BETWEEN POLITICS AND SCIENCE

Assuring the Integrity and Productivity of Research

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Science Policy: Structure and Boundaries

> Our survival depends on our ability to judge things by their results and our ability to establish relations of confidence and responsibility so that we can take advantage of what other people know. We could not live in modern society if we did not place confidence daily in a thousand ways in pharmacists, surgeons, pilots, bank clerks, engineers, plumbers, technicians, lawyers, civil servants, accountants, courts, telephone operators, craftsmen and a host of others. . . . Democracy is like nearly everything else we do; it is a form of collaboration of ignorant people and experts.
>
> – E. E. Schattschneider, *The Semi-Sovereign People* (1960: 137).

Introduction

To E. E. Schattschneider's characterization of democracy as a form of collaboration among the ignorant and the expert we might add, so too is science policy. The nature of science policy as a delegation of authority from patron to performer has befuddled both from the very beginning. Ignorant patrons worry about getting their money's worth for their delegation of funds and authority to the researchers. Expert researchers face the similarly unenviable task of performing for patrons who might not appreciate it.

Another way of inquiring about the centrality of delegation in science policy is by asking how nonscientists get scientists to do what we, as citizens, have decided. By focusing on the "how?" of science policy rather than the "how much?" of research funding, this line of inquiry appears to step away from the traditional center of conflict in science policy. It is true of course that budgets are the epicenter of political debate about research and the sites where analysts have found expression of differing political priorities (e.g., Barfield 1982). But science policy analysis often has been too involved in the question of "how much?" to the neglect of the question of "how?" The question of "how?" must be

asked and answered for any answer to "how much?". Moreover, in the developing "steady state" of research funding, "how much?" is a question settled by increments at the margin of an overall budget and "how?" becomes ever more important (Cozzens, Healey, Rip, and Ziman 1990). My account of science policy is concerned with the structure of science policy – with its relatively durable processes and institutions – rather than with its budgets, which may be alternatively incremental or irrelevantly volatile.

The first section of this chapter describes an analytical framework for examining problems of delegation known as principal-agent theory, which has become an important analytic tool for casting the relationship between politics and science.[1] Roughly put, principal-agent theory as applied to science policy means the government is the principal who requests the agent – science – to perform certain tasks because the principal is not capable of performing them directly. The agent performs the delegated task, out of self-interest, but with some of the consequential benefits accruing to the principal as well. Because of the implicit exchange in this delegation, principal-agent theory is also known as ideal contracting theory. The centrality of the research contract or grant, provided by a public institution to a private institution or individual performing scientific research, is prima facie evidence that principal-agent theory should be an important analytical method for science policy. The contracting aspect also hints at some ways of managing the problems of mutuality and of stability across the boundary between politics and science.

An account of science policy must be informed by an account of policy making in general. The institutions of governance were not created to govern science alone. The apparatus of science policy has historically been largely the apparatus of economic policy, health policy, security policy, and so forth.[2] We need some understanding of the broader structure of policy making in order to understand the specific structure of science policy making. Fortunately, principal-agent theory provides such a broader structure because, as discussed below, the problems of delegation are not limited to scientific agents, although they may be exacerbated by them.

An account of science policy must also be informed by an account of science in general. The idea of science as an objective enterprise populated by an apolitical elite informed the first generation of science policy studies.[3] Political scientist Harvey Sapolsky (1975: 79) argued that "advocacy" in this literature "often substituted for analysis," and it consequently failed to produce useful policy instruments or to generate much cumulative scholarship. The contrary idea of science as a political enter-

prise populated by an interested elite followed, promoted by journalistic accounts of the scientific establishment.[4] But this idea overplayed the difference between the ostensibly political behavior of scientists and our expectations of an apolitical science. The role of this literature was its repositioning of science as an interest, but its value was mostly shock value. Its message still appealed to the earlier model: if only scientists would be less venal and live up to their creed, then science policy would be better made.

The second section of the chapter appeals to a better account of science, called constructivism. Derived from techniques in anthropology and sociology and from newer answers to old problems in the history and philosophy of science, constructivism takes an empirical and skeptical – indeed, a scientific – view of science.[5] The constructivist approach casts science as a social activity much like any other occupation or profession, and it provides social explanations for why this particular profession manages to produce knowledge that is reliable. Constructivism is valuable for science policy because, contrary to claims by those who have examined only its margins, constructivist studies of science provide the close, empirical, reasoned, and unvarnished account of scientific work that is necessary for informed policy making.

Additionally, the constructivist perspective on science leads to a helpful perspective on the problem of boundaries between politics and science. For as clearly as principals and agents seem to map on to politicians and scientists, respectively, the principal-agent approach does not exhaustively or exclusively demarcate the conceptual territory they inhabit into politics and science. Constructivism provides a more nuanced approach to the boundary between politics and science straddled by science policy. Indeed, if science were as entirely objective and politics as entirely venal as the early model suggested, then science policy would be impossibly reduced to the simple appropriation of funds and the mindless following of advice. Pragmatism demands the ability to account for institutions of science policy such as those discussed in this book, and constructivism fits this bill. These institutions are the boundary organizations I introduce in later chapters, and they satisfy the need for nuance in science policy by satisfying principals on both sides of the boundary.

Structure: the Problem of Science Policy

In 1884, the eminent geologist, ethnologist, and explorer Major John Wesley Powell appeared before a special commission of Congress investigating the organization of government science. Powell testified to the Allison Commission that because institutions conducting scientific re-

search required constant modifications by those conducting the research, "[i]t will thus be seen that it is impossible to directly restrict or control these operations by law" (quoted in Guston 1994a: 38).

In his testimony, Powell pointed at the primary analytical fact of science policy: scientists know things about the conduct of research that politicians and administrators do not. This fact is too often either used as an apology, as Powell did, for a laissez faire policy for science, or overlooked entirely. The asymmetry of information between those who would conduct research and those who would govern it presents the central problem of science policy. It means the patrons of research have a hard time understanding whether the recipients of their largesse are doing their bidding, and if so, how well. It also means the recipients have a hard time providing evidence of their integrity and their productivity to their patrons.

The asymmetry of information between performers and patrons is not unique to science policy. Rather, it is characteristic of all delegatory relationships. For the purposes of this book, it matters little whether the study of science policy is conceived as merely a subset of the study of delegation, or whether science policy is in some way unique. Any singularity of science policy is likely derived from the position of science in the nexus of claims about a unique epistemological status, the unpredictability of advance, the difficulties in discerning productive consequences, and the delivery of products through such intermediaries as educated students or an external market rather than directly to the principals. Any of these claims, if true, would serve simply to increase the asymmetry of information. Thus, the problem of delegation may loom even larger for the principal of research.

Other scholars have recognized the centrality of the problem of delegation in science policy. In discussing "forms of patronage," theorist Stephen Turner (1990a) has suggested that politicians looking to fund science suffer from a mismatch in the distribution of knowledge and discretionary power. To substitute for the elements of science they cannot grasp, these politicians focus on trust, public attestations, personal relations, and metonyms of overall performance such as financial accountability. Patrons trust researchers based upon their personal relations or their ability to demonstrate adherence to their public statements or standardized rules of accountability. This trust substitutes for the ability of patrons to understand the substance of research and to trace its impact. In a more literary vocabulary, Turner, like Powell, describes the logical outlines of the principal-agent view of science policy.

The advantage of principal-agent theory over Turner's formulation is its facility in formalizing and generalizing discussions of delegation. Bor-

rowing from transaction-cost economics, principal-agent theory takes a contractual approach toward explaining organizations and hierarchies.[6] That is, the relationship between institutions, or among individuals within an institution, can be described as if those parties had entered into a contract specifying the rights and obligations of each party. Such a contractual perspective has metaphoric significance in science policy. Analysts and policy makers often speak of a "social contract for science" as the promise of science "to deliver goods to society in return for its patronage with no strings attached" (Rip 1990: 399). The contractual perspective also has a great deal of procedural significance for science policy because of the centrality of contracts and grants in the actual relationships between sponsors (principals) and performers (agents).

As in more formal models, in principal-agent theory it is important to specify what parts of the model correspond to which parts of the real world. The principal is an actor who requires a task to be performed but lacks the ability to perform it directly. The agent is an actor to whom the principal delegates the performance of the task, or with whom the principal engages in a contract for its performance. In commercial application, clients or consumers of goods and services are the principals, and professionals and other producers are the agents. I cannot grow vegetables, so I delegate the task of providing them to my greengrocer and sign a contract for the delivery of fruits and vegetables. I cannot practice medicine, so I delegate to my physician the task of healing me when I am ill, and we agree to the provision of diagnostic and prescription services. Politicians cannot perform research, so they delegate to scientists the task of investigating the natural world and contract for the performance of inquiries, analyses, innovations, etc. Table 1 provides a summary of such principal-agent relationships.

Anywhere there is a delegatory, contractual, or representative relationship, there is potential to apply the principal-agent perspective. Think of Dashiell Hammett's *The Maltese Falcon*, a story in which the problems of agency plague the characters. Casper Gutman, also known as "The Fat Man" and played by Sydney Greenstreet in John Huston's familiar movie version, is a very rich man who has spent seventeen years pursuing the valuable statuette. He has been betrayed by agents acting on his behalf, including the sultry blonde of many names who tries to seduce the twitchy private detective Sam Spade (played, of course, by Humphrey Bogart). Gutman suspects that Spade can help him retrieve the falcon, but he wonders if Spade is the right agent. Gutman tests Spade by offering him alcohol. Spade passes the test by accepting a full glass from Gutman, who mutters, "I distrust a man that says when. If he's got to be careful not to drink too much, it's because he's not to be trusted

Table 1 *Typical Principal-Agent Relationships*

Principal	Agent
Patron	Performer
Customer	Greengrocer
Patient	Physician
"The Fat Man"	Sam Spade
Voters	Politicians
Politicians	Researchers
Congress	NIH
Congress	Researchers

when he does" (Hammett [1929] 1972: 94). After deciding Spade is the right agent, Gutman must still assure himself that, after he provides Spade with information about the falcon, Spade will not double-cross him and use the information to take the falcon for himself. So Gutman offers Spade a smaller amount of money down, and a share of the profit from the eventual sale of the statuette, the burden of which Gutman will bear.

"The public is like a very rich man," Schattschneider (1960: 139) mused, "who is unable to supervise closely all of his enterprise. His problem is to learn how to compel his agents to define his options." Gutman's problem with Spade is like the public's problem in representative government. Table 2 illustrates how this problem of agency is iterated throughout representative government, as the public chooses representatives, who create executive agencies and delegate authorities to them as the next agent. Executive agencies perform some of the requirements of the delegated authority directly, and they let grants and contracts to other performers, who in turn are their agents.

The delegation from the government to the scientific community is the most abstract principal-agent perspective in science policy. Although at this level there is no single principal or agent, we can still discuss the systems for expressing goals for the relationship and assuring they are being pursued. At finer levels of resolution, we can view particular institutions within those systems: legislative bodies, executive agencies, and other public and private research performers such as universities, firms, and hospitals. The legislative bodies are usually specified as the principals for executive agencies, as well for all of their subsequent agents.

Table 2 *Iterated Principal-Agent Structure of Representative Government*

Principal	Agent
Electorate	Representatives
Representatives	Executive Agencies
Executive Agencies	Grantees/Contractors

That is, the agencies are themselves principals in providing grants and contracts to the performers, but since the funding agencies are agents of the legislature, the performers are also agents of the legislature.[7] At the finest level of resolution, individual principals and agents become visible: the chairpersons of legislative committees, the administrators and professionals in the executive agencies, and the individual researchers. This level of analysis is important for two reasons. First, organizations are not unitary actors, and the activities of principals can provide resources to particular agents within an organization who are more likely to share their perspective. Second, because analysts can sharpen the resolution of the principal-agent perspective to individual relationships as embodied in actual grants, contracts, legislation, policy statements, and other less tangible social relations, the application of principal-agent theory bears little risk of reifying the institutions it examines.[8]

Where the principal is governmental, it usually needs its agents to produce things that the market does not produce in an optimal quantity, for example, the public good of scientific knowledge upon which technological innovation is supposed to be predicated. Without the ability to refer to market prices for the delegated chore, the principal is assumed to be relatively ignorant of the manner and cost of production (Tullock 1966, Niskanen 1971, Turner 1990a). But even where there might be a market, for example, in vegetables, the asymmetry of information between the agent who supplies my vegetables and myself is apt to be large. Keeping track of the growing season, knowing how to identify produce of the finest quality, and other tacit knowledge of greengrocery is difficult for me to learn. Given the challenge of providing technical information and analysis to legislators who usually have a generalist's background, the assumption of an asymmetry of information about research between principals and agents is not hard to justify.[9]

Further, the agent may not share the goals espoused by the principal, but might prefer to conduct research on anything intellectually interest-

ing or personally lucrative, regardless of its technological potential. The traditional research practice of "bootstrapping," reserving funds intended for one project for use in another, usually a more exploratory project, is an example. 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. My ignorance of medicine may lead me to choose a physician who provides me with an intoxicating but otherwise ineffective elixir over one who offers a debilitating but eventually effective treatment.

The potential conflict of goals and the asymmetry of information create two regular problems of delegation known, through terms derived from their original use in insurance theory, as "adverse selection" and "moral hazard."[10] In the adverse selection problem, the principal has difficulty selecting the appropriate agent because of an original lack of expertise or information. It is difficult and costly for the principal to discover which potential agent most completely shares the principal's goals. The classic example is when persons most likely to apply for health insurance are those most likely to require it, thus costing the insurer money. Or the least expensive vegetables may be of the poorest quality or grown with the greatest amount of pesticides. Or a mercenary detective like Sam Spade may not be trusted with a priceless bauble.

In the moral hazard problem, the delegation by the principal provides not only an incentive to perform the required task, but also an incentive to cheat, shirk, or otherwise act unacceptably. It is difficult and costly for the principal to know whether the agent will continue to pursue the principal's goals after the principal has made the delegation of authority. The classic example is the incentive to commit arson that fire insurance perversely provides. Or physicians may perform medically unnecessary procedures, for which they are remunerated, only to make it appear as though they are thorough diagnosticians. Or the Fat Man, in hiring Spade to recover the Maltese Falcon, may have given Spade the opportunity to abscond with the bird.

There are a variety of ways of solving these problems. Indeed, I maintain that how the relationship between government and science is structured to solve these problems is the most important way to measure change in science policy, and Chapter 6 offers a new periodization of science policy based on such a change. Historically, principals began by simply grappling with these problems and educing, as Turner describes, attestations and other signifiers of integrity and productivity. After World War II, both patrons and performers tacitly agreed that integrity and productivity were automatic products of an autonomous scientific community. But if this agreement breaks down, as it did from the late

1970s, the patron has other options, such as requiring a degree of monitoring and reporting by the performer. For example, providers of fire insurance employ investigators to discover the causes of fires on insured property and thus deter arson. Providers of health insurance employ physicians to examine applicants and attempt to enforce restrictions against preexisting conditions to prevent coverage of persons likely to be ill. None of the characters leaves Spade's residence, so that each may monitor the others while the falcon is delivered to them. My task here is to explore the steps the public patron of research has taken to assure the integrity and productivity of the scientific agent.

A set of related questions about who should conduct publicly funded research exemplifies adverse selection in science policy. In the United States, these questions were already contentious by the 1880s, as politicians and scientists argued over the character and conduct of research in civilian agencies such as the Coast and Geodetic Survey versus military agencies such as the Hydrographic Office of the U.S. Navy, or the Geological Survey's practice of contracting out the analysis of collected material to university scientists rather than government employees (Manning 1988; Guston 1994a). In the immediate postwar period, the questions were primarily over whether government agencies or universities would conduct the bulk of federally funded research continuing from the War.

Examples of decisions about the choice of agents include, among others, choices between: military versus civilian research; intramural versus extramural research; mission or programmatic research versus disciplinary research; large firms versus small firms; and peer-reviewed versus earmarked (or pork barrel) research. In practice, such decisions are rarely unidimensional; that is, decisions about military versus civilian research, for example, often contain elements of the choice between peer-reviewed versus earmarked research as well.

Although these choices are important ones in science policy, this book will not dwell on them because they are derivative of the questions of integrity and productivity, properly conceived. Questions of the integrity and productivity of science must be asked and answered across each of the dimensions of choice listed. Patrons must be able to assess the integrity of research, regardless of its location intramurally or extramurally. They must be able to perceive the productivity of research, whether it is conducted by military or civilian agencies.

Furthermore, the choice of agents is a problem that can extend far outside the domain of science policy, because a great deal of even basic research is conducted in pursuit of missions and is therefore competitive not with other research projects but with other projects in pursuit of that particular mission.[11] That is, there is a step prior to the selection of

a research agent that involves selecting a method for achieving a mission, be it research, procurement, a service program, or some other expenditure. This step, taken in public health policy, security policy, energy policy, and elsewhere, needs to be informed by an understanding of the integrity and productivity of research. In this sense, science policy as understood here must inform policy decisions in these other substantive fields.

Regardless of the agent selected, problems of the conduct of research by that agent still remain. Even agents who espouse the goals of the principal, and who are provided with incentives to keep them aligned with that goal, might conduct their research sloppily or fraudulently or might pursue other goals and interests that divert them from the contractual ones. The two primary concerns of the patrons of research about this moral hazard are: 1) how the patron can tell that the research will be conducted with integrity; and 2) how the patron can tell that the research will be conducted with productivity.

The governmental principal's concern with productivity is evident. Research must at least push back the frontiers of knowledge and often must also contribute to higher education, military security, public health, economic advantage, or other missions. The government would not want to squander public monies by funding the meanderings of researchers. As Price (1979: 80) wrote about the authority delegated to researchers, "it depended on the continued confidence among elected politicians in the assumption on which the tacit bargain was founded – that basic research would lead automatically to fruitful developments."

But what of the patron's concern with the integrity of research? It has two elements. First, the principal is concerned to the extent that it affects the productivity goal. Research is the basis for myriad applications ranging from regulations to new drugs to military hardware. Fraudulent research can compromise the integrity of these applications and threaten the policy goals to which they contribute.[12] Moreover, fraudulent research may simply waste the time of other researchers.[13] Second, the principal has a symbolic or ideological concern for the integrity of science. Since the twin birth of liberal democratic thought and modern science in the English Enlightenment, science has been held as something of an exemplary community of freedom and cooperation in the pursuit of a common goal. As a result, it has served as a model of integrity for the larger society and of the efficacy of instrumental action and values, upon which representative government relies (Ezrahi 1980; 1990). If the public cannot trust science to have integrity, what can it trust?

The concepts of integrity and productivity, however, are not fixed, but they will vary over time and circumstance, as will what is taken as evidence of integrity and productivity. Yet it will remain in the durable

interests of the principal to be concerned about the integrity and productivity of sponsored research, regardless of the fact that the specific questions and answers relevant to those concerns will be flexible and even contested. Similarly, it will remain in the durable interests of the agents to be able to demonstrate integrity and productivity.

In scholarship about politics and public policy, principal-agent theory is usually applied to the relationship between a congressional committee and a particular executive agency under its jurisdiction. An important starting point is economist William Niskanen's *Bureaucracy and Representative Government* (1971), which stands as a neoclassical translation of the Weberian warning against the "overtowering" expert bureaucracy (Weber 1946: 232).[14] Niskanen adopts the standard assumption of political-economic analysis – that individuals act to maximize their utility – to model the relationship between a legislative committee and an executive agency. In this "bilateral monopoly," a single bureau promises a set of expected outputs in exchange for a budget granted by a single patron committee. The asymmetry of information is the crucial characteristic of Niskanen's bilateral monopoly. The bureaucrat needs little information, most of which can be garnered from the preferences legislators reveal in campaigns, proposed legislation, floor debates, and other public pronouncements. The patron, however, needs information to set a budget, and yet has little access to such information because there is no market price to use as a reference for bureaucratic services. Bureaus therefore command budgets larger than those that, had legislators possessed complete information, would provide the greatest net benefit. In such situations, Niskanen reasons, bureaus consume too much of their patrons' resources, and they produce too much as well.

Legislative principals can manipulate institutions and incentives to align the bureaucratic agent's goals with their own. For example, structural changes can increase competition among bureaucracies that produce similar outputs. Multiple bureaucracies fracture the bilateral monopoly on one side, making information about the costs of production more available 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 also breaking the bilateral monopoly at the legislative end.[15]

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; emphasis in the original). Niskanen therefore recommends incentives to encourage

agents to cut costs while maintaining output, such as rewarding thrifty bureaucrats with a portion of the budgetary cuts, or 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 scenario of sharing residuals with cooperative bureaucrats is the logic behind Gutman's offer to Spade of a percentage of the sale of the bird. It is the logic behind the institution of Inspectors General in all the cabinet departments and many of the independent agencies, as well as the offices created to investigate scientific misconduct.[16] It is also the logic behind sharing royalties from licenses with the federally funded inventor to help assure scientific productivity.[17]

The Two-way Street

In the principal-agent literature on the relationship between Congress and the bureaucracy, Niskanen represents what is generally known as the "bureaucratic dominance" school of thought, which emphasizes the asymmetry of information and the consequent and costly autonomy of the bureaucracy. There is also a school of thought favoring "congressional dominance," which argues that Congress possesses sufficient tools – budgets, new authorizing legislation, confirmations, and oversight – for liberating information and otherwise bending the bureaucracy to its political will. A detailed examination of the debate between the two schools is not in order here, especially since the concept of schools on this issue simplifies both the empirical work and the principal-agent framework behind it too greatly.[18] But the debate is symptomatic of a broader pathology, albeit a curable one, of the principal-agent framework. As Morris Ogul and Bert Rockman (1990: 21), two long-time observers of the struggle between Congress and the bureaucracy, conclude, "The logic of the principal-agent relationship . . . is its great advantage; its stylization of facts is its vulnerability."

Terry Moe, an important clarifier of the application of the principal-agent framework for the study of political institutions, provides a helpful metaphor to elaborate a more subtle use of it. He describes principal-agent relations as "a two-way street" and argues that each of the schools has elaborated only one direction.[19] 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, an additional perspective more sensitive to the ebb and flow between principal and agent is called for. After all, we are interested in the relationship between politics and science, and no relationship monopole exists. At the very least, the inquiry must consider the reciprocal hazards of the agent-

principal framework, if you will: How does the agent demonstrate that the research is conducted with integrity? And how does the agent demonstrate that the research is conducted with productivity? These questions are part of the problem of mutuality between politics and science that Price overlooked.

In my analysis of science policy, I attempt to address the problem of mutuality. But I also go further to combine the political-economic approach of principals and agents with more sociological insight.[20] This insight is derived from the constructivist approach to the study of science. Constructivism, or social constructivism, maintains that science is not the simple result of an immediate and objective understanding of the natural world. Rather, science has an invariably social component, which makes it subject to a wide array of influences, from the theory-laden aspect of observations, to the demographic characteristics of the scientific community, to the interpersonal, organizational, and technological processes through which knowledge is certified.

Sheila Jasanoff (1992) describes a small set of central tenets of the constructivist view of science that are important to understand about science as it impinges on policy arenas. First, constructivism holds that scientific claims are contingent upon certain local or background conditions of production. It is difficult – some would say impossible – to separate what is contingent about science from what is not. Second, constructivism holds that the process of inscription, by which scientists represent reality through a series of highly mediated interactions with machines, is problematic. Nature does not speak directly to scientists, but scientists use machines to write down what they manage to wring out of experiments. Third, constructivism holds that, because there are contingent components and practices embedded in scientific claims, these claims may be deconstructed by revealing these contingencies. Scientists involved in controversies, adversaries engaged in legal or policy proceedings, and scholars applying constructivist methods use similar techniques of deconstruction.[21] Fourth, constructivism holds that one particularly common manner of deconstruction (related to inscription) is "experimenters' regress," in which critics reveal the contingencies of experiments such that no experiment could actually stand up to scrutiny. Thus, the certainty of the critical processes of replication and falsification is illusory because no pair of experiments could be truly identical. Finally, in large part as a consequence of these other tenets, constructivism holds that what counts and what does not count as science does not correspond to any essential endowment of necessary and transcendent characteristics. Rather, constructivism encourages analysts to observe how participants themselves attempt to demarcate science from nonscience.

Although all of these tenets of constructivism are relevant to my analysis, and the third one is particularly important to understanding the role of Congress in my case studies, the final tenet is crucial for my overall approach. As sociologist Thomas F. Gieryn (1995: 393–94; emphasis in the original) writes, "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." A good deal of the empirical work upon which my analysis is based has involved watching, in various ways, how the participants involved in science policy tasks related to the integrity and productivity of science go about their work.

Boundary Work and the Problem of Stability[22]

Whether by Popper's falsification, Merton's norms, or Kuhn's paradigmatic consensus, claims for the demarcation of science from nonscience help construct and preserve the cognitive authority of science (Gieryn 1995).[23] Thus, the constructivist argument that boundaries between science and non-science are not essential, but instead provisional and ambiguous, has important consequences. One is the fear that constructivism erodes the cognitive authority of science. Some believe the tenets and methods of constructivism admit or legitimize a dangerous relativism. If constructivists manage to convince people that science is not a rational, objective, truthseeking and, indeed, truthfinding enterprise, then science will have lost its role as ultimate arbiter of Nature and its competitive position against religion, politics, and other traditionalist enterprises for providing a world view.

On a less prosaic but more immediate level, a feared consequence of constructivism is that it opens the tent of policy to the nose of the camel of irrationality. This fear is a variant of Yaron Ezrahi's more robust (but less apprehensive) argument about the decay of the productive relationship between science and liberal democracy in the Anglo-American tradition.[24] It is also at the root of policy-relevant discussions by a number of antagonists in the recent, so-called science wars.[25] For example, the editors of *The Flight from Science and Reason* argue, "The health of liberal democracy depends on the general use of reason. Reason must not be the cognitive tool of the few: if the integrity of science and reason are undermined among the majority, then democracy itself is in peril" (Gross, Levitt, and Lewis 1996: 491).

Central to these fears is the threat of instability – that the objective role of science is necessary to prevent human activities from undermining the rational foundations of society and freeing them to slip down into

some abyss of unreason. This threat is akin to political scientist Langdon Winner's (1977) concept of "apraxia" – the danger of large-scale technological failure should certain conditions for the management of technology, such as technocratic forms of governance, not be fulfilled. It holds that if science gives an inch to the dark forces of irrationality, there is no remaining fallback, no next position of compromise or stability. Philosopher Mario Bunge (1996: 110) argues, "Spare the rod and spoil the charlatan. Spoil the charlatan and put modern culture at risk. Jeopardize modern culture and undermine modern civilization. Debilitate modern civilization and prepare for a new Dark Age."[26]

The appropriate response to the charge that constructivism paves the road to the abyss should be a careful cartography of the admittedly unfamiliar territory that lies beyond our often scientistic and exclusionary liberalism, but does not descend so fearsomely. Indeed, a second possible consequence is that constructivist studies of science can improve the position of science in society, for the ultimate benefit of society and perhaps of science as well. By clearly portraying science as it is practiced, constructivism can recover the human face beneath science's rationalist mask. And even if the creature revealed is a little disfigured, greater familiarity with this phantom will bring sympathy rather than contempt. This hope grounds the work of a number of scholars who argue that constructivist approaches can actually aid the rationalists' perennial goal of scientific literacy and the public understanding of science.[27] It also grounds constructivist perspectives on questions usually addressed by more traditional approaches to science policy. For example, Jasanoff's (1990) study of science advisory committees to U.S. regulatory agencies finds, in situations where scientists and policy makers insisted on the realist's clean demarcation between the science and the policy components of their task, policy making became more difficult. When those boundaries were intentially blurred, policy making was easier.[28]

Such cartographies of constructivism in the policy arena are critical for providing an alternative to realist accounts and fears. It is thus imperative for constructivism, both intellectually and politically, to map the toeholds, ledges, plateaus, or even the vast plains that are accommodatingly stable and level. They may overlook the abyss, but they are not slippery slopes into it.

Scholars drawing on the concept of boundary work have seemingly intuited this problem and concretized aspects of it, finding toeholds without reifying the boundary. The primary example is the identification by Star and Griesemer (1989: 393) of "boundary objects" that are "both plastic enough to adapt to local needs and constraints of the several parties employing them, yet robust enough to maintain a common iden-

tity across sites." These boundary objects allow members of different communities to work together around them, and yet maintain their disparate identities. Boundary objects can be such things as tomatoes, which are familiar to both me and my greengrocer, but which convey different specific meanings to the horticulturalist selling them and to the shopper looking to make a marinara sauce. Or they can be things like the Maltese falcon, which represents wealth and prestige of legendary proportions to the Fat Man, but to Sam Spade is a way of paying the bills and saving his own skin. Or they can be things like an article in a research journal, in which unsubstantiated claims may represent research fraud to a congressional investigator, but merely unwarranted speculation to a research colleague.

The use of boundary objects, however, is almost infinitely flexible. Sociologist Joan Fujimura therefore expands them into "standardized packages," which are more adept than boundary objects at stabilizing facts. A standardized package "is used by researchers to define a conceptual and technical work space which is less abstract, less ill-structured, less ambiguous and less amorphous" (Fujimura 1992: 169). The standardized package combines boundary objects with common methods in more restrictive but not entirely definitive ways. Unlike boundary objects, standardized packages are robust enough to change local practices. But as "interfaces" among a set of actors from diverse social worlds, standardized packages emphasize the collaboration of those actors to "get work done" and simultaneously to maintain their integrity in their respective social worlds. Although Fujimura's focus remains on the use of standardized packages among scientists, the diverse social worlds that mutually partake in them can easily be populated by policy makers as well. Under such circumstances, examples of standardized packages might include a patent for a recombinant bacterium, which a researcher pursues because it represents priority in discovery after long years of research. But the same patent also represents to a policy maker the prospect of a commercial pharmaceutical, and the policy maker treats this kind of research more generously as it produces more patents.

In the next incremental step, sociologist Kelly Moore (1996: 1598) broadens the scope of analysis from objects and their aggregates to organizations, likening the latter to the former in their ability to "provide both an object of social action and stable but flexible sets of rules for how to go about engaging with that object." In Moore's historical account of public interest organizations such as the Scientists' Institute for Public Information, the relationship between politics and science is the crucial site of boundary work. These organizations allowed scientists to present themselves both as members of a knowledge community and

as advocates, bridging the boundary between politics and science while allowing both enterprises to continue operating without substantial change.[29] The organization became the boundary object.

Each of these elaborations contributes to a discussion of stability, but none provides a compelling general hypothesis of how the objects, packages, or institutions stabilize the potential chaos of the politics/science boundary.[30] As Gieryn (1995) points out, the extent and productiveness of boundary work in Jasanoff's account varies among cases. In such a collection – one example of boundary work is too much, one too little, and one just right – there are no instructions to follow in reproducing the right measure. For Star and Griesemer, as long as there is some local agreement over these boundary objects, the boundary may be relatively stable around them. But this stability is based entirely on consent, and there is a large and unexplained gap between the fully consensual boundary object and the other means of satisfying conflicting social worlds, including "imperialist impositions of representations, coercion, silencing and fragmentation" (Star and Griesemer, 1989: 413).[31] And Moore's (1996: 1621) conclusion that "the boundaries between science and politics were redrawn [by the public interest organizations] to suggest that the content of science was untainted by subjectivity but that scientists had a moral obligation to serve the public good" still does not allow her to explain why some of the organizations she studied succeeded and others collapsed. It remains an open question how the politics/science boundary becomes, or can become, stabilized.

In this book, I introduce the concept of a boundary organization as one route to stabilization. Boundary organizations are institutions that straddle the apparent politics/science boundary and, in doing so, internalize the provisional and ambiguous character of that boundary. Negotiating these elusive qualities becomes the daily work of the boundary organization, which in fact involves the use of boundary objects and standardized packages as a collaboration between the interests of the principal and those of the agent. The success of the boundary organization is judged by principals on either side of the boundary, both of whom rely on the boundary organization to provide them with necessary resources. A successful boundary organization will thus succeed in pleasing two sets of principals and remain stable to external forces astride the internal instability at the boundary. The success of the organization in performing these tasks can then be taken as the stability of the boundary, while in practice the boundary continues to be negotiated at the lowest level and the greatest nuance within the confines of the boundary organization.

My concept of the boundary organization differs in subtle but important ways from German political scientist Dietmar Braun's (1993) description of intermediary agencies. In his comparative study of mission agencies that sponsor research in the United States, Great Britain, France, and Germany, Braun appropriately critiques the dyadic structure of principal-agent theory and situates the mission agencies as intermediaries between a political system and a scientific system. He concludes that this three-part or triadic structure improves the communications between politics and science, but continues to concede significant power to science over the choice of research. Rip (1994) has a similar vision of the dual nature of research councils between the scientific community and the government, embodying values from both sides. Moreover, Rip (1994: 12–13) generalizes from this observation, arguing that "because they have two patrons, the state and the scientific community, the research councils are relatively independent with respect to either of them."

The analyses by Braun and Rip, however, are more apt for the European agencies studied than they are for the United States (where Braun studies NIH and Rip generalizes about NIH and NSF). The European agencies or research councils are semipublic, whereas NIH is a fully public, governmental institution. The available resources for all three parties in the triad are therefore somewhat different, and the extent of "capture" (Rip 1994: 8) of the councils by the scientific community varies greatly among nations. Moreover, these authors attribute to the research councils narrower functions – apart from funding research. Braun introduces the triadic structure merely to account for complexity, and the need of his intermediary agency for the scientific system is limited to the latter's providing reputational assistance to the former. In the case of the boundary organization, however, the professionals in the agency and the scientists on the outside collaborate to produce mutually instrumental boundary objects and standardized packages. To the extent that Rip focuses on the consequences of the dual nature of the research councils, he argues that it permits them an independence they can exploit in an entrepreneurial way. Although the boundary organization may behave entrepreneurially, its key characteristic is the stability it induces in the science/politics boundary by successfully internalizing the boundary negotations.[32]

This boundary organization also differs from the boundary-spanning organization previously defined in the sociology of organizations (Aldrich 1979; Bozeman 1987). The concept of the boundary-spanning activities helps explain how organizations insulate themselves from ex-

ternal political authority. Organizations engage in such activities to exploit opportunities or respond to threats from their environment. The boundary organization I elaborate draws its stability not from isolating itself from external political authority, but precisely by making itself accountable and responsive to opposing, external authorities. The boundaries most important to the sociologist Aldrich, for example, are those of the organization itself, which determine its membership. The most important boundaries here are the ones between science and politics that the organization internalizes in order to be flexibly undifferentiable from either politics or science.

Historical Prelude: the Allison Commission

To explore the plausibility of my approach, which emphasizes the problems of agency between politics and science, I provide a brief historical probe of the Allison Commission of the 1880s. Although the cast of characters in this episode is radically different than today's – small-government Democrats, free-spending Republicans, and well-traveled, interdisciplinary geologists – their concerns and the interplay of their institutional interests will be remarkably familiar. That, of course, is the point. This century-old struggle reveals that the asymmetry of information and the concern for the integrity and the productivity of science are durable elements of the structure of science policy.

The Allison Commission was an ad hoc, joint committee of Congress established in July 1884 to examine the organization of the federal research effort.[33] The Allison Commission, eponymous for its chairman Senator William Boyd Allison (R-IA), was formally known as the Joint Commission to Consider the Present Organizations of the Signal Service, Geological Survey, Coast and Geodetic Survey and the Hydrographic Office of the Navy Department. The Signal Service of the Army Department included the Weather Bureau and maintained a school of meteorological training at Fort Myer, Virginia. The Geological Survey of the Department of the Interior mapped public lands and conducted research in geology, ethnology, archaeology, and paleontology under the direction of Major John Wesley Powell. The Coast and Geodetic Survey, created in 1807 and thus one of the oldest federal bureaus, charted coastal waters and lands and conducted a transcontinental triangulation. The Hydrographic Office also conducted coastal mapping, but its domain was restricted to foreign coasts. Together, these four agencies accounted for the vast majority of the federal government's very modest budget for research, spending about $3 million in annual appropriations in the years before the billion dollar budget.

The ostensible reason for Congress's study of these agencies was their apparent overlap in jurisdiction. Conflict roiled between the Hydrographic Office and the Coast Survey, spurred by the Secretary of the Navy, who believed everything on the water belonged to the Department of the Navy, and reasoned the Coast Survey should not perform coastal mapping but be restricted to geodesy – the science of measuring the size and shape of the earth. The Coast Survey's geodetic work, however, conceivably overlapped with the finer resolution mapping performed by the Geological Survey. Meanwhile, the bureaucratic location of the Signal Service was under dispute. Many members of Congress felt it did not belong under military supervision in the War Department, but should instead be a civilian agency because its purposes were no longer military. Supporters of the status quo argued that making scientific observations in remote places, on a regular schedule, and with the required precision necessitated military discipline – a problem of delegation unto itself. Given the uncomfortable jurisdictional problems and the scientific character of each of the disputed agencies, prominent voices from the scientific community called for at least the consolidation of the surveys or even for the creation of a new Department of Science.

To answer these and other questions, the six members of the Allison Commission delved into the minutiae of bureaucratic detail, from the rate of subsistence payment to employees of the Coast Survey to the quality of the coffee at Fort Myer.[34] But they also heard testimony from the scientists and bureaucrats about the nature of research and its quality as conducted by their agencies. Major Powell, for example, lectured the Commission on the two classes of scientific work conducted by the government: the "constructive work" of "applied science," performed, for example, by the Army Corps of Engineers (which was not under scrutiny); and the "original investigation" that "purely scientific institutions" such as the Geological Survey, the Coast Survey, and the Signal Service were "designed for." Because such scientific institutions required constant modification as dictated by scientific inquiry, Powell argued, "[i]t will thus be seen that it is impossible to directly restrict or control these scientific operations by law." Powell was not alone in this opinion, but at least he was far more subtle than other scientists who supported the bureaus. In a mode still common today, these scientists personalized the asymmetry of information, accusing members of Congress of ignorance and antiscientific attitudes. "It is a shame," one scientist wrote, "that a Congressman whose brain is not more than two kitten power can kick [the Coast Survey] around like a foot ball [sic]" (quoted in Guston 1994a: 30). Powell and his supporters reasoned that because scientists had a monopoly on the information needed to direct their