1968:16). Among the GAO recommendations adopted by DHEW was the reestablishment of the IPA system and the creation of a standard or model IPA, which resulted in the number of IPAs increasing to 72 by the end of 1968 and the commercialization of at least seventy-five medical inventions by 1977 (U.S. Congress 1979a:57,48). Nevertheless, in November 1977 DHEW undertook a review of the IPA system, expressing concern that “the use of IPAs encourages exclusive licensing and thereby sacrifices the agency’s broad objective of influencing the availability and cost of HEW-supported inventions...[and that] the use of IPAs is conceptually inconsistent with any objective other than rapid commercialization” (U.S. Congress 1979a:48). Even as the technology policy legislation that is the focus of the remainder of this chapter was being planned, DHEW was reconsidering its commitment to IPAs once again.

2. The broader scope of technology policy

Beyond DHEW’s wanderings through patent policy, the history of the amending of the technology transfer clause of the social contract for science has a broader scope than that of the challenges to the self-regulation clause. Whereas, the latter was largely contained by the relationship between a congressional committee and NIH; the former involved policy initiation by the President and strategic action within the Congress directed both internally and externally. Although some of the issues to be dealt with in the new legislation, like patent policy, had been on the table since the original NSF debate, the immediate impetus for their serious reconsideration was the activity of President Carter’s advisory Committee on Industrial Innovation, part of his broad-based Domestic Policy Review.
In an October 31, 1979 message to Congress on industrial innovation, President Carter touted industrial innovation as “an essential element of a strong and growing American economy. It helps ensure economic vitality, improved productivity, international competitiveness, job creation, and an improved quality of life for every American” (Carter 1981:2070). Carter offered nine initiatives covering various aspects of technology policy for industrial innovation, including “enhancing the transfer of information,” “increasing technical knowledge,” and “strengthening the patent system.” He proposed a Center for the Utilization of Federal Technology “to improve the transfer of knowledge from Federal laboratories” and he acknowledged his support for uniform government patent rules and the retention of patent ownership by universities and small businesses on government-sponsored inventions, “in recognition of their special place in our society” (Carter 1981:2071).

In announcing these proposals, Carter also announced a step away from the linear model of innovation and the social contract for science:

Innovation is a subtle and intricate process, covering that range of events from the inspiration of the inventor to the marketing strategy of the eventual producer. Although there are many places in the chain from invention to sale where we have found modification of Federal policy to be appropriate, there is no one place where the Federal government can take action and thereby ensure that industrial innovation will be increased (Carter 1981:2073-74).

Seeing multiple points of federal policy intervention, none among them necessary and sufficient for increased innovation, Carter transcended the postwar rhetoric and began to amend the social contract for science.

The legislative activity liberated by this transcedence spanned the 1980s. In the year following Carter's initiative, Congress passed the Stevenson-Wydler Technology Innovation Act (P.L. 96-480) to assist technology transfer and the Bayh-Dole Patent and Trademark Laws (P.L. 96-517) to overhaul the patent system. Congress fine-tuned Bayh-Dole in 1984, and revised Stevenson-Wydler with the
Federal Technology Transfer Act (FTTA) of 1986 (P.L. 99-502). Together with other acts of Congress involving patents, small businesses, and cooperative research and development projects, the government began actively encouraging and supervising the scientific community across a range of variables in the innovation process. With the incentives applied by the new legislation and the subsequent institutionalization of expectations, the social contract for science began to look less like a tacit agreement and more like an explicit contract. Similarly, the procedures implemented by NIH began to run up against the limits of flexibility for science and the demands of procurement.

The sections below recount the most important pieces of legislation from Stevenson–Wydler to the FTTA and how each of these acts play out the logic of principal-agent theory, as the political community began to redistribute the residuals of efficient operation to the members of the scientific community who were successfully engaging in innovation.\(^{23}\) In the following chapter I will describe the implementation of relevant parts of the legislation at the intramural labs at NIH and how this implementation provides a new overlay to the reward system of science.\(^{24}\)

3. Technology Innovation Act of 1980

Just as individual cases of scientific misconduct—poorly handled by scientific peers, universities, and funding agencies—spurred Congressman Dingell to question the scientific specialness enshrined in the social contract for science, deteriorating economic conditions in the late 1970s caused other members of Congress to question whether the United States was in fact reaping the return on its substantial investment in scientific research. Was the organization of incentives and sectoral associations of R&D in the U.S.—an organization that separated science from social goals—an effective and

\(^{23}\) For other summaries of this legislation, see West Publishing (1991) and Bagur and Guissinger (1987).

\(^{24}\) There are approximately 750 national laboratories, usually distinguished by their sector of operation: the government-owned, government-operated (GOGOs) and the government-owned, contractor-operated (GOGs). The intramural laboratories at NIH are GOGOs, and with a basic research budget of almost $1 billion in 1991, they rival the size of the largest DOE GOCO labs. The NIH intramural program is the world’s largest performer of biomedical research.
efficient way of producing the expected benefits? Congressional involvement was broad: the science committees, the small business committees, and the judiciary committees of both chambers, as well as the Joint Economic Committee, were all active. The result was bipartisan support for what was essentially an incipient technology policy that included specific inducements to enhance the productivity of research. The original social contract for science, founded in the wake of the Second World War, founndered in the economic crisis that was the "moral equivalent of war."  

Although "the nature of the innovation process and the consequences of technological innovation have received little attention in the United States until recently" (U.S. Congress 1979c:18-19), at least seven hearings in the House and Senate in 1979 and 1980 addressed the up-and-coming topic. This activity was derivative of the idea that science could not continue to be separated from social and political goals, as opposed to the idea in the social contract for science. The idea of integrating science was reified in the organization of the Senate committees and embodied in Senator Adlai E. Stevenson. Stevenson was the chairman of the Senate Commerce Committee's Subcommittee on Science, Technology, and Space, and he also sat on the Senate Banking Committee, where his subcommittee had jurisdiction over the Export Administration and the Export-Import Bank and oversaw the declining fortune of U.S. exports. The yoking of science with commerce in the organization of the Senate, and Stevenson's dual role in science and international economics, helped create legislation that itself linked science and commerce, and science and international economics.  

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25 Representative Jim Lloyd (D-CA), a member of the House Science and Technology Committee, borrowed President Carter's language about the energy crisis to describe the urgency of acting on industrial innovation.
27 Interview with Steve Merrill. Staff on the House side dispute the contention that the Senate organization is superior in this regard, however, suggesting that the focused organization in the House allows its members to work successfully and, at times, almost invisibly, at the margins. See the account of Bayh-Dole below.
Within one hour of President Carter’s message to Congress on industrial innovation, the science committees and small business committees of both chambers held a joint hearing. Gaylord Nelson (D-WI), of the Senate Small Business Committee, presided. He pointed to social realities uncharted by the old map, a set of "unprecedented economic problems: inflation, energy, and declines in productivity and economic growth" and suggested that "our best hope for resolving these problems is to speed up the process of innovation that has been the trademark of this country since its beginnings." Senator Nelson also cited the by now all-too-familiar discrepancy between the success of the U.S. in acquiring Nobel prizes (103 in physics, chemistry, and medicine at the time compared to 3 by Japan) and its failure in promoting economic growth and productivity. "Despite this dominant achievement in pure science, there is a serious imbalance in terms of economic returns" (U.S. Congress 1979e:1-2). The primary witness at the hearing, Secretary of Commerce Juanita Kreps, summarized the challenge to both the country and the reality mapped out by the social contract for science: "In short, the preeminence of our industry and of our scientific and technological capabilities is being called into question" (U.S. Congress 1979e:14).

In reporting to the House of Representatives on the results of the several hearings on industrial innovation and on the bill (S. 1250) that was their focus, the Committee on Science and Technology recognized that "[t]echnology innovation is the process by which industry generates and diffuses new and improved products and processes. It is a vital component of economic growth [in which the] U.S. has traditionally been the leader....However, the extent of this lead may be diminishing in relation to past U.S. industrial performance and vis-a-vis foreign industrial performance" (H. Rpt. 96-1199:3). A number of indicators pointed to an eroding U.S.

28 George Brown, who at the time was the chairman of the Subcommittee on Science, Research, and Technology, had introduced H.R. 4762, the House version of S. 1250. A companion bill to S. 1250 had been introduced in the Senate--the Science and Technology Research and Development Utilization Policy Act (S. 1215)--by Schmitt, Howard W. Cannon (D-Nev), and Stevenson (D-IL), which became part of Bayh-Dole.
position, including a decline in real research and development (R&D) spending since the mid-1960s, as well as in the ratio of R&D spending to GNP; a decline in the rate of patents of domestic origin; a decline in productivity gains compared to foreign competitors; and a slipping trade balance even in R&D-intensive products. "It became apparent," testified J. Herbert Hollomon:

that the strong U.S. policy emphasis on R.&D., even now still only slightly less than one half of the world's total, had not produced the results its earlier proponents had proclaimed....[This is] a lesson that is by now well-learned, as a result of mistakenly equat[ing] R.&D. with innovation among policymakers....We have not appreciated the complexity of the process (U.S. Congress 1979c:20).29

With the decay of these available indicators of innovation, it was unclear that the technological promise of the social contract for science was being fulfilled.

In an effort to leverage the declining real R&D spending, the committee "recognized that there is a need to improve the productive capacity of the nation's Federal laboratories by utilizing them more fully, not only as R.&D. centers for the Federal mission agencies, but also as national resources—resources that State and local governments, as well as the private sector, can turn to for sound scientific and technical know-how" (H. Rpt. 96-1199:7).30 Witnesses stressed the necessity for "active" engagement in technology transfer (U.S. Congress 1979b:27). They also attacked the one-variable model of technology transfer: "Significantly increased R&D funding does

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29 Hollomon had been Assistant Secretary of Commerce in the Kennedy Administration and in that position had conducted an extensive review of the federal role in technological innovation. Despite the review, it was "other nations [that] began to understand and appreciate the importance of innovation and to perceive R.&D. as only one part of the complex process required to bring the new into practical and widespread use" (U.S. Congress 1979c:19). Hollomon argued that the U.S. did not adopt this attitude because of its unique postwar position, which it began to lose to the aggressive emulation and adaptation of its spinoff policy by other countries. He also linked economic indicators such as declining productivity increases and the trade imbalance to technological failures such as the nuclear accident at Three Mile Island as part of the technological malaise in the U.S.

30 The effective utilization the federal laboratories had been on the agenda at least since hearings by the Research Subcommittee in 1968, which considered how other agencies tap into the labs. A 1978 House Science Committee print discusses two reports by the Council of State Governments on technology transfer to state and local governments. Technology transfer to industry was a relative newcomer. See the testimony of Albert H. Teich in U.S. Congress (1979b:3-23).
not necessarily increase innovation, but better planning, accountability and follow through of R&D does” (U.S. Congress 1979b:620). The committee also understood that because technology transfer was not part of the mission of research agencies (except NASA), “work performed in this capacity is not often relevant to professional promotion within the organization. In fact, career development of staff engaged in technology transfer is sometimes detrimentally affected because it is spent on activities other than those specified in position descriptions upon which promotions are based” (H. Rpt. 96-1199:33). To this end, the Science Committee urged “the Federal Government [to] strive to transfer federally owned or originated technology to State and local governments and to the private sector.” The committee “intended that this policy will provide the basis for the inclusion of technology transfer programs in the mission requirements of every Federal agency engaged in R.&D. activities” (H. Rpt. 96-1199:32). The House Science Committee thus tried to restructure the missions of research agencies to align internal agency incentives with its goals, encroaching on Vannevar Bush’s prescribed freedom of personnel and policy decisions.

The committee further recognized “that technology transfer must, to be effective, consist of more than information dissemination. Technical assistance, often in the form of person-to-person assistance, is also required” (H. Rpt. 96-1199:35). “The best way to transfer technology is to transfer the people who developed it or to help those people who developed the idea take it all the way to the marketplace” (U.S. Congress 1979c:108). Steve Merrill, a former professional staff member for the Senate Committee on Commerce, Science, and Transportation, which handled the bill on the Senate side, suggests that the act was indeed part of new congressional thinking that the old model of transfer--cheap and automatic--was over. Instead, the federal government was considering partial support for private sector activities in innovation.31 To this end, Stevenson-Wydler attempted to encourage cooperative research between

31 Interview with Steve Merrill, former professional staff member, Senate Committee on Commerce, Technology, and Transportation, Washington, DC, January 2, 1992.
universities and industry. By some measures, the industrial support of academic research had declined substantially since the immediate postwar period and was seen as part of the competitiveness problem.\textsuperscript{32} Some even attributed the decoupling of universities and industry to the federal government’s R&D funding policy itself, as the federal government squeezed out private investment in academic research (U.S. Congress 1980c:viii).\textsuperscript{33} To remedy the situation, Stevenson-Wydler proposed the establishment of Centers for Industrial Technology as sites for university-industry cooperation and loci for technology transfer. Such a proposal began to question the organizational segregation of basic research proposed by Steelman, and the “special protection and specially assured support” proposed by Bush for pure research.

Still, some witnesses before the interested committees criticized the proposed legislation because it dealt “only with one part of the innovation process, R&D inputs.” The innovation process was broader than R&D, and included other crucial steps; some witnesses, for example, thought that R&D was the step least in need of attention, and that a federal innovation policy would better address regulatory policy (U.S. Congress 1980c:65-66). For the Centers for Industrial Technology to work, patent laws would have to be changed. Dwight M. Baumann, director of the Center for Entrepreneurial Development at Carnegie-Mellon University, argued, “To provide economic stimulus and value to taxpayers, federal research developments should be passed along to businesses that can apply them to product development” by granting exclusive (but conditional) licenses on federal patents (U.S. Congress 1979c:175). Stevenson-

\textsuperscript{32} Jacob E. Goldman, then senior vice president and chief scientist at Xerox Corporation, testified that the ratio of federal support to industrial support of university research had increased from approximately 8:1 in 1960 to 30:1 in 1967, and had stabilized at about 20:1 in 1975 (I find these ratios to be 10:1, 29:1, and 20:1; by 1979 the ratio had declined to 18.5:1 and by 1986 it had surpassed the 1960 ratio to 9.5:1—NSB 1991:306). Lewis M. Branscomb, then vice president and chief scientist at IBM (and former director of the National Bureau of Standards) presented a slightly different view, comparing the ratio of total industrial R&D to federal R&D in 1962 (about 1:2) to that in 1980 (about 1:1) (This trend has continued; in 1991 the estimated government-industry ratio was 1.2:1—NSB 1991:308). See U.S. Congress (1979c).

\textsuperscript{33} In the context of the argument above that Vannevar Bush attempted to emphasize the “bilateral monopoly” aspect of the social contract for science, this attribution rings true.
Wydler provided these for the Centers, but Bayh-Dole—the parallel track of technology policy in 1980 (see below)—complemented Stevenson-Wydler to provide additional elements, not limited to input, that emphasized the personal nature of technology transfer.

Nevertheless, Stevenson-Wydler marked the beginning of the end of the technology clause of social contract for science, and it did so with resounding support. Although the House and Senate moved the bill back and forth several times over specific amendments, both chambers passed the bill with bipartisan support. The major provisions were largely in line with Carter’s domestic policy initiative. Finding that “antitrust, economic, trade, patent, procurement, regulatory, research and development, and tax policies have significant impacts upon industrial innovation and development of technology” (94 Stat. 2311), Stevenson-Wydler challenged the promise of technological and economic benefits from an exclusive focus on R&D and it expanded the scope of interest to institutional and other variables.

Stevenson-Wydler declared, in contrast to the epiphenomenal result of spinoff, that “[i]t is the continuing responsibility of the Federal Government to ensure the full use of the results of the Nation’s Federal investment in research and development. To this end the Federal Government shall strive where appropriate to transfer federally owned or originated technology” (94 Stat. 2318). Likewise, it was “the sense of Congress that departments and agencies, including the Federal laboratories, whose missions are affected by, or could contribute to, the programs established under this Act, should...support or participate in activities or projects authorized by this Act” (94 Stat. 2316). The act required the major federal labs to establish their own Offices of Research and Technology Applications (ORTAs) to deal with “off the shelf” technologies—patents owned by the government that were languishing without private

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34 The Senate passed S. 1250 on May 28, 1980. The House passed an amended version on September 8. On September 26, the Senate concurred with some House amendments and disagreed to others, and on October 1, the House finally either concurred in or receded from the remaining amendments.
interest. It required the President to award a National Technology Medal, in supplement to the National Science Medal, for “outstanding contributions to the promotion of technology or technological manpower for the improvement of the economic, environmental, or social well-being of the United States” (94 Stat. 2319). The act also established the Center for the Utilization of Federal Technology, in the National Technical Information Service, to serve as “a one-stop shopping center” for information about federal technologies. The act “establishe[d] a clear Federal mandate to promote industrial technology...and enhanced government-business cooperation” (Carter 1981:2379-80).

The Stevenson-Wydler Act thus took a few short steps along the principal-agent dynamic to reduce the moral hazard of funding unproductive research. Primarily, the Act emphasized the technology development missions of the federal laboratories in an effort to align the internal incentives of the organizations with the intent of Congress in delegating authority and appropriating funds to their scientists. It also mandated ORTAs, whose professionals—employed to transfer off-the-shelf technologies—would be in a position parallel to that of inspectors general in that their job is to enhance the productivity (rather than the integrity) of scientific programs by acting from the outside. The Act also made a tiny though not unremarkable statement by placing the National Technology Medal into the reward system of science.

4. Bayh-Dole Act

Whereas, the Stevenson-Wydler Act concentrated on new institutions in the federal research establishment, the Bayh-Dole Act began to overhaul the checkerboard federal patent system to provide specific incentives for federally funded scientists and engineers to engage in technological innovation. The checkerboard system resulted,

35 The Centers for Industrial Technology were similarly oriented, in that they would provide an institutional context aligned directly with congressional interests in technological innovation and industrial development, but I do not deal with them because of their failure to be implemented fully by the Reagan Administration.

36 For a history of the development of federal R&D patent policies, see U.S. Congress (1976b; 1976c).
at least in part, from the failure of Bush and his allies to pass a science foundation bill before other research agencies such as PHS and the Office of Naval Research became active. Each agency was therefore free to establish its own rules for granting title on federally funded inventions. But no civilian agency established the "liberal" patent policies Bush advocated until NASA did in its organic act of 1959.37 Because of the changes it advocated in the patent system, Bayh-Dole was more contentious than Stevenson-Wydler, although it too eventually achieved wide congressional support.

Bayh-Dole had origins similar to Stevenson-Wydler in the Carter Administration's Committee on Industrial Innovation, but influential congressional hearings began in 1976 before the Subcommittee on Domestic and International Scientific Planning and Analysis of the House Science Committee. Appearing before the subcommittee, Deputy Assistant Secretary of Commerce for Product Standards Howard I. Forman set the tone for this and future hearings by presenting chairman Ray Thornton (D-AK) with a medal from the National Inventors Hall of Fame. The obverse of the medal bears the passage from article I, section 8 of the Constitution of the United States describing the power of Congress to grant patents to encourage the sciences and the useful arts. The reverse bears Abraham Lincoln's dictum, "The patent system added the fuel of interest to the fire of genius." Forman continued his dramatic testimony by drawing an analogy between the patent system and the baby whom King Solomon had ordered divided between the two petitioning mothers: like the baby, proprietorship cannot be divided productively, Forman argued. The government should therefore grant exclusive licenses. Forman even accused the U.S. government of being guilty of the same kind of suppression of patents that populists often accused large corporations of doing: the government held title to some 28,000 patents and failed to provide licensing terms that resulted in no more than five percent of these patents' commercialization (U.S. Congress 1976a:4-11).

37 NASA was also just at the edge of civilian and military research and had an explicit technology development mission.
Another witness at this early hearing, Norman J. Latker, patent counsel to the Department of Health, Education, and Welfare (HEW), described the problem of the status quo of research and industrial innovation: “the closing of the enormous gap between the new fields of knowledge...and their practical implementation by industry...has been left to random and haphazard execution” (U.S. Congress 1976a:556). Latker had been the chairman of an ad hoc Subcommittee on University Patent Policy for the Federal Council on Science and Technology of OSTP. Addressing the problem of university-business cooperation in R&D, Latker testified that “[t]o overcome these barriers to technology transfer, it was deemed essential to the subcommittee that the Government persuade universities to provide a management capability within the institution that will serve as a focal point for identification, receipt, and prompt protection of the invention results of university research for later dissemination to industrial concerns” (U.S. Congress 1976a:647). Contrary to Vannevar Bush’s idea that the government would provide money and no other guidance or interference in university affairs, Latker defined a new role for the government and for universities in organizing for technology transfer, rather than simply for research. Indeed, one of the criteria for negotiating the IPAs at DHEW that Latker helped to implement was assuring that the university had some active technology transfer capacity.

Birch Bayh (D-IN) of the Senate Judiciary Committee took such testimony to heart. He feared that the U.S. had lost its edge in developing new technologies, as indicated by the new $14.9 billion trade deficit in manufactured goods in 1978. He reiterated statistics that fewer than four percent of the nearly 30,000 federally owned patents were licensed. “This is very little return on the billions of dollars we spend every year on research and development” (U.S. Congress 1979a:1-2). Likewise, Robert W. Kastenmeier (D-WI), of the House Judiciary Committee, expressed almost single-minded faith in manipulating the patent system as an intervening variable.
between research and industrial productivity: "The annual inflation rate is now
approaching 20 percent with the other economic news equally grim. At the heart of the
American economy's ability to overcome this situation is our patent system" (U.S.
Congress 1980b:1). Citing the belief of economists that these woes of the U.S.
economy resulted from a failure of its industries to keep up with the productivity gains
of its competitors, the House Judiciary Committee asserted that "the effective
commercialization of government financed research is becoming an ever more important
issue for those who are concerned with industrial innovation" (H. Rpt. 96-1307, pt.
I:2).

Witnesses still discussed the 1968 GAO report that found that "inventors [with
HEW funding] were stuck with hundreds of potential therapeutic agents on their
shelves" but could not interest industry in them because of the inability of industry to
protect its risky investment with exclusive licenses (U.S. Congress 1978b:55; U.S.
Congress 1979a:37; and see V.C.1 above). According to former NIH director Donald
S. Fredrickson, "[T]he real credit for [Bayh-Dole] goes to the people who had earlier
had put together the NIH patent system which gave to the institutions the right to have
the inventions and to license them, and that of course was so damn successful and
obviously desirable that that was then swept...into all government agencies."38
Fredrickson probably overestimates the effect of DHEW's IPAs given the critical
review the department was simultaneously conducting. Nevertheless, the IPAs did
seem to provide an important precedent and Bayh-Dole proposed to spur even greater
commercialization by allowing small businesses as well as non-profit organizations
such as universities to obtain title to inventions that had been made with federal grants
or contracts with relative ease. The legislation also specified that the organizations
granted title to these inventions were to share royalties with individual inventors and to

38 Interview with Donald S. Fredrickson, former director, NIH, Bethesda MD, January 5, 1993.
use the remaining funds to underwrite the legal costs of patenting and licensing, and to support additional research and education.

A former senior staff member at the House Science Committee recalls that the impetus for the Bayh-Dole Act was “to get the fruits of the federal dollars harvested by the private sector.” This staffer also says that—akin to the logic of the principal-agent model and the change in the social contract for science—that “scientists were the tool to try to get the job done.” The job was industrial productivity through innovation. The committee saw no reason that federally funded scientists should not be “keeping one eye cocked toward plying the research they were working on.” Furthermore, the revenues derived from patents and licenses were also a way to formalize the “bootstrapping” of basic research. “Bootstrapping” refers to the practice, often reported in sponsored research, of diverting resources from a research contract or an applied project, to a basic research project. It is part of the moral hazard of research productivity. Although the act specified general categories to which license income needed to be applied, laboratory directors would generally have fewer constraints on the use of such money than on the (licit) use of grant or contract money. As one former senior staff member said, there is “nothing more dear to a lab director’s heart than having a pot of unlabeled money—nothing.” Having unlabeled money was important, staff thought, “because science needs to be vibrant.”

Although the goal of stimulating technological innovation was widely shared—as evidenced by Stevenson-Wydler—there was significant opposition to manipulating patent law to achieve that goal. Law and policy regarding the disposition of federal patents varied across agencies. NASA, which had made the most of the spinoff rationale, had the most liberal patenting and licensing policies, although these policies were by no means streamlined (Doctors 1969). The House Science Committee hoped

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39 The staffer does mean “plying” as “selling”, rather than “applying” as in “making practical.”
40 Interview with Robert C. Ketcham, former chief of staff and general counsel, House Science, Space, and Technology Committee, Chevy Chase, MD, January 17, 1992.
to achieve a uniform policy across the agencies--of which there were some twenty-six different patent policies--and since NASA already had a liberal policy, legislation was needed to bring the other agencies to that level.41 “[B]ut [t]he general feeling in Washington was that if the government was paying for it, the government should own it.”42

Between the House Science Committee and a uniform federal patent and licensing policy was Jack Brooks (D-TX), second senior Democrat on the House Judiciary Committee. Brooks believed that the patent proposal in Bayh-Dole:

violates a basic provision of the unwritten contract between the citizens of this country and this government; namely, that what the government acquires through the expenditure of its citizens’ taxes, the government owns. Assigning automatic patent rights and exclusive licenses to companies or organizations for inventions developed at government expense is a pure giveaway of rights that properly belong to the people (H. Rpt. 96-1307 pt.I:29).43

Brooks’ use of the “unwritten contract” language here is significant rather than merely fortunate. Subject to a social contract, science was presumed to be a public rather than a private enterprise. NIH had a public health mission. The change in the social contract for science was also a change in the unwritten contract Brooks saw, one that began to privatize what had been public property.

Brooks further argued that no organizations were currently turning down federal funds because they could not easily obtain patent rights over any inventions, and unless supporters of the measure planned on increasing federal R&D expenditures beyond the half-share the government already provided, he “fail[ed] to see how

41 The act needed to amend, among others, the Atomic Energy Act of 1954, the National Aeronautics and Space Act of 1958, and the Federal Nonnuclear Energy Research and Development Act of 1974, some of the landmark pieces of legislation in postwar science policy. Representative Toby Moffett (D-CT) opposed the idea of a uniform set of patent policies because the current policies were each based on considered congressional opinion that was more likely to be appropriate for the specific technologies each agency dealt with than a single, uniform standard (H. Rpt. 96-1307, pt.II:24)
42 Interview with Robert C. Ketcham.
43 Again, Price’s dictum applies, as does Ezrahi’s (1990:chs.10-12) warning of the “privatization of science.”
enactment of the bill would lead to increased production” (H. Rpt. 96-1307 pt. I:29). Supporting this type of technology transfer by granting exclusive licenses, Brooks argued, could restrict the number of producers and marketers of technology and thus restrict competition.

Furthermore, Brooks feared that by appearing to leverage publicly funded R&D, the legislation would decrease private R&D. “It is an entrepreneur’s dream,” argued Brooks. “If he is successful, the government gives him highly profitable monopoly rights over the product; and, if he fails, well, he hasn’t lost anything. There is simply not sufficient evidence to support the creation of another welfare fund for private business which can only serve as a disincentive for private investment.” As if understanding the nature of the expiring social contract for science, Brooks predicted that the bill would also change the emphasis of federal R&D from “intellectual and technological innovation for the general welfare of the people to one which emphasizes the profit incentive underlying commercialization in the marketplace” (H. Rpt. 96-1307, pt. I:30-31).

A former senior staffer at the House Science Committee suggests that the committee was able to succeed with this legislation, despite such opposition by Brooks, by pursuing a strategy of working the “margins,” where questions of technology policy were still located in 1980. When the committee prepared to report the bill, the House parliamentarian questioned its filing for an open rule for one of such potential controversy. But, the staffer relates, nobody was banging on his door about the work,

44 Senator Russell P. Long (D-LA) was an ally of Brooks on this matter. Long saw “not even a shred of evidence to support” the position that current patent policies caused delays and stifled innovation. Long conjured an old metaphor from Nobel economist Wassily Leontief that granting patent rights to a federal R&D contractor was no more reasonable than allowing a federal road contractor to collect tolls. As former chairman of the Monopoly Subcommittee of the Senate Small Business Committee, Long had held hearings on federal patent policy as early as 1959 (U.S. Congress 1980a:463-64).
45 Elsewhere, Brooks cited the testimony of Admiral Hyman Rickover that the bill, if enacted, would actually impede industrial innovation (H. Rpt. 96-1307, pt. II). Was Brooks right? Private R&D spending rose through the early 1980s but has stabilized and even declined of late. Some economists argue Brooks’ point, that there are long-term detrimental effects of emphasizing proprietary technology transfer rather than diffusion of public technologies that could indeed decrease private R&D spending. See, for example, Stewart (1987).
and he feared that applying for a protective rule would attract undo and damaging attention. The ploy succeeded in the House and an amended version passed the Senate, with the House finally accepting the Senate’s version by voice vote.

The Bayh-Dole legislation meant that the government had created a broad system of incentives to federally funded scientists to perform more productive science, where productivity was to be measured in patents and commercial licenses. By allowing universities, other non-profits, and small businesses to obtain title to inventions from sponsored research, and by requiring them to use a portion of any subsequent licensing revenue for inventor rewards, Bayh-Dole applied explicit incentives like those suggested by principal-agent theory. Whereas, the simple, social contract for science that existed previously was linked to the expectation of technological benefit; the new, more explicit arrangement was linked to some measure of actual performance. Bayh-Dole also provided that federal agencies, including federal laboratories such as the government-owned, government-operated (GOGO) laboratories like NIH, could grant exclusive licenses.

5. Executive activity--Packard report

This technology policy, to have federally funded R&D join the battle for industrial innovation and economic competitiveness by expanding the possibilities for technology transfer, continued to gain momentum into the Reagan Administration. In 1982, Congress passed and President Reagan happily signed the Small Business Innovation Development Act (P.L. 97-219). The act established the Small Business Innovation Research (SBIR) program, which required federal agencies with annual R&D budgets exceeding $100 million to set aside 1.25% of extramural funding for small businesses. The following year, Reagan signed a memorandum on

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46 See Arrow (1991) for a brief discussion of how, in the real world, contracts between professionals (agents) and clients (professionals) may not often take performance into account as well as they could.
48 After a series of running battles with the university lobby, SBIR passed 353-57 in the House and by voice vote in the Senate. Before passage, the set-aside had been reduced from 3% to the accepted
government patent policy, directing federal agencies to extend, within existing law, to all contractors the rights granted to non-profits and small businesses by Bayh-Dole to claim title to inventions made with federal funds.

Also in 1983, the Reagan Administration's Office of Science and Technology Policy (OSTP) issued a report conducted by its Federal Laboratory Review Panel. "[T]he Panel found that the Federal laboratories have several serious deficiencies, and consequently, a number of laboratories do not meet the quality and productivity standards that can be expected of them" (OSTP 1983:v). In addition to raising age-old questions of science administration such as multi-year and discretionary funding for laboratories and a separate science and technology personnel system within the civil service, the Panel also recommended that the federal laboratories be encouraged to interact more with industry and with universities, and be permitted greater freedom of action from agency heads in Washington. The Panel viewed the federal laboratories as occupying a niche intermediate between that of industry and academia, and although the role of the laboratories was generally to conduct basic and applied research, they could "[d]evelop commercial products only when that work has industry cooperation and is directly related to the laboratory's unique capabilities" (OSTP 1983:2; emphasis in the original).

Based in part on the findings of the Federal Laboratory Review Panel, the Trademark Clarification Act of 1984 (P.L. 98-620) amended the Bayh-Dole Act to

1.25%. Even after this reduction, there were attempts to exclude particular research programs from the set-asides. All exclusions were rejected, but amendments for excluding health-related research (offered by Henry Waxman, D-CA) and NIH research (offered by O'Brien, R-IL) were rejected more narrowly (164-193 and 169-228, respectively) than was the amendment excluding defense research (80-295). Former NIH director Fredrickson describes how the research agencies opposed SBIR, but that now they "realize what a magnificent program it is, and how fortunate that we were convinced by the Congress that this was desirable." Interview with Donald S. Fredrickson. Between 1982 and 1992, some $2.5 billion in competitive R&D funds were allocated by SBIR. The set-aside has now been raised to 2.5% and the definitional size of small businesses has been expanded. Also see Anuskiewcz (1992) for a review of some specifics of the program.

49 The warrant for such a separate system was the discrepancy between private sector and civil service salaries for technical personnel, particularly physician-scientists in the intramural laboratories of NIH. Such questions have been raised perennially, perhaps beginning with Steelman (1947).

50 This prescription largely corresponds to the cooperative research and development agreements (CRADAs), which were to be authorized in the Federal Technology Transfer Act of 1986.
extend technology transfer activities. The Act permitted the directors of government-owned, contractor-operated (GOCO) laboratories to make decisions about the awarding of licenses and permitted contractors to use patent royalties for R&D, for employee incentives, or for education. Seeking to turn the federal laboratories into engines of local economic development akin to Route 128 in Massachusetts and the Forrestal Center at Princeton University (S. Rpt. 98-662), the Act also allowed GOCOs run by universities and nonprofit organizations to retain title to inventions, as universities and small businesses had for their own research under Bayh-Dole. The Act also codified President Reagan's executive order allowing private companies, regardless of size, to obtain exclusive licenses of federal technologies. Together with Stevenson-Wydler and the original Bayh-Dole, the 1984 amendments enlisted federally funded researcher in all sectors into a more formal relationship with the federal government in which the performance of research productivity would in fact be measured and rewarded.

D. The Federal Technology Transfer Act of 1986

1. Origins of FTTA

In 1971, a small group of DOD laboratories joined together to form a consortium for technology transfer. In 1974, the group expanded to include laboratories from non-defense agencies and renamed itself the Federal Laboratory Consortium for Technology Transfer (FLC). The FLC does not transfer technology, but rather it provides a network among its member laboratories and develops methods for establishing relationships between laboratories and client groups (FLC n.d. [1986]). By 1984, FLC had attracted more than 300 of the federal laboratories from eleven agencies as members (U.S. Congress 1985c:5), although until then neither NIH

51 Although new research, e.g., Luger and Goldstein (1991) suggest that such centers of local development are very difficult to duplicate.
52 The Cooperative Research Act (P.L. 98-462) was another technology policy initiative in 1984. This act eliminated some antitrust implications for companies wishing to engage in joint, precompetitive R&D and established a number of public-private consortia, most prominently the Semiconductor Research Corporation (SEMATECH).
nor NASA laboratories had participated. In October 1984, FLC invited a staff member from the House Science, Space, and Technology Committee to its semiannual meeting in Seattle to discuss the implementation of Stevenson-Wydler. The problems articulated at the FLC meeting included the lack of career paths at the ORTAs created by Stevenson-Wydler, and the continuing inapplicability of technology transfer activities to personnel and advancement decisions for researchers, despite congressional intent. The staff member returned to Washington, transformed FLC's claims into a questionnaire, and surveyed 75 laboratories and organizations. The results of the survey confirmed the claims, and the staff member began putting the results of the survey into legislative form.

This effort led to H.R. 3773, the Federal Technology Transfer Act, which would amend Stevenson-Wydler to allow GOGO laboratories to enter into cooperative research and development agreements (CRADAs) with nonfederal organizations and mandate that federal employees receive a portion of the royalties from licenses of their patented inventions. The bill would also establish FLC as a federal agency. H.R. 3773 built upon the Uniform Patent Procedures Act (S. 2171), a bill introduced in the previous Congress by Robert Dole (R-KA) along with the amendments to the Bayh-Dole Act, which had first suggested the use of "cooperative research and development arrangements" for public-private collaborative research in the GOGO laboratories (see S. Rpt. 98-662:5).

Hearings on the revision of Stevenson-Wydler were held by the Senate Commerce, Science, and Transportation Committee and the House Science, Space, and

53 By the end of 1986, FLC had 328 member laboratories, including only three from the Department of Health and Human Services: the Centers for Disease Control (CDC) in Atlanta, the Medical Devices Laboratory in Bethesda, and the National Institute for Occupational Safety and Health in Cincinnati. By this time, there were also ten member laboratories from NASA. See FLC (n.d. [1986]).
54 Interview with senior staff member, House Science, Space and Technology Committee, Washington, DC, January 26, 1992.
55 A Department of Commerce report in 1984 had contrasted the success of some universities in spawning local development through research parks and the lack of ability of the national laboratories to cooperate with industry.
Technology Committee. Attention was limited to these two committees, and not extended to the judiciary and small business committees as before, because the science committees had established their jurisdiction with the original Stevenson-Wydler Act.\textsuperscript{56} The Senate hearings, before Slade Gorton (R-WA) and his Science, Technology, and Space Subcommittee, were held in Seattle, which is also one of the regional nodes of FLC. Gorton's concerns were the same as his colleagues five years earlier, the fate of industrial innovation in the United States. He cited a finding by the Presidential Commission on Industrial Competitiveness that U.S. industry had lost market shares in seven of ten high-technology sectors studied (U.S. Congress 1985b:1). Eugene E. Stark, Jr., the chairman of FLC, testified that technology transfer from the federal laboratories was operating at "less than 50 percent effectiveness" and that the permission to engage in technology transfer activities extended by Stevenson-Wydler had to be transformed into a mandate (U.S. Congress 1985b:3-4).\textsuperscript{57} The subcommittee also heard testimony from many user-groups which evaluated Stevenson-Wydler positively, but encouraged the subcommittee to go beyond it.

Hearings before Doug Walgren's (D-PA) Subcommittee on Science, Research and Technology of the House Science Committee were similar. Members, including Walgren and Stan Lundine (D-NY), lamented the expansion of the trade deficit into high technology areas, for example the $6.8 billion deficit in electronics in 1984 (U.S. Congress 1985c:4). Lundine also cited the Presidential Commission on Industrial Competitiveness, which recommended that federal R&D be managed with more concern for commercial applications. John P. McTague, deputy director of OSTP, continued hammering at the technology clause of the social contract for science as well, testifying that R&D funding alone was inadequate for industrial innovation, because the aggregate (public and private) U.S. R&D investment of $110 billion was larger than the

\textsuperscript{56} Interview with senior staff member, House Science, Space and Technology Committee, Washington, DC, January 26, 1992.

\textsuperscript{57} Although it was not made clear of how effectiveness was measured.
investments of Japan, West Germany, France and the United Kingdom combined. “[W]e must get multiple payoffs from these Federal programs,” McTague argued. For technology transfer from the federal laboratories to be successful, the law needed to “encourage the mingling...of cultures rather than create mechanistic structures and rigid formulas” (U.S. Congress 1985c:9). 58

2. FTTP and institutional conflict

Unlike the Senate, which had been in Republican hands since shortly after the passage of Stevenson-Wydler, the Democratic House took the opportunity of amending Stevenson-Wydler to attack the Reagan Administration’s implementation of the act. Walgren charged the Administration with failing to implement major provisions of Stevenson-Wydler, including the National Industrial Technology Board and the Centers for Industrial Technology, and he attacked the Administration’s proposals to abolish parts of the National Science Foundation and the Department of Commerce having mandates under the act. The Reagan Administration budgets had been kind to basic science, but damaging to applied research and development, particularly in the civilian sector. 59 Reagan Administration officials, like McTague who testified in the hearings, at times emphasized what seemed to be a return to the old model. 60

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58 The Senate’s report on the bill also cited the Packard report and a report of the National Governors’ Association in arguing that the U.S. “can no longer afford the luxury of isolating its government laboratories from university and industry laboratories” (S. Rpt. 99-283:2).


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60 Smith (1990) characterizes the science policy of the Reagan Administration as a return to the consensus that had been modestly disrupted by the push for relevance and the anti-war and environment movements of the early 1970s. Wilson (1983), however, considers the Reagan administration’s
described how basic research expenditures had grown more than 55 percent in the previous four years, and how budget cuts had shifted development work back to industry, where it was more appropriate and effective. McTague recapitulated Vannevar Bush’s paean to “[b]asic research, or the pursuit of frontier knowledge,” as the central government interest (U.S. Congress 1985c:9).61

Partisan attitudes and divided government played a significant role in the congressional perspectives and politics of FTFA. Although Administration documents such as the Packard Report and the Commission on Industrial Competitiveness seemed headed in the same direction as the Democratic House, there had been proposals emanating from the Office of Management and Budget (OMB) to begin privatizing the federal laboratories, including NIH. The Reagan Administration had a noted hostility to the Department of Commerce as a niche for suspected advocates of industrial policy, and saw FTFA and the establishment of FLC as incipient industrial policy. Some Republican members of Congress saw FTFA as a Democratic ploy to expand federal programs.62 In fact, Democratic staffers saw linking the laboratories to industrial interests as a way of preserving them against the forces of budgetary stringency. “The more relevant [the laboratories] are, the closer to industry, the more advocates they can have.”63 One example of this advocacy occurred with the National Bureau of Standards (NBS), the budget of which the Reagan Administration wanted to cut below a freeze level. The proposed cuts included a $3 million steel program. Staff contacted

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61 One House staffer viewed the Administration’s relative generosity toward basic research as the “rather perverse” policy child of Reagan’s first budget director, David Stockman, who could not be against science entirely so he supported basic research as an excuse not to fund applied research and development.
62 Interview with Robert C. Ketcham.
63 Interview with senior staff member, House Science, Space, and Technology Committee, Washington, DC, January 26, 1992. Indeed, at about this time it started to become clearer that the defense roles of many of the national laboratories would shrink in the future. The Department of Energy became particularly aggressive in 1986 and 1987, for example, as the Office of Health and Environmental Research at Los Alamos pushed for involvement in a human genome project (Cook-Deegan 1991; OTA 1988).
representatives from the steel industry, who in turn contacted members with steel interests in their districts and declared the NBS budget a key steel vote. Federal laboratories only exist in a small percentage of districts, but their potential industrial partners are more widespread and more adept at political action. Such manipulation of the institutional relations of science is a far cry from Vannevar Bush’s science “free from the influence of pressure groups.”

The House Democrats and Senate Republicans also argued for six months over the appropriate level of rewards for federal employees whose inventions are licensed. But Dole, the majority leader in the Senate and who had been active on the topic since 1980 and had introduced similar legislation the previous Congress, was the chief sponsor of the current Senate version. Bob Michel (R-IL), minority leader in the House, was interested in the bill, primarily because it would play well in Peoria, his home district where the Department of Agriculture had its Northern Regional Research Center (which is a member laboratory of FLC). In fact, Michel was instrumental to the success of the bill in the House: he handled the veto threat from the Reagan Administration, which was not circulated on time.

3. Instrumental use of scientists

The technology policy legislation of the 1980s shows a willingness to use scientists as instruments of policy. In Stevenson-Wydler and its wake, “scientists were the tools” to overhaul innovation, the engine of the economy. The general fact of this instrumental use was not counter to the conditions of the social contract for science, but the reduction of the broad agreement to a set of incentives and side payments for

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64 This situation may be exacerbated by the increasingly “interlocking” nature of the biomedical research community in academic and government labs and new biotechnology industries. Kenney (1986) writes about a new “university-industrial complex” in biomedicine and Krimskey (1992) describes “academic entrepreneurship,” cataloguing the industrial activity of a large fraction of biomedical researchers. A large number of new biotechnology firms have also sprung up in the Maryland suburbs around NIH.

65 Interview with senior staff member, House Science, Space, and Technology Committee, Washington, DC, January 26, 1992.
performance was. Under the social contract for science, the free interaction of scientists was supposed to deliver the scientific capital for technological innovation. Finding that the level of technological innovation based on this organization did not yield satisfactory economic performance, politicians decided to intervene in the reward system of scientists.

Extending the principle of payments for performance in a professional relationship that Bayh-Dole applied to federally funded inventors in universities (Arrow 1991), FTITA established royalty sharing for federally employed inventors at a minimum of 15 percent of royalty income (regardless of whether the inventor remains a federal employee). The payment is limited to $100,000 per year for all inventions, unless the President makes an exception in the form of a Presidential award. But members of Congress also realized that "royalty sharing alone, although effective, is an imperfect tool in promoting technology transfer. The process of turning an invention into a successful commercial product is complex, and involves the work of more than just the inventors" (H. Conf. Rpt. 99-953). The act therefore provided for cash awards to others intimately involved with the technology transfer process. FTITA also requires laboratories to "ensure that efforts to transfer technology are considered positively in laboratory job descriptions, employee promotion policies, and evaluation in the job performance of scientists and engineers" (100 Stat. 1790 §4(a)(3)).

In form, these incentives of the new technology policy correspond to the Niskanen's idea that to limit the problem of moral hazard in delegation, the principal should authorize the redistribution of residuals to agents who cut down on waste and inefficiency. In this case, research that does not lead to technological innovation, measured not merely by patents but by exclusively licensed patents, is effectively defined as Niskanenian waste. Federally funded researchers, in any sector of

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66 This is also in contrast to the instrumental, but rather passive, use of scientists by the state described by Mukerji (1989).
67 Although the Senate was direct about its intention not to set precedent for royalty sharing in the private sector (S.Rpt. 99-283:13).
performance, can profit from the redistribution of residuals in the form of royalty payments if they do not produce wasteful, i.e., unlicensed research.

Staff involved in this legislation had assumed that it would change the motivation of scientists somewhat because it manipulated the possible sources of funding for scientists. But they also believed there was very little concern about the possibility of influencing the reward system of science. Staff denied that any expectation that scientists would in fact transfer technology was built into Stevenson-Wydler and FTTA, but rather believed that the law gave scientists the permission and the encouragement (rather than the mandate) to do the bidding of Congress. In this view, technology transfer had become part of the mission of the agencies but not necessarily a duty of individual scientists, although they would be rewarded for engaging in the practice. Staff intended to protect scientists at the federal laboratories who wanted to conduct undirected research from people looking over their shoulders, while also giving the scientists who wanted the opportunity to follow their research through to commercialization the opportunity and a reward for doing so. The legislation provided “a way of thinking; it doesn’t mean it’s a way of doing.” The acts contain no sanctions against “wasteful” scientists, but they do reward productive ones.

Staff also deny, despite a belief that FTTA “is a tool,” that expectations for commercial relevance are being pushed higher by the act. Instead, the real motivation for relevance is the budgetary climate, and staff believe this factor is being perverted by the Office of Management and Budget, which had originated the idea to privatize NIH and has more recently triggered rumors that it would require research agencies to earn

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68 There was also some concern for the competitive nature of scientific employment. As noted above, the Packard report found a severe discrepancy between salaries in private sector research and in civil service laboratories. Rewards from FTTA could, at least in part, promise to make up for some of this lack. FTTA also reduced the distinction between academic researchers, who from Bayh-Dole could profit from inventions and who could collaborate with industry, and federally employed researchers. Thus, FTTA attempted also to solve the problem of “brain-drain” from the federal laboratories.

69 Interview with Robert C. Ketcham.
part of their keep with CRADAs. Such an unfortunate policy would, staff believe, begin to turn the federal labs into "job shops." Nevertheless, there was "some skewing [of research] that we wanted to happen." Given the relatively small size of rewards\(^{70}\) and, in some laboratories like NIH, the sympathy between the research and industrial agenda, the danger was thought to be small and any problems could be addressed post \textit{hoc} \(^{71}\).

The drafters of FT TA were somewhat concerned about conflicts of interest that could derive from relations between federal employees and private firms. After discussing the issue with agency representatives, the drafters decided that the agencies involved were too different for specific legislative language regarding conflicts. Instead, they added language to the act to have agencies issue their own regulations as needed. But the act "makes no changes in the conflict of interest laws affecting Federal employees or former Federal employees....Agencies have the flexibility under this section to establish standards for cooperative research agreements which prevent former employees from benefitting unjustly from their former employment" (S. Rpt. 99-283:10). The problem they recognized was that "the patent without the inventor is useless," so the congressional goal was to get the invention out and rely on the agencies to protect their own integrity.\(^{72}\) Nevertheless, the Congress recognized that "[t]his incentive approach is an innovation in the Federal Government which should be monitored. Accordingly, the new section 13 includes three provisions to insure that "royalty income over and above a laboratory's normal budget does not adversely affect the laboratory's primary mission" (S. Rpt. 99-283:12).

E. Conclusion

\(^{70}\) The total amount of revenue collected by the government for licenses in 1985 was $1.6 million (S. Rpt. 99-283:6).
\(^{71}\) Interview with senior staff member, House Science, Space, and Technology Committee, Washington, DC, January 26, 1992.
\(^{72}\) Interview with senior staff member, House Science, Space, and Technology Committee, Washington, DC, January 26, 1992.
The social contract for science postulates both the instrumental use of scientists by the political community, and the reverse relationship as well. These reciprocal demands existed in some sort of equilibrium, so long as the U.S. economy performed well. There was no sense disturbing it, whether a causal relationship existed or not, while the purported effects of scientific research on the economy were apparent. In a crude analogy, there was no sense questioning Ptolemaic astronomy as long as the planets were in more or less the right place. The prediction of the postwar paradigm of science policy was that the U.S. economy would be productive, innovative and dominant. When the economy began to lose its dominance through an attributed decline in innovation and productivity, the politicians began to question why the equilibrium remained undisturbed.

With a wide consensus, politicians crafted a set of incentives for members of the scientific community to contribute more directly to technological innovation and the economy. These incentives show that the old model of the relation of science, technology, and the economy that was implicit in the social contract for science, was being superseded. Politicians recognized that science does not simply flow automatically and cheaply from the laboratory to the market place. As the legislation says, the flow has to be encouraged; and as the bureaucrats would say, by "incentivizing" people to do it. "Incentivizing" meant treading into the reward system of science and introducing the possibility that some scientists could be induced to follow a system of commercial, rather than scientific, rewards of the kind anticipated by principal-agent theory. They would be paid for productive performance. The irony of this change is that, as people are decrying the changes in the social contract for science wrought by the commercialization of university and government research--and citing Vannevar Bush as the author of that contract--the changes actually revert to arrangements closer to Bush's original script.
The attempts at managing the moral hazard of scientific productivity as described here was done through legislation, rather than through the threat of legislation, as in the scientific integrity case. Nevertheless, its success is still dependent upon the details of implementation and boundary work within NIH. The following chapter examines those details, suggesting that the boundary work performed by the scientists and nonscientists engaged in technology transfer at NIH defines a boundary in some ways similar and in some ways different from the legislation.
VI. Technology transfer, two: technology transfer and the boundary between science and politics

A. Introduction

The tentative technology policy legislation of the 1980s established a set of incentives for scientists to promote the commercialization of their research. They were encouraged to go about “plying” their wares. This chapter describes how the intramural laboratories at the National Institutes of Health (NIH) implemented these incentives, which provide an alternative pathway to the reward system for scientists. In addition to this intrusion into the reward system, the implementation of technology transfer legislation has had at least three other effects. It has created an institutional context in which scientists and nonscientists collaborate to bring the fruits of research to the market. It has created within this institutional context a cadre of these nonscientists, technology transfer professionals, whose duty it is to conduct something akin to shuttle diplomacy between the domains of science and politics. And it has created a set of new indicators such as licenses, royalty income, and cooperative research and development agreements that could be used for the assessment of NIH intramural programs. Together these effects comprise the rejection of the clause of the social contract for science that presumed “automatic” technology transfer. Their workings also help bound the influence of Congress and of the Federal Technology Transfer Act (FTTA).

This chapter examines the implementation of FTTA at NIH with an emphasis on these effects. FTTA provides an institutional context for scientists and these new professional, nonscientists to perform boundary work—to apply the new incentives and to produce the new indicators. The first part of the chapter briefly addresses the development of technology transfer at NIH immediately before and after the Stevenson-Wydler Act of 1980. The second part of the chapter describes the impact of the new biotechnology industry on NIH technology transfer, and describes the implementation of FTTA that led to the creation of the Office of Technology Transfer (OTT). The third
part of the chapter examines the developing model of technology transfer that has been established through the activities of OTT and how the new model alters the boundary between science and politics. Part of this alteration, to be discussed in the concluding section, involves a variety of policy issues including conflicts of interest among intramural investigators, fair access by private firms to cooperative opportunities with intramural investigators, and the fair pricing of drugs and devices derived from cooperative research. These issues represent new aspects of “political” science that many in NIH fear will attract still greater political involvement in the future.

B. “Up to the fence”: the evaluation and transfer of medical technologies

1. “The translation gap”

The previous chapter noted how biomedical research received little attention in the preparation of the Stevenson-Wydler Act of 1980. To a great extent, the neglect was mutual; that is, NIH seemed to be operating rather independently of the legislation in the early 1980s. A 1982 technical memorandum on Technology Transfer at the National Institutes of Health prepared by the congressional Office of Technology Assessment (OTA) mentions neither Stevenson-Wydler nor the Bayh-Dole Act.1 Instead, OTA identified two primary foci of technology transfer at NIH: the consensus development conferences and other activities managed by the Office of the Medical Applications of Research (OMAR); and the clinical research programs at institutes such as the National Cancer Institute and the National Heart, Lung, and Blood Institute. Discussion of technology transfer to industry, as opposed to these examples of information dissemination to medical practitioners consumed a mere six paragraphs of the memorandum’s 116 pages.

1 Recall that Stevenson-Wydler had exhorted the laboratories to include concern for technology transfer. Bayh-Dole permitted the government-owned, government-operated (GOGO) labs to grant exclusive licenses, which legislated for the intramural program at NIH what had been practice with the extramural program.
The origin of this mutual neglect seems to be in the fact that NIH was, through these mechanisms identified by OTA, engaged in technology transfer activities of its own initiative, rather than waiting for explicit legislative prompting. Encouraged by informal contacts with important members of Congress and given the opportunity by a crisis in medical technology, NIH had established a system for the assessment and transfer of medical technologies several years prior to the advent of Stevenson-Wydler. Donald S. Fredrickson, former director of NIH (1975 to 1981) explains how the germs of the idea were planted:

[W]hen I first became director, even before I was sworn in, I was met by Ted Kennedy and Jake Javitz, who at that time were on this technology transfer kick, and that was 1975. And I thought about what they said. And I came back, and during those few months before I knew about what Asilomar was, I sat down and actually wrote a paper. I was going to write a series of papers to lay out NIH policy, and I never completed but one.²

Fredrickson entitled the proposed series of papers, "The purposes of the National Institutes of Health." Tapping an old but effective metaphor, he entitled the sole paper he drafted "On the translation gap" (Fredrickson 1975).³

In the paper, Fredrickson described his perception that the medical profession, for the second time in the twentieth century in America, was on trial. At the first trial during the Progressive Era, "[g]uilt was pronounced for proliferation of quackery and the defendant was remanded to the custody of biomedical science for reform. The probationer and trustee prospered in this new relationship" (Fredrickson 1975:1). The medical profession was going through its second trial in 1975. "It was alleged that the

² Interview with Donald S. Fredrickson, former director of NIH, Bethesda, MD, January 5, 1993. Kennedy (D-MA) was at the time chairman of the Health Subcommittee of the Senate Labor and Public Welfare Committee. He was also active in the technology assessment movement and on the board of governors of OTA. Javitz (R-NY) was the ranking minority member of the Senate Labor Committee and particularly involved in health care and aging issues. The congressional contact that Fredrickson describes is related to the idea that Congress has some element of control over its bureaucratic agents based on the confirmation process; Fredrickson indicates that this control seems based on information exchange and making congressional preferences clear.
³ The issue must have been important to Fredrickson, as he drafted the paper in the first month his directorship.
fruits of the revolution [in biomedicine] were being distributed unequally among the populace and with indifference to mounting strain upon the public purse” (Fredrickson 1975:1). Given the seriousness of the allegation, Fredrickson communicated his “own perceptions of where action is particularly needed includ[ing] (1) realistic assessment of the boundaries between biomedical research and health care, with particular attention to necessary extensions of the research continuum in the direction of clinical investigation without imperiling the research that must precede it” (Fredrickson 1975:2). The NIH director thus set out the problem of the translation gap in language similar to that adopted in this analysis--the boundary between the scientifically governed domain of biomedical research and the politically charged domain of health care.4

To address the translation gap, Fredrickson created a vocabulary that would be reified in the NIH consensus conferences and would also resonate with the later technology transfer legislation:

Our responsibility, however, is to understand more completely and to improve the somewhat informal system whereby consensus is reached concerning the validity of the interventions arising from our research. Are there better linkages between the institutions, centers, and other clusters of experts we subsidize and the editorial and collegial processes whereby discoveries are deemed proper for general application? How can we extend the continuum of biomedical research across perceivable [sic] gaps in translation (Fredrickson 1975:6)?

The translation gap was an area between the lab table and the examining table that could not be traversed easily. Fredrickson wanted NIH to consider formalizing what had been the informal or tacit processes that moved medical applications from one to the other. He anticipated accomplishing this formalization by reconfiguring the relationships among scientists to bridge the translation gap. As he recalls,

And that what we had to do was define, perhaps extend to a certain definite point, the boundaries of NIH’s interests and activities and

4 Fredrickson (1975:1) had made the logically prior point that the Federal government was in fact the largest purchaser of health care.
interaction with other government agencies that would make scientists participate in assuring the interested community of the meaning of the inventions and the discoveries, and in evaluating them from a scientific standpoint. But not to put value judgments on them. That was a very important thing at that time in my mind. We had to make the scientists come right up to the fence, but don’t get reaching over [into] health care.5

2. “A balanced fence?”: the consensus development program

Fredrickson’s opportunity to walk biomedical scientists up to the fence arose in 1976 when the director for cancer prevention walked into his office and complained that one of the National Cancer Institute (NCI) scientists had accused NCI of causing cancer rather than preventing it. The American Cancer Society was running a large mammography program under NCI contract, and this scientist claimed that the program was flawed because the radiation doses were too high. Fredrickson suspended the mammography program “until we’ve had an exercise here to attempt to find out what the hell it is we all know and don’t know. And that was the birth of the NIH consensus exercises.”6 The first Consensus Development Conference on breast cancer screening was held in September 1977.

Since that time, NIH has held more than 80 consensus conferences to assess and disseminate information about medical technologies.7 Reports of the conferences are published in the Journal of the American Medical Association as part of the effort to disseminate information about the technologies discussed to medical practitioners. The conferences are managed by OMAR, informally established in 1977 coincident with the first conference and formally established by the Department of Health, Education and Welfare (DHEW) in October 1978 (OTA 1982). OMAR’s other functions have included coordinating technology assessment and transfer activities among the

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5 Interview with Donald S. Fredrickson. The “fence” seems to be the fact-value distinction. Also see n. 9 below.
6 Interview with Donald S. Fredrickson.
7 A list of conferences may be found in IOM (1990).
institutes, divisions and centers of NIH and monitoring the effectiveness of such activities.\textsuperscript{8}

According to Fredrickson, the consensus development program "has succeeded, and it proved to allow me to make my point. Scientists would walk up to the fence, provided the fence is a balanced one."\textsuperscript{9} However, a balanced fence to scientists may be exclusionary or inappropriately separatist to others. Studying OMAR's consensus development program, sociologists Gerald Markle and Daryl Chubin find that the conferences inappropriately exclude direct discussions about economic, social and ethical questions related to new medical technologies--as well as experts in these questions. This exclusion occurs because the program "is designed around an inadequate model of science [that] assumes that a strict separation of factual from value issues is possible, and further, that objective evidence compels experts to converge on the 'correct' decision" (Markle and Chubin 1987:20) That is, the consensus development program is misguided not only because it does not reach over the fence, but also because it presupposes the existence of the fence in the first place. The program is in this view an institution of the social contract for science.

Likewise, the Institute of Medicine (IOM 1990) has recommended that the program should be expanded to include economic, social, and ethical issues. Even its creator, Donald Fredrickson, has said that he has "gotten to a point in [his] dotage where [he] went before the current group who run these programs and suggested that cost analysis had to be made a part" of consensus development. Fredrickson added, however, that one reason for the proposed inclusion of cost analysis is "because that's also become a new science."\textsuperscript{10} In other words, methods of economic assessment have managed to cross from the politics side of the fence to the science side.

\textsuperscript{8} The relationship between technology assessment and technology transfer is important, but inappropriate to discuss in detail here. See OTA (1982).
\textsuperscript{9} Interview with Donald S. Fredrickson. Also see Fredrickson (1978) for a description of the first consensus conference.
\textsuperscript{10} Interview with Donald S. Fredrickson.
As a result of Fredrickson's efforts and the crisis in mammography screening, OMAR, a somewhat centralized office dealing with medical applications and technology transfer, already existed when Stevenson-Wydler mandated that the large government laboratories should establish Offices of Research and Technology Applications (ORTAs). OMAR became the ORTA for NIH and remained at the focus of NIH technology transfer activities until 1986.

C. "Asking for trouble"?: toward commercial technology transfer

1. NIH and commercial biotechnology

After Congress passed the Federal Technology Transfer Act (FTTA) of 1986, which authorized cooperative research and development agreements (CRADAs) between intramural research laboratories and laboratories in private firms and universities, the focus of technology transfer at NIH began to shift away from OMAR and its emphasis on assessing and diffusing information to health care practice, and toward an emphasis on commercial technology. Clearly this shift was not entirely caused by the passage of FTTA. Between Stevenson-Wydler and its 1986 amendments, the biotechnology industry had blossomed almost overnight by the granting of a patent to Stanley Cohen of Stanford University and Herbert Boyer of the University of California, San Francisco, for recombinant DNA techniques.11 Indeed, a number of the persons at NIH responsible for technology transfer who spoke with me cited the importance of the new industry and its connection to the patent system, newly revised in 1980. According to Reid G. Adler, director of the Office of Technology Transfer at NIH, "The biotech industry is basically the product of the patent system."12

11 The "Cohen-Boyer patent" actually consisted of two patents, a process patent granted in December 1980 and a product patent not granted until after some dispute in 1984. Stanford University administers the patent, which it licenses in a nonexclusive but royalty-bearing fashion. For an overview of patenting and biotechnology issues, see Kenney (1986:app.2) or Areen, King, Goldberg, and Capron (1984:ch.1).

12 Interview with Reid G. Adler, director, Office of Technology Transfer, NIH, Rockville, MD, January 17, 1992. One OTT professional, however, took a broader but similar view of the artificiality of the biotechnology industry: "[the] biotech industry owes its lifeblood to the Bayh-Dole Act, the
Until FTTA, NIH had maintained a well-deserved and deeply entrenched reputation for standing aloof from commercial interests. Donald Fredrickson “started [at NIH in 1951] in a time when if you had a patent, you couldn’t even get the royalties here. We kept double-arms’ length from industry in every possible way.”\(^\text{13}\) The Cohen-Boyer patent was not universally praised, even and perhaps especially by biologists. Thomas Mays, the TDC for NCI and a Ph.D. geneticist recalls that when he was an active scientist, “some of us didn’t like it.”\(^\text{14}\) Fredrickson relates that early in the patent process, Stanford contacted him about the application and the perspective of NIH. “I knew that there were certain people who were violently opposed to patenting that idea, and so was I a little bit, but that’s not a bias you can carry into running an agency.” So Fredrickson tried to work through Federal Advisory Committee Act procedures in putting together opinions, and presented the case for patenting the process to DHEW Secretary Joseph Califano, who was initially “very hostile [to the idea], but he eventually came around.”\(^\text{15}\)

Even after the Cohen-Boyer patent, NIH had a “desultory” patent branch in its office of general counsel. NIH did maintain a longstanding Patent Board to deal with extramural inventions,\(^\text{16}\) but biomedical research was not “philosophically attuned” to patenting.\(^\text{17}\) As Fredrickson recalls, OMAR provided only modest assistance to


\(^\text{13}\) Interview with Donald Fredrickson. Another long-time NIH scientist recalls his first unwitting trip to a drug firm, where he learned more than he had been able to impart, and then learned that he could not accept the firm’s travel money and that he would have to take annual leave for the day he spent (Rall 1990). Several other interviewees with long years of NIH service had similar recollections of an extreme distance from industry.

\(^\text{14}\) Interview with Thomas D. Mays, Ph.D., J.D., director, Office of Technology Development, NCI, Bethesda, MD, January 7, 1993.

\(^\text{15}\) Interview with Donald S. Fredrickson.

\(^\text{16}\) The Patent Board administered the extramural Institutional Patent Agreements (IPAs), which NIH negotiated with grant-receiving institutions individual to permit them to patent NIH-funded research. IPAs were cited in the debate leading to the Bayh-Dole Act, which then obviated the need for them. See V.C.1.

\(^\text{17}\) Interview with Philip S. Chen, Jr., Associate Director for Intramural Affairs, NIH, Bethesda, MD, January 22, 1992.
potential licensers in familiarizing them with NIH-held patents. But licensing activities themselves were handled centrally for the government by the National Technical Information Service (NTIS), which was not particularly proactive. Before FTFA, "government labs were sleepy little places where patents were concerned." Before the 1980s progressed, there was "a significant increase in NIH interactions with industry, owing in large part to the rise in biotechnology industries fostered by NIH-supported discoveries" (Chen 1987). One example of this interaction is the major NIH licensing success of the pre-FTTA period: the license on the acquired immune deficiency syndrome (AIDS) test kit. The AIDS test kit is credited to NCI researcher Dr. Robert Gallo, based on research performed in his laboratory and in the laboratory of Dr. Luc Montagnier of the Pasteur Institute in France. Gallo's research was announced in April 1984 and the test kit for detecting presence of the virus was ready for testing nine months later (Panem 1988). The AIDS test kit has been so successful commercially, because of the unfortunate world-wide demand, that it has accounted for over $25 million of the $36 million generated by the licensing of all

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18 Interview with Donald S. Fredrickson.
19 Interview with Philip S. Chen, Jr.
20 Interview with senior professional, OTT, Rockville, MD, January 17, 1992.
21 The saga of the AIDS test kit continues to this day. Gallo and Montagnier had been embroiled in what was then seen merely as a priority dispute over the discovery of the virus that causes AIDS, named LAV by the Frenchman and HTLV-III by the American. But there were also some concerns over the possible misappropriation by Gallo of research materials borrowed from Montagnier. Since the validity of a patent is contingent in part upon a clear description of the discovery and identification of the discoverers, there was also a dispute over the patent and the expected millions of dollars in royalties. The issue was thought to have been settled by a treaty signed by President Ronald Reagan and Prime Minister Francois Mitterand agreeing on a joint claim for Gallo and Montagnier in discovering the virus and a royalty-sharing agreement in which the bulk of the revenue would be donated to an international AIDS foundation and the remainder split between the co-inventors' institutions. However, after an extensive investigation by Chicago Tribune reporter John Crewdson prompted a formal investigation by NIH into Gallo's conduct, the Office of Research Integrity (ORI) has recently issued a report finding Gallo guilty of making false statements in the published account of his discovery of HTLV-III. Although ORI's findings are subject to appeal, such false statements could be found to be crucial to the patent of the test kit, and the agreement may be threatened by the misconduct finding. For priority disputes in science, see Merton (1973:ch.18). For the Gallo-Montagnier feud, see, for example, John Crewdson, "Probe finds fraud in AIDS studies," Chicago Tribune (August 11, 1991), p. 1; and Philip J. Hilts, "Federal inquiry finds misconduct by a discoverer of the AIDS virus," The New York Times (December 31, 1992), p. 1.
22 More controversy swirled around the announcement of Gallo's discoveries by Secretary of Health and Human Services Margaret Heckler. See Panem (1988) and Shiitls (1987), among others.
federal patents since 1987.\textsuperscript{23} The robustness of the biotechnology industry is demonstrated by the bulk of the remaining license revenues, some $8.7 million generated by 115 inventions, which are earned from biotechnology inventions at NIH and four other biotechnology-intensive agencies (GAO 1992:34).\textsuperscript{24}

The rate at which NIH scientists report inventions seems to be linked to events in the biotechnology industry rather than to FTTA. The rate began to increase sharply in 1983 (approximately 0.040, no units given) and continued to increase until 1988 (about 0.180), after which it fell back to 1986 levels (about 0.110) (GAO 1992:24). GAO attributes this pattern to: 1) the rapid rise of biotechnology industries based on the 1980 Cohen-Boyer patent; and 2) the creation of new invention-reporting areas by the decisions of the Patent and Trademark Office in 1985 and 1987 to allow the patenting of plants and animals (GAO 1992:27).

In August 1985, NIH director James Wyngaarden made one internal accommodation to the changing commercial environment of biotechnology by lifting the ban on consulting by NIH employees.\textsuperscript{25} Previous to Wyngaarden's action, employees could have no financial relationships with outside firms. Since, consulting can be done on the scientists' own time (annual leave, evenings or weekends), and employees are limited to a maximum of $25,000 per year in consulting fees (with a maximum of $12,500 from a single company) and are prohibited from owning equity in companies for which they consult.\textsuperscript{26} Scientists cannot serve on boards of directors, but are

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\item \textsuperscript{23} This despite the contribution of 80\% of the total licensing revenue to the international foundation and sharing the remainder with the French.
\item \textsuperscript{24} The four other agencies are three PHS agencies, the Alcohol, Drug Abuse, and Mental Health Administration (ADAMHA), the Centers for Disease Control (CDC), the Food and Drug Administration (FDA), and the Agricultural Research Service (ARS).
\item \textsuperscript{25} Actually, the first change came in 1980, when NIH scientists were first permitted to provide lectures on material in the public domain to industry for an honorarium. This permission was overturned by the Ethics Reform Act of 1989, which prohibits government employees from receiving honoraria for speeches, appearances, or articles (Chen 1991; 1987). This act has recently been partially overturned by a court decision which found that restrictions on honoraria for activities unrelated to the jobs of federally employees violated their free speech.
\item \textsuperscript{26} During the period from August 1987 to August 1988, 124 NIH scientists (about 11\% of the 1100 full-time permanent staff) consulted with companies, earning a total of less than $700,000, or about
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permitted to serve on scientific advisory committees if not more than one third of the members of such committees were from NIH. This provision was meant to avoid the appearance of “NIH spin-offs with favored access to NIH intramural research” (Chen 1990:3). Employees were also prohibited from transferring any government information that was not already in the public domain (e.g., published or presented at meetings).27

The push to allow consulting was in large part due to the growing competition between intramural researchers and their extramural counterparts. As historian Charles Weiner writes, “Biology had been transformed [by the new patented techniques] and so have been the biologists. Starting very rapidly in the early 1980s, academic biologists never before involved with industry became consultants, advisors, founders, equity holders, and contractees of new biotechnology firms” (Weiner 1993 forthcoming:22; emphasis in the original).28 In addition to the new industrial contacts and the fact that academic salaries had overtaken government salaries by the end of the 1970s, the Bayh-Dole Act of 1980 augmented the academic advantage by allowing university researchers to profit from patents supported by federal research dollars. It was a difficult situation for NIH staff, regarded “as the moral equivalent of university faculty” (Chen 1990).

Intramural scientists also argued that consulting allowed them to participate better in the

$5,500 per scientist. NCI had the largest number of active scientists (60), earning an average of just over $7000 (NIH 1988:17).

27 A focus of one of Representative John Dingell’s (D-MI) hearings in 1990 was a chain of financial misdealings and conflicts of interest in the NCI lab of Robert Gallo. Apparently, Gallo had been made aware of possible conflicts as early as 1986, but failed to take effective action and the misdealings continued. The General Accounting Office (GAO) received allegations of the conflicts, began an investigation in December 1988, and found the allegations substantive in March 1989. Dingell’s oversight subcommittee joined forces with GAO in August 1989, and held a hearing in April 1990 on the issue (U.S. Congress 1990a). By the end of July 1990, the scientist under investigation, Syed Zaki Salahuddin, had been indicted by the U.S. Attorney in Baltimore, MD, for felony conflict of interest and illegal receipt of gratuity charges. Salahuddin pleaded guilty to two misdemeanors, paid a $12,000 fine, and became a tenured professor at the University of Southern California (Holden 1992). In a strikingly similar story, Prem Sarin, another scientist in Gallo’s lab and a witness at the April 1990 Dingell hearing, was convicted on three felony counts of false statements to the government and embezzlement. In both cases, Dingell was highly critical of internal NIH investigations (Hamilton 1991a; U.S. Congress 1991a).

broader scientific community as it expanded into the commercial sector (Chen 1990; 1988; Rall 1990). In effect, they argued that a ban on consulting intruded on the free association of scientists implicit in the republic of science.

One result of this policy was to publicize the commercial potential of NIH research (Chen 1988). Another was to usher in the possibilities of vexing conflicts of interest. By the early 1980s, the changes in biology were beginning to cause—and the changes in legislation were validating—relationships between academic researchers and private firms that raised serious questions about the commitments of academics and practices in their laboratories (Kenney 1986). Rather than hold the line against conflicts of interest in any form, NIH pushed the ethical envelope in order to keep up with the Jones’s in academia. NIH rules were, however, more strict than university rules regarding stock ownership or board membership, for example.

Some fourteen months later, however, the newly permissive environment with respect to consulting fostered difficulties when Congress passed FTTA and created the possibility that an intramural scientist consulting for a company could also be involved in cooperative research. NIH did promulgate a rule forbidding simultaneous consulting and CRADA activity, although there was controversy over whether a “cooling off” period should be interposed between the termination of a consulting arrangement and commencing a CRADA. The Office of General Counsel believed such a moratorium was necessary, but after intense discussion, director Wyngaarden decided that “such a cooling off period would place a severe impediment to the progress of science” (Chen 1991:3).29 “In retrospect,” Chen has said, “I believe our lives would have been somewhat simplified had the Federal Technology Transfer Act predated the liberalization of our outside work rules rather than the reverse” (Chen 1990).

29 The rationale is strikingly similar to the process of “trivializing” and “compartmentalizing” ethical issues in the history of biotechnology as described by Weiner (1993 forthcoming).
2. Implementing FITA

To implement the complex of authorities and incentives of FITA, director Wyngaarden assigned Philip S. Chen, Jr., associate director for intramural affairs, to craft a plan. Chen recommended that the extramural Patent Board be reconstituted as a new Patent Policy Board to develop policies and procedures for technology transfer at NIH and two other Public Health Service (PHS) agencies. Wyngaarden created the new Board on 9 April 1987 and designated Chen the chairman. Membership on the Board includes senior scientists and administrators from some of the categorical institutes and from other PHS agencies. The Board is organized into several subcommittees, two of which--the CRADA subcommittee and the royalty distribution subcommittee--address the major responsibilities of labs under FITA. A third subcommittee addresses issues of training for scientists. A fourth subcommittee is composed of Technology Development Coordinators (TDCs), a network of professionals created to represent the technology transfer efforts at the various institutes, centers, and division (ICDs).

In late 1987, NIH was planning to establish a Division of Invention Development within the Office for Intramural Affairs as an "administrative and organizational locus." The division was expected to "serve as the primary focus for implementation and information exchange on invention reporting, patent application, licensing, foreign patent filing, and training of NIH intramural scientists in technology management matters. It would also be the principal interface between interested companies and the NIH intramural research community" (Chen 1987). With a slight name change, the Office of Invention Development (OID) was created in 1988,

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30 The two agencies joining NIH in the Patent Policy Board are ADAMHA and CDC, the other PHS agencies with the largest research programs. FDA did not join NIH, ADAMHA, and CDC in these activities immediately because of its largely regulatory character.
32 Interview with Philip S. Chen.
replacing OMAR as the ORTA under the original Stevenson-Wydler (GAO 1989). Led by Chen, OID had two additional staff members (Chen 1988).

Initially, the Patent Policy Board, OID, and the TDCs constituted the entire organization for commercial technology transfer. According to one of the persons involved in these initial efforts, "there was too much to implement immediately." The first steps beyond the nascent organization consisted of developing NIH-wide training sessions and manuals for the intramural scientists and initiating the NIH/ADAMHA-Industry Collaboration Forum, an annual event to link intramural scientists with commercial interests first held in October 1988. Policy development at the time was reactive, responding to the many problems as they cropped up. The few people in the Intramural Office responsible for implementing FT TA also had to combat a general skepticism about the legislation from within the ranks at NIH. "Everybody had been pretty educated that you couldn't do" what FT TA was telling the scientists to do. More than once, the phrase "you're asking for trouble" rang out as implementation proceeded, because there was a palpable fear that FT TA would change science.  

One aspect of this cultural difficulty in implementing FT TA was the conflicting signals being sent by Congress about scientific conduct and conflicts of interest on the one hand, and increasing commercial ties on the other hand. Two hearings by Ted Weiss (D-NY), in September 1988 and June 1989, focused on conflicts of interest in scientific research and their possible relationship to scientific misconduct (U.S. Congress 1988c; U.S. Congress 1989c). In response, in December 1988, NIH held a retreat on "Conflicts of Interest in Collaborations with Industry," with participants

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33 Privileged interview, Bethesda, MD, January 8, 1993.
34 Privileged interview, Bethesda, MD, January 8, 1993.
35 Weiss expressed his fear that commercial ties would make universities too tumultuous for scientific pursuits. He quotes Tocqueville on one of the ill effects of the frenzy of democracy: "'In the midst of this universal tumult--this incessant conflict of jarring interests--this continual striving of men after fortune--where is that calm to be found which is necessary for the deeper combinations of the intellect?'" (U.S. Congress 1989c:2). For more on Tocqueville's view toward the conflict between democracy and science, see Guston (1993).
from NIH, industry, academia and other government agencies (NIH 1988). One outcome of the retreat was a policy statement on CRADAs and licensing that outlined the responsibilities of researchers and technology transfer professionals (Chen 1991). NIH held another conflict of interest meeting in June 1989 that focused on issues in extramural research (see Palca 1989) and an open meeting in November 1990 on Conflicts of Interest in Clinical Evaluation of Commercial Products (see NIH 1990).

3. The Office of Technology Transfer: "we make it happen!"

By the time of the December 1988 retreat on conflicts of interest, Reid G. Adler had been hired to direct OID. Adler, a former research biochemist at NIH, is also an attorney and has taught and published on biotechnology issues (see U.S. Congress 1991b). Over the next year, under Adler's leadership, the three-person OID was transformed into the PHS Office of Technology Transfer (OTT), which now serves NIH, ADAMHA, CDC and FDA. Adler had a vision of gathering under one roof all of the licensing, marketing, and patenting activities, instead of relying on NTIS for licensing and the Office of General Counsel's patent branch for patenting.37

OTT's current organization corresponds to Adler's original vision. The office is largely a collection of non-scientists whose job it is, to see that technology transfer occurs. The first professional whom Adler brought into OTT was Nina Siegler, a certified financial analyst with a Wall Street background and a specialty in biotechnology. Siegler had come to Washington, DC in 1987 to start her own firm, which had failed. She saw the opportunity at OTT as another chance to start up a company. With her Wall Street experience, Siegler took charge of the Technology Management Branch of OTT, responsible for matching NIH research with potential commercial collaborators or licensees--in other words, marketing. She also worked on integrating the Patent Branch, which had been on the NIH campus in Bethesda rather

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36 It was at this retreat that the differences between director Wyngaarden and the Office of General Counsel over the cooling-off period were aired.
37 Privileged interview, Bethesda, MD, January 8, 1993.
than downtown with the rest of the Office of General Counsel, into OTT.\textsuperscript{38} The Technology Management Branch is also the information services branch, and it began making contacts with outside databases, maintaining internal databases, and coordinating activities with the office of the director of NIH.\textsuperscript{39}

The second professional brought to OTT was Sandra L. Shotwell, who had been a licensing associate at Stanford University and as such was involved in licensing the second of the Cohen-Boyer patents (the product patent).\textsuperscript{40} Shotwell became the chief of the Technology Licensing Branch, and her major task was to gain administrative control over licensing for OTT from NTIS,\textsuperscript{41} which had not been performing well in the biotechnology licenses because, for example, it failed to understand the problems of licensing replicating organisms.\textsuperscript{42} As OTT grew, it drew on the resources of the TDCs and provided them with expertise.

By the end of 1992, OTT had expanded to include the three branches--Patent,\textsuperscript{43} Technology Licensing, and Technology Management--with twenty-three professionals and one administrative officer. OTT has assumed responsibility for negotiating licensing agreements for inventions made under CRADAs and advises NTIS on licensing other non-CRADA inventions. With the training subcommittee of the Patent Policy Board and the TDCs, OTT assists in educating scientists in their FTTA responsibilities and opportunities.\textsuperscript{44} OTT has created an computerized database called the “invention tracking system,” which maintains information on the status of PHS inventions, CRADAs, licenses, domestic and foreign patent applications, and other

\textsuperscript{38} Interview with Nina Siegler.
\textsuperscript{39} Interview with professional staff, OTT, Rockville, MD, January 17, 1992.
\textsuperscript{40} Interview with Sandra L. Shotwell, chief, Technology Licensing Branch, OTT, January 8, 1993.
\textsuperscript{41} Interactions with NTIS continue because, as OTT is now trying to develop complete expertise in-house in foreign patent filings, licensing, and marketing, it is actively taking cases back from NTIS. Other agencies are taking back cases as well, at least in part because the agencies are in a better position to deal with the scientists.
\textsuperscript{42} Interview with Nina Siegler.
\textsuperscript{43} The Patent Branch was formally transferred in May 1991.
\textsuperscript{44} Educational material includes a 167-page loose-leaf manual to instruct researchers in the organization and process of technology transfer. See PPB (1992).
information.\textsuperscript{45} OTT also hosts the annual PHS Technology Forum and an annual forum with the Pharmaceutical Manufacturers Association, which provide opportunities for scientists and industry representatives to exchange ideas and information about possible collaborations or technologies (see below), and OTT publishes an annual directory to PHS technology transfer resources (e.g., OTT 1992; 1991).

This organization provides the context in which the technology transfer professionals at OTT, their colleagues in each ICD (the TDCs), and NIH intramural scientists collaborate to move research from the laboratory to the commercial sector. Technology transfer does not happen by the automatic processes assumed by the social contract for science. Nor does it happen by a fluid connection of scientists, engineers, and industrialists with neatly defined tasks of research, development, and marketing. Implementing the incentives provided by Congress in FTTA, NIH created a institution to align the interests of its intramural scientists with those of Congress. It also created a set of professionals whose own task was to assure that these interests would be aligned on a day to day basis. How does technology transfer happen? “We make it happen!” says one senior OTT professional.\textsuperscript{46}

D. Technology transfer and boundary work

1. Introduction

This section discusses in detail how the professionals at OTT, the TDCs, and the scientists collaborate to make technology transfer happen. The details of this work are complex and governed by arcane features of federal regulations, legislated

\textsuperscript{45} Telephone interview with Mike Miller, public affairs specialist, OTT, Rockville, MD, January 27, 1993. During the last six months, OTT has conducted its own management review and survey of NIH and other PHS agencies, in part directed at determining what further information needed to be entered into the invention tracking system. One goal of the review was reconciling OTT records with those at NTIS, the Patent and Trademark Office, and the various institutes. Another goal was to make sure that internal controls, as specified by the Federal Managers Integrity Act (FMIA), were being followed (FMIA sets out procedures through which financial officers report to the President on the internal control of programs, e.g., costs, timetables, productivity). Miller says that the tracking system has been shared with the National Aeronautics and Space Administration (NASA) and with the Army.

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46 Interview with senior professional, OTT, Rockville, MD, January 17, 1992.
incentives, and scientific practice. These complexities create tensions or conflicts that can interfere with the performance of technology transfer. How the scientists and nonscientists resolve these tensions--how they perform their boundary work (see IV.B.1) while implementing FTTA--helps determine the extent to which Congress can control bureaucratic performance and scientific productivity.

Regardless of these tensions, there are indicators of a great increase in technology transfer activities since FTTA created the incentives for these activities. CRADAs currently managed by OTT numbered 48 in 1988, 89 in 1989, 110 in 1991, and 125 in 1992.\textsuperscript{47} From October 1986 to September 1988, NIH had received $3.9 million in royalty payments (GAO 1989:28).\textsuperscript{48} Income from NTIS-signed licensing agreements was $4.2 million in 1987; $5.7 million in 1988; $4.7 million in 1989; $5.6 million in 1990; $13 million in 1991;\textsuperscript{49} and $11.9 in 1992.\textsuperscript{50} Income from licensing agreements signed at OTT has risen from $10,000 in 1990 to $90,000 in 1991 and almost $700,000 in 1992.\textsuperscript{51} The average number of inventions licensed per year by NIH rose from 17.5 in the fiscal years 1981-1986 to 29.6 in 1987-1991 (GAO 1992:29).\textsuperscript{52}

\textsuperscript{47} Numbers from GAO (1989), GAO (1991a), OTT (1991) and OTT (1992x), respectively. GAO (1989) reports data as of February 1989 for NIH only, and excludes ADAMHA, CDC, and FDA, which apparently had no CRADAs at the time. GAO (1991a) reports data for fiscal year 1989 by department only, so the total includes an unknown number of CRADAs from the three other PHS agencies in addition to NIH. OTT (1991) was published in November 1991, but it gives no date for the measurement of CRADA numbers. OTT (1992) is current to July 27, 1992. Both of these reports include the other PHS agencies. Excluding them, NIH had 76 and 87 active CRADAs in 1991 and 1992, respectively. At any given time, a large number of CRADAs are under negotiation or review. For example, GAO (1989) reports that 87 CRADAs were in draft form of some sort for HHS, compared to the 89 active. Many of the smaller IDCs that had no active CRADAs as listed in OTT (1992) had one or more in the process when I contacted their TDCs in January 1993.

\textsuperscript{48} The royalties were primarily derived from the AIDS test kit, and all royalties were received from licenses taken before the 1986 Act that were handled by NTIS.

\textsuperscript{49} Includes a one-time AIDS test kit payment of $6.3 million, settling a law suit that covered additional license revenues from 1987 to 1990 (GAO 1992:34).

\textsuperscript{50} Interview with Sandra L. Shotwell. Again, most of this money is derived from the AIDS test kit.

\textsuperscript{51} Interview with Sandra L. Shotwell. Most of this income is derived from sign-up fees rather than from royalties, and all of it is derived from OTT-managed rather than NTIS-managed licenses. OTT signed its first license agreement in mid-1990.

\textsuperscript{52} This increase was larger than any agency but ARS, with an average increase of 14 compared to 12.1. The average value of 29.6 licenses per year is higher than any other agency; ARS is second with 18.2 and NASA third with 14.2 (although NASA declined over the period from 23.8). NIH share of average licensed inventions rose from 25.5% in the period prior to FTTA to 34.1% in the period after
Administration for biomedical research has never before kept such a detailed set of potential indicators as the number of CRADAs and royalty income. I mean here by "potential indicators" an index of activity that could be taken as a measure of something else, e.g., the success of FTTA or productivity or output of intramural research. The production of these potential indicators, however, requires the collaboration of scientists and nonscientists. One CRADA, as I will show below, requires a complex interaction among scientists, a TDC, and others at OTT and on the Patent Policy Board, to be added to the index of activity. In this interaction, these collaborators are also performing boundary work that defines in practice what constitutes science and what constituted non-science.

Below I examine the boundary work associated with FTTA. First, I discuss briefly the relationship between TDCs and scientists. Second, I discuss boundary work for inventing, patenting, and licensing. In the third section, I discuss how boundary work is performed around CRADAs. In the context of these discussions, I also introduce policy issues such as conflicts of interest, fair access to cooperative opportunities, and fair pricing of licensed products, that I will return to in the conclusion of the chapter. As suggested above, this boundary work has two consequences: it helps define the limits of congressional control over bureaucratic performance and scientific productivity; and it helps define the limits of science and non-science. These consequences are related to the extent that as tasks or products are defined as scientific, they are more easily defined as beyond congressional or political control; and as political control is defined over tasks, they are harder to define as scientific.

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(GAO 1992:29). NIH achieves this share with approximately 10% of federal intramural R&D obligations and approximately 1.4% of the total number of federally employed scientists and engineers (GAO 1992:19).
2. Technology development coordinators (TDCs)

There is one designated TDC for each of the 21 institutes, centers, and divisions (ICDs) in NIH. There is also one for each of the two ICDs in ADAMHA,\textsuperscript{53} as well as for FDA and CDC.\textsuperscript{54} Some ICDs have offices of technology development with a large number of professionals and support staff. NCI, for example, is the largest ICD and the most active in technology transfer as defined by FITA.\textsuperscript{55} The designated TDC, Thomas D. Mays, is a Ph.D. geneticist and an attorney, and he directs an office that has grown from three and one-half full-time equivalents (FTEs) to eighteen currently. NCI has 25 active CRADAs (PHS 1992) and about 450 patent applications, some one-quarter to one-third of the NIH total.\textsuperscript{56} Maryann Guerra, the TDC for the National Institute of Allergy and Infectious Diseases (NIAID--in which AIDS spending is focused), has an administrative background.\textsuperscript{57} She began working on implementing FITA when the scientific director of NIAID sat on the original Patent Policy Board. Guerra does not commit all her time to technology transfer duties, but she directs a five-person staff (two Ph.D.s, two assistants, and one support person) who manage 21 CRADAs, 125 active patent applications, and 65 licenses.\textsuperscript{58}

On the other end of the spectrum are ICDs having only one person who spends as little as 5-10\% of his or her time on technology transfer issues.\textsuperscript{59} Some of these part-time TDCs have other administrative positions; some perform research. Thomas

\textsuperscript{53} ADAMHA's IDCs are the National Institute of Mental Health (NIMH) and the National Institute of Drug Abuse (NIDA).
\textsuperscript{54} I interviewed 14 of the 23 TDCs at NIH/ADAMHA. These 14 manage 74 of the 93 CRADAs active in NIH/ADAMHA as of July 1992. Of the 9 not interviewed, 7 represented ICDs that have no active CRADAs. One of the remaining ICDs has a number of CRADAs, but the groundrules for interviewing could not be settled before this chapter was prepared.
\textsuperscript{55} NCI's FY1993 budget authority as approved by Congress is just under $2 billion (AAAS 1993:62). Intramural funding is about 20\% of this total.
\textsuperscript{56} Interview with Thomas D. Mays.
\textsuperscript{57} NIAID's FY1993 budget authority as approved by Congress is just under $1 billion (AAAS 1993:62). Intramural funding is about 20\% of this total.
\textsuperscript{58} Interview with Maryann Guerra.
\textsuperscript{59} The difference among the ICDs was most striking to me as a researcher. Although I was aware of the vast difference in scale among the intramural programs, even the technology transfer efforts range from 10\% FTE to soon to be 30 FTEs, or more than two orders of magnitude. Expertise did not seem similarly distributed.
E. Ingalls of the National Center for Research Resources (NCRR) has no appropriated funds for intramural work, because NCRR is funded largely on a fee-for-service basis by the other ICDs. If NCRR researchers spend too much time on collaborative work, its client ICDs may complain that the Center is not spending enough time on service. Nevertheless, NCRR has had 4 CRADAs over the years, none of which are currently active, and about 100 patents. Steven M. Galen of the Clinical Center (CC) is similarly situated. Less than 3% of the CC’s budget is spent on research. Galen has managed a few CRADAs, but none are currently active. Like NCRR, service is more important than technology transfer; CC needs to keep its approximately 500 hospital beds full. The in-house nature of the Center’s clinical programs makes Galen think that emphasizing commercial technology there “would be like Bell Labs’ going competitive.”

Regardless of the size or the role of the ICD, however, the TDCs were virtually unanimous in saying that they could do more for technology transfer if only they had the time to allocate to it. Mays will be adding four more staff to his technology transfer enterprise at NCI. Galen says that the Clinical Center will soon be hiring a full-time TDC, and that with aggressive work even a couple of people focusing on technology transfer could recoup ten times their salary in private contributions to research. To the extent that part-time TDCs are diverted by conflicting duties, FTTA is underimplemented.

One of the areas into which most of the TDCs would expand their efforts is education and outreach to the scientists. Whereas, in some of the smaller institutes education is of little importance because, as Galen says, he has known personally the half-dozen potential collaborators at the CC for twenty years; the medium-sized and

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60 Interview with Thomas E. Ingalls, TDC and program analyst, NCRR, Bethesda, MD, January 6, 1993.
61 Interview with Steven M. Galen, TDC and deputy executive officer, CC, Bethesda, MD, January 4, 1993.
62 Interview with Thomas D. Mays.
63 Interview with Steven M. Galen.
large ICDs have active educational programs for their scientists and feel that they could do more. Stephen Ficca of the National Heart, Lung and Blood Institute (NHLBI), for one, does not do “as much [education] as we would like to do.” Most of the TDCs have met at least once with the laboratory and branch chiefs in their institute, and some continue to do so on a regular basis. Some TDCs send newsletters or bulletins out to their scientists and encourage them to attend training sessions that OTT provides. One TDC even announces the receipt of royalty checks from licensed technologies. NCI and NIAID have held training sessions to supplement what OTT provides:

> We’ve had seminars, where we’d teach them. We’ve brought in patent attorneys to tell them what is a patent, so you know when you’re working at the lab, when you’ve discovered something, and when you should be aware to file a patent application. We’ve had seminars on CRADAs: when do you get into a CRADA? why do you get in a CRADA? So we try to help them along the way and advise them, you know, so that they know when they should be involved in tech transfer and encourage them to do so.

Nevertheless, the TDCs seem to feel that training sessions do not help scientists learn the ropes of technology transfer because the sessions would compete with other “scientific seminars” offered by NIH and would be seen as administrative in nature. Mays’ office at NCI has conducted an informal telephone survey that suggested the scientists wanted very practical training. Mays thinks that the TDCs “can’t turn everybody into lawyers” and that it is “illusory” to think that a formal course would help the scientists perform better.

Education and outreach is also a sensitive question because attitudes about commercial technology transfer still vary widely among the scientists:

> At the NIH there’s two philosophies about this, essentially. I tell you, there’s more than two, but there’s two extremes: one is the people

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64 Interview with Stephen Ficca, TDC and executive officer, NHLBI, Bethesda, MD, January 4, 1993.
65 Interview with TDC at a small ICD, Bethesda, MD, January 4, 1993.
66 Interview with Maryann Guerra.
67 Interview with TDC at a medium-sized ICD, Bethesda, MD, January 8, 1993.
68 Interview with Thomas D. Mays.
[who] are overly enthusiastic about this, and they push CRADAs like it’s a religion; and you have the opposite point of view, that sees this technology transfer act basically as a great demon that has descended upon the field of fundamental research and will destroy basic research because it will motivate people to do things for money instead of for the science. And so people feel that way are very much opposed to CRADAs.69

Reid Adler, director of OTT, sees a normal distribution of attitudes on the part of scientists toward FT TA: some are entrepreneurs; others think it is an abomination; but most could take it or leave it.70 A few of the interview subjects postulated that the variance in response to FT TA could be explained generationally. The older scientists, who had been socialized at a time when biology and commerce were more distant, reacted hostilely to FT TA. The younger scientists, who matured with the tools of molecular biology and the influence of the biotechnology industry, were both more accepting and more interested. Several of the interview subjects suggested that in their ambivalent response to FT TA, intramural scientists were no different from their university colleagues who responded to the Bayh-Dole Act with a similar combination of hostility and opportunism. For these reasons, among others, TDCs “don’t say, ‘go out and find an industrial partner!’”71 In this sense, TDC activities seem to match congressional expectations of providing a way of thinking rather than mandating a way of doing (see V.D.3).

3. Inventions

a. inventions and paperwork

One of the areas in which scientists need to be educated is the process of reporting an invention, filing a patent, and licensing the technology to a private firm. Although scientists may believe that knowing when to report an invention and file a patent application is part of professional judgment,72 it is not the case that scientists

69 Interview with Stephen Fieca. These caricatures are strikingly similar to the two sides in the initial postwar debate over the federal role in research, as described by Parsons (1946) or Price (1956).
70 Interview with Reid G. Adler.
71 Interview with TDC at a medium-sized ICD, Bethesda, MD, January 8, 1993.
72 Interview with a Senior Researcher at a small ICD, Bethesda, MD, January 7, 1993.
know when they have discovered something.\textsuperscript{73} As Guerra said in the passage above, “We’ve brought in patent attorneys to tell them what is a patent, so you know when you’re working at the lab, when you’ve discovered something, and when you should be aware to file a patent application” (emphasis added).\textsuperscript{74} This need to know when one has discovered something is particularly acute among foreign scientists who have been exposed only to patent arrangements in their home countries, most of whose patent systems differ from that of the U.S. in that the U.S. system is based on filing date rather than discovery or reduction to practice.\textsuperscript{75} TDCs remind scientists that they should not publish their work without first filing an invention report, because prior disclosure can invalidate a patent application.\textsuperscript{76} Scientists are also encouraged to “use bound notebooks which are dated and witnessed (read and understood) on a daily (or at least weekly) basis by someone such as your Laboratory Chief or another qualified scientist or technician” in order to protect their “claim regarding conception of the invention” (PPB 1992:16).

Staff members in Mays’ office at NCI often talk with scientists who have had ideas, in order to inform them about inventorship and filing patent applications. Scientists know that they have a responsibility to report inventions, but their proper execution of this responsibility depends on their experience. Their experience is in turn derived from previous activities, or perhaps even from questions that OTT people or TDCs will raise at scientific presentations (e.g., “have you filed on that?”). To drive the point home, Mays admits that patenting is not transparent, even for lawyers. “Tech

\textsuperscript{73} See, for example, Edge and Mulkay’s (1976) discussion about the problematic discovery of the pulsar.

\textsuperscript{74} I followed this up briefly after the interview ended when Guerra had no more time. She emphasized that scientists do need to be taught, in the technology transfer context, what a discovery is.

\textsuperscript{75} Interview with TDC at a medium-sized ICD, Bethesda, MD, January 8, 1993. Foreign scientists are permitted to share inventorship if they are at NIH under a federal appointment; otherwise, they sign away their rights when they accept other status at NIH. Foreign scientists constitute about roughly one-fourth of all researchers at NIH.

\textsuperscript{76} OTT does not, however, request that scientists refrain from publishing in order to secure a patent.
transfer is only accomplished by the use of certain agreements....[Invention disclosure is] highly interactive."\(^{77}\)

In working together--collaborating--with scientists, TDCs generally feel that their task is primarily administrative, to facilitate the paperwork that needs to be done, and to alleviate the burden on the scientist. Guerra says,

If you want to be successful in tech transfer in the government--scientists come to the government, and we have great scientists here, because they don’t want the hassles of administration and writing grants, doing that. They want to be able to be in their labs. And they give up other things to do that. They give up money, space, you know, a lot of things you might have if you worked outside of the government. And so we try to do what we can to make their lives as good as we can in the lab.\(^{78}\)

Or, as another TDC describes it, “It’s customer service.” The TDC steps in so the scientist can get away from the paperwork and back to the science.\(^{79}\) One TDC who is also a scientist understands that technology transfer does not happen without the talking and the paperwork, but he thinks that his being a scientist as well has helped him deal with scientists and try harder to make the process work for them.\(^{80}\)

TDCs are not, however, able to relieve scientists of the entire burden. Scientists still have to work with the TDCs and often with patent attorneys and licensing specialists from OTT. Scientists recognize patenting as time-consuming and not particularly rewarding, especially when the alternative is pushing ahead in their research and advancing their careers. But for those scientists who are interested in cooperating with private firms, patents increase the chances of attracting research partners because they can offer proprietary protection from the outset.\(^{81}\) Without the assistance of TDCs, who in effect legalize and formalize what the scientists could only

\(^{77}\) Interview with Thomas D. Mays.
\(^{78}\) Interview with Maryann Guerra.
\(^{79}\) Interview with TDC at a small ICD, Bethesda, MD, January 4, 1993.
\(^{80}\) Interview with TDC at a small ICD, Bethesda, MD, January 8, 1993.
\(^{81}\) Interview with Acting Section Head at a large ICD, Bethesda, MD, January 8, 1993.
attempt do if they were trusted not to cheat, the scientists would not have the wherewithall to do the patenting themselves.\textsuperscript{82} And although scientists do not want the frustration of revising patent applications and keeping exacting notebooks to detract from research time, they realize that their colleagues in industry share these distracting responsibilities. Scientists are also beginning to realize that patents are about protection as well as exclusion.\textsuperscript{83} That is, their proprietary interests over their research directions and materials can be protected legally, thereby assisting their scientific claims to priority and their access to professional rewards. The incentive provided by patents therefore works on a variety of levels, but it includes assisting scientists in their pursuit of rewards within the credibility cycle by giving them proprietary resources to attract research partners and proprietary protection of their scientific claims.

b. assessing and evaluating technologies

After the scientists and the TDCs collaborate on the invention disclosure and the patent application, the process shifts focus to OTT. But recently, OTT has felt pressure to evaluate or assess potential patents before actually applying for them. Despite its rapidly growing and expert staff, OTT can process only so many patent applications; and filing fees on patent applications are not trivial. OTT has taken two approaches to navigate around these constraints: first, the Office attempts to delegate assessment and evaluation back to the ICDs; and second, it has invented a process to bring the patenting and licensing decisions together at OTT, so that an invention with little chance at being licensed profitably will not be patented at government expense.

The larger ICDs can handle the burden of evaluation and assessment, but the smaller ICDs do have problems with this responsibility.\textsuperscript{84} NIAID evaluates and assesses inventions through an internal technical evaluation advisory committee:

\textsuperscript{82} Interview with Richard J. Youle, section chief, National Institute for Neurological Disease and Stroke, Bethesda, MD, January 6, 1993.
\textsuperscript{83} Interview with TDC at a medium-sized ICD, Bethesda, MD, January 8, 1993.
\textsuperscript{84} Interview with TDC at a small ICD, Bethesda, MD, January 4, 1993.
It’s an internal committee made up of four extramural scientists, and four intramural scientists, [and] the deputy director of the institute. It’s headed up by Jane Biddle, and we have...one representative from OTT in licensing and one representative from another institute....And we established that [committee to] have a...body of scientists at NIAID that started to understand tech transfer. Because they are involved in this once a month review, they now become pretty familiar with inventions. And we hoped that it would serve two purposes. One is that it would help us evaluate inventions to figure out when are they good, when do they have potential, when should we invest federal funds for this. Because it’s expensive now...so you don’t want to be filing patents on things that are just going to sit on a shelf. And [two] to teach some scientists about tech transfer, so that if they go back in their lab, they are familiar with it, they understand it, they can maybe do some positive discussions with their peers. And we have one scientist who absolutely hates any kind of committee work, absolutely detests it. [He] called me up and asked me to be removed from one committee and to be put on this committee because he thought it was a worthwhile committee, one of the few worthwhile committees in the government. So that was when I felt we really reached a peak--that somebody actually asked to be on a committee. A scientist asking to be on a committee!\textsuperscript{85}

At OTT, the Patent Branch is active from the initial receipt of invention disclosure forms through the evaluation of both domestic and foreign applications. The sheer volume of business is a problem, however.\textsuperscript{86} One scientist suggested that he could come up with more patentable ideas than OTT could handle.\textsuperscript{87} Sometimes the Patent Branch will receive an invention disclosure from a TDC shortly before publication. In such cases, it is OTT policy not to request that scientists delay publication, but instead the “office goes into warp drive” to finish the application.\textsuperscript{88} This policy helps OTT keep on the good side of the scientists involved,\textsuperscript{89} but it also suggests the tensions inherent in technology transfer between the scientists’ priority of publication and the OTT priority of patent application.

During its first year, OTT introduced the Technology Management Team (TMT) to facilitate the collaboration of the principals involved in technology transfer and

\textsuperscript{85} Interview with Maryann Guerra.
\textsuperscript{86} Interview with senior professional, OTT, Rockville, MD, January 17, 1992.
\textsuperscript{87} Interview with a Senior Researcher at a small ICD, Bethesda, MD, January 7, 1993.
\textsuperscript{88} Interview with Reid G. Adler.
\textsuperscript{89} Interview with professional, OTT, Rockville, MD, January 17, 1992.
evaluate technologies. Each TMT consisted of the inventor, the TDC, an OTT patent professional, an OTT marketing professional, an NTIS licensing professional, and other relevant program personnel. Together, they would devise a strategy of patent prosecution and licensing. The TMT also served an educative function, communicating the details of the process to the scientists and bringing some of them who had previously had bad experiences with technology transfer back into the fold. The scientists who participated would, if treated with respect, be willing to give time from their research to speak with the patent attorneys and other administrators. Originally, OTT established a TMT for each new invention. This coverage was necessary because the TMT was a “problem-solving routine” and, early on, patent applications were of generally low quality and each new invention would “open a new can of worms.” But as OTT began receiving twenty or thirty new inventions each month, and as the experience level of staff and scientists rose, OTT began to use TMTs only when a problem arose, because it lacked staff time to prepare and schedule meetings.90

Currently pressured by the costs of patenting, OTT has reinstituted something like the TMTs. In the past year, it has initiated the “cross-function team concept,” where patenting and licensing personnel91 work together to make a “business decision” about which patents to pursue. The cross-function team assesses the technology for a potential market in order to set technology transfer priorities. If the cross-function team gives a technology a low priority score, the inventor can revise the invention report or, if OTT rejects the technology, the inventor can seek title on his or her own.92 One datum in the team’s decision is a response by the inventor to a question on the invention report about the potential market of the invention. The question poses some difficulty for scientists, however, because not only do scientists lack expertise in estimating a

90 Interview with Nina M. Siegler.
91 The functions are crossed in another sense. Patent professionals at OTT maintain portfolios in a particular technological area; licensing professionals maintain portfolios in a particular market segment. This division is an attempt to create a more effective “sales force.” Interview with Nina Siegler.
92 Interview with Sandra L. Shotwell.
potential market, but they also suffer from a potential conflict of interest in responding to the question: if the market is deemed insufficient, OTT may reject the invention and allow the rights to devolve to the inventor. In such cases, the government forgoes all potential revenues should the inventor succeed in marketing the invention, but the inventor assumes all the burdens of patenting and licensing. But generally lacking the time, resources, and expertise to assume such burdens, such inventions may often fall by the wayside. Tensions resulting from the expense of patenting, the time-consuming nature of the technology transfer process, and the success of NIH intramural inventors in burdening OTT with inventions, forces choices these choices by cross-function teams about invention development based on commercial potential.

c. licensing, royalties, and motivations

Once an invention has been patented, OTT attempts to license it. Licensing is streamlined if the invention is derived from research performed under a CRADA, because the private partner has usually already been granted an option on an exclusive license. In such cases, the licensing process moves directly into negotiations over terms of the license.\footnote{Interview with professional, OTT, Rockville, MD, January 17, 1992. This streamlining is part of the incentive for the private research partner who, before FTTA, would have had to wait out a formal process involving public notice in the \textit{Federal Register} in order to get an exclusive license on a federally owned patent through NTIS.} OTT uses several techniques to find licensees for inventions not developed in CRADAs. One technique is OTT's version of a scientific meeting, with a marketing twist: the PHS Technology Transfer Forum and the NIH/PMA Technology Transfer Conference.\footnote{PMA is the Pharmaceutical Manufacturers Association, a trade group.} OTT invites potential licensees to these fora, at which scientists who have available technologies or who would like to enter into CRADAs present their research in poster format. Another technique is to secure listings for available NIH technology in private databases and trade journals. In the past year, these listings have helped OTT bring in more licenses.\footnote{Interview with Sandra L. Shotwell.}
The third technique is for OTT licensing specialists to do legwork. On one side, they maintain contact with the scientists who perform research in their portfolio area, e.g., cancer or AIDS. The specialist may meet with laboratory chiefs to review their technologies and inquire if any companies have contacted the scientists directly, rather than having gone through OTT. The specialist may also conduct walk-throughs of laboratories to identify at an early stage potentially valuable technologies. On the other side, licensing specialists attempt to be proactive in marketing inventions. They identify potential market segments, write letters to potential licensees, and make follow-up phone calls. Once a potential licensee is identified, the party files a license application and the licensing specialist evaluates the application, bearing statutes in mind.6 If there are several interested parties, the applications are evaluated on a competitive basis. After a licensee is chosen, further negotiations with OTT determine the terms of the license. When terms are agreed to, the license is sent through the appropriate signature pathway at NIH. The licensing specialist will follow the progress of the executed agreement, to determine whether the licensee has attempted to bring the technology to market in a diligent manner and at a fair price. If the licensee does not comply, the government can modify or terminate the license agreement.7

One standard part of exclusive licensing agreements is a fair pricing clause, which asserts that "PHS may require LICENSEE to submit documentation in confidence showing a reasonable relationship between the pricing of a Licensed Product, the public investment in that product and the health and safety needs of the public" (OTT 1992:366).8 The clause has been very controversial, but it is important when one considers that the government pays for research on therapeutics and diagnostics, and then pays through Medicare and Medicaid for them to be used. The issue of the fair pricing of government-developed drugs achieved notoriety with the

6 There is special consideration given to small businesses and domestic producers.
7 Interview with professional, OTT, Rockville, MD, January 17, 1992.
8 But the agreement does not "restrict the right of LICENSEE...to obtain a reasonable profit" (OTT 1992:366).
ruckus over the cost of AZT, one of the two drugs approved by FDA to treat persons with HIV infection. NIH also had an opportunity to discuss fair pricing with Bristol-Myers Squibb Company regarding the other drug approved for AIDS treatment, ddl. NIH may have lost an opportunity for a CRADA or license here and there because of the clause. But to date, the “clause causes more consternation” than outright conflict; it could become a bigger issue in the future, however, particularly when gene therapy techniques advance to the point where one dose is a sufficient treatment:

But you know...let’s suppose that gene therapy works. And let’s suppose the delivery system gets more sophisticated, so you’ll be able to use a pre-packaged syringe to inject cells, let’s say reproducing cells like stem cells. Well, holy hell! One injection, boom! What do you have? Cystic fibrosis? Okay. Wshh! That fixes that problem. Next patient! How do you price out what that therapy costs? Do you say what it costs, or what it’s worth? If you do what it’s worth, it could be very expensive. If you do what it costs, it could be very inexpensive. So, to me, the pricing thing could become a very big issue in the near future.

In addition to the problem of hesitancy on the part of potential licensees and private research partners, the fair pricing clause is a problem because NIH has no statutory authority or experience in determining what a fair price is. Nevertheless, NIH has been criticized for doing too little, yet it is the only agency with a fair pricing clause.

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99 The pharmaceutical company Burroughs-Wellcome received the exclusive rights to market AZT as an AIDS treatment, despite the role that NIH scientists had in determining its effectiveness. Public Citizen brought suit against Burroughs-Wellcome, and also named the government a co-defendant. The government took the side of Public Citizen. The government was then dropped from the suit and agreed to license its rights to a generic drug maker, Barr Laboratories, Inc., which challenged Burroughs-Wellcome’s patent. The U.S. District Court in Washington, DC, dismissed Public Citizen’s suit, but Burroughs-Wellcome v. Barr goes to trial this year. See Wolfman (1993) as well as Ackiron (1991). Several of the interview subjects who discussed the fair pricing clause mentioned AZT. One of the smaller ICDs also had a fair pricing controversy with a drug being clinically tested for an AIDS-related opportunistic infection. Interview with TDC at a small ICD, Bethesda, MD, January 8, 1993.

100 Interview with Sandra L. Shotwell.

101 Gene therapy is a field pioneered by Dr. French Anderson, formerly of NHLB institute, who was among the very first intramural researchers to take advantage of CRADAs and exclusive licensing under FTTA. In gene therapy, patients who lack particular proteins or enzymes in their systems are treated with cells that have had the gene for producing that protein inserted into their own genetic material. When these cells survive and reproduce in the patient, they produce the missing protein.

102 Interview with Stephen Ficca. Ficca’s point has a sharper edge with the Clinton Administration’s recent forays into childhood vaccinations.

103 Interview with Thomas D. Mays.
Addressing the fair pricing issue, particularly within the context of the new Administration's concern about health care costs, has been a subject of ongoing concern and review by an NIH Advisory Committee.\textsuperscript{104}

Once an invention is licensed, the licensee usually pays a fee up front and makes royalty payments after it has been marketed. Intramural scientists are entitled to a share of the income from licenses, according to FTTA at least 15% of the income (with a $100,000 annual cap) before the remainder is transferred to laboratory or agency research, education, or technology transfer programs, or cash rewards for noninventors. The royalty distribution at NIH is more generous than the minimum, providing 25% of the first $50,000 of income, 20% of the next $50,000, and 15% of income over $100,000 (GAO 1992). Despite the fact that NIH has the highest average annual income from inventions of all federal agencies (GAO 1992:44), it also has the lowest percentage of total invention income shared with inventors--6.8% compared to 15% at ARS, 24.6% at the Navy, and 71.6 at NASA (GAO 1992:47).\textsuperscript{105} Of the invention income not distributed to inventors (slightly more than $11 million at NIH), NIH used 48% (or $5.3 million) to defray technology transfer expenses, spent 31% (or $3.4 million) on research and education activities and employee awards, and had not distributed the remainder (GAO 1992:53).

Both the royalty payments to inventors and the percentage spent on research and education activities and employee awards were intended by the authors of FTTA to be incentives to engage in technology transfer activities. Although the GAO found that the reporting rate for inventions by NIH employees increased significantly after 1986, as noted above, it did not attribute this increase to the incentives of FTTA. The TDCs,

\textsuperscript{104} Interview with Sandra L. Shotwell. It has also been the subject of a recent report of the Office of Technology Assessment (OTA 1993).

\textsuperscript{105} Actually, I'm not sure how GAO determines this 6.8% figure, because the minimum distribution is 15% before expenses. The income from the AIDS test kit may be so extraordinary that even after the inventor, Dr. Gallo, caps out at $100,000 (or just more than $600,000 of total invention income), that the earnings vastly exceed the sum of the earnings of lesser inventions and reduce the average distribution below the mandated minimum.
scientists, and OTT personnel interviewed in this research corroborate this lack of
efficacy by the incentives. For example, Reid Adler believes that the royalty-sharing
incentives are largely “illusory” to anyone with common sense because biomedical
research projects require long years of development and, usually, a time-consuming
and expensive approval process by FDA.106 Stephen Ficca says that “philosophically
[royalty sharing] certainly is a beneficial mechanism, [but] in practice, there aren’t that
many people that are benefiting from the royalties. It’s a small group of people.”107
Guerra adds that not only are there few inventions actually earning royalties, but that
“most of [the scientists] aren’t motivated by that--if they were motivated by personal
income, they wouldn’t be working in the government.”108 There is also some belief
that the $100,000 cap on royalties (for any number of inventions) is “pretty draconian,”
especially when it is combined with conflict of interest regulations that prohibit
intramural scientists from profiting from the royalties earned from book sales.109

These generally small, infrequently applied incentives then do not seem to drive
scientists toward technology transfer, perhaps particularly because of the effort required
to initiate the process with a patent application. “Nobody likes the patent application,
but the royalties help kill the pain,” says one OTT professional.110 Nevertheless, says
one scientist who earned about 5% of his salary last year in royalty payments, “you
hope for a home run.”111 By the accounts of the participants, the motivation to engage
in technology transfer through patenting and licensing seems to come from the
opportunity to help move one’s research beyond the laboratory. Despite initial fears
that the royalties would cause scientists to change the orientation of their research,

106 Interview with Reid G. Adler.
107 Interview with Stephen Ficca. GAO quantifies Ficca’s surmise: 31.65% of federally employed
scientists and engineers are engaged in R&D; 1.59% of all scientists and engineers report inventions;
0.17% of all scientists and engineers receive royalties.
108 Interview with Maryann Guerra.
109 Interview with senior professional, OTT, Bethesda, MD, January 17, 1992. The professional added
that this prohibition was remarkable, considering that even criminals--"Son of Sam" was cited--can do
so.
110 Interview with professional, OTT, Rockville, MD, January 17, 1992.
111 Interview with Richard J. Youle.
FTTA "isn't changing science in the sense that it's changing research."\textsuperscript{112} Over time, scientist have realized that patents are the only way that their inventions are going to have the impact on health care they hope for.\textsuperscript{113} Scientists are indeed understanding that papers do not change medicine, but patents do.\textsuperscript{114}

4. Cooperative research and development agreements (CRADAs)
   
a. creating the CRADA

The invention disclosure, patenting and licensing process described above provides a formalized and interactive version of the traditional process of scientific discovery and communication. In a similar way, the CRADA provides a formalized version of the traditional process of scientific collaboration. Stephen Ficca, involved in the first CRADA at NIH, describes the process of inventing formal cooperation:

[O]ur institute was the first institute at NIH to actually put a CRADA in place under the auspices of the new act. And that occurred because Dr. French Anderson had been working directly with Phil Chen and with the Office of General Counsel. But because there were no guidelines in place, things were sort of getting off to a very slow start. And the problem was that there seemed to be a tremendous potential here, and investors were very much interested in getting this off the ground, and NIH was responding very slowly at the time....I got involved then because I was one person who could actually act as a facilitator to pull together general counsel, our attorneys, the attorneys for the company--at that time it was...Genetic Therapy, Inc., GTI...--and Dr. Anderson. And we all sat down in a room together, all the attorneys from the company...attorneys from General Counsel, Phil Chen, myself, Dr. Anderson. And we pretty much spent the whole day in the conference room from maybe 8:30 in the morning till, I don't know, 4:30, 5:00 in the afternoon, banging out the very first collaborative [sic] research and development agreement. Now, what made this kind of unique was that...the original Stevenson-Wydler Act...really had to do with patents and royalties and things of that nature. And what the [revision of the] Stevenson-Wydler Act did, it created this instrument called the CRADA, the collaborative [sic] research and development agreement. That's essentially what it did.\textsuperscript{115}

\textsuperscript{112} Interview with TDC at a medium-sized ICD, Bethesda, MD, January 8, 1993.
\textsuperscript{113} Interview with Thomas D. Mays.
\textsuperscript{114} Interview with Richard J. Youle.
\textsuperscript{115} Interview with Stephen Ficca.
Needless to say, this type of “banging out” CRADAs was not the way to proceed on each one. One of OTT’s first tasks was to create a model CRADA, boilerplate language from which collaborators could work toward more specialized agreements.

Having the model CRADAs, however, does not make the process carefree. Like patenting and licensing, CRADAs are time-consuming endeavors—so time-consuming, in fact, that one TDC may even suggest to researchers that they attempt alternatives to CRADAs for cooperating with private research partners such as material transfer agreements (MTAs), to avoid the burdens of CRADAs.116 But generally, TDCs try to alleviate the problems CRADAs may offer:

Coming from an administrative background, I feel very strongly that the role of administrators is to help the scientists get the research done. So the philosophy that I use in tech transfer is...to help them get involved in tech transfer to the greatest extent possible. I’ve found one of the downsides to the scientists of being involved in tech transfer...is the time it takes. They want to be at the bench. They don’t want to be bothered with the administrative crap that goes with it. They don’t want to negotiate agreements. They just want to be able to...collaborate with who they want, when they want. And so...the goal of our office is to have people there that do all that for them. If they want to enter an agreement, they come to us and say, “I want to enter an agreement.” Then Hank [Safferstein], who is...head of my CRADA section, he starts negotiating with the companies. He helps them draft the research plan. He helps modify the CRADA, make the CRADA.117

As with inventing, patenting and licensing, the role of the TDC is to collaborate with the researchers to “draft the research plan” and “make the CRADA”:

[S]omebody will come and say, “I have this research project and I’ve talked to a couple of people. We’re trying to get here. What do we do?” And we basically tell them what to do--tell them how to set up a research plan. [We help] them structure a research plan. We tack onto that all the legal boilerplate that we require for a CRADA. We send it to the company, talk to the company, explain the boilerplate to them, ask them to describe what they’re going to contribute to the research plan, reevaluate it...So...we’re like taxi cab drivers, essentially. Somebody says, “I want to go to 45th and 2nd Avenue.” No problem! We know the directions. We can get you there. And that’s basically what we

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116 Interview with TDC at a small ICD, Bethesda, MD, January 8, 1993.
117 Interview with Maryann Guerra.
do...It was slight hyperbole to say "an idea to reality." We try to facilitate the process.\textsuperscript{118}

Once the TDC has "banged out" the CRADA proposal, there is again a set of reviews, most prominently the CRADA subcommittee of the Patent Policy Board, which clears all CRADA proposals. The process has recently been altered to allow an OTT professional and a professional from the Office of General Counsel to review CRADA proposals prior to the monthly CRADA subcommittee meetings and smooth out any rough edges before the subcommittee judges it.\textsuperscript{119}

The research plan, mentioned by both TDCs above, is the substantive part of the CRADA and a focus of the boundary work of defining the domains of politics and science. In order to prevent intramural labs from becoming industry shops, scientific exchanges are required for CRADAs and not just monetary contributions. TDCs agree that CRADAs are usually developed for "scientific reasons, scientific excellence," and are derived from contact between scientists "on a scientific basis."\textsuperscript{120} In other words: "[O]ne of our objectives is not to enter into CRADAs because of additional resources. We do that now because it helps our scientists, but if my scientists were coming to me saying... 'I'm only getting into CRADAs because I want money,' I would be opposed to trying to get CRADAs for them. We want them to enter into CRADAs that complement their science."\textsuperscript{121} To ensure the scientific component, CRADA proposals are cleared through a technical advisory committee in the ICD (PPB 1992).

Nevertheless, the comments by the TDCs reveal that there is some tension between the need for money and assistance in intramural laboratories and the ideal of scientific partnerships. The ideal is not something that is expressed in FTFA, and the possibility

\textsuperscript{118} Interview with Stephen Ficca. See above in the licensing section for the "idea to reality" quote.
\textsuperscript{119} Interview with TDC at a small ICD, Bethesda, MD, January 8, 1993.
\textsuperscript{120} Interview with TDC at a medium-sized ICD, Bethesda, MD, January 8, 1993.
\textsuperscript{121} Interview with Maryann Guerra.
of productive CRADAs may be limited by the boundary work of determining which
proposed CRADAs do and do not have sufficiently scientific components.

b. fair access and identifying research partners

One of the possible rough edges for CRADAs now and in the future is the
problem of providing fair access to intramural collaborators to interested private firms.
FTTA does not require that the selection of CRADA partners follow standard federal
contracting procedures. Indeed, it was the intent of the legislation to streamline the
process for this particular application. Under some circumstances, for example when
interested parties are unknown or when competition will serve the public interest in a
particular project, announcements of CRADA opportunities may be placed in the
Federal Register or the Commerce Business Daily (PPB 1992). Otherwise, partners
are generally identified by the scientists themselves, according to their own criteria. As
Stephen Ficca describes about the first CRADAs at NHLBI:

[T]here was a lot of concern that we were not giving fair access to other
companies, and there was a lot of concern that we had the potential of
cornering the market on gene therapy with one company. And in
defense of that, I said, "here's what we did." And we went out and
looked for people. And...we looked for other companies. Nobody
wanted to collaborate with us on this project.

Q. How did you look for them? How did you identify them?

Well, we identified them by going through a file. There were two
ways, actually. French [Anderson] already knew all these people in the
first place...But also, somebody in the Office of Technology Transfer
did a search for us and gave us a list of companies who were doing gene
therapy. And so we contacted each of them. And, one of them was a
chief rival of French, and had no intentions of collaborating with him
one iota.....Some of the other companies were just moving in a different
direction. French was using retroviruses and they were using a totally
different vector or delivery system, and they weren't interested in that.
And basically...nobody else believed this was a good
investment....That was it. Now, it's a totally different matter. If today
we were to do that, I think you'd find that people would be clamoring at
the door. And in fact, that's the case.122

122 Interview with Stephen Ficca.
One scientist who is involved in about half a dozen CRADAs in various stages of development says that he has generally identified partners by word of mouth, as part of a conscious strategy. He asks colleagues who the leaders in the field are, and he tries to identify exactly who the individual scientists are who perform the relevant research. He then tries to "seduce" these scientists with his scientific problem. This scientist works on a therapeutic for a disease prevalent in the Third World, so the potential market is not a wealthy one. Instead of dealing with businesspersons in companies who would focus on the potential profitability of a CRADA, the scientist instead uses this "grass-roots strategy" to find research partners. Although CRADAs initiated in this manner have not usually involved private funding for his laboratory, they have provided him with opportunities he otherwise would not have had. His experiences demonstrate the other side of the problem of scientific criteria and monetary support: his project is scientifically interesting, but he has to hustle to attract money.

Some TDCs describe positive experiences with advertising for CRADA partners. One TDC had an advertisement in the Federal Register that attracted several inquiries from interested firms. One company, the interest of which the TDC had known before, was selected for the CRADA. Another company submitted a proposal that might have been superior to the one chosen, but it came in after the closing date. In this case, the TDC was looking for the broadest possible CRADA to support an existing patent, so the TDC felt that competition could not hurt. Other TDCs describe bad advertising experiences:

[I]n fact what they’ve done in a couple of cases is to not even advertise a particular research project, but advertise that they’re interested in looking for collaborators, or “here’s a list of all these research areas that we’re doing. Anybody’s who’s interested in collaborating with us, contact so-and-so.” We have used that technique a little bit but without much success. We’ve had people contact us about drug delivery systems. But it turned out that the drug delivery system was how to

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123 Interview with Acting Section Head at a larger ICD, January 8, 1993.
124 Interview with TDC at a small ICD, Bethesda, MD, January 8, 1993.
deliver drugs to the local pediatrician. It wasn’t the kind of drug
delivery system we were talking about.¹²⁵

Advertising for CRADA partners has the benefit of not only providing the
appearance of fair access from the perspective of potential partners, but it also can help
scientists avoid appearances of conflicts of interest:

[O]ne of my investigators came to see me and said a company called
him, a big company, and said that they wanted to collaborate with him
and that in exchange for the collaboration that they would give him a
very lucrative position when he left the government and all sorts of
those other things. He said, “what do I do about that?” I said, “you go
back and tell them you’re not interested, and then we’ll go see Phil
Chen.” So we did that. And Phil was very empathetic. And...he said,
“but I really...would like to collaborate with somebody.” So I said
okay. We then set up an advertisement.¹²⁶

However, in protecting the scientist and the ICD in this way, advertising does to some
extent take the choice about collaborations out of the hands of the intramural researchers
involved, an effect noticed by the TDCs;¹²⁷

And so we...asked people to submit a research plan, and we would
evaluation those research plans, and...the company that had the best
research plan, that’s who we’d collaborate with. But part of that was,
that it also had to be...in the best interests of the public and the
government....

Q. You identified the companies the same way....

No...this was totally different....[W]e advertised in the Federal
Register...as sort of an open competition. And then we...put...an
outside review group together. The investigator wasn’t involved in this
decision as much. In the final analysis, the investigator had to agree,
but we had an outside review group go over the applications, the
proposed research plans. And then the review group recommended the
best plan for the government and what they thought was in the most
public interest.¹²⁸

¹²⁵ Interview with Stephen Ficca.
¹²⁶ Interview with Stephen Ficca. Nevertheless, there are no rules governing post-government
employment of scientists engaged in CRADAs.
¹²⁷ Interview with TDC at a medium-sized ICD, Bethesda, MD, January 8, 1993.
¹²⁸ Interview with Stephen Ficca.
Another problem with advertising is that it further burdens the timetable of competitive research and competitive industry. One scientist experienced the loss of two potential licensees because of advertising troubles. In one instance, a company indicated its interest in licensing the scientists' patent exclusively, but was told that an advertisement would first have to placed through NTIS in the Federal Register.\textsuperscript{129} NTIS never actually placed the advertisement. Therefore the clock had not started on identifying other interested parties. The potential licensee then became impatient and the license fell through. In a second instance, another potential licensee was waiting for the results of the advertising, but wanted to know beforehand what the upfront and royalty payments might be. Such information could not be provided because it would have constituted negotiating before the end of the advertising. That company, as well, became impatient and the advertisement attracted no other potential licensees.\textsuperscript{130} As one TDC suggested, advertisements make selecting CRADA partners dangerously indistinguishable from procurement. "There's that timeliness to science," and "you plug it into the contract system and you've killed CRADAs—well, maybe not killed them, but...you can't act on the moment."\textsuperscript{131} Another added, "Technology transfer is supposed to assist, not hold back, research;" "the research can't wait."\textsuperscript{132}

c. contact, funds, the market and motivations

Regardless of their feelings about the pricing clause, fair access and advertising, paperwork and so forth, there is a general consensus among the participants in technology transfer from the NIH intramural laboratories regarding which aspects of CRADAs are important and which are unimportant motivators for the scientists involved. Scientists are most interested in CRADAs because they allow them access to materials and expertise that otherwise would be closed off for proprietary reasons; and scientists appreciate the small addition of resources to their laboratories that CRADAs

\textsuperscript{129} This was the pre-FTTA procedure.
\textsuperscript{130} Interview with Senior Researcher at a small ICD, Bethesda, MD, January 7, 1993.
\textsuperscript{131} Interview with TDC at a medium-sized ICD, Bethesda, MD, January 8, 1993.
\textsuperscript{132} Interview with Thomas D. Mays.
can provide. These benefits, however, do not seem to be a significant contributor to recruiting and retaining scientists for intramural research.

The first major payoff for scientists engaged in CRADAs is resources for their laboratories, particularly money for personnel and access to materials. "For scientists, getting bodies on board is more important" than other incentives. Although CRADAs will often come with money for laboratory personnel, sometimes personnel ceilings from the Office of Management and Budget (OMB) that limit the number of full-time equivalents at the ICD can stand in the way. But the marginal contribution that CRADAs can make to funding a laboratory are important. For one scientist, CRADA money accounts for about 5-10% of his operating budget. With budgetary constraints, particularly on discretionary spending and laboratory supplies, even this money at the margin is very important to maintain the laboratory.

The second major payoff for CRADA participants is the opportunity to exchange proprietary or unique materials to which, without the protection afforded by the CRADA, they would have no access. For one scientist, the primary benefit of the CRADAs he has signed is the access to expertise and proprietary information he could neither generate in his laboratory nor find in a non-proprietary setting. Another researcher could not have even started his line of research without the CRADA, because the necessary protein would not have been available and "it would be a pain" to purify it even if it had been. Although other options, called material transfer agreements (MTAs), are available to protect and facilitate the exchange of reagents and research materials, some scientists are almost "forced" into CRADAs by companies looking to protect their investment, because only in CRADAs can guarantees for rights of first refusal for exclusive licenses be extended. In this way, the stronger potential

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133 Interview with TDC at a small ICD, Bethesda, MD, January 8, 1993.
134 Interview with TDC at a small ICD, Bethesda, MD, January 6, 1993.
135 Interview with Richard J. Youle.
136 Interview with Acting Section Chief at a larger ICD, January 8, 1993.
137 Interview with Richard J. Youle.
138 Interview with TDC at small ICD, January 8, 1993.
research partners can have a great deal of leverage over intramural researchers and the federal government.

This exchange of materials seems to be an important mechanism in more fully integrating the intramural scientists into the scientific community. Proprietary protection allows intramural scientists to work with external colleagues at an earlier stage in the project.\textsuperscript{139} Maryann Guerra emphasizes the restrictions that intramural scientists had labored under before FTTA: “I think we were motivated by the fact that interacting with industry is an important thing to, and the government was not allowed to do that before. And so our scientists were at a disadvantage because they couldn’t have that opportunity. CRADAs allowed that opportunity.”\textsuperscript{140} Nevertheless, the contact with the broader scientific community via CRADAs is probably not as important as other normal mechanisms such as attending professional meetings.\textsuperscript{141}

Despite the opportunities that CRADAs offer to scientists for augmenting their laboratories and their scientific roles, there is a general consensus that these opportunities do not significantly affect the ability of NIH to recruit and retain intramural scientists. At the Clinical Center, for example, TDC Steven Galen believes that the opportunities provided to scientists by FTTA help, but only because everything helps. FTTA is not a predominant influence because clinicians can get funds from other sources and because clinical studies are so expensive that they can be done in few places other than NIH. In Galen’s limited experience with CRADAs, the researchers get involved for the collaborations themselves, not the money that may come with the collaborations.\textsuperscript{142} Thomas Mays at NCI--the most active ICD--agrees with this assessment. Mays finds it hard to believe that the money that may come from collaborations is an aid to recruitment and retention. Rather, the opportunity to see research through to development in the context of a CRADA seems to be the primary

\textsuperscript{139} Interview with TDC at medium-sized ICD, January 8, 1993.
\textsuperscript{140} Interview with Maryann Guerra.
\textsuperscript{141} Interview with Richard J. Youle.
\textsuperscript{142} Interview with Steven M. Galen.
motivator.\textsuperscript{143} Scientist Richard Youle also agrees that CRADA opportunities are not important in keeping scientists at NIH rather than at medical schools or in the private sector.\textsuperscript{144}

To the contrary, it may even be the case that in some research areas, the ability to collaborate with private sector partners “puts [intramural scientists] into the mainstream more, but that gives them opportunities outside the government” that could even accelerate brain drain from NIH.\textsuperscript{145} Intramural scientists often feel “chafed” by the unusual combination of opportunities and constraints of being government scientists.\textsuperscript{146} Although the scientists could not pursue their goals in any other context, they still have difficulties adjusting to the time-consuming process and the apparent lack of rewards. One scientist “absolutely” finds that his time is being drawn away from research because he has to scramble to maintain his CRADA. Although his TDC relieves some of this burden, the scientists ends up on the phone with the TDC instead of with the research partner. Not only does he lose time at the bench, but his own hands-on research work has become more oriented toward tasks, however mundane, that complement the CRADA. The scientist must convince the postdoctoral researchers in his laboratory that doing the work he does would not be good for their careers. In fact, it may not be good for the scientist’s own career: he is currently up for tenure and is concerned that CRADAs and patents do not come up in the review. Although he cannot yet say that his career has actually suffered, he feels his future would be more secure if he spent his time publishing papers rather than hustling the CRADA. He also says his CRADA relationship has kept him “from making wise financial decisions; [but] that’s not why I’m at the NIH.”\textsuperscript{147} If the constraints are not in balance with the opportunities for cooperative research, FTIA will not have removed the threat that

\textsuperscript{143} Interview with Thomas D. Mays.
\textsuperscript{144} Interview with Richard J. Youle.
\textsuperscript{145} Interview with Thomas E. Ingalls.
\textsuperscript{146} Interview with TDC at a small ICD, Bethesda, MD, January 8, 1993.
\textsuperscript{147} Interview with Acting Section Chief at a larger ICD, Bethesda, MD, January 8, 1993.
scientists will flee intramural research, and it may have exacerbated it to some small degree.

Neither is it clear that the CRADA mechanism is an important way to keep intramural scientists in touch with broader social goals. As noted above with royalties, NIH intramural scientists may already be more concerned than other scientists with social goals like public health because they are public servants, working for less money and recognition than their university and private sector colleagues and within the context of a mission agency.148 Stephen Ficca, the TDC involved in the first CRADA, “would have to say that keeping in touch with the major social issues is not one of the main things that the CRADAs bring. What it does, it allows them to follow through on those things. Because I think they’re already in touch with social issues” (emphasis added).149 Nevertheless, as one scientist indicated, CRADAs are important to him because they keep him in touch with the value of applied research, which is a social goal. Basic research, he believes, is “honorable” and NIH should provide an environment that emphasizes basic research, but it is also important to keep in contact with the applied research that CRADAs allow.150 Or, as Maryann Guerra maintains, “[Scientists] like to see it go to the next step. But ...we’re not supposed to do applied research here. We’re basic research. And we’re surely not product-oriented....[B]ut it doesn’t mean that they don’t want to see that get done.”151 And as noted above, scientists are realizing that patents keep scientists in contact with the social values of health because of their ability to change medicine.

Ficca’s idea of “follow through” probably yokes these slightly divergent opinions together: for scientists who are predisposed to applied research or whose research will lead to a drug or a device with incremental effort, the CRADA seems a reasonable extension that allows them to follow through on plans they already have; for

148 Interview with TDC at a small ICD, Bethesda, MD, January 4, 1993.
149 Interview with Stephen Ficca.
150 Interview with Richard J. Youle.
151 Interview with Maryann Guerra.
scientists who are not so predisposed, the CRADA provides little connection or attraction. “Follow through” also provides a bridge to the question of recruitment and retention. Although few if any saw an impact of FTTA, one TDC did speculate that in the future, as NIH implemented its new intramural rules on animal care that would increase costs faster than funding seemed to be rising, that researchers would seek CRADAs for their animal research.152 Yet to the extent that much animal research is related to testing, this, too, seems like a issue of “follow through” that would be available to researchers, but not necessarily something that might make them choose between intramural and extramural research. “I guess I don’t see the mechanism as being crucial to keeping our scientists in touch with the broader social issues. I see it as being an important in planning how to conduct and how to carry out an effective and efficient, rapidly evolving research program.”153

Although CRADAs can, by allowing access to proprietary materials, allow intramural scientists to become more complete participants in their wider scientific community—that has, since 1980, included more and more industrial scientists—CRADAs also expose scientists to the vicissitudes of the market. In some cases, research can suffer; in others, it can be validated. When research suffers, it can be as crude as when scientists whose work is very product-oriented cannot find partners because profitability is the sole selection criterion and the potential market for the product is deemed unprofitable: “It’s the biggest problem I have.”154 But the difficulties can also be more subtle. For example, NHLBI is in the position of trying to establish a CRADA for the public benefit of developing a new device, but the institute could benefit itself only if it becomes, from the company’s perspective, a competitor:

Normally under a CRADA, if something is developed by coinventors, the government will own the product and [grant] an exclusive license. But this company we’re talking to now has got these [devices]. Under

152 Interview with TDC at a small ICD, January 8, 1993.
153 Interview with Stephen Ficca.
154 Interview with Acting Section Chief at a larger ICD, Bethesda, MD, January 8, 1993.
any circumstances, they’re saying they will own the patent, and we
won’t get any royalties. And they’ll say, “but you can use the research
data any way want to.” The only reason we would use the research data
is to develop a [device] to treat the patients, but they already have the
patent on it. So what are we going to do, get an exclusive license? I
mean, it makes no sense....On the other hand, we wouldn’t not do it,
because they’re going to help us with the cost of the research, I think.
But if...they are going to move a product into the community hospitals
or into the doctors’ office that’s going to help the patients, then we feel
that’s what we should do.155

But the same profit motive that can trouble research can also grace it. CRADAs
offer the opportunity for companies to deem a line of research valuable, in a sense
validating the intramural scientist involved. The primary CRADA of one senior
researcher involved a medical device requiring a great deal of development work. He
and an outside physician worked on the device and patented it, but then the outside
physician dropped the project. The researcher sought intramural sponsorship, which
was difficult because many of the relevant medical specialists thought the device was a
silly idea. The researcher also realized that the device would need to be manufactured
in a cleanroom, so he sought a private collaborator, which presumably would not
collaborate unless it thought the project was potential profitable. The researcher found
interested companies, but for a variety of procedural reasons, he has yet to sign a
CRADA. Nevertheless, he feels validated and rewarded by the level of industrial
interest the device has received.156

d. public-private partnership and conflicts of interest

Although the participants in technology transfer activities generally rejected the
idea that CRADAs allowed the scientists to enhance their connection to social goals--
with the stated exception of emphasizing the value of applied research--there is clear
agreement and evidence that CRADAs do enhance the influence of market values.
Indeed, the statement regarding applied research can be read in exactly this way. But

155 Interview with Stephen Ficca. Also note the apparent tension here among the commercial, the
public health, and the scientific aspects of the project.
156 Interview with Senior Researcher at a small ICD, Bethesda, MD, January 7, 1993.
when participants reject the notion that social goals are enhanced and simultaneously accept the notion that market influence occurs, they are distinguishing between the social and the economic. They are encouraged to make this distinction by FTCA, because although it opened the doors to formal cooperative research with universities and nonprofit organizations as well as with profit-seeking firms, the mechanisms FTCA provides are limited to economic incentives. The history and trajectory of the legislation was sensitive to a growing national consensus about a decline in technological innovation and industrial innovation. In its implementation, this sensitivity has remained: of the 93 CRADAs listed by PHS (1992) for NIH and ADAMHA, only 4 do not include profit-seeking firms. The CRADAs therefore represent a somewhat occluded view of promoting the public health, encouraging public-private partnership in which the public health is promoted only through private action.

Some implications of CRADAs as public-private partnerships are not lost on the participants, but they do seem to think that they are in control of the situation. Although they recognize that “you need these drug companies” to do the research, they also feel that they are able to get what they need--money, good scientists, and expertise--from their private partners. It is clear to many that CRADAs are part of an industrial policy, but they view cooperative research as an improvement on the previous adversarial relations, one that will now allow the United States to compete internationally. The breakdown of the old system that separated the public and private spheres might be somewhat dishonorable, but the United States was only able “to be honorable because we were loaded.”

Stephen Ficca shares this perspective that there is something for everyone in cooperative research:

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157 Interview with Richard J. Youle.
I think it’s important...to have a legitimate mechanism to...collaborate with the private sector at a time now [when] biotechnology is an industry of a future. It is--I hate to say this--but I think Ross Perot did say this in one of his talks...and that’s not just something he invented. I think it’s an observation that many people have made. And I think that government-private sector collaborations are going to be critical....[W]e’ve spawned lots of biotechnology industry here at the NIH, and I think that without the CRADA mechanism, and without a wise use of that mechanism, it would flounder....Eventually the industry would grow, but it grows better when we have that ability to collaborate, and [when] we have a legitimate mechanism to leverage. And they leverage on us. They come in and they can get the research done, and we then leverage on them because they can help offset some costs. That’s legitimate, because they’re going to benefit. We benefit. The public benefits. Everybody benefits. If it didn’t exist, there’d be something else that would have to be invented. I think it is necessary. 158

Likewise, Thomas Mays understands that the FTAA is something of an industrial policy for the biotechnology industry. He emphasizes the proper handling of fair access to cooperative opportunities, the fair pricing clause, and conflicts of interest as sensitive areas of this new policy that could be as vulnerable as other such policies. 159

NIH has struggled with the issue of conflict of interest since almost the passage of FTAA. Not the least of the problems were the congressional investigations into conflicts and financial misconduct in the laboratory of NCI researcher Robert Gallo, discussed above. But NIH has also attempted the tortuous task of rulemaking to resolve the potential conflicts in public-private partnerships in biotechnology. In September 1989, NIH issued a “request for comment on proposed guidelines for conflict of interest policies” (NIH 1989). Directed at assisting universities and other NIH extramural grant recipients develop their own policies, the NIH proposed guidelines received a hostile response from a collection of university and medical school faculty and administrators, and from biotechnology firms and venture capitalists. According to the analysis by NIH of the 771 responses, 41% of the respondents said that the guidelines would have a negative effect on research or

158 Interview with Stephen Ficca.
159 Interview with Thomas D. Mays.
technology transfer; 17% said that the guidelines were counter to the government’s recent efforts to encourage the exploitation of research (e.g., Bayh-Dole and FTIA); 20% said that the guidelines would hamper communications and a similar percentage said the guidelines would damage the emerging biotechnology industry (NIH 1990). The general mode of argument was a utilitarian one that did not deny the legitimacy of the NIH’s role in regulating conflicts nor the potential problem. Instead the respondents focused on what they foresaw as damage to the pursuit of other goals like technology transfer and the development of the biotechnology industry. In these arguments, the respondents constituted an interest group—rather than a group of disinterested scientists or a group of self-regulating professionals—whose formation was precipitated by legislation including Bayh-Dole and FTIA (Guston 1990c).

After this repudiation by its constituency, NIH tried again, sending new draft guidelines to PHS in May 1990. Like the earlier guidelines, the new draft would apply to all types of research, from basic to clinical. PHS overruled NIH, however, limiting the application of the draft guidelines to clinical research and limiting financial interests to those “directly and predictably affected by a clinical trial” (Palca 1990:1237). Representative Ted Weiss, who had been pursuing the conflict of interest issue since his hearings in 1988, criticized the approach of NIH as too weak.

Intramural scientists are generally governed by the same conflict of interest regulations as govern all federal employees. Although consulting was allowed by director Wyngaarden’s 1985 ruling, federal scientists cannot engage in CRADAs with companies for whom they are consulting. Scientists may not even work toward a CRADA while they are paid consultants. Also:

Scientists who enter into a CRADA will be somewhat limited in their freedom to collaborate with others because of their confidential relationship with the CRADA partner. Therefore, NIH/ADAMHA scientists must be careful during conversations with others not to reveal
proprietary information purposefully or accidentally in the absence of a written confidentiality agreement (PHS 1992:117).\textsuperscript{160}

In any case, all NIH intramural scientists are also covered by interim guidelines on conflicts of interest.

For the participants in technology transfer from the intramural laboratories at NIH, conflicts of interest are a small but nagging problem that always threatens to become a larger problem. Mays says that the public trust is taken seriously, and that the process tries to err on the side of caution.\textsuperscript{161} For scientists proposing CRADAs, there is a disclosure form and a certification process that affects the scientists and their families (that is applicable to scientists in the entire laboratory, not merely to the few directly engaged in the CRADA). Scientists do occasionally feel hamstrung by the requirements and would prefer more flexible arrangements,\textsuperscript{162} and ICDs may lose CRADA opportunities because scientists would prefer to maintain their consulting relationships.\textsuperscript{163} But the rules regarding disclosure and consulting seem firmly established.

The area of developing policy regarding conflicts of interest is with clinical research:

The big problem here is the integrity of the research. If the company comes in, and...if their product is being evaluated, and they're going to benefit from by the outcome of the trial, then if they're involved in the evaluation of the data of that trial, then there's always the potential of that either that there will be some very positive interpretations of the data, or that people will view the integrity of the trial as being compromised by that.\textsuperscript{164}

\textsuperscript{160} This admonition seems stern for scientists who may be used to communicating freely with their colleagues according to some norm of communism (Merton 1973:ch.13). But communication is also subject to counternorms (Mitroff 1974) or contextual (Ziman 1990) and is sensitive to contexts in which normal behavior may be somewhat curtailed (Zuckerman 1977). Indeed, in the normal course of discovery, scientists will refrain from communicating in order to ascertain more completely what their discovery is and in order to ensure priority (see Edge and Mulay 1976; Mitroff 1974).

\textsuperscript{161} Interview with Thomas D. Mays.

\textsuperscript{162} Interview with Richard J. Youle.

\textsuperscript{163} Interview with TDC at a small ICD, Bethesda, MD, January 4, 1993.

\textsuperscript{164} Interview with Stephen Ficca.
To address this problem of perceived or actual conflict, the CRADA subcommittee of the Patent Policy Board has been focusing on data monitoring in clinical trials. A data monitoring board has been established to oversee multi-center clinical trials, and companies with a financial interest in the outcome of the trials are usually excluded from data analysis.

   e. indicators

If it is the case that the technology policy of the 1980s, including Bayh-Dole and FTTA, promotes the instrumental use of scientists to bolster technological productivity and innovation in biotechnology as argued in chapter V, then it would presumably be the case that Congress would oversee its policy instruments to determine if they are actually promoting the sought-after ends. Indeed, this presumption is correct. GAO has followed FTTA closely, commenting on the implementation status of the act (GAO 1991a; 1989), the licensing activities of the agencies covered by the act (GAO 1991b), and the effectiveness of the royalty sharing provisions (GAO 1992). The House Science, Space and Technology Committee has also held a number of hearings on FTTA implementation (U.S. Congress 1991b; 1990b; 1989b).

In the testimony at each of these hearings, witnesses presented the number of CRADAs entered into by NIH as a key datum. In the 1991 and 1990 hearings, witnesses also presented the number of patents or patent applications filed as an important datum, and in 1991 the witness mentioned as well the income derived from licenses on those patents. The GAO reports, as a whole, emphasize data of the same type: invention reporting rates, licensing rates, income-producing inventions, and so forth. Given that FTTA created the opportunities for such data to be generated, and indeed invented the CRADAs themselves, will these data become indicators for the evaluation of the scientists and the agencies performing technology transfer, and if so, what might be the impact?
FTTA rejects the idea that personnel evaluations at the research agencies are to be dependent upon technology transfer activities, but the law is specific in stating that: “Each laboratory director shall ensure that efforts to transfer technology are considered positively in laboratory job descriptions, employee promotion policies, and evaluation of the job performance of scientists and engineers in the laboratory” (100 Stat. 1790). In my research, I found little indication that such activities are taken into account for personnel evaluation, and I found some apprehension that technology transfer activities would detract from the publishing and other activities upon which tenure decisions are based. One TDC asserted that some ICDs had incorporated technology transfer activities into personnel evaluations, but that mostly technology transfer had been incorporated into other administrative routines such as the review of publications and conference papers for patentable inventions. But most said that they were unaware of any such consideration, and no TDC with whom I spoke affirmed that engaging in CRADAs or patenting inventions had become important to personnel evaluations in his or her ICD. The scientists supported this finding. For example, one was not aware of any role of technology transfer activities in personnel decisions; one expressed anxiety that his current tenure review did not mention anything about CRADAs or patents and asserted he be more comfortable if it did; another merely understood that there are “brownie points” for patenting.

The participants are somewhat divided over the idea of using these data as indicators for personnel review. For example, Maryann Guerra feels that “scientists don’t want their markers to be inventions or how many cooperative agreements they have. And I don’t think they should be recognized...or applauded or punished for what they do...when it comes to tech transfer.” Thomas Mays acknowledges that

165 Interview with TDC at a small ICD, Bethesda, MD, January 4, 1993.
166 Interview with Richard J. Youle.
167 Interview with Acting Section Head at a large ICD, Bethesda, MD, January 8, 1993.
168 Interview with Senior Researcher at a small ICD, Bethesda, MD, January 7, 1993.
169 Interview with Maryann Guerra.
CRADAs, patents and royalty monies have been used as indicators, but believes their use must be seen in the context of questions about other indicators. Publication counting, for example, is a difficult question. Which is better: five scholarly tomes or 100 pieces of thinly sliced research salami? The AIDS test kit counts as one patent, but it also accounts for millions of dollars of royalty income and perhaps millions of lives saved.\textsuperscript{170} Others see patents as: one possible “measure” of “success;”\textsuperscript{171} a reasonable measure within limits;\textsuperscript{172} an item for a curriculum vita that measures and publicizes a research accomplishment, to be used akin to publications as an indicator;\textsuperscript{173} an “objective” evaluation of the commercial merit of a research project—in contrast to an article, which a scientist can always publish in a second-rate journal;\textsuperscript{174} and a real contribution when “some of science is posturing.”\textsuperscript{175}

Nevertheless, there is great sensitivity to preserving the variety of research styles at NIH and the flexibility of the individual scientists to pursue research as they see fit:

I guess I’m sort of parochial [but] I don’t see productivity being measured in the number of CRADAs that you have, or the number of patents that you’ve filed....Traditionally the kinds of things that we look at to judge our quality of research have been various other mechanisms such as outside peer review...publications and citations. Looking at contributions they’ve made to the field....Now, to the extent that a patent represents the culmination of that basic research, or...a real-life application...there is that danger. Because if...people will find out their livelihood depends upon the numbers of patents that they’ve filed, and that will certainly influence the research that they do, and things that they work on, and that would be a disaster. That would be, long run, a big problem for this country. That’s my opinion.\textsuperscript{176}

\textsuperscript{170} Interview with Thomas D. Mays. Indeed, some institutions have begun limiting the number of publications that applicants for grants or candidates for advancement can submit in support of their candidacy, in the hopes of reducing the incentive to build c.v.’s through the strategy of the “least publishable unit.”
\textsuperscript{171} Interview with TDC at a small ICD, Bethesda, MD, January 8, 1993.
\textsuperscript{172} Interview with Steven Galen.
\textsuperscript{173} Interview with Thomas Ingallis.
\textsuperscript{174} Interview with Senior Researcher at a small ICD, Bethesda, MD, January 7, 1993.
\textsuperscript{175} Interview with Richard J. Youle.
\textsuperscript{176} Interview with Stephen Ficca.
[Why] they're here is because they can do research and some of the research they do will never result in a product. It will never be something that is going to have a patent attached to it. So, yes the Federal Technology Transfer Act is critical. But [the Act does not] say, "okay everything you do now, you should have an invention, and now you should have a product." So the legislation needs to be there to allow what's done here as as basic research institution to get out of here, but not to displace or to compromise what's being done here. And that's what we worry about happening, is that they mix the objectives. Basic research is what we're supposed to do here, and tech transfer should never override or supersede that primary objective. And if it does, the Federal Technology Transfer Act will be the worst piece of legislation that was ever passed for public health. I mean it really will. Because what will happen is that the research will be directed by outside parties that are going to make money, and the important things that are discovered here that are never going to make any money, but eventually lead to a vaccine, you know, fifteen years down the road, won't happen any more.\textsuperscript{177}

NIH is moving gently into this area of indicators, because it does not want to make scientists feel as if they must be performing technology transfer.\textsuperscript{178} The typical scientist at NIH wants to do research, not necessarily technology transfer,\textsuperscript{179} and many lines of research are not amenable to product development: "You're not going to get a drug out of Drosophila homeoboxes."\textsuperscript{180}

CRADAs, patents, and royalty income could also conceivably be used to indicate the productivity or quality of the individual ICDs at NIH, and of the intramural program in general. Participants seem to understand the reasonableness of finding indicators for programmatic success, and most think that technology transfer is important for the future of the intramural program, but they are skeptical and even fearful of the application of such indicators. One TDC acknowledges that Congress must ask questions about patents and CRADAs and hopes that the awareness of this political interest does filter down to the level of the individual scientist.\textsuperscript{181} Another

\textsuperscript{177} Interview with Maryann Guerra.
\textsuperscript{178} Interview with TDC at a small ICD, Bethesda, MD, January 8, 1993.
\textsuperscript{179} Interview with TDC at a small ICD, Bethesda, MD, January 6, 1993.
\textsuperscript{180} Interview with Richard J. Youle.
\textsuperscript{181} Interview with TDC at a small ICD, Bethesda, MD, January 8, 1993.
acknowledges that when the economy is bad, research cannot be taken for granted as a good: “basic science for basic science is just not going to work....You look to the tangible. CRADAs are tangible....It suggests value...in a lot of different arenas.” It responds to the demand, “show us what you’ve done.” Thomas Mays agrees that being able to leverage on external resources gives scientists better opportunities which could lead to better research, more products, and greater political success.

However, just as not all individual research programs are attuned to technology transfer, not every ICD program lends itself to CRADAs and patenting.

[T]here are some organizations and some labs that aren’t technology transfer type of organizations. You know, not everybody’s going to be able to do CRADAs. I mean there may be some labs that will file a lot of inventions and have a lot of patents, but they’re not labs that...industry wants to collaborate with. And then there are some labs that industry wants to collaborate with that aren’t going to have a lot of inventions.

Furthermore, the role of technology transfer is different at some of the ICDs, such as the Clinical Center, that have service functions. ICDs also range in size of their intramural programs from a few laboratories to NCI, which consumes some $400 million per year, about 40% of all NIH intramural research.

OTT recognizes that the variety of the ICDs does present certain problems, among them deciding which technology transfer functions can be decentralized to the ICDs and which OTT must perform. The variety may even therefore result in uneven access to technologies at the ICDs, although not necessarily in an expected direction. That is, it is not perfectly clear whether ICDs that rely more on OTT or their own expertise would provide better access. Similarly, it is not perfectly clear what the impact of using technology transfer activities as a comparative indicator among

182 Interview with Steven Galen.
183 Interview with Thomas D. Mays.
184 Interview with TDC at a medium-sized ICD, Bethesda, MD, January 8, 1993.
185 Interview with Maryann Guerra.
186 Interview with Sandra L. Shotwell.
institutes would be. Under budgetary pressures, one of two scenarios is possible: Congress or OMB measures the income derived from CRADAs and royalties and subtracts that amount from ICD programs; or, discovering through CRADA, patent, and royalty indicators those ICD programs which are more productive than others, Congress or OMB gives more money to the ICDs with a greater ability to leverage it (or subtract more from those that do not demonstrate this leveraging ability).

When I conducted the first round of my technology transfer interviews in early 1992, it was rumored that OMB was going to implement exactly this first scenario. Reaction to the idea was hostile. Dr. James O. Mason, Assistant Secretary for Health and director of PHS, believes that although Congress is justified in questioning research expenditures and emphasizing technology transfer responsibilities, the “the well will dry up if everyone gets on the technology transfer bandwagon.” If the system works perfectly under FT TA, then “the investigator pushes back the frontiers of science” and private enterprise commercializes the results.187 Maryann Guerra expresses a similar sentiment:

I think that what is important is...that resources given to the NIH or to the...government research institutions not be diminished because of outside income. If they ever start looking at how much money we’re bringing in from these sources, and then start reducing budgets for that, it will kill the innovative and the basic culture that’s here at NIH. NIH is great because they have scientists here that are devoting their lives to science. They’re not, as I said, financially motivated.188

However, if the legislative motives that produced FT TA are still intact, the second scenario seems more likely, in fact, than the first. This logic is, of course, the same logic that drives the increasingly important debate over the relative shares of military and civilian research.189 It is also an issue in the running battle over intramural

188 Interview with Maryann Guerra.
189 See, for example, Alic, et al. (1992) for the idea of the ratio of productivity between military and civilian R&D investment.
and extramural research, as one TDC recognized. The federal government continues its large investment in science because it believes it to be productive for the economy and for the public health. FTTA is premised on the continuity of basic research for its productive value. Although Congress may be known for actions making little sense, it makes less than little sense to punish budgetarily programs that are productive. Rather, it seems more likely if FTTA indicators are used for budgeting decisions, that money will be pared from programs that are not as productive. The result could be some kind of Matthew Effect, where the generally larger, more commercially relevant ICDs continue to grow at the expense of the generally smaller, less visible ICDs with smaller commercial constituencies.

E. Conclusion

Through the Federal Technology Transfer Act, the Congress created explicit incentives for scientists in the NIH intramural program to engage in technology transfer activities. These incentives supplement the normal reward system of science, providing a second track to the credibility cycle and undermining the technology transfer premise of the social contract for science. NIH has been particularly adept at applying these incentives, in part because the agency had been engaging in its own technology transfer activities prior to the Stevenson-Wydler Act, and also in part because the revolution in biotechnology occurred in parallel to the legislative changes authorizing commercial technology transfer.

The technology transfer programs of OMAR at the end of 1970s and beginning of the 1980s were part of the social contract of science; they went “up to the fence” but did not cross over into politics and did not allow politics to enter. FTTA, however, tore down the fence. One particularly interesting boundary that was breached was that between research interests and commercial interests, which Vannever Bush

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190 Interview with TDC at a small ICD, Bethesda, MD, January 8, 1993.
and John Steelman had hoped to segregate. This research shows that patents are becoming vital parts not only the promotion of technology transfer, but also of the pursuit of research agendas, research materials, research partners, and the psychic returns from knowing that one’s work has had an impact on medical applications.

Not only did the legislation authorize this change in the credibility cycle, but its implementation provided for active roles for nonscientists in areas that had previously been the exclusive domain of scientists. Scientists and nonscientists must collaborate to determine what a discovery or an invention is, so it may be patented. They must collaborate to choose a research partner with whom the intramural scientist can engage in a CRADA. They must collaborate to design a research plan for the CRADA application.

In the process of these collaborations they also attract more political attention in a number of ways. Participants in technology transfer realize that FITA is some kind of industrial policy. Yet they are uncomfortable when they must make their procedures, like selecting a CRADA partner, more formal in order to extinguish the possibilities of ambitions, profits and lusts motivating such selections and pre-empt the political attention that such conflicts of interest would undoubtedly attract. They are also uncomfortable with the fair pricing clause, which may come under increasing scrutiny with the new Administration’s health care policies. Indeed, the biomedical research system is being linked, through FITA and the collaboration of scientists and nonscientists, to the health care system as it had not been before. But this linkage is occurring only through market mechanisms that emphasize only marketable research.

These collaborations are also creating what may become the new indicators of research productivity. In beginning to work on technology transfer legislation in the late 1970s, members of Congress were already paying attention to patents. With Bayh-Dole and FITA, they now have more reason to examine the patenting record of research in the national laboratories and in the universities. They can also measure
licensing income from these patents, and the number of cooperative research
agreements. In testimony, agency representatives already present such data as
indicators of their activity and commitment. Although people at NIH fear that the
research programs will be penalized for their success by having their budgets reduced
according to the money they bring in through licenses and CRADAs, the opposite
seems rather more likely: that research programs with a comparative advantage in
technology transfer will be emphasized by return-conscious politicians, and the
programs and institutes that are more commercially relevant will prosper, perhaps at the
expense of those that are not. Coupled with the limiting of technology transfer to
market mechanisms, the “privatization” of intramural research is again a possibility, but
in a slightly different way.

Conflicts of interest, fair access, fair pricing, and the existence of potential
indicators are all political elements that are newly and clearly associated with the
conduct of intramural research. By engaging in boundary work that attempts to define
CRADAs by scientific rather than financial criteria, and as scientific projects rather than
industrial policy, technology transfer people at NIH hope to avoid the politics of
procurement and industrial policy. But by engaging in boundary work that produces
indicators, NIH invites political scrutiny, political analysis, and political influence on all
of the activities that produce these indicators. The boundary work thus attempts to limit
political influence when it can, but because of the opportunities provided in FTTA, and
because the activities of the scientists themselves in their pursuit of research materials
and research partners produce patents and CRADAs, the boundary work also invariably
extends political influence.

Yet in the midst of all this damage to the separatism of the social contract for
science, scientists at NIH are also being granted more opportunities to exchange
knowledge, materials, and research with a broader segment of the scientific
community. By formalizing some aspects of scientific work, politics has allowed
scientists to engage in a freer exchange of freer intellects. This interaction is also
mediated by nonscientists. Just as the destruction of the old social contract for science
in the misconduct case led to a new relationship in which politicians helped to ensure
the integrity of science, so too is a new relationship evolving in which politicians help to
ensure the productivity of science. FITA seems to have created a relationship that both
constrains and extends science as a sphere embedded in social relations.
VII. Conclusion: science and technology policy in the nonmodern world

A. Introduction

The way that science is governed in the United States has changed. The contemporary government of science involves more monitoring, more incentives, more apparent linkages to political goals, and more involvement by nonscientists in what has traditionally been the domain of scientists. In a word, the government of science has become more political. It was surely political before, but the set of changes examined in this dissertation are discrete because they involve the attempted manipulation of politics within what had been defined as a scientific sphere devoid of politics.

I have investigated these changes by focusing on two aspects of the relationship between Congress and the National Institutes of Health (NIH) in the 1980s: (1) the integrity of scientific research; and (2) the productivity of scientific research. Prior to the 1980s, the integrity of science was not subject to political scrutiny. The relationship between government and science was structured on the belief that scientific integrity was a question for scientists alone to address within the context of a self-regulating community. In the 1980s, it became increasingly clear to certain members of Congress that scientists alone could not manage the integrity of their enterprise. Subsequent congressional efforts instigated the creation of an Office of Scientific Integrity (OSI) at NIH to monitor, investigate, and adjudicate questions of scientific misconduct. Similarly, in the 1980s, Congress took a new tack in attempting to assure the productivity of science. As the economy slowed through the late 1970s, no longer could the simple model of a free and automatic flow of technology from federally funded research be assumed. Technology policy legislation created incentives for federally funded scientists to contribute more demonstrably to technological innovation, and NIH created an Office of Technology Transfer (OTT) to administer these incentives.
The research presented in this dissertation has concentrated on the creation of these two new institutions, OSI and OTT, and the monitoring and incentives they implemented, from both the congressional and the bureaucratic perspectives. OSI was formed by an act of bureaucratic pre-emption. Representative John Dingell, who had investigated and publicized not only the poor performance of scientists, universities, and NIH in maintaining scientific integrity, but also the discrepancy between this performance and the promise of self-regulating science, wanted to legislate an office of research integrity. He failed, but NIH director James Wyngaarden established OSI anyway. OSI then entered the stream of debate over defining and adjudicating misconduct in science, a debate which eventually shifted the locus of control of both the definition of misconduct and the procedures to adjudicate it from norms and peer groups to administrative law. Nevertheless, this shift was propelled by criticism from scientists themselves.

OTT was formed by NIH to help implement the Federal Technology Transfer Act (FTTA) of 1986. Congress had recognized in the Stevenson-Wydler Act of 1980 that government-owned, government-operated laboratories such as NIH needed internal organizations to facilitate technology transfer. Although NIH already had an Office of Medical Applications of Research, the increasingly commercial nature of biomedical research through the 1980s reinforced the congressional intention that government laboratories contribute to technological innovation for economic development. FTTA allowed civil service scientists to share in royalties derived from their patented inventions, and allowed them to engage in cooperative research and development agreements (CRADAs) with private research laboratories, thereby supplementing their own laboratory resources. NIH created OTT to implement these new responsibilities under FTTA. Applying the incentives requires the collaboration of scientists and nonscientists in areas of traditionally scientific activity such as defining a research plan and selecting a research partner. These collaborations also produce potential indicators
of scientific productivity, such as the number of patents and licenses, and the amount of royalty income.

This research informs three related (and increasingly abstract) levels of analysis in the dissertation: (1) congressional-bureaucratic relations; (2) the social contract for science; and (3) the relationship between science and politics. The details of the relationship between Congress and NIH extend the discipline’s understanding of the “two-way street” between congressional control and bureaucratic independence. They also signify a change in the more general attitude toward science, from a social contract for science that specified sharp boundaries between a political sphere and an apolitical, scientific sphere, to a new relationship with boundaries blurred by links between politics and the political within science. This change, in turn, characterizes the newly recognized embeddedness of science in social relations and the problem of science and technology policy in the nonmodern age. The remainder of this final chapter will discuss the conclusions that can be drawn from this research for each of these three levels of analysis.

B. Congressional-bureaucratic relations

1. Conceiving of Congress and science

In the introduction to this dissertation, I argued that political science has largely ignored science as an object of study except to the extent that it has examined the impact on politics by the exogenous force of science and scientists (I.B). The handful of studies on congressional-bureaucratic relations involving bureaus with scientific expertise—which could in principle address the reciprocal impact of politics on science—have concentrated on bureaus, such as the former Atomic Energy Commission, with primarily economic or regulatory rather than scientific orientations. By studying congressional relations with NIH, the mission of which is to perform basic and applied
research in support of the public health mission of the Public Health Service (PHS), I have extended the study of congressional-bureaucratic relations to a new domain.

I have also conceptualized the congressional-NIH relationship in terms of principal-agent theory, and the two cases of the integrity and the productivity of science as problems of moral hazards. The principals in Congress rely on their agent-scientists to conduct their research honestly and in a manner that contributes to technological innovation. The reliance of science policy practice and analysis on literal grants and contracts and on the social contract for science warrants further pursuit of the moral hazard and adverse selection aspects of science and technology policy.

When the primary relationships between Congress and the research agencies were established in the wake of World War II, the integrity and productivity of science were articles of faith exemplified by both *Science: The Endless Frontier* and *Science and Public Policy*. These articles are also the premises of the social contract for science (II.C.3). But as a map to the social reality of the late 1970s and 1980s, the social contract for science was not sufficient for members of Congress. Based on interests that had been aligned forty years in the past, it was too simple an agreement for the complexity of both politics and science. Congress and NIH proceeded to build the monitoring and incentive structures expected in a principal-agent relationship that had been missing from the social contract for science.

That these structures were created is at least a minor testament to the ability of Congress to apprehend, to critique, and to influence bureaucratic structure and performance. Yet the control derived from the creation of these structures was limited by negotiations in agencies over implementation, as well as by the limited ability of the Congress to address smaller policy currents in the context of a large volume of more turbulent issues. These limitations to congressional control also elucidate some of the limitations to the standard principal-agent approach to congressional-bureaucratic relations as well.
2. Congressional tools and techniques
   
a. overseeing a scientific bureaucracy

The scientific nature of NIH did not prevent congressional committees from successfully overseeing bureaucratic operations in assuring the integrity of science; nor did it prevent Congress from legislating changes in the reward system of science through the technology policy acts of the 1980s. As the Fountain inquiry into NIH accountability demonstrates, congressional oversight of bureaucratic science is not unprecedented (II.C.4.c). But Representative Dingell's oversight of scientific integrity pried open new areas of scrutiny by using financial accountability as a rationale to investigate the political nature of the relations among scientists. Similarly, the War on Cancer is just one of many attempts to direct scientific output (II.C.4.d). But in the technology policy acts of the 1980s, legislation reached into the reward system of science by making technology transfer a laboratory mission and by rewarding scientists directly for measurable contributions to technological innovation, and the way science is now being done is consequently changing.

Explaining why this influence is not deterred by the scientific nature of NIH is an interesting challenge, but as shown it involves a number of factors. The scientific bureaucracy is not an “overtowering” Weberian bureau or a dominant bureau of Niskanen's analysis because Congress has created a number of instruments that gather data and translate bureaucratic practices and products into information for members of Congress. For example, investigative reports by GAO, OIG, and the Secret Service provided fodder for Dingell, and staff resources through agency personnel on detail, the AAAS congressional fellows program, and general increases since the Legislative Reorganization Act of 1970 strengthened legislative committee oversight, allowed Dingell to conduct his own informed and well-orchestrated investigations. A second reason was that both cases occurred in a context that encouraged members to use these resources for the fundamental reexamination of the basis of policy. The state of the
economy had reached crisis proportions: the "moral equivalent of war" encouraged more government experimentation and intervention in the economy (V.C.2); and the "politics of fiscal austerity" made oversight and the elimination of waste, fraud and abuse in any area attractive legislative endeavors (III.C).

The third and perhaps most important reason why the scientific nature of NIH did not limit effective congressional scrutiny or legislation, is the fact that members of Congress were able to identify the politics within the science that both legitimated congressional involvement and provided fulcra from which to move policy. Representatives Dingell and Weiss identified politics hiding beneath a veneer of professional norms: researchers, universities, and agencies acting out of self-interest, even out of ambition, profit and lust, rather than pursuing the truth with a skeptical and disinterested mind (III.D.2). The technology policy legislation identified just this element in science as well: it sought the intersection of self-interest and innovation--patents--and leveraged policy at that crucial point. The political strategy, in effect, converted science into any other kind of politics.

b. logical action

The logic of the principal-agent relationship was played out with precision in congressional activities studied here. Members of Congress responded to the moral hazard of fraudulent research first by reminding the scientific community of the necessity to respond to its professional norms. But in their continued investigation of cases of alleged misconduct (generally brought to the attention of their committees by whistleblowers--or fire-alarm pullers), Representatives Dingell and Weiss articulated a coherent and compelling critique of the supposed self-regulating system of science, publicizing the unreliability of professional norms in maintaining the integrity of science. They were assisted by the full panoply of congressional surveillance and information devices that reduced the information asymmetry between the legislative principal and the expert agency. They responded a second time by attempting to
legislate an office in a position administratively superior to NIH, but failed for reasons unrelated to scientific integrity. Yet this congressional activity, even in the absence of legislation, prompted NIH to establish an office that would monitor scientific and investigate and adjudicate cases of misconduct.

Congress (and the President) also responded to the moral hazard of unproductive research. The technology policy legislation of the 1980s created an incentive system that, in effect, gave scientists commissions for performing research leading to technological innovation. The legislation encouraged scientists to “ply” their research in an attempt to “skew” it somewhat more toward the practical by extending patent rights to them and sharing royalty revenues with them (V.D.3). Not only did the legislation create incentives for individual scientists to engage in such conduct, but it created in the aggregate a set of indicators that measure how research institutions take advantage of the opportunities provided by these incentives, and that could measure the productivity of scientific research. Implementing the legislation provides readily available information for its evaluation (VI.D.4.e).

3. Extent of bureaucratic independence

   a. pre-emptive strike vs. anticipated reaction

Congressional intervention, however unprecedented, logical and informed it may be, does not necessarily translate into congressional control or dominance. For example, agencies can engage in pre-emptive action that is distinguishable from an anticipated reaction dominated by congressional preferences. Dingell failed in his initial legislative attempt to create an office of research integrity. Why he failed is a complicated story, the account of which differed between the press and participants. Yet NIH director Wyngaarden created OSI in a “pre-emptive strike” (III.D.4). How does preemption differ from anticipated reaction? As with the military origin of the metaphor, preemption may not be one’s absolute preference, but it is closer to one’s preference than the other available options--capitulation or waiting to fight off the
attack. Anticipated reaction seems closer to capitulation. More perfect information about the preferences of legislative principals is required for an anticipated reaction than for a preemption: in order to react the way the opponent desires, one must know what the opponent wants. Preemption, in contrast, is satisficing behavior. An OSI, under Wyngaarden’s jurisdiction, was preferable to allowing Dingell to create an office, or allowing the OIG to gobble up jurisdiction. Dingell did not create OSI; nor did he force Wyngaarden to do it. Dingell created a strategic environment for Wyngaarden in which some action was preferable to no action. Nor did Wyngaarden create OSI to suit Dingell, although he wanted to satisfice him as well. Wyngaarden behaved according to a reasonable decision rule for limited information.

b. boundary work and implementation

The details of implementation, particularly the boundary work that continues even and perhaps especially after legislation, also intercedes between congressional action and direct bureaucratic response. This finding is by no means novel, but I think this conceptualization may be. The principals make legislative decisions about the extent of their influence—to which scientists the incentives apply, how large the incentives should be, what procedures need to be followed for implementing the incentives (i.e., establishing CRADAs or licensing inventions). These are first-order decisions about the placement of the boundary between politics and science (or any other aspect of bureaucratic or expert production). But for reasons at best indirectly related to the legislation, the reach of politics is extended or contracted. Intramural researchers get a sense from the NIH administration that technology transfer is important; others feel that it will kill basic research. Technology development coordinators are more or less ambitious, more or less deferential to scientists, more or less able to devote time and resources to recruiting scientists for and educating them in technology transfer. Rarely if ever are technology transfer activities incorporated into performance reviews for researchers, contrary to the plain language of FTFA. The
incentives, even though they are prescribed by Congress, are self-administered by the agents and are marginal (in the economic sense) to their other rewards. Despite making little reference to congressional action, this boundary work expands or contracts the extent of congressional influence.

c. nonlegislation vs. legislation

It is tempting to compare the two cases of the integrity and the productivity of science in order to compare the extent of congressional influence in cases without and with legislation (respectively). Although I do not believe that any systematic conclusions may be drawn about this question from the dissertation, a few interesting observations may be made. In terms of outcomes, both kinds of activities led to the same ostensible result: the creation of new offices of similar size in their early days of 1989 and similarly situated in a controversial boundary issue. But the two offices could not have been adjudged any more differently: OSI, without a legislative foundation, came under scathing attacks from critics without and within NIH. OTT, on the other hand, has helped make NIH an acknowledged leader in federal technology transfer.

Can the differing success be attributed to the legislative basis for OTT as NIH’s Office of Research and Technology Applications (ORTA) under Stevenson-Wydler? The legislation, it would seem, has given the technology transfer program at NIH some measure of legitimacy that the scientific integrity program has lacked, and has had to seek, for example, in the PHS Advisory Committee on Scientific Integrity (see IV.C.7). But even more than providing legitimacy, the legislation seemed to provide technology transfer with a constituency. That is, the technology policy legislation created property rights (or more accurately reassigned them) and thereby created

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1 See Jasanoff (1990) for the role of advisory committees and regulatory peer review in political legitimacy of agency decisions. This advisory committee advised the Office of Scientific Integrity Review, which was the department office that oversaw OSI; but as OSI encountered continuing trouble before its demise, it too was considering establishing an advisory committee.
interests and people willing to act on them. When NIH tried to issue regulations on conflicts of interest, for example, public comments emphasized apparent contradictions between the issued regulations and the technology transfer legislation. This opposition to the proposed regulations drew from a wide variety of professions whose members had coalesced around the new proprietary interests in federally sponsored research (VI.D.4.d). This interest group is OTT’s constituency.

Clearly it is harder for an overtly regulatory office like OSI to develop a constituency than for a service- and production-oriented office like OTT. But those characteristics of the offices are constructed: both offices have regulatory or constraining functions and both have productive or enhancing functions (see VII.D below). In other words, an OSI could have been created that emphasized the integrity of the scientific literature and other scientific institutions rather than this one that emphasized individual investigations. Such an OSI could have inherited a constituency from congressional action, or could have built one had this role been prepared for them. Conversely, an OTT could have been created without its constituency, or the OTT created could have alienated or ignored this constituency.

A significant part of creation these constituencies lies beyond the substantive area of the issue. Although it does seem to be easier to create constituencies through property rights when there are commercial products potentially involved, the court in the Abbs challenge to OSI’s policies and procedures could have found that scientists do have the property rights in grants, reputations, faculty positions, etc., that Abbs claimed, or Congress could have assigned such rights (see IV.D.5). The debate and consensus-building prior to legislation, and the legitimate closure of this debate represented by the legislation itself, are important in constructing the constituency as well. This observation that the legislation process is construction, however, leads back to the issue of legitimacy. No congressional instigation or bureaucratic preemption--which appear as personal, arbitrary, and private actions--can substitute for the
impersonal, considered, and public technology of legislation. The legitimacy drawn from this mechanical character of legislation may in fact be one of the important differences between congressional influence with and without laws.

4. Principal-agent ethnography

As often described in the literature on congressional-bureaucratic relations, the logic of principal-agent theory is robust but its mastery of empirical details is thin. Detailed interview data, a "principal-agent ethnography," can put the flesh of politics onto the dry bones of the theory (III.B.4). The principal-agent ethnography shows legislators trying to act like principals by creating bureaucratic "screwdrivers" and using scientists as policy "tools." It shows them being more successful with the information produced for them by expanded staff resources and special information agencies. It also shows bureaucrats acting like agents, but also within limits of information and their own preferences, not just for budgets but for procedural jurisdiction, for example, over the monitoring of scientific integrity. The principal-agent ethnography discovers what information the actors involved had at their disposal, and what they did with it, rather than relying on proxy data or assuming that bureaucrats have information that is complicated or ambiguous.

The principal-agent ethnography also shows, however, that even when the form of the principal-agent relationship is satisfied, control does not follow. One problem is that of watching the watchers: does the OIG play by fair rules in monitoring NIH for Congress, and can Congress determine if the OIG is monitoring properly (III.D.3)? A second problem is the layers of organization across which bureaucratic slack may be cumulative—the iterated principal-agent problem. Signalling NIH through hearings or

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2 I call it a "technology" of legislation to emphasize these impersonal, considered and public characteristics of the activity. Such visible activities have greater legitimacy in liberal-democratic culture (Ezrahi 1990). Science and technology support liberal-democratic culture in part by making hidden relationships visible, accounting for causes so the causes may be held accountable. In this sense, for example, political science may render the bureaucratic strategy of preemption visible, accounting for the creation of OSI, so public judgment may hold responsible actors accountable.
draft legislation about congressional preferences may elicit a response from “factions” at the bureau (III.C.2). But it is not the same as ensuring that a particular hierarchy will take form or that a particular definition of scientific misconduct will be implemented—even if the hierarchy and definition are written in bills and passed by Congress—because there are more bureaucratic layers that can be and are less responsive to Congress. Instigating the pre-emptive creation of a centralized monitoring office for scientific integrity is not defining what misconduct is; attempting to authorize an office and a definition of misconduct is not specifying the adjudicatory policies that the office should adopt. Instructing the national laboratories to make technology transfer part of their missions is not the same as mandating that technology transfer activities be incorporated positively and negatively into the evaluation of federally employed scientists; giving the scientists incentives to engage in tech transfer is not the same as giving incentives and resources to the TDCs to assist them fully. This organizational iteration means there is always another round of bureaucratic interpretation and implementation, regardless of the frequency or formality of congressional intervention. The iteration continues to the street-level bureaucrat (Lipsky 1980), in this instance the scientist at the bench. Boundary work in implementation is the work that determines at which iteration and with what increments the political influence waxes and the expert influence wanes.

A third problem revealed by the principal-agent ethnography is that the information “asymmetry” appears not as unbalanced as supposed. Bureaucrats can be uninformed about legislative preferences and activities to an extent that affects their reaction. They can make attempts to clear actions with the Hill that go without comment. And although they may not engage in anticipated reactions, they still use congressional prompting to help set their agenda, presumably so they can pre-empt any likely, adverse congressional action. Uninformed bureaucracies can lead to two results: the possibility of greater direct congressional control; or poor attempts at pre-emption or
anticipated reaction that lead to more conflict. An even more complete "principal-agent ethnography" would closely examine the bureaucratic information devices, e.g., legislative liaisons, with which the presumed agents monitor their principals.

These features revealed by the principal-agent ethnography—the general form of the principal-agent relationship, the lack of control due to iteration and implementation problems, and the information problems that the agents may also share with the principals—suggests the possibility that transposing the assumption of correspondence between "principal" and "Congress," and between "agent" and "bureaucracy" might be a fruitful line of inquiry. For example, if it is the case that the legislative principal establishes information agencies to keep tabs on the bureaucracy, to assist it in getting what it wants out of its agents, then is the bureaucracy not acting like a principal when it implements policies to produce certain indicators that will assist it in getting what it wants out of Congress? Since Congress wants those indicators as well, this mutual interest would lead to a more cooperative interpretation of the relationship than the standard conflict between bureaucratic dominance and congressional dominance encouraged by the literature. Perhaps it is even a more interesting problem how the bureaucracy structures its relationship with Congress, without legislative authority, now that Congress has large-scale information, analysis and surveillance systems at its disposal. The "two-way street" between Congress and the bureaucracy as described in this dissertation exists and is well-travelled. The traffic is probably even heavier than seen here.

C. Social contract for science

1. The old social contract for science

This dissertation has argued that there was a social contract for science. It held that the federal government will provide resources to the scientific community and allow the scientific community to retain its decisionmaking mechanisms, and in return
expects forthcoming but unspecified technological, economic, and other benefits. It represented a postwar consensus about the nature of science and therefore about the nature of science policy. Identifiable in both major postwar reports on the federal role in research, the social contract for science was a tacit acceptance of and faith in the integrity and productivity of science. Given the postwar enthusiasm for science and technology, it was a low-cost alternative to any more specific guarantee of scientific integrity and productivity. Its acceptance and faith were in turn rationalized by a model of science as a self-regulating system similar to an economic market and a model of the relationship between scientific and technological innovation as automatic and free. It was a contract in that a quid pro quo—the separation of science from politics in exchange for technological benefits—was desired by both contracting parties. But it was social in that the mechanisms underlying this exchange—self-regulation and spin-off—were implicit and automatic.

Although it was challenged episodically over the years since its creation, the social contract for science was not fundamentally damaged by loyalty tests, by inquiries conducted by the Fountain Committee, by the War on Cancer, or by the controversy over the safety of recombinant DNA research. These episodes failed to overturn the tacit acceptance of self-regulation and spin-off because they failed to formalize any monitoring or incentives in the integrity or productivity of research and because they failed to touch the politics internal to science. However, the establishment of the formal principal-agent relationships that touch the interests internal to science corresponds to the demise of the old social contract for science.

2. Revisiting the two premises
   a. the self-regulating scientific community

The old social contract had been elaborated around a premise that the scientific community has a market-like organization that, among other functions, served to deter misconduct and catch and correct any failures in maintaining standards of scientific
integrity. After hearings by Representative Gore and by Senator Hatch, legislation instructed NIH and the biomedical community to shore up their internal mechanisms for ensuring the function of this self-regulating organization. But later hearings by Representative Dingell “deconstructed” the premise of self-regulating science. He showed via his subcommittee’s investigation that claims to self-regulation really masked self-interest, power, and conflicts. In creating OSI, NIH director Wyngaarden was trying to preserve the social contract for science by creating a mechanism that, even if it did not satisfy the market model of science, would at least be operated by scientists in a scientific mode.

Controversy over the definition of scientific misconduct and over the procedures used to adjudicate cases of misconduct, however, led to the failure of Wyngaarden’s attempt to preserve the social contract for science. With a variety of repertoires that plausibly defined scientific misconduct, scientists effectively lobbied NIH to select a definition that emphasized the intent of the accused rather than norms or standards of scientific conduct. This emphasis allowed the forfeiture of demographic dominance by scientists—for only scientists could define the norms and standards of scientific conduct, whereas scientists have no privilege over determining the intent of a person (IV.C). Similarly, scientists pressured for the adoption of a “legal-adversarial model” of adjudication over a “scientific dialogue model” and thus adopted a process emphasizing administrative law rather than a peer review (IV.D). Formal monitoring and procedures, instigated by Congress but assisted by scientists, thereby replaced the implicit reliance on scientific integrity under the social contract for science.

b. automatic and free technology transfer

The second premise about which the social contract for science had been elaborated was that the self-regulating scientific community would produce the best science, leading automatically and without political intervention, to technological innovations. The premise could be maintained, regardless of whether the underlying
mechanism of technology transfer actually followed the linear or the spin-off model, as long as the prediction of a robust and technologically superior economy held true. But the economic prediction began to fail in the late 1970s, and Congress passed legislation designed to intervene in technological development at points other than simply the funding of research—including intervening in the reward system of science (V.C).

NIH had been sensitive to concerns about technology transfer, but it had also attempted to maintain the "fence" between politics and science, health care and research. The predecessor to OTT adhered to this "fence" model of the social contract for science (VI.B). But the technology transfer policies tore the fence down and linked intramural research with commercial biotechnology and, through these firms and their products, to health care. These new linkages are mediated by administrators who are concerned about their potential effects on the research performed at NIH. But the administrators are also sensitive to political demands for demonstrable productivity, and they are aware that the technology policy legislation is an industrial policy as well. The linkages are, after all, largely contingent on the marketable potential of the research. In encouraging such linkages to be established, Congress recognized that, contrary to the premise of automatic and free technology transfer in the social contract for science, technology transfer instead had to be legislated, administered, marketed, licensed, and rewarded. In order to forge these linkages in each instance of technology transfer, scientists and nonscientists must cooperate on a wide array of tasks including some that have been traditionally defined as scientific (VI.D).

3. The new social contract for science

a. arriving at a new social contract for science

The creation of OSI and OTT and their implementation of policies for scientific integrity and productivity marked the end of the social contract for science as it had been established in the immediate postwar period. The social contract for science did not have to die with the creation of OSI and OTT, however. That is, because of the
flexibility in the principal-agent relationship described above (VII.B), the congressional instigation of monitoring and incentives could conceivably have been implemented in a fashion that preserved rather than eroded the social contract for science. This was, in fact, the intention of NIH director Wyngaarden in creating OSI in the office of the director, where it could be staffed by scientists and run in a manner not only consistent with but one incorporating scientific ideas of defining and adjudicating misconduct.

Indeed, if it had not been for the controversial veto of the NIH reauthorization bill that contained the legislative authorization of OSI and a definition of misconduct—as well as the fetal tissue provision which drew the veto—such an office might have resulted. To a certain extent, the question is still being negotiated at OTT, but the process of the collaboration of scientists and nonscientists to produce potential indicators of the productivity of science seems already well-entrenched, and the problems of fair access and fair pricing are being handled with the clear idea that technology transfer has reached across the fence into industrial policy and health care.

If we believe that the old social contract has been eroded or superseded, what is the character of the relationship that has been built in its stead, or replaced it? Is there a new social contract for science?

The new relationship is not radically different in the sense that the integrity and productivity of science are no longer important to Congress or society generally. In fact, with continued economic stress and higher expectations for the contributions of science and technology in the new Clinton Administration (Clinton-Gore 1993), they seem to be even more important. The new relationship could conceivably be called a contract because it too maintains a quid pro quo: support is still given to scientific research in the expectation of technological benefits. But the new relationship has been altered. "Social," rather than meaning "implicit," now means "through monitoring, incentives, and other social interactions between politics and science." In other words, a new social contract for science could be written as follows: the political community
agrees to provide resources to the scientific community and will assist and reinforce its decisionmaking mechanisms and in return expects forthcoming and measurable technological benefits. Compared to the old social contract for science, the new one emphasizes the direct participation of the political partner in achieving the terms of the contract, and it highlights the expectation that performance of the terms will be apparent rather than promised.

b. defining a client?

The old social contract for science differed from the social contracts for the professions like law and medicine in that the former was supposed to be a necessary condition for the advancement of science and the progress of technology; whereas, the latter types were merely the best arrangements to service a clientele in a Liberal, pluralist state (I.A.2.c). The profession of science also supposedly differed from other professions in that it lacked just such a clientele (II.B.3.c). But the changes that have led to the new social contract for science can lead to rejecting both suppositions.

As described above, the integrity and productivity of science are important for both the old and the new social contracts for science. One of the differences between the two, however, is that the new social contract for science displays how important the integrity and productivity of science are for the political partner to the contract. The integrity of science was important enough to Senator Hatch and Representatives Gore, Dingell, and Weiss to become a prominent item on their oversight agenda and to inspire legislative activity. For Dingell, in particular, there were certainly potential targets of his subcommittee that could have yielded a larger return in the waste, fraud and abuse account, or a higher ratio of rascals exposed per investigator-hour. What Dingell and his colleagues determined by their inquiries, however, was that the self-regulatory system of science did not protect the integrity of science well enough for congressional purposes, which for example included protecting controversial science such as AIDS research that supported their policy goals. In the case of the productivity of science,
members of Congress determined that the automatic and free mechanism of spin-off
was not productive enough for their purposes, either. Those purposes are much
clearer—the strength of the economy and thus their jobs as protectors of prosperity.

The identity of the purposes is secondary to their existence. Once these
purposes exist, and once members of Congress act on them, science has lost its status
as the profession without a clientele. Congress is no longer the patron of science, but
its client, at least with respect to the issues of integrity and productivity, which are
central to any professional relationship. Instead of paying the scientists to do
whatever it is that scientists do, and hoping that some good comes out of it, the
Congress-as-client has begun to pay scientists on a contingency basis through the
technology transfer legislation. Such contingency payments are still only a small
marginal percentage of total payments, but the analogy holds. Similarly, instead of
relying on scientists to check on and regulate the conduct of themselves and their
colleagues, the Congress-as-client has encouraged the creation of organized and
legalized regulatory bodies. The new clientele relationship still exists with the prior
patronage relationship in the middle ground between entitlement and procurement, but
clientelism is closer to the latter than is patronage.

c. linkages

Representative George Brown suggested in the passage quoted in the
introduction (I.A.1) that the current problem of science policy is to envision and create
the linkages between science and society that will become the supporting structure for
the new social contract for science. I have argued that, beginning in 1980,

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3 Congress stands as an immediate surrogate for the client of the larger society as well.
4 Indeed, there is legislation pending in the Senate that would specify that the national laboratories of
the Department of Energy would have to spend some 20% of their appropriations on cooperative
projects with industry such as CRADAs. See Branscomb (1993 forthcoming).
5 Also recognizing just this situation, even NAS (1992) recommended the creation of a Scientific
Integrity Advisory Board, modelled to some extent after organizations like the National Accounting
Standards Board, which would centralize scientific integrity activities under the auspices of a more
established organization such as the National Academy of Public Administration.
policymakers and scientists have been creating these linkages: to administrative law in the case of the integrity of science; and to commercial technology innovation in the case of the productivity of science. To the extent that these linkages have already been created, the science policy system is already operating under a new social contract for science. The current problem of science policy may in fact be to envision and create more linkages. But it is also to recognize three events that have already occurred: 1) the old social contract for science has eroded; 2) it has already been replaced by a new social contract for science as outlined above; and 3) the details of the new social contract for science are being negotiated at the margins of the science policy system among scientists and nonscientists who are themselves forging those links and defining the new boundaries.

Even though the linkages to administrative law that are being forged to maintain the integrity of science helped overturn the old social contract for science, they are inconsistent with the model of science that underlies the new social contract for science. This model recognizes the politics within the sphere of science—a realm where administrative law rather than peer review or reputational effects applies. But the linkages developed to define and adjudicate scientific misconduct are based on the explanatory repertoire proposed by Charles Babbage (IV.C.1.a and IV.C.7). This repertoire, even as it reduces the demographic dominance of scientists by removing them from their status as peers to a status of expert witness, minimizes the ostensible role of interests and politics within the scientific community by attributing misconduct to psychopathology rather than anomie, socialization, or a general failure of honesty or duty. This repertoire may protect science against some intrusions and also against some internal strains because its determination of who is unacceptable can be very

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6 That new linkages exist has been noticed in policy analyses, e.g., GUIR (1986), but the linkages have not generally been cast as a significant part of the alteration of the entire relationship.
7 This is the conclusion of the Carnegie Commission on Science, Technology, and Government (1992), although the report gives very few concrete recommendations for how those linkages can be created rather than how the social goals may be determined.
public and very severe. But the retention of this contradiction presages continued problems for the scientific community, particularly with issues of conflicts of interest, which the Babbage repertoire does not recognize as a difficulty for the integrity of science.

The linkages being formed regarding the productivity of science are more cognizant of political forces and, for example, more sensitive to conflicts of interest because of the directly commercial nature of technology transfer. The vast majority of CRADAs at NIH occur with profit-seeking firms, and participants in technology transfer suggest that although scientific criteria rank high in creating CRADAs, they are generally not formed unless there is a strong expectation of a commercializable product at the end of the line. Commercial potential is now also becoming a device for prioritizing invention reports for possible patents from intramural research. This emphasis on commercial technology transfer means that the linkages that have been established between science and society have been forged primarily with a portion of society known as the market, with its own set of values that can feed back into the research. This linkage appears to the partial—since the market is embedded in social relations—exclusion of linkages to other portions of society that embrace other values. Connection to the market is important because it is profit-seeking behavior that is able to provide the actual money for incentives to federal scientists. But the difficulty here lies in the potential conflicts between profit-seeking behavior and the mission of public health.

If there is a new social contract for science that makes the political community, and perhaps Congress, more explicitly a client of a professional scientific community, and that increases the opportunities for linkages between the two communities, what kinds of further policies are likely to arise? The new social contract for science, clientelism, and linkages suggest that science and technology policy will be more active, more formal, and more oriented around boundary institutions and boundary
work. This kind of policy does seem to be emerging, on both the congressional and bureaucratic sides. In Congress, for example, Representative George Brown created a Task Force on the Health of Research in the 102nd Congress to advise his Committee on Science, Space and Technology. The Task Force, itself a kind of boundary institution acknowledging the assistance of more than a dozen advisors from committee and OTA staff as well as from universities, professional societies, private firms, and the National Academy of Sciences, “was convened...to define a more active role for the Committee in the forging of science policy for the national good” (U.S. Congress 1992:v).

Although the tone of its report is relatively critical of the scientific community and the ideas that have undergirded decades of science policy, one major innovation suggested by the Task Force is the shifting of the focus of science and technology planning in the Executive Office from the Office of Management and Budget (OMB) to the Office of Science and Technology Policy (OSTP) (U.S. Congress 1992:14-16). OSTP is itself a boundary institution, a kind of liaison between the scientific community and the White House. It has close informal links with the National Academy and, at least in its incarnation under the Bush Administration, its President’s Council of Advisors for Science and Technology was one of the few (if not the only) group of private citizens that met regularly with the President. Moreover, Brown’s Science Committee also has legislative jurisdiction over OSTP. Shifting science and technology policy from an “OMB-dominated process” to an OSTP-dominated process would not just help create “a more strategic process oriented toward the conduct, goals, and users (not just the performers) of research” (U.S. Congress 1992:14). It would also serve the activist goals of Brown’s Committee by providing it the greater access it would have to OSTP policymaking than it has to OMB policymaking. This access would probably be derived from informal as well as formal sources, as the Science Committee and OSTP are closer to each other and the rest of the science policy
community than is OMB, and as the Science Committee has itself created a committee of private citizens to advise the chairman.

The second major innovation suggested by the Task Force is "integrating performance assessment into the research process" (U.S. Congress 1992:16-18). This innovation is exactly the chore performed by the incentives provided under the technology policy acts of the 1980s. The acts define patents, licenses, and CRADAs as good performance and provide rewards in the research process for researchers and laboratories who perform well by these criteria. The Task Force acknowledges that "Federal policy has been less [than] effective, however, in monitoring the progress of research programs toward particular goals" and catalogues a number of performance problems—including "[d]iscontent between promised and actual performance" and "[l]ack of criteria for measuring the effectiveness of ongoing research programs" that are clearly problems that can be addressed within a principal-agent framework (U.S. Congress 1992:16).

These activities of the House Science Committee make more sense within the context of the new social contract for science, the client relationship, and the emphasis on linkages, that emerges from the analysis of the issues presented in this dissertation. Indeed, these activities seem in this context, as did Brown’s call for a new social contract in the passage cited in the introduction, almost a little late.

D. Politics and science

1. Another view of science and the market

In chapter II and elsewhere, I have referred to the description of the scientific community given by Michael Polanyi as a self-organizing community of freely interacting individuals, requiring autonomy from political interference like the economic free market, as part of the intellectual basis of the old social contract for science. By a perhaps interesting historiographic conjunction, Michael Polanyi’s brother Karl Polanyi
(1957 [1944]) wrote a remarkable treatise on the market economy and its relationship to society. Although the ostensible purpose of the *The Great Transformation* is to account for the collapse of Nineteenth Century civilization and the subsequent rise of fascism from the resulting chaos, Karl Polanyi also provides a rich description of the complex relationship between the supposedly free market and the supposedly *laissez-faire* politics of the era. Whereas, Michael Polanyi’s work invokes an economic model to distinguish science from society; Karl Polanyi’s work is an economic model for the reintegration of science and society.

The intellectual roots of Karl Polanyi’s argument about the connectedness of the market and society are empirical inquiries in history and anthropology: “The outstanding discovery of recent historical and anthropological research is that man’s economy, as a rule, is submerged in his social relationships” (Polanyi 1957 [1944]:46). Markets exist in a plentitude of social environments, but not all of these markets become market economies, yet alone self-regulating economic systems. “It was not realized that the gearing of markets into a self-regulating system of tremendous power was not the result of any inherent tendency of markets toward excrecence, but rather the effect of highly artificial stimulants” (Polanyi 1957 [1944]:57). State intervention, initially in the form of legislation that freed money, land, and labor to become commodities, but later in the form of more complex regulatory measures, provided the necessary stimulation. As a product of social relations, malleable political institutions, and other historical contingencies, this state intervention in shaping markets meant that the “economic system [itself] was submerged in general social relations” (Polanyi 1957 [1944]:67).

Not only was the self-regulating market (to whatever extent the market actually achieved that status) created by and submerged in social relations, but it was eventually destroyed by them as well, Polanyi argues. The self-regulating market demanded that every aspect of society—money, land (Nature) and labor (people)—become an
accessible, mobile commodity. But “[t]o allow the market mechanism to be the sole
director of the fate of human beings [labor] and their natural environment
[land]...would result in the demolition of society” (Polanyi 1957 [1944]:73). Instead,
through a two-fold policy of extending the market and extending forms of social
protection contrary to the market, society demolished the self-regulating market that it
had itself been constructing.

I suggest that much the same can be said for science. Although the idea of a
self-regulating scientific community, as a self-regulating market, continues to have
great currency and normative appeal, historical and anthropological studies point the
way toward a conception of science as itself “submerged in social relations.” Similarly,
there is a complex developmental relationship between science and society that cannot
be squeezed into the reductionist, modernist conception of a self-regulating science and
a laissez faire society.

2. The state and science
   a. regulating science

Under the old social contract for science and its underlying premises that the
integrity and productivity of science were automatically derived from the proper,
autonomous functioning of the scientific community, the only proper role for the state
was as a disburser of funds. Any other activity risked being labeled “regulatory” in one
way or another, interfering with the market of science. Early Cold War security
concerns regulated the interaction of scientists across international borders, as did
export controls in the 1980s (Foerstel 1993). Human subjects protections and animal
care guidelines regulate the conduct of scientific experiments, as did the recombinant
DNA guidelines. Such regulations constrain the objectification of persons, animals,
and the environment in ways similar to the economic regulation described by Karl
Polanyi which constrained the commodification of labor and land. There are also equal
opportunity, drug-free workplace, and hazardous waste disposal regulations that
influence laboratories. All of these regulatory efforts were initially met with opposition, even disdain, from many among the scientific community. To a certain degree, these were some of the strings that Frank Jewett, who had opposed the creation of the National Science Foundation on the grounds that receiving government money would ultimately lead to following government regulations rather than scientific interest, had feared.\textsuperscript{8}

These strings have also been attached in the cases of integrity and productivity in science. OSI's definition of scientific misconduct, the enforcement of which can lead to punishments for offending scientists up to and including debarment and could spur criminal investigations for filing false claims with the government, regulates the conduct of individual scientists. The adoption by professional societies, universities, and other research institutions of codes of professional conduct--in many cases an attempt at "pre-emptive" self-regulation--are nevertheless regulatory of the conduct of individual scientists as well (Steneck 1993). The Public Health Service requirement that institutions applying for grants have in place institutional guidelines to manage misconduct allegations and investigations regulates the institutions of scientists and their local methods for resolving issues of scientific integrity. These regulatory activities also spurred a great deal of reference to and discussion of many of the other regulatory regimes for science mentioned above (Abelson 1991).

Although the application of incentives to encourage technology transfer does not restrain or regulate science in the way that policies to insure the integrity of science do, it nevertheless intrudes on the free market organization of science in at least two ways. The primary concern is that the introduction of proprietary interests into research raises problems of free communication among researchers, for fear of breaching proprietary agreements or inappropriately disclosing proprietary material prior to filing for patent protection. Another concern is that incentives will divert scientific attention and

\footnote{See U.S. Congress (1986c) for a complete overview of the "regulatory environment" of science.}
resources away from fundamental research that will provide the basis for long-term scientific discovery and technological innovation and toward short-term, commercially viable products. Both of these possible results of the application of commercial incentives threaten the "free play of free intellects" (Bush 1980 [1945]:12) in the market of science.

To some extent, the social contract for science encourages the primary consideration of the regulation or constraint of science, in much the same way that the classic political Social Contracts consider how individual conduct must be regulated to prevent bellicose or simply unfair situations in the State of Nature. Again, this was Jewett's concern. He worried that, to abuse Locke (1963:392), if scientists consent to travel on the king's highway, they are obliged to obey the kings laws. Yet obeying the law is only one-half of the social contract, for they also gain the protection of the king from highwaymen.

b. extending science

Just as Karl Polanyi's description of the political economic interaction has two parts--regulating the market and extending the market--so does the description of the "political-scientific" interaction. Congressional and NIH efforts in the integrity and productivity of science have not been merely regulatory or intrusive or controlling. They have also extended science, as the new social contract for science suggests, to "assist and reinforce its decisionmaking mechanisms."

In addition to regulating the conduct of individual scientists, the introduction of formal mechanisms for assuring the integrity of science may also enhance the ability of individual scientists to trust other scientists, because expectations of conduct are now more formal, more clear and, presumably, easier to meet. Being able to trust the results of others is, according to Michael Polanyi and to Harriet Zuckerman, the primary basis of the social contract for scientists (II.B.3.c) The clarification of expectations and their presumed, subsequent fulfillment can also help make the reward system of science
more efficient by assuring that scientists who garner the trappings of success through plagiarism, fraud, or other forms of misconduct are less likely to succeed at the expense of their more scrupulous but less well-rewarded colleagues. By improving the credibility of the credibility cycle (II.C.3.c) that allocates scientific resources, such regulations improve the quality of scientific processes and outputs. The formalization of processes for the adjudication of misconduct allegations may also limit the application of power on an individual and arbitrary basis, as the episodes of misconduct investigated by Representatives Dingell and Weiss suggested (III.D.2). Formalization may also help protect the scientific community from damaging internal tensions, at the invitation of scientists (IV.C.6).

Similarly, the incentives applied to federally employed scientists have extended the market of science primarily by allowing intramural scientists to cooperate with scientists in industry, in effect eliminating sectoral boundaries that had prevented the free exchange of information and materials. Many scientists engage in CRADAs or sign materials transfer agreements in order to gain access to techniques, reagents, or proprietary material that they would otherwise not be able to use (VI.D.4.c). Patents provide proprietary protection for scientists, which they can then use to secure their traditional goals of exchange, collaboration, and priority. Once the biotechnology revolution helped created a large pool of scientific talent, working on top-notch science, in the private sector, the constraints on public-private cooperation effectively cleaved the biomedical research community. But Bayh-Dole and FTTA, by allowing new mechanisms of cooperation, re-extended the community, even as it had the constraining effects discussed above.
E. Science and technology in the nonmodern world

1. The risk of fascism?

One must wonder about the consequences of such political participation in both extending and constraining science. After all, Karl Polanyi describes the collapse of Nineteenth Century civilization, the destruction of its market economy, and the rise of fascism from the rubble of ambivalent political interference. Does this mean that the failure of the old social contract for science will lead to a similar tragedy with respect to science?

That risk, as Robert K. Merton (1973:ch.13) recognized in his impassioned defense of scientific norms, exists. Merton was of course writing about the abuse of science by the same fascist forces, the ascent of which Karl Polanyi explains. That risk is likewise apparent to Gerald Holton (1993), who also invokes Nazism and its anti-science Romantic philosophy in (a perhaps harsh) juxtaposition with the essays of Vaclav Havel and George Brown (1992). Ezrahi (1990:290) also makes the link to fascism, what he calls the “aestheticization of politics,” but he is more hopeful that “America may yet produce a uniquely liberal-democratic variant of aesthetic politics.”

I think the key to understanding the extent of the threat inherent in the embeddedness of science, or science in the non- or post-modern world, lies in two realizations: 1) that the separation of politics and science was an artificial one to begin with; and 2) that even a socially constructed science can still be a stabilizing force for democratic politics. I will not provide a complete argument for either of the realizations, but I will sketch briefly where such argument would go.

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9 Neither Holton nor Ezrahi concede, of course, that anti-science Romanticism needs to be anti-technology; in fact, both realize that technology was vital to Nazi politics. Holton suggests that the Nazis invented the concept of Aryan science, which he suggests is a version of the contemporary belief in the social construction of science, to fill the necessary step before useful technology. Ezrahi views Nazi science and technology as a purely aesthetic type, iconographically represented in the flying machine, which would in some literal sense allow Romantic flights of fancy.
2. The promise of nonmodern science?
   a. the artificial separation

   Arguing that the separation between politics and science was an artificial one to
begin with is a historical argument that would parallel Karl Polanyi’s investigation of
the construction and destruction of Nineteenth Century market economy. In the United
States, it might involve a close study of the history of federal science policy, not
concentrating necessarily on the size of the programs of the government, but rather on
the role of the government in fostering independent scientific institutions, establishing
the preconditions for scientific exchanges, and so forth. It would take the above
section on extending science (VII.D.2.c) and project it backward onto federal science
policy.¹⁰ One particularly interesting concatenation of linkages to explicate would be
the wartime innovations of scientific institutions in the U.S., from the National
Academy of Sciences by the rump Republican Congress of the Civil War, to the
creation of the National Research Council during World War I, to the watershed science
policy innovations of World War II. Such an investigation could also benefit from
developing the Publius-Vannevar Bush analogy (II.C.2.a).

   Shapin and Schaffer (1985) have taken the task of demonstrating the artificiality
of the separation between politics and science in a different direction, to the beginnings
of modern science in Enlightenment England. They argue that the Seventeenth Century
dispute between Thomas Hobbes and Robert Boyle over the nature of scientific
knowledge, which they claim largely invented the exclusive of domain experimental
science, was settled in Boyle’s favor in only a contingent manner. Latour (1991)
interprets Shapin and Schaffer’s account as a “political constitution of truth” in which
science and politics have been defined as separate institutions, sharing only legitimacy.
Latour believes that both the Enlightenment transition and the current one are not

¹⁰ To some extent, Dupree (1957) accomplished this, but his categories are firmly historical rather
than political, social, or economic.
historical, per se, but conceptual. Although we have diligently pursued the lines of the Hobbes-Boyle dispute for three centuries of so-called modernity, we never succeeded in separating the two spheres of politics and science completely—in finishing the argument. “The nonmodern world...is simply the realization that we have never entered the modern world” (Latour 1991:17).

Therefore, we should not despair that expectations of a responsible and productive scientific community and a supportive but distant political community have been dashed. The abrogation of the political constitution of truth—or the social contract for science—buries only its artificiality, that is, its status as an artifice. The possibilities of reconciling interdisciplinary problems such as crime and the environment (Latour 1991:7), or solving the problems of scientific misconduct or technology transfer, lie in the breach.

The branches of politics and science that Hobbes and Boyle separated to open the modern age are now intertwining again in a new, “nonmodern” age in which we accept that both social and technical forms exist among both the political and scientific communities (Latour 1991:16-17). For scientific integrity, this means denying the distinction between social and technical norms in the Merton and Zuckerman repertoires and turning to either a Babbage or Schmaus repertoire. It also means accepting the assistance of administrative law and congressionally inspired regulations in determining misconduct in science and thus the boundaries of good science and the scientific community. For scientific productivity, this means rejecting the facile distinction between market interactions and scientific interactions and realizing that patents propel the professional interests of scientists as well as have an impact on health that publications do not. It also means facilitating the collaboration of nonscientists and scientists in the tasks of planning research, selecting research partners, and identifying discoveries. Recognizing the artificiality of the separation between politics and science permits the conceptualization of new solutions to old problems.
b. stability of social constructed science

The ability to reconceptualize old problems and presumably to solve them allows even socially constructed science—that science which is not clearly differentiated from politics—to provide support and stability to liberal-democratic politics. As Yaron Ezrahi (1990) argues, science provides two types of support for liberal democracies: one is a model of nonviolent and progressive consensus formation; the second is that science clarifies relationships, both physical and social, to allow the kind of instrumental action, and the accounting and accountability, that people expect of their liberal-democratic governments (see I.C.6).

If a reconceptualized vision of a "political" science helps solve the problem of pollution, for example, then the instrumental expectations of citizens should be fulfilled, even more so then without that reconceptualization, because the problem ameliorated is one that was particularly vexing under the old conceptualization. If the instrumental expectations of the citizens are fulfilled with technologies that are themselves the result of some sort of representative process, even one that at the least is encouraged by Congress to assist nonprofit institutions and small businesses and to assure fair prices, then it is yet for the better.

Similarly, it was not the intrusion of politics that led to the politicization of science and that threatens the role of science as a model polity. The politics were always there, unrecognized by society and political science. Once these politics are exposed, as they have been, science cannot be a model polity unless some meliorative action is taken. Instead of allowing the arbitrary, personal, and private collegial system to determine the answers to questions of scientific integrity, politics and science have decided together to allow administrative law—a regular, depersonalized, and public process to do so. Both can only benefit, as science can now be portrayed as a social system that now shares even more of the values of liberal-democratic culture. If science is made more democratic, it cannot but be a better model for democracy.
In other words, it may be the case that our conception of science has changed, but that does not mean that science itself has changed. For Holton's fear to be realized, we must change science because our conception of science has changed. But more than that, we must change science in a particular and ill-conceived way. This dissertation has shown how our conception of science has in fact changed, and how we have changed science based on that changed conception. But my analysis shows that the particular changes, at least in these two crucial cases of the integrity and productivity of science, have not been so ill-conceived. Indeed, although both changes have regulatory aspects, they have in both cases served to reconstruct and extend the scientific community in a way that allows its members to pursue their goals more flexibly and with more certainty. A scientific community with a democratically enhanced system of integrity and a system of productivity better linked to social goals could only better serve the roles of model polity and clarifier.

3. Conclusion

In revealing these aspects of the integrity and productivity of science, this dissertation has demonstrated not only the social embeddedness of science, but more importantly the realization of this embeddedness by authoritative political actors. This dissertation has, in effect, documented the passage of science and technology policy into the nonmodern world.

The social contract for science was not merely a justification of self-regulating science and *laissez faire* science policy. It was more an accommodation of the modern political constitution of truth that separated the political and scientific realms. Science and technology policy in the nonmodern world will be tricky business. Without this accommodation, it will depend more upon the interactions between parties who recognize the new relationship and their colleagues and opponents who do not. The relationship between politics and science will be characterized, not by the separation implied in the old social contract for science, but by boundary-straddling institutions
such as OSI and OTT. It will be characterized, not by disinterested delegation by Congress, but by increasingly critical and competent scrutiny.

The political community still retains its interest in a scientific community with integrity and productivity. To protect its interest, the political community will take more control, and where it cannot control, it will take account. The relationship between politics and science in the nonmodern world will be one that reflects this emphasis on accountability. The new boundary institutions will be called upon by both “sides.” Both “sides” will increasingly integrate their own operations for control and accountability with perspectives and talents from the “other side.” If the challenge in the modern age was implementing the putative separation between the enterprises of science and politics, then the challenge of the nonmodern age will conceptually and observationally distinguishing between two increasingly similar enterprises.


Kohlstedt, Sally Gregory and Margaret W. Rossiter, eds. 1986. *Historical Perspectives on American Science: Perspectives and Prospects,* Baltimore: Johns Hopkins.


_______. 1990b. *Guidelines for the Conduct of Research at the National Institutes of Health*. Bethesda: NIH.


Pinch, Trevor and Wiebe E. Bijker. 1987. "The social construction of facts and artifacts: or how the sociology of science and the sociology of technology might benefit each


Rall, J. Edward. 1990. "Conflict of interest as it relates to intramural scientists at the National Institutes of Health." The Endocrine Society, Atlanta, June 20.


