CHAPTER 1

INTRODUCTION

Science Wars and Policy Wars

When considering the importance of science in policymaking, common wisdom contends that keeping science as far as possible from social and political concerns would be the best way to ensure science's reliability. This intuition is captured in the value-free ideal for science—that social, ethical, and political values should have no influence over the reasoning of scientists, and that scientists should proceed in their work with as little concern as possible for such values. Contrary to this intuition, I will argue in this book that the value-free ideal must be rejected precisely because of the importance of science in policymaking. In place of the value-free ideal, I articulate a new ideal for science, one that accepts a pervasive role for social and ethical values in scientific reasoning, but one that still protects the integrity of science.

Central to the concerns over the use of science in policymaking is the degree of reliability we can expect for scientific claims. In general, we have no better way of producing knowledge about the natural world than doing science. The basic idea of science—to generate hypotheses about the world and to gather evidence from the world to test those hypotheses—has been unparalleled in producing complex and robust knowledge, knowledge that can often reliably guide decisions. From an understanding of inertia and gravity that allows one to predict tides and the paths of cannonballs, to an understanding of quantum mechanics that underlies the solid state components of computers, to an understanding of physiology that helps to guide new medical breakthroughs, science has been remarkably successful in developing theories that make reliable predictions.

Yet this does not mean that science provides certainty. The process of hypothesis testing is inductive, which means there is always a gap between the evidence and the theory developed from the hypothesis. When a scientist makes a hypothesis, she is making a conjecture of which she is not certain. When the gathered evidence supports the hypothesis, she is still not certain. The evidence may support the theory or hypothesis under examination, but there still may be some other theory that is also supported by the available evidence, and more evidence is needed to differentiate between the two. The hypothesis concerns a great many more instances than those for which we will carefully collect data. When we collect more data, we may find that seemingly well-confirmed hypotheses and theories were false. For example, in the late nineteenth century, it was widely accepted that chemical elements could not transform into other elements. Elements seemed to be stable in the face of any efforts at transmutation. The discovery of radioactivity in the early twentieth century overturned this widespread belief. Or consider the theory of ether, a medium in which it was once commonly believed light traveled. Despite near universal acceptance in the late nineteenth century, the theory of ether was rejected by most physicists by 1920. Going even further back in history, for over 1,500 years it seemed a well-supported theory that the sun revolved around the Earth, as did the fixed stars. But evidence arose in the early seventeenth century to suggest otherwise and, along with changes in the theories of mechanics, overturned one of the longest standing and best supported scientific theories of the time. After all, how many times had humans seen the sun rise and set? And yet, the theory was ultimately incorrect. Data can provide evidential support for a theory, but can never prove a scientific theory with certainty. Aspects of the world that were once thought to be essential parts of scientific theory can be rejected wholesale with the development of new theories or the gathering of new evidence.

Because of the chronic, albeit often small, uncertainty in scientific work, there is always the chance that a specific scientific claim is wrong. And we may come to know that it is wrong, overturning the theory and the predictions that follow from it. The constant threat of revision is also the promise of science, that new evidence can overturn previous thought, that scientific ideas respond to and change in light of new evidence. We could perhaps have certainty about events that have already been observed (although this too could be disputed—our descriptions could prove inaccurate), but a science that is only about already observed events is of no predictive value. The generality that opens scientific claims to future refutation is the source of uncertainty in science, and the source of its utility. Without this generality, we could not use scientific theories to make predictions about what will happen in the next case we encounter. If we want useful knowledge that includes predictions, we have to accept the latent uncertainty endemic in that knowledge.

The chronic incompleteness of evidential support for scientific theory is no threat to the *general* reliability of science. Although we can claim no certainty for science, and thus no perfect reliability, science has been stunningly successful as the most reliable source for knowledge about the world. Indeed, the willingness to revise theories in light of new evidence, the very quality that makes science changeable, is one key source for the reliability and thus the authority of science. That it is not dogmatic in its understanding of the natural world, that it recognizes the inherent incompleteness of empirical evidence and is willing to change when new evidence arises, is one of the reasons we should grant science a prima facie authority.

It is this authority and reliability that makes science so important for policy. And it seems at first that the best way to preserve the reliability of science is to keep it as far from policy as possible. Indeed, the realm of science and the realm of policy seem incompatible. In the ideal image of science, scientists work in a world detached from our daily political squabbles, seeking enduring empirical knowledge. Scientists are interested in timeless truths about the natural world rather than current affairs. Policy, on the other hand, is that messy realm of conflicting interests, where our temporal (and often temporary) laws are implemented, and where we craft the necessary compromises between political ideals and practical limits. This is no place for discovering truth.

Without reliable knowledge about the natural world, however, we would be unable to achieve the agreed upon goals of a public policy decision. We may all agree that we want to reduce the health effects of air pollution, for example, or that we want safe, drinkable water, but without reliable information about which pollutants are a danger to human health, any policy decision would be stymied in its effectiveness. Any implementation of our policy would fail to achieve its stated goals. Science is essential to policymaking if we want our policies concerning the natural world to work.

This importance of science in achieving policy goals has increased steadily throughout the past century in the United States, both as the issues encompassed by public policy have expanded and as the decisions to be made require an increasingly technical base. As science has become more important for policy, the relationship between science and policy has become more entangled. This entanglement exists in both directions: science for policy and policy for science. In the arena of policy for science, public funds allocated for doing science have grown dramatically, and these funds require some policy decisions for which projects get funded and how those funds will be administered. In the arena of science for policy, increasing numbers of laws require technically accurate bases for the promulgation of regulations to implement those laws. These arenas in practice overlap: which studies one chooses to pursue influences the evidence one has on hand with which to make decisions. In this book, however, my focus will be largely on science for policy.

While the entanglement between science and policy has been noted, the importance of this entanglement for the norms of science has not been recognized. As science plays a more authoritative role in public decision-making, its responsibility for the implications of research, particularly the implications of potential inductive error, increases. Failure to recognize the implications of this responsibility for science, combined with the desire to keep science and policy as distinct as possible, has generated deep tensions for our understanding of science in society.

These tensions are evident in the increased stress science has been under, particularly with respect to its public role. Some commentators note an increasing strain on the "social contract" between science and society (see, for example, Guston and Keniston 1994). This strain was made manifest in the 1990s when two public debates erupted over science: the "Science Wars" and the sound science–junk science dispute. Both can be taken as emblematic of science under stress in our society.

The Science Wars, as they are often called, centered on the authority of science. They were about whether or not science should be believed when it tells us what the nature of the world is, about whether or not science should have more public authority than other approaches to knowledge or belief. For those outside the world of science studies, these are astonishing questions to raise. If one wants to know something about the natural world, it seems obvious that one should ask scientists. While few in science studies would actually dispute this, the claim has been made that the knowledge produced by science has no special authority above and beyond any other approach. In other words, the claim is that science and its methods have no special hold on the ability to uncover and speak truth; they simply have more funding and attention.

The sound science–junk science war, in contrast, does not question the special epistemic authority given to science in general, or the overall reliability of science for answering empirical questions. Instead, this dispute is about which particular piece(s) of science should shape policy. When is a particular body of scientific work adequately "sound" to serve as the basis for policy? Debates in this arena center on how much evidence is sufficient or when a particular study is sufficiently reliable. The arguments focus on such questions as: How much of an understanding of biochemical mechanisms do we need to have before we regulate a chemical? How much evidence of causation is needed before a court case should be won? How much of an understanding of complex biological or geological systems do we need before regulatory frameworks intervene in the market to prevent potential harm? The idea that science is the authoritative body to which one should turn is not questioned; what is questioned is which science is adequate for the job, or which scientific experts are to be believed by policymakers, Congress, and the public.

While both of these disputes are symptomatic of deep concerns surrounding the public role of science, neither has been able to produce a satisfactory approach to understanding the role of science in society or what that role might mean for the norms of scientific reasoning. This is, in part, because both disputes began with the presupposition that science is a distinct and autonomous enterprise developed by a community of scientists largely in isolation from public questions and concerns. Such an understanding of science and scientists inhibits a clear view of how science should function in society. Both in the academic arena of the Science Wars and in the policy arena of the sound science–junk science dispute, the discussions shed little light on the deep questions at issue, even as the existence of the debates indicated the need for a more careful examination of the role of science in society and its implications.

The Science Wars

The Science Wars were an academic affair from start to finish. A particular critique of science, known as social constructivism, began in the 1970s and gathered steam and fellow travelers in the 1980s. The social constructivist critique was essentially an assault on the authority of science, particularly its apparently privileged place in producing knowledge. Social constructivists suggested that scientific *knowledge* (not just scientific institutions or practices) was socially constructed and thus should be treated on a par with other knowledge claims, from folklore to mythology to communual beliefs

(Barnes and Bloor 1982). There simply was no deep difference between one set of knowledge claims and another, social constructivists argued, and thus scientific facts held no special claim to our acceptance.

As this critique was developed throughout the late 1970s and 1980s, other criticisms of science began to coalesce. For example, feminists noted that few scientists were women, and that many scientific claims about women had been (and continued to be in the 1980s) either explicitly sexist or supportive of sexist beliefs (Fausto-Sterling 1985; Longino 1990). Feminists wondered if science done by women would be different, producing different conclusions (Harding 1986, 1991). It was unclear whether sexist science was always methodologically flawed or bad science (as it sometimes was), or whether sexist science simply relied upon different background assumptions, assumptions which in themselves did not clearly put the scientific quality of the work in doubt. If the latter were the case, then an emphasis on unpacking the background assumptions, which often arose from the surrounding culture, seemed to support the notion that science was in fact a social construct, or at least heavily influenced by the surrounding society and its prejudices. Although feminists and social constructivists disagreed about much, their arguments often pointed in a similar direction—that scientific knowledge consisted of socially constructed claims that were relative to a social context. Only those *within* a particular social context thought the claims produced had any special authority or believability.

By the early 1990s, some scientists began to take umbrage with these criticisms, particularly the apparently strong form of the social constructivist critique, that in general science had no special claim to being more believable than any other knowledge claim. As scientists began to engage in this debate, the Science Wars erupted. An early salvo was Lewis Wolpert's *The Unnatural Nature of Science* (1992), which devoted a chapter to responding to relativist and social constructivist views of science. The debate really heated up in 1994, however, with the publication of Paul Gross and Norman Levitt's *Higher Superstition: The Academic Left and Its Quarrels with Science.*[1] As one sympathetic reader of the book notes, "This unabashedly pugnacious work pulled no punches in taking on the academic science critics. . . . Naturally, those criticized on the 'academic left' fired back, and so the science wars were joined" (Parsons 2003, 14). The polemical nature of Gross and Levitt's book drew immediate attention from scientists and fire from its targets, and the accuracy of Gross and Levitt's criticisms has been seriously questioned. (Roger Hart [1996] is particularly precise in his critique of Gross and Levitt for simply misunderstanding or misrepresenting their targets.) Now scientists and their critics had a text over which to argue.

The Science Wars took an even nastier turn when Alan Sokal, a physicist, decided to attempt a hoax. Inspired by Gross and Levitt's book, he wrote a paper in the style of postmodern social constructivism and submitted it for publication in a left-leaning social constructivist journal, *Social Text*. The paper was entitled "Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum Gravity," and was a parody of some constructivist work, citing and drawing heavily from that work. The editors were thrilled that a physicist was attempting to join in the discussion, and they published the piece in 1996.[2] Sokal then revealed he had written the work as a hoax to unmask the vacuity of this kind of work (see Sokal 1998). Many cheered Sokal's effort; after all, hoaxing is a venerable tradition in the natural sciences, where hoaxing has revealed some of science's most self-deceived practitioners.[3] But in the humanities, there is little tradition of hoaxing as a deliberate attempt to catch a colleague's suspected incompetence.[4] Scholars in those fields take for granted that a person's work, ingenuously put forth, is their own honest view, so others cried foul at Sokal's violation of this basic norm of intellectual honesty. The gulf between the critics of science and the scientists only grew wider.

However, as Ullica Segerstråle notes, in many of the forums of debate for the Science Wars, it was hard to find anyone actually defending the strong versions of the social constructivist claims (Segerstråle 2000, 8). The plurality of views about science among both science studies practitioners and scientists themselves became increasingly apparent as the decade came to a close. In the end, the Science Wars petered out, perhaps having moderated the views of some academics, but having had little impact on the public perception or understanding of science.[5]

So what was the debate about? Why did critics of science attack science's general authority? I think the debate arose because of tensions between science's authority and science's autonomy. As I will discuss in chapter 3, the autonomy of science, the isolation of science from society, became a cornerstone of the value-free ideal in the 1960s. On the basis of the value-free nature of science, one could argue for the general authoritativeness of its claims. But an autonomous *and* authoritative science is intolerable. For if the values that drive inquiry, either in the selection and framing of research or in the setting of burdens of proof, are inimical to the society in which the science exists, the surrounding society is forced to accept the science and its claims, with no recourse. A fully autonomous and authoritative science is too powerful, with no attendant responsibility, or so I shall argue. Critics of science attacked the most obvious aspect of this issue first: science's authority. Yet science is stunningly successful at producing accounts of the world. Critiques of science's general authority in the face of its obvious importance seem absurd. The issue that requires serious examination and reevaluation is not the authority of science, but its autonomy. Simply assuming that science should be autonomous, because that is the supposed source of authority, generates many of the difficulties in understanding the relationship between science and society.

That the relationship between science and society was an underlying but unaddressed tension driving the Science Wars has been noted by others. Segerstråle, in her reflections on the debate, writes, "But just as in other academic debates, the issues that were debated in the Science Wars were not necessarily the ones that were most important. One 'hidden issue' was the relationship between science and society. The Science Wars at least in part reflected the lack of clarity in science's basic social contract at the end of the twentieth century" (Segerstråle 2000, 24–25). Rethinking that social contract requires reconsidering the autonomy of science. Once we begin to rethink the autonomy of science (chapter 4), we will need to rethink the role of values in science (chapter 5), the nature of scientific objectivity (chapter 6), and the process of using science in policymaking (chapter 7). The Science Wars demonstrated the tension around these issues with its intensity, but shed little light on them.

Policy Wars: The Sound Science–Junk Science Dispute

While the Science Wars were playing out, the place of science in policymaking was the focus of a completely separate debate in the 1990s. Rather than centering on the authority of science in general, the debate over science in policy centered on the reliability of particular pieces of science. As noted above, science is endemically uncertain. Given this uncertainty, blanket statements about the general reliability of science, and its willingness to be open to continual revision, are no comfort to the policymaker. The policymaker does not want to be reassured about science in general, but about a particular piece of science, about a particular set of predictions on which decisions will be based. Is the piece of science in which the policymaker is interested reliable? Or more to the point, is it reliable enough? For the policymaker to not rely on science is unthinkable. But which science and which scientists to rely upon when making policy decisions is much less clear.

The difficulties with some areas of science relevant for policy are compounded by their complexity and by problems with doing definitive studies. Because of their complexity, policy interventions based on scientific predictions rarely provide good tests of the reliability of the science. A single change in policy may not produce a detectable difference against the backdrop of all the other factors that are continually changing in the real world. And the obvious studies that would reduce uncertainty are often immoral or impractical to perform. For example, suppose we wanted to definitively determine whether a commonly occurring water pollutant caused cancer in humans. Animal studies leave much doubt about whether humans are sufficiently similar to the animal models. The biochemical mechanisms are often too complex to be fully traced, and whether some innate mechanism exists to repair potential damage caused by the pollutant would be in doubt. Epidemiological studies involve people living their daily lives, and thus there are always confounding factors, making causation difficult to attribute. A definitive study would require the sequestering of a large number of people and exposing them to carefully controlled doses. Because the latency period for cancer is usually a decade or more, the subjects would have to remain sequestered in a controlled environment for years to avoid confounders. And large numbers of people would be needed to get statistically worthwhile results. Such a study would be immoral (the subjects would not likely be volunteers), expensive, and too unwieldy to actually conduct. We cannot reduce uncertainty by pursuing such methods. Nor does implementing a policy and seeing what happens reduce uncertainty about the science. Real world actions are subject to even more confounders than are controlled for in epidemiological studies. Even if cancer rates clearly dropped after the regulation of a pollutant, it would be hard to say that the new regulation caused the drop. Other simultaneous regulations or cultural changes could have caused the cancer rate to decline at the same time as the drop in exposure to the pollutant.

Thus, in addition to the generic uncertainties of science, science useful for policymaking often carries with it additional sources of uncertainty arising from the biological, ecological, and social complexity of the topics under study. Yet this chronic uncertainty does not mean that policymakers should go elsewhere for information. Instead, it puts increased pressure on assuring that the science on which they depend is reliable.

Tensions over the role of science in policy increased as the reach of regulation grew and the decisions stakes rose throughout the 1970s and 1980s. As will be discussed in the next chapter, by the 1970s dueling experts became a standard phenomenon in public policy debates. As I will discuss in chapter 7, attempts to proceduralize the policy decisionmaking process were supposed to rein in the impact of these contrarian experts, but by the 1990s it was apparent the problem was not going away. In fact, it seemed to be worsening, with public debates about technical matters occurring increasingly earlier in the policymaking process.

New terms arose in attempts to grapple with the crisis. Although scientists had used the phrase "sound science" to refer to well-conducted, careful scientific work throughout the twentieth century, its opposite was often "pseudoscience," which masqueraded as science but had not a shred of scientific credibility. Examples of pseudoscience included tales of extraterrestrial sightings, claims about extrasensory perception or psychic abilities, and astrology. Pseudoscience as such has not been taken seriously in the policy realm and has not been a source of policy controversy. Rather than these more outlandish concerns, the realm of policy was focused on the reliability of a range of ostensibly reasonable scientific claims. Even as scientific expertise became the basis of many decisions, from new regulatory policy to rulings in tort cases, increasing concern was raised over the quality of science that served as a basis for those decisions. As tension brewed over the role of science in policymaking and skeptics over certain uses of science became more vocal in the early 1990s, a new term entered into the lexicon of science policy commentators: "junk science."

Although the term "junk science" appeared occasionally before 1991, Peter Huber's *Galileo's Revenge: Junk Science in the Courtroom* popularized the notion of junk science. Huber's critique centered on the use of evidence in tort cases and decried the shift in the Federal Rules of Evidence from the Frye rule of the 1920s, which stated that only scientific ideas reflecting the consensus of the scientific community were admissible in court, to more recent, laxer standards that allow any expert testimony that assists in the understanding of evidence or determination of fact. Huber argued that this was a catastrophe in the making and that we needed to strengthen standards back to the Frye rule.

Huber relied upon an autonomous image of science to decide what counted as sound science, stating near the close of his book, "as Thomas Kuhn points out, a scientific 'fact' is the collective judgment of a specialized community" (Huber 1991, 226). For Huber, what the specialized community deems sound science *is* sound science. Yet Kuhn's idea of a specialized community consists of scientists working within internally determined paradigms—sets of problems and ideas that scientists alone, separated from any social considerations, decide are acceptable. (Kuhn's influence in this regard will be discussed further in chapter 3.) Under this Kuhnian image of science as isolated and autonomous, one could presume that there might exist a clear and "pure" scientific consensus to which one could refer, and on which one could rely. Any science outside of this clear consensus was "junk," even if later it might prove its mettle. Initially, the very idea of junk science depended on an autonomous and isolated scientific community, inside of which one could find sound science, and outside of which lay junk science.

Thus, the same conceptual framework that led to the Science Wars, the idea of science as autonomous and isolated, shaped the sound science–junk science debates. Like the Science Wars, the resulting debates in the policy arena have produced few helpful insights. Instead, they merely changed the rhetoric of policy disputes. As experts with obvious credentials continued to disagree about apparently scientific matters, Huber's term "junk science" expanded from the courtroom to all public debates over technical policy issues. Rather than argue that an opposing view had an insufficient scientific basis, one could dismiss an opponent by claiming that their views were based on junk science, which looked like science, but which would be proven wrong in the near future. Conversely, one's own science was sound, and thus would prove to be a reliable basis for decisionmaking in the long run.

The idea that sound science was a clear and readily identifiable category, and that its opposite, junk science, was also easily identified, ran rampant through public discussions. As this language permeated policy debate, it became a mere rhetorical tool to cast doubt upon the expertise of one's opponents. In a revealing study by Charles Herrick and Dale Jamieson, the use of the term "junk science" in the popular media from 1995 to 2000 was examined systematically (Herrick and Jamieson 2001). They found that the vast majority of studies tarnished with the term did not have any obvious flaws (such as lack of peer review or appropriate publication, lack of appropriate credentials of the scientists, or fraud), but were considered "junk science" because the implications of the studies were not desirable. For example, studies were called junk science because the results they produced were not "appropriately weighted" when considered with other evidence, the studies came from a source that was simply presumed to be biased, or undesirable consequences that might follow from the study were not considered. Thus, by the end of the decade, the term "junk science" had come to be used in ways quite different from the original intent of designating work that fails to pass muster inside the scientific community, denoting instead science that one did not like rather than science that was truly flawed.

Despite the muddling of the notions of sound and junk science, much effort has gone into finding ways to sort the two out in the policy process. For example, the Data Quality Act (or Information Quality Act, Public Law 106-554, HR 5658, sec. 515) was passed in 2000 and charged the Office of Management and Budget (OMB) with ensuring "the quality, objectivity, utility, and integrity of information . . . disseminated by Federal agencies," including the information that serves as a basis in public record for regulatory decisions.[6] However, there are deep tensions generally unrecognized at the heart of such solutions to the sound science–junk science problem. In an essay by Supreme Court Justice Stephen Breyer, published in *Science*, Breyer emphasized the need for "sound science" in many then-current legal cases: "I believe there is an increasingly important need for law to reflect sound science" (Breyer 1998, 538). While the importance of sound science is clear, how to identify what constitutes sound science in any particular case is a challenge, Breyer acknowledged. This is in part because the ideal for sound science contains contradictory impulses, as can be seen in Breyer's concern that "the law must seek decisions that fall within the boundaries of scientifically sound knowledge and approximately reflect the scientific state of the art" (Breyer 1998, 537). As noted above, earlier standards of evidence, following the Frye rule, demanded that scientific testimony reflect the consensus of the scientific community. While such a standard might clearly determine the boundaries of scientifically sound knowledge, it would often exclude state-of-the-art science, which would encompass newer discoveries still in the process of being tested and disputed by scientists. Every important discovery, from Newton's theory of gravity to Darwin's descent by natural selection to Rutherford's discovery of radioactivity, was disputed by fellow scientists when first presented. (Some may note that in high stakes discoveries, expert disputation can become a career unto itself.) Yet many cutting-edge scientists have strong evidence to support their novel claims. State-of-the-art science and scientific consensus may overlap, but they are not equivalent. If we want to consider state-of-the-art scientific work in our decisionmaking, we will likely have to consider science not yet part of a stalwart consensus.

In the 2000s, the rhetoric around science in policy changed again, this time to focus on "politicized science" rather than junk science. The Bush administration's handling of science and policy led to these charges, particularly as concern over the suppression of unwanted scientific findings arose.[7] Rather than introducing junk science into the record, the worry is that sound science is being distorted or kept out of the public record altogether. Thus, the debate over the role of science in policymaking continues, even if under an altered guise. Regardless of the form it takes, debate over sound science and junk science (or politicized science) centers on the reliability of science to be used in decisionmaking.

The introduction of new jargon, however, has not helped to clarify the issues. As with the Science Wars, more heat than light has resulted. And ironically, despite the parallels between the two disputes, neither dispute seems to have noticed the other. The Science Wars were a debate among academics interested in science and science studies; the sound science–junk science dispute is a debate among those interested in the role of science in policy and law. One was about the standing of science in society; the other is about which science should have standing. These two disputes involve different texts, participants, and issues, and we should not be surprised that no general connection was made between them. Yet the origins of these two disputes can be found in the same set of historical developments, the same general understanding of science and its place in society. Both disputes and their conceptual difficulties arise from assuming that a clearly defined, authoritative, and autonomous scientific community that hands to society fully vetted scientific knowledge is the correct understanding of science's role in society. Getting to the heart of this understanding—centering on the autonomous and authoritative view of science—will be central to finding a workable resolution to the continuing dispute over the role of science in public policy. It will also challenge the norms for scientific reasoning in general.

Overview, Context, and Limits of the Book

This book will not challenge the idea that science is our most authoritative source of knowledge about the natural world. It will, however, challenge the autonomy of science. I will argue that we have good grounds to challenge this autonomy, particularly on the basis of both the endemic uncertainty in science and science's importance for public decisionmaking. In order to protect the authority of science without complete autonomy, I will articulate and defend ways to protect the integrity of science even as scientific endeavors become more integrated with the surrounding society. By considering carefully the importance of science for public policy, I will argue for important changes in the norms that guide scientific reasoning. In particular, I will argue that the value-free ideal for science, articulated by philosophers in the late 1950s and cemented in the 1960s, should be rejected, not just because it is a difficult ideal to attain, but because it is an undesirable ideal.[8] In its place, I will suggest a different ideal for scientific integrity, one that will illuminate the difference between sound science and junk science, and clarify the importance and role for values in science. I will also argue that rejecting the value-free ideal is no threat to scientific objectivity. With these conceptual tools in hand, a revised understanding of science in public policy becomes possible. I will argue that understanding scientific integrity and objectivity in the manner I propose allows us to rethink the role of science in the policy process in productive ways, ways that allow us to see how to better democratize the expertise on which we rely, without threatening its integrity.

Key to this account is the growth in science advising in the United States. Prior to World War II, involvement of science with government was sporadic. Wartime, such as World War I, produced spurts of activity, but rather than producing a long lasting science-government relationship, these episodes developed the forms of the relationship that would be cemented after World War II. That war was the watershed, when science established a permanent relationship with government, both as a recipient of federal support and as a source for advice. Yet the road since World War II has not been smooth. Chapter 2 will detail both how the forms of science advice originated and the ups and downs of science advising since then. Although the specific avenues for advising have shifted in the past fifty years, the steadily expanding importance of science for policymaking will be apparent.

This continual expansion is crucial to note because even as scientists were becoming more central figures in policymaking, philosophers of science were formulating an understanding of science that would turn a blind eye toward this importance. Chapter 3 will examine how the idea of the science advisor came to be excluded from the realm of philosophy of science. In particular, I will examine how the current ideal for value-free science came into existence. Although some normative impulse to be value-free has been part of the scientific world since at least the nineteenth century, the exact form of the value-free ideal has shifted. At the start of World War II, most prominent philosophers rejected the older forms of the ideal as unworkable. The pressures of the cold war and the need to professionalize the young discipline of philosophy of science generated a push for a new value-free ideal, one that was accepted widely by the mid-1960s, is still predominant among philosophers, and is reflected by scientists. I will describe

how this ideal came into existence and how it depends crucially on a belief in the autonomy of science from society.

Chapter 4 begins the critique of this value-free ideal. As we will see in chapter 3, the current value-free ideal rests on the idea that scientists should act as though morally autonomous from society, in particular that they should not consider the broader consequences of their work. Chapter 4 disputes this claim, arguing that scientists must consider certain kinds of consequences of their work as part of a basic responsibility we all share. Because of this responsibility, the value-free ideal cannot be maintained. Values, I argue, are an essential part of scientific reasoning, including social and ethical values.

This raises the question of how values should play a role in science, a question addressed in chapter 5. There I lay out a normative structure for how values should (and should not) function in science, and I argue that at the heart of science values must be constrained in the roles they play. The crucial normative distinction is not in the kinds of values in science but in how the values function in the reasoning process. While no part of science can be held to be value-free, constraints on how the values are used in scientific reasoning are crucial to preserving the integrity and reliability of science. By clearly articulating these constraints, we can see the difference between acceptable science and politicized science, between sound and junk science.

If science is and should be value-laden, then we need an account of objectivity that will encompass this norm, an account that explicates why we should trust specific scientific claims and what the bases of trust should be. In chapter 6 I provide that account, arguing that there are at least seven facets to objectivity that bolster the reliability of science and that are wholly compatible with value-laden science. We can have objective and value-laden science, and explicating how this is possible clarifies the basis for science's reliability and authority.

Returning to the nuts and bolts of science in policymaking, chapter 7 concerns how we should understand the needed integrity for science in the policy process given the pervasive role for values in science. I will argue that attempts to separate science from policy have failed, but that the integrity of science can be brought into focus and defended in light of the philosophical work of the previous chapters. With a more precise view of scientific integrity, we can more readily understand the sound science–junk science debates and see our way through them.

Finally, in chapter 8 I present some examples of how these considerations lead to a different understanding of science in policy, and how that understanding includes an important role for the public in the practice of policymaking. In particular, I will address the problem of how to make the proper role of values in science accountable to the public in a democracy.

Philosophers might immediately object to the trajectory of this argument on the grounds that I am confusing the norms of theoretical and practical reason. In philosophy, this distinction divides the norms that should govern belief (theoretical) and those that should govern action (practical). The basic distinction between these realms is that values should not dictate our empirical beliefs (because desiring something to be true does not make it so), even as values might properly dictate our actions (because desiring something is a good reason to pursue a course of action). John Heil (1983, 1992) and Thomas Kelly (2002), for example, have challenged the sharpness of this distinction, and while I will draw from some of their work, I will not attempt to resolve the general tensions between theoretical and practical reason here. Instead, I will argue that (1) simply because science informs our empirical beliefs does not mean that when scientists make claims based on their work they are not performing actions; and (2) the intuition that values should *not* dictate beliefs is still sound. Indeed, I will argue that values dictating belief would violate the norms of good scientific reasoning, thus preserving an essential aspect of the distinction between theoretical and practical reason. But dictating beliefs is not the sole role values can play. And the actions of scientists as voices of authority cannot be handled properly by merely concerning ourselves with theoretical reasoning. Making claims is performing an action, and some concerns of practical reason must be addressed. How to do this without violating the core norms of theoretical reason is at the heart of this book.

The arguments I will present in the following chapters have been developed against the backdrop of current discussions in philosophy of science, particularly on values in science. In addition, there has been some philosophical attention to science in public policy since 1990, although this has not been a central area of concern (for reasons discussed in chapter 3). Before embarking on the trajectory I have laid out above, it will be helpful to situate the arguments to come among this work.

The most careful examiner and defender of the value-free ideal for science since the 1990s is probably Hugh Lacey. In his 1999 book, *Is Science Value-Free?*, Lacey develops a three-part analysis of what it means to be value-free. He distinguishes among autonomy (the idea that the direction of science should be completely distinct from societal concerns), neutrality (the idea that the results of science have no implications for our values), and impartiality (the idea that scientific reasoning in evaluating evidence should involve only cognitive and never social or ethical values) (Lacey 1999, chap. 10). Lacey strongly defends the impartiality thesis for science, arguing for a strict distinction between cognitive (for example, scope, simplicity, explanatory power) and noncognitive (for example, social or ethical) values, and for the exclusion of the latter from scientific reasoning. Lacey's conception of impartiality captures the current standard core of the value-free ideal, as we will see in chapter 3. He is more moderate in his defense of neutrality and autonomy, arguing that neutrality is only a plausible ideal if one has sufficiently diverse "strategies" or approaches to research within disciplines, something Lacey finds lacking in many areas of current scientific practice, particularly in the arena of plant genetics (Lacey 2005, 26–27). Autonomy in research is even more difficult to assure, as the importance of funding in science has grown (see chapter 2). And recent careful reflection on policymaking for science seems to suggest that autonomy may not be desirable in the ideal (see, for example, Guston 2000; Kitcher 2001).[9] While I will not address the issues of neutrality and autonomy here, I will be directly critiquing the ideal of impartiality, which Lacey views as logically prior to the other two. If my criticisms hold, then all three theses of value-free science must be rejected or replaced.

Hugh Lacey is not the only philosopher of science who has defended the value-free ideal for science while examining areas of science crucial for policymaking. Kristen Shrader-Frechette has held a steady focus on the role of science in policymaking, providing in-depth examinations of nuclear waste handling and concerns over toxic substances, and using these examples to develop concerns over the methodological flaws and weaknesses of some risk analysis processes (Shrader-Frechette 1991, 1993). Her views on the proper role for values in science have also followed the traditional value-free ideal. For example, in *Risk and Rationality* (1991), she argues that, "although complete freedom from value judgments cannot be achieved, it ought to be a *goal* or ideal of science and risk assessment" (44). In *Burying Uncertainty* (1993), when describing "methodological value judgments," she considers the traditional epistemic values, which are acceptable under the value-free ideal for science, and problems of interpretation with them (27–38). She explicates clearly how the reliance on these values can create problems in risk assessment, but no alternative norms for scientific reasoning are developed. It might seem she undermines the value-free ideal in her book *Ethics of Scientific Research* when she writes, "Although researchers can avoid allowing bias and cultural values to affect their work, methodological or epistemic values are never avoidable, in any research, because all scientists must use value judgments to deal with research situations involving incomplete data or methods" (Shrader-Frechette 1994, 53). However, the importance of the value-free ideal becomes apparent when Shrader-Frechette equates objectivity with keeping the influence of all values to a minimum, and still only "methodological" (or epistemic/cognitive) values are acceptable (ibid., 53). In this book, I will disagree with Shrader-Frechette on this point, arguing that the value-free ideal needs to be rejected *as an ideal*, and making a case for a replacement set of norms for scientific reasoning. In addition, Shrader-Frechette contends that scientists are obligated to consider the consequences of their work because of a professional duty as scientists (Shrader-Frechette 1994, chap. 2). I, however, will argue in chapter 4 that the obligation to consider the consequences of one's choices is not a duty special to a profession or role, but a duty all humans share.

The work of Helen Longino is probably the closest to my own position on how to understand the proper role of values in science. Rather than starting with a focus on policymaking, Longino has worked from the feminist philosophy of science literature that developed out of feminist critiques of science in the 1980s. In *Science as Social Knowledge* (1990), she lays out a framework for understanding the ways in which values can influence science, particularly through the adoption of background assumptions. She distinguishes between constitutive and contextual values in science, arguing that both influence science in practice and content (4). Longino develops her account of values in science by examining the functioning of background assumptions in scientific research relating to gender. To address the concerns about the objectivity of science raised by these examples, she suggests that we think of science as an essentially social process, and she develops a socially based view of objectivity that can inform how science should function (ibid., chap. 4).[10] While her work serves as a useful starting point for more in-depth discussions on the role of values in science and the social nature of science, I depart from her framework in several ways. First, I want to provide a more closely argued account for how the adoption of an ostensibly empirical background assumption could "encode" values, work I have begun elsewhere (Douglas 2000, 2003a). Second, I do not utilize her distinction between contextual and constitutive values in science because I want to maintain a focus on both the scientific community and the broader community within which science functions, and dividing values into the internal and external at the start obscures one's vision at the boundary. In addition, as Longino herself argues, the distinction can provide no ground for ideals for science, as it is a thoroughly porous boundary between the two types of values. (This issue is discussed further in chapter 5.) Finally, while I appreciate and utilize Longino's emphasis on social aspects of science, I think we also need clear norms for *individual* reasoning in science, and this book aims to provide those. Thus, while my views on objectivity, as articulated in chapter 6, draw insight from Longino, I do not rest the nature of objectivity on social processes alone.

In addition to these philosophers who have grappled with the role of values in science, two writers have provided important insights on the role of science in policymaking. I see this book as expanding on the insights from these earlier works. Sheldon Krimsky, for example, has contributed much to the discussion on science and technology in public life and policymaking, focusing on the biological sciences and their import (Krimsky 1982, 1991, 2000, 2003; Krimsky and Wrubel 1998). Krimsky's discussions are wide ranging, and only some pick up on the themes of this book, as most of his work centers on the relationship between the public and the uses of new technology. The book that most closely relates to the concerns to be considered here is *Hormonal Chaos*, an overview of the endocrine disruptor debate, where Krimsky addresses problems of the acceptance of evidence as a basis for both science and policy (Krimsky 2000). While his discussion of that particular debate is rich, the general implications for understanding science in policy are not fully developed. For example, in order to get us beyond the sound science versus junk science debate, Krimsky briefly mentions a new ideal, "honest science," which he describes as "science that discloses financial interests and other social biases that may diminish the appearance of objectivity in the work" (ibid., 187). While this sketch is suggestive, it needs further development. Which interests are relevant and why? Why is the exposure of interests important to the integrity of science? How does this fit with the ideal of value-free science, in which one's interests are not to interfere with the interpretation of evidence? Answering these questions with more in-depth normative work is one of the purposes of this book. I will propose that it is not a full disclosure of interests that is needed for the integrity and objectivity of science, but an explicit and proper use of values in scientific reasoning. Not all interests are relevant to the doing of science, and some kinds of influence arising from interests are unacceptable, even if disclosed. Situating the scientist with respect to his or her interests is a good start, but not normatively sufficient.

In contrast to the work of Krimsky, Carl Cranor's *Regulating Toxic Sub-*

stances (1993) is more focused on the topic of the general use of science in public policy. Cranor examines the implications of accepting (or rejecting) certain levels of uncertainty in science to be used as a basis for policy, and provides a careful account of the processes of risk assessment and the uncertainties involved. I take up his focus on the trade-offs between underregulation and overregulation and expand their reach beyond how public officials and administrators should think about science to how scientists and philosophers of science should think about science, given science's central importance in the policy process. In particular, I will address how these insights lead to rethinking our understanding of norms for scientific reasoning, the nature of objectivity, and how to differentiate junk science from sound science.

Scientists and philosophers still largely hold to the value-free ideal.[11] Some claim that the value-free ideal is essential to the authority of science, to objectivity, or to the very possibility of having reliable knowledge (for example, Lacey 1999, 223). The critiques of the value-free ideal to date have been based on its untenability rather than its undesirability. I will take the latter road here and provide an alternative ideal in its place. Having a clearer understanding of how values should, and should not, play a role in science, working from the foundations of both moral responsibility and proper reasoning, should provide a clearer framework with which to examine the role of science in policymaking.

Thus, this book is about how scientists, once engaged in a particular area of research, should think about the evidence, and should present their findings, given the importance of science in policymaking. This area has reached a philosophical impasse of sorts. The value-free ideal requires that ethical and social values have no influence on scientific reasoning in the interpretation of evidence. But works like Longino's and Cranor's suggest that something is quite amiss with this ideal, that values and interests are influential for scientists, and perhaps properly so. What we need is a reexamination of the old ideal and a replacement of it with a new one. The ideal I propose here is not just for the social functioning of science as a community, but for the reasoning processes of individual scientists, for the practice of science and for the giving of scientific advice.

In addition to this philosophical literature on values in science and science in policy, there are related bodies of work that will not be directly addressed in this book. For example, Shrader-Frechette (1991, 1993), as well as Douglas MacLean (1986), have also done considerable work on which values shape, or should shape, our management of risk. This work argues for more ethically informed weighing of the consequences of policy decisions, suggesting that qualitative aspects of risk, such as its distribution, the right to avoid certain risks, voluntariness, and the valuation of life, are crucial to a complete understanding of risk in our society.[12] In this book, I do not engage in debates over which values in particular should inform our judgments concerning risk, but instead focus on how values in general should play a role in the science that informs our understanding of risk.

To finish setting the bounds of this book, a few caveats are in order. First, some hot-button issues will not be discussed. For example, debates over science education have been chronically prominent in the past few decades, as first creationism, and now intelligent design, seek to challenge the content of science education through school boards and textbook disputes, rather than through the scientific debate process of journal publications and conferences. My concerns here are not with what science to teach to the young, but with what science to depend upon to make decisions in public policy.[13]

Second, the book focuses exclusively on the natural sciences as a source of desired expertise. My neglect of the social sciences, such as psychology, sociology, and economics, arises partly from the need for less complexity in the book, and partly from a desire to avoid the debates over the scientific standing of the social sciences. Social sciences also raise unique problems of reflexivity, as the subjects of the research can read and understand the research, and alter their behavior as a result. How the ideas I develop here would apply to such contexts awaits future work.

Finally, while my treatment brings philosophical attention and analysis to the role of science in public policy, an issue much neglected by philosophers in the past forty years, science is a changing institution in modern society, and many have noted that the most important issues of science in society may not center on science and *public* policy as we enter the twenty-first century. Since the 1990s, the sources of funding for science and the institutional settings for research have been changing, with potentially important consequences (Greenberg 2001; Krimsky 2003). Over half of the funding for scientific research in the 2000s comes from private sources. With intellectual property concerns keeping much private research private, one may wonder whether the issues I address here are really salient. Such a critic would have a point, but until philosophy of science ceases to define itself solely in epistemological terms, such issues can hardly be addressed.

I see this book as part of a reorientation of the discipline of philosophy of science, to begin to take seriously again a philosophical (that is, conceptual and normative) examination of science as it functions in society in *all* of its aspects. With such a reorientation, we will be better able to address the issues presented by these changes in scientific practice, and the implications for policy for science, broadly construed.

CHAPTER 2

THE RISE OF THE SCIENCE ADVISOR

WITH THE CURRENT OMNIPRESENT NEED for science advice, how to ensure the soundness of such advice has become an ongoing source of difficulty in government. Yet the need for sound science advice was not always obvious. At the beginning of the twentieth century in the United States, there were no regular avenues for science advice to the government, much less regular contact between scientists and policymakers. Although the National Academy of Sciences had been founded during the Civil War to provide science advice to the government, after the Civil War the academy drifted from advising prominence to being primarily an honor society for the nation's budding scientific community. By 1900, there were some scientists embedded within governmental agencies with particular needs, but the larger scientific community had no relationship with the government. This pervasive absence of academic scientists in the halls of power was not a source of concern for policymakers, politicians, or academic scientists.

A century later, the relationship between science and government has blossomed. Even with the chronic debates over which science is most trustworthy, the need for regular science advice is unquestioned. In the United States, federal laws and federal regulations help to structure and ensure the quality of scientific advice, and many avenues and standing bodies provide such advice. Although there are debates over whether science advisors are heeded too much or too little, the need for science advice from a broad base of scientists is obvious. How did this dramatic shift in the perceived importance of science advice come about? In this chapter, I will chronicle the rise of the science advisor in the United States, from an occasional partner in governmental endeavors to a mainstay of the policy process.[1]

Several aspects of this story are of particular note. First, World War II is not the beginning of the story, as is often assumed. Crucial tentative steps and experimentation with how to develop a science-government relationship took place prior to World War II. Second, the successes from the pre–World War II era served as the basis for the rapid scaling up that takes place post-1940. This should give hope to any who wonder whether new institutional mechanisms for grappling with science advising in a democracy can take hold (mechanisms which will be explored in chapter 8). Rapid expansion can occur, even as institutions remain fluid. Third, the scientist-advisor becomes a pervasive feature by the late 1950s, the same time that philosophers of science come to ignore this important role (as we will see in chapter 3). Finally, the tensions that would drive the sound science–junk science debates discussed in the last chapter are apparent by the 1970s, even as earlier problems with science advising are resolved.

The rise of the science advisor was not a simple, linear path. Prior to World War II, science advising in the United States occurred in fits and starts, producing uneven success. World War II was the watershed event that made permanent a close relationship between science and government. Once the need for a strong relationship was cemented, the question remained what shape that relationship should take. The success of advising during the war depended on close, personal ties that could not be sustained in the context of the burgeoning science and government relationship after World War II. Although the war demonstrated the pervasive importance of science to government, it did not establish how the relationship between science and government should be structured. Further experimentation and evolution in that relationship took place as the institutionalization of the relationship deepened. Key templates were drawn, however, from the first half of the twentieth century, most notably the national lab, the contract grant, and the science advisory committee.

Science and the U.S. Government Prior to 1940

A relationship between science and government in the United States was not created wholesale from the crucible of World War II. The makings of that relationship had roots at least as deep as the nineteenth century, such

that by 1900 the importance of having some scientists in government was clear. As Dupree (1957) wrote in his chronicles of science and the federal government, "In the first years of the twentieth century a government without science was already unthinkable" (288). The science that was a part of many government activities was located in a scattered federal research establishment, with agencies as diverse as the Geological Survey, the National Bureau of Standards, the Department of Agriculture, and the Bureau of the Census.[2] In the first decade of the twentieth century, the Public Health Service was added to this pantheon. These diverse bureaucratic entities engaged in scientific research to help guide policy decisions and to assist with scientific issues of public interest. Thus, having some needed scientific activity within the federal government was a given, but the relationship was generally ad hoc and internal, generating scientific research within the government when and where it was deemed necessary. Ties to the broader scientific community were largely absent.

In addition to this budding federal research establishment, there was a little utilized mechanism for seeking external scientific advice. In 1900, the sole formal science advisory body in existence was the National Academy of Sciences (NAS), created as a separate entity from the U.S. government during the Civil War to provide a standing resource for external science advice. According to its charter, the NAS was to respond "whenever called upon by any Department of the Government" to requests to investigate scientific issues. The NAS was to have its expenses reimbursed for such work, but was to receive no compensation (Dupree 1957, 139–40); thus was the independence of the academy to be assured. However, by the end of the nineteenth century, the inherent passivity embedded in the charter—to not respond unless called upon—had turned the NAS into a standing honor society, not an active advisor to the government (ibid., 294).

The compartmentalized nature of federal research and the inactivity of the NAS created new challenges with the coming of World War I. In fact, when the need for outside expertise became apparent after the sinking of the *Lusitania* in 1915, it was not to the NAS that the government first turned. Instead, Secretary of the Navy Josephus Daniels contacted Thomas Edison (who, as an engineer, was not considered a proper candidate for the NAS) to help organize a committee of experts to screen the numerous unsolicited inventions that were being sent to the navy to aid with the coming war effort (Dupree 1957, 306; Kevles 1995, 105). In addition to the unpaid screening efforts of the newly formed Naval Consulting Board (NCB), Edison and Daniels persuaded Congress that monies were needed to fund research directed by the NCB, research that could be more properly targeted at the needs of the navy than the whims of America's many inventors. The $1 million appropriation was the beginning of the Naval Research Laboratory (Dupree 1957, 307).[3] National Academy members were nowhere to be seen on the NCB, which was composed primarily of engineers. When asked why the NAS was so absent from the NCB, one inventor quipped, "Possibly because they have not been sufficiently active to impress their existence on Mr. Edison's mind" (quoted in Kevles 1995, 109).

This state of affairs aggravated at least one NAS member, astronomer George Ellery Hale, who began pushing for more NAS involvement with the war effort. The National Academy proved too stultified to be of assistance to the government, and the National Research Council (NRC) was formed in 1916 as a more active arm of the NAS (Dupree 1957, 309; Kevles 1995, 112). By 1917, the NRC and the NCB were in direct competition over which body could produce a reliable submarine detector first (Kevles 1995, 116). Both groups had some success developing early sonar devices, although scholars dispute whether these early devices or the convoy system was more important in protecting shipping from submarine attacks.[4] What *was* successful was the mobilization of a large-scale research effort targeted at a particular problem. The NRC was made a permanent body in May 1918.

As World War I ended, the NRC sank into inactivity with respect to the government, instead focusing on helping to bring U.S. scientists from various scientific societies together (Dupree 1957, 329). The growth in science funding and institutions for the next decade took place largely in the booming industrial labs of the period. The one major effort to provide support for basic research in the United States, the National Research Fund proposed by Hoover in 1926, was supposed to draw contributions from the private sector, but it was a "complete and immediate failure" (ibid., 342). Against this backdrop of government malaise with respect to science stands the bright spot of the National Advisory Committee on Aeronautics (NACA). Formed in 1915 to help the United States keep pace with developments in the new arena of flight, NACA was composed of twelve presidential appointees, with members drawn from the army, navy, Weather Bureau, Bureau of Standards, the Smithsonian, and the public—that is, nongovernmental scientists with relevant expertise (ibid., 287). By 1920, NACA had its own lab at Langley Field, which included one of the world's few wind tunnels (Zachary 1997, 85). The National Advisory Committee on Aeronautics also collaborated with universities in conducting their research (Dupree 1957, 334).

In the 1920s, NACA and its lab thrived and its annual budget grew from $5,000 in 1915 to $500,000 by 1927 (Kevles 1995, 105; Dupree 1957, 334).[5] The advisory committee provided a successful model for how government could support scientific research, attract good outside advice, and remain flexible in the face of change.

With the onset of the Great Depression in 1929, scientific research suffered. Budgets for science dropped precipitously by 1933 throughout government, as well as in industrial labs and universities. Indeed, some believed that science had helped to cause the Depression by making production so efficient that workers were no longer needed. Even among the many who still valued science, there was little impetus to fund science, a provider of long-term societal benefits when acute crises of employment and hunger were at hand (Dupree 1957, 345–47). Despite the decline of funding for science, the apparent need for good science advice grew during this period, particularly with a New Deal emphasis on careful planning and use of resources. With the NRC and the NAS largely quiescent, when a request came from Agriculture Secretary Henry A. Wallace for advice on the reorganization of the Weather Bureau, the NRC replied that it could provide little help with the task. Instead, it suggested that a more flexible policy-oriented body be formed within the auspices of the NAS. By executive order, President Roosevelt created the Science Advisory Board (SAB) in 1933, a committee of prominent scientists in academia and industry who would create and run subcommittees focused on all manner of scientific issues important to government. Massachusetts Institute of Technology President Karl Compton was named chairman of the SAB, with other board members drawn largely from the natural scientists who were part of NAS. Within two years, over one hundred scientists had served on SAB subcommittees, and numerous reports and recommendations had been made to the government on the organization of research and other technical questions (Compton 1936, 35–36).

Yet the successes of the SAB in providing advice to the government's scientific bureaucracy were overshadowed by two prominent failures. First, the SAB pushed for funding to employ out-of-work scientists in research endeavors for the public interest. The proposal was ultimately rejected, much to Compton's disappointment (Dupree 1957, 353–57; Pursell 1965, 346). Second, internal divisions arising in 1934 over the appointment of non–academy members to the SAB undermined its ability to be effective (Pursell 1965, 347–48). When the more broadly based National Resources Board (NRB) began to look at the need for science research planning, the apparent need for the SAB abated. The SAB was abolished by the end of 1935, and its planned successor body in the NAS rapidly faded into oblivion (Dupree 1957, 358).

Even though the SAB failed to provide the desired permanent avenue for science advice, it did presage many of the issues that would arise in the coming decades. First, even as it faded from the scene, the SAB predicted that the need for science advice would only grow. As noted in its second annual report,

> With government in a stage of transition from the more passive and regulatory part played in the past, to one of more intelligent and broad supervision and initiation, it is the concern of every citizen that there be available to government the most competent and impartial advice which can be found. The endurance of our traditional form of government will depend in increasing measure upon the quality of expert judgment, tempered with experience, which is available to government, and the willingness of government to follow such judgment. (Quoted in Compton 1936, 31)

How to obtain trustworthy advice would be an issue throughout the coming decades, as would how to keep science advisors from being abused. Indeed, Compton noted the presence of tensions still remarked upon today in the science advising process: "As fact-finding agencies the scientific services should be free to produce results that are not discolored by the opinions and snap judgments of policy-making political groups who may wish to put the dignity of 'science' behind their plans in order to win public approval" (ibid., 32).

An additional important shift in the relationship between science and government arose from the work of the NRB, altering the landscape of how science was viewed by the government.[6] While the SAB had drawn its ranks almost exclusively from natural scientists, the NRB included social scientists and educators as well, thus bringing together a more comprehensive view of research activities and planning possibilities. Interested in understanding and assisting all of these intellectual enterprises together, the NRB came to view research as a national resource. It broadened concern for research beyond government scientists focused on particular problems to include scientists in industry and academia, and suggested that all of these research activities were a national concern and thus collectively of interest to the government. As noted by Dupree (1957), this was a major shift in the view of science: "The logic of the broadened view of research as a national resource led to a concept of the government's responsibility extending beyond its own [science] establishment" (360). By the late 1930s, the idea that government should fund science beyond the activities of government scientists was being put into practice by pioneering agencies such as the National Cancer Institute (created in 1937) and NACA (ibid., 366). And one key way in which these agencies provided external science funding was through the contract research grant, which provided funds to scientists for particular research projects. This allowed for both autonomy for the scientist and accountability to the agency providing the funding. It was a mechanism that addressed many of the concerns over science funding between scientists and the government, one whose use would expand greatly in the coming decades.

As World War II approached, the SAB had been dissolved, with no mechanism for general science advising replacing it. Despite the SAB's call for the importance of science advice, new mechanisms and pathways for advising would have to be developed in the crisis of World War II. But the decades leading up to the war provided much of the raw material with which the science-government relationship would be so greatly expanded, from national labs to advisory bodies to contract research grants.

The Watershed of the Second World War

World War II, the largest conflict in human history, initiated a new relationship between science and government in the United States. Central to this change was the fact that the importance of science for American survival and prosperity were amply illustrated during the war. The stunning successes of radar, penicillin, and most dramatically, the atomic bomb, made the country aware of how powerful an ally science could be. This obvious importance would translate into permanent government funding sources for nongovernment scientists in the post–World War II era. In addition, the need to have scientists close to government decisionmakers also became apparent. Without technical men in the halls of power, many of the science-based successes of the war would not have come to pass, as the effort would not have been expended to bring those endeavors to fruition. Neither the funding of nongovernment scientists nor the use of external science advisors was entirely new, but the scale and intensity of such interactions vaulted these relationships to a prominence not seen before.

What was needed to unite and grow the scattered endeavors of the 1920s and 1930s into an all-out war effort was the right person in the right position. That person was Vannevar Bush, who would become President Roosevelt's personal liaison to the science community. Made president of the Carnegie Institution of Washington in January 1939, Bush had previous experience in both industrial science (as a director of the company Raytheon) and academic science (as professor of electrical engineering and subsequently vice-president of MIT) (Zachary 1997, 39–45, 84). In addition, by October 1939 he was chairman of NACA, and thus understood both its functioning and its successes (ibid., 99; Dupree 1957, 370). Bush embodied the experience needed to organize a war effort across the arenas of science in the United States. Although Bush was a Yankee Republican, Democrat Roosevelt found in Bush the link he needed to the scientific community. When Bush met personally with Roosevelt in June 1940 about forming a new organization to mobilize the full scientific community in the United States for the coming war effort, Roosevelt quickly signed off on everything for which Bush asked (Zachary 1997, 112; Kevles 1995, 297). As summarized by Bush's biographer, "In minutes, the president had promised Bush a direct line to the White House, virtual immunity from congressional oversight, and his own line of funds. The seeds had been sown for their extraordinary relationship" (Zachary 1997, 112). Solely by Roosevelt's authority was the National Defense Research Committee (NDRC) formed, operating entirely out of presidential discretionary funds, and thus not requiring any congressional oversight.

The members of the NDRC were drawn from the government departments of war, navy, and commerce, plus members from the National Academy of Sciences and other esteemed scientists, with Bush as the chairman.[7] Bush recruited James Conant (president of Harvard), Karl Compton (president of MIT and former head of the Depression-era SAB), and Frank Jewett (president of the NAS and head of AT&T's Bell Laboratories) to help him run the fledgling council. The crucial task of the NDRC was to structure the giving of government support to nongovernment scientists in a way that would not threaten the autonomy so valued by the scientists (Greenberg 1999, 79; Zachary 1997, 114–16). Bush adapted the contract research grant approach from NACA for wartime, and began the dispersal of funds on a much larger scale than ever seen before. The formal framework of the contract research grant, within which science and government could work closely together and still tolerate each other, changed the traditionally frosty relationship between the scientific community and government officials. With contract research grants, the government could pay scientists who were not government employees to conduct research, which allowed university scientists to do crucial pieces of work for the government without forcing the scientists to leave the university setting. In the first year of NDRC's existence, it had a budget of over $6 million and authorized over one hundred research contracts to universities and industrial labs (Zachary 1997, 117; Kevles 1995, 298).

Although large compared to previous grant-funding operations, within a year it was apparent to Bush that the NDRC would not be sufficient for the war effort. By the spring of 1941, even before the United States formally entered the war, the research projects supported by the NDRC had grown, and mere research by the nation's top scientists no longer seemed adequate to meet pressing defense needs. Development of research into usable assets was also needed, which would require substantially more money. In response, Bush helped create the Office of Scientific Research and Development (OSRD) in May 1941, which subsumed the yearling NDRC (Kevles 1995, 299–300; Zachary 1997, 129). The OSRD received its appropriations from Congress, allowing much larger sums to be funneled through the office, sums adequate to develop microwave radar, proximity fuses, solid fuel rockets, and penicillin into usable tools for the military.

This expansion of research effort occurred *before* the United States committed to the largest of all scientific projects of the war, the Manhattan Project. It was not until the fall of 1941 that the crucial report from Britain arrived in the states with the analysis that made the atom bomb project look feasible. Prior to this, there were deep concerns over the technical possibility of generating sufficient fissile material for a weapon, as it was thought both that more fissile material was needed than is the case and that the difficulties in generating the material were likely insurmountable. The British report (known as the MAUD report[8]) made it clear how feasible producing an atom bomb was, thus prompting a serious commitment to the project in late November 1941, just before Pearl Harbor. By mid-1942, the Manhattan Project had been taken over by the military, and large-scale national labs were being planned to complete the project, with military security. (The army took over the directing of construction for the project sites—Oak Ridge, Hanford, and Los Alamos—and the majority of the funding) (Hewlett and Anderson 1962, 73–74). Thus, the OSRD's scale of effort had little to do with the Manhattan Project, and speaks to the enormity of all the other research efforts occurring during the war. Indeed, the costs for the radar systems alone ($1.5 billion) rivaled the costs for the entire atom bomb project ($2 billion) (Kevles 1995, 308). For many of the physicists involved in these projects, the bomb project was not the central activity of the war.

As Kevles notes, "The atom bomb only ended the war. Radar won it" (ibid., 308).

In addition to the research efforts set up by OSRD at existing universities, new large national labs were created at Argonne, then Oak Ridge and Los Alamos. Although the idea of a national laboratory had a long history in the United States, dating back at least to the founding of the Naval Observatory in 1842, and continued in such efforts as NACA's lab at Langley Field, the size of the labs originated in the 1940s tends to obliterate their predecessors from view (see Westwick 2003, 27). The earlier, smaller labs served as inspiration for the World War II labs, although the national lab idea had never been implemented on so grand a scale.

The scientific endeavors during World War II also changed how scientists viewed both the government and the process of research. Scientists found having such grand efforts, and the resources involved, a wonderful experience (Greenberg 1999, 98–99), and they also enjoyed having access to the halls of government. But the relationship between Bush and Roosevelt was primarily a personal one. No formal systems that could continue it beyond wartime, and outside of the trusting relationship between Bush and Roosevelt, had been established. Although the need for having men of science in close contact with men of power seemed clear, how this was to work beyond the scope of the war was not.

More obvious at the close of the war was the need for continued federal funding of science. Only Frank Jewett argued against some continuation of the new, pervasive, and close ties between science and government. Jewett suggested in 1945 that government funding of science was not needed because the "amounts of money involved in support of first-class fundamental science research—the only kind that is worthwhile—are infinitesimally small in comparison with national income," and thus could be supported by private sources (quoted in Greenberg 1999, 141). However, as Greenberg notes, "Jewett was a minority of one" on this issue (141). The rest of American scientists enjoyed the possibilities that large-scale federal funding presented to them, and political leaders, shaped in part by Bush's *Science: The Endless Frontier*, desired to make continued use of the security and largesse science could provide the country.

Thus, it was generally accepted at the end of the war that science and government would have to maintain the close ties developed during the war. However, how these ties were to be institutionalized was not clear. With a sense of urgency, but not a definitive plan, decentralization of the science-government relationship characterized the immediate postwar

period. But unlike the previous crises of the Civil War and World War I, where close affiliations (or attempts at institutionalizing a link) between science and government faded and drifted after the wars' end, the need for the ties went unquestioned and thus eventually the ties became stronger after World War II. Although there were debates over the form of that relationship in the postwar years, the general effort to find acceptable institutional avenues between science and government never abated. For this reason, the Second World War is seen as a watershed in the history of American science policy.[9]

The Ascent and Institutionalization of Science Advisors (1945–70)

As World War II drew to a close in 1945, and Bush published *Science: The Endless Frontier*, it became clear that new administrative structures were needed between science and government. The personal relationship Bush had enjoyed with the president had died with Roosevelt that April. Truman had much less interest in or tolerance for the scientists who ran the OSRD. The organization could not survive the war, in any event, having been premised upon the extreme conditions of wartime, and built for speed and secrecy. New organizations would be needed for the postwar era. But the debates in the years immediately after the war over the details of the new National Science Foundation and over the control of atomic energy consumed the efforts of scientists and policymakers. These debates would shape the science policy landscape for decades, but prevented any efforts at creating a comprehensive approach to policy for science, or to science advice for policy. Instead, approaches and organizations were developed as needed, leading to the pluralistic, but pervasive, system we have today.

The debate over the founding of the National Science Foundation has been elaborated in detail elsewhere (see, for example, Kleinman 1995; Bronk 1975). Although there was little disagreement on the need for a federal agency that dispersed contract grants to scientists throughout the country, disagreements did arise over the precise shape of such an agency. Questions such as whether geographic distribution should be a consideration in funding decisions, whether the social sciences should be included in the NSF funding stream, and most importantly, the amount of control the president should have over the NSF, thwarted a quick resolution on legislation. Indeed, the issue of presidential control most delayed the creation of the NSF, preventing final legislative passage until 1950. Truman refused to sign any legislation that did not place the head of the NSF directly under his authority (as opposed to under the authority of a board of trustees).

While this debate delayed the creation of the NSF, other avenues of funding for science sprang up to amply fill the void—particularly through the newly created Office of Naval Research (ONR) and Atomic Energy Commission (AEC).[10]

As possible avenues for research funding proliferated (including NSF, ONR, AEC, NACA, and the National Institutes of Health [NIH] by 1951), creating a patchwork of funding sources rather than a single unified source, the avenues for science advising also proliferated, albeit at a slower pace. Bush, lacking good personal ties to Truman, felt no longer needed by the White House, and left in early 1946 (Zachary 1997, 302–9). No science advisor took his place; instead, science advising moved to the agency level. The most prominent of the time was the General Advisory Committee (GAC) to the AEC. The creation of the AEC was perhaps the most important political success of the scientific community after World War II, as it ensured civilian (rather than military) control over nuclear power.[11] The Atomic Energy Act of 1946, which created the AEC, also mandated the creation of the GAC to provide advice to the AEC. Chaired by Robert Oppenheimer from 1946 to 1952, and then by I. I. Rabi from 1952 to 1956, the GAC boasted prominent scientists such as Glenn Seaborg, Lee DuBridge, and Enrico Fermi as members. Although the GAC's opposition to the hydrogen bomb was not heeded (see Herken 2000, chap. 3), it had a powerful influence over policy, particularly in its early years. As Hewlett and Duncan (1962) described the committee, with its "distinguished membership," "it spoke with the voice of authority" (46). The first chair of the AEC, David E. Lilienthal, "fell into the habit of asking the GAC for all kinds of advice" (Lapp 1965, 104). While the GAC was the primary advising body to the AEC, the AEC also developed additional science advisory bodies, such as the Advisory Committee on Reactor Safeguards, as needed (Brooks 1964, 75).

In addition to the AEC's GAC, other advising committees of scientists also popped up in the post–World War II era. Most concerned themselves with advice on the direction of science funding. One early example, instigated by Bush's efforts to generate continuous science advising for the direction of military scientific research, was the Joint Research and Development Board, created in 1946. The board oversaw an elaborate collection of advisory committees, drawn from scientists in civilian life (Price 1954, 144–45). While the board allowed scientists to maintain contact with the military in the postwar years, it ultimately proved unworkable, both because the military was unwilling to allow the board to have final decisionmaking authority over research projects and because the board was not structured

well to serve the needs of the military (ibid., 145–52; Smith 1992, 49). The board was ultimately abolished in 1953, but the loss of science advice to the military as a whole would not last. In 1956, the Defense Science Board was formed, an institution that successfully served the military in the decades following (Smith 1992, 50–60).

Although scientists were experimenting with multiple ways to advise various parts of the government, the five years after World War II had failed to produce any clear avenue for science advice for the president. Truman did not create an official science advisory body in the White House until 1951. War was still an impetus for bringing scientists into closer contact; the Korean War, in this case, was the prime motivator for Truman's request to William Golden for a report on better ways to utilize civilian scientists (Herken 2000, 55). Golden recommended that Truman create the position of a personal science advisor to the president, with an attached advisory committee (ibid., 55; Smith 1990, 111–12). However, the governing board of the newly formed NSF was concerned over the possible existence of the presidential science advisor, and initially opposed the plan on the grounds that such an advisor would undercut the function of the NSF to formulate national science policy (Bronk 1974, 117). In response to these concerns among scientists, Truman created the Science Advisory Committee (SAC), consisting of eleven prominent scientists, but he placed it within the executive branch's Office of Defense Mobilization (ODM), not in direct contact with the president (Burger 1980, 7–8). Although SAC was supposed to be a source of advice for both the president and the ODM director, Truman rarely consulted it (Smith 1992, 163). In the meantime, the NSF did little to direct national science policy. With the pluralistic funding structure in place, and an explicit demand that the NSF focus on basic research only (avoiding the defense research controlled by the Department of Defense [DOD] and AEC and leaving medical research to the NIH), the NSF worked on setting up its own administrative procedures in the 1950s and abandoned efforts to direct general science policy (Walker 1967; Bronk 1975).

With the change of president in 1952, the role of scientists in government again shifted. Because of an inherent interest in science advice, Eisenhower made better use of the Office of Defense Mobilization's SAC, requesting their advice on issues such as intercontinental ballistic missiles and the alert status of U.S. bombers (Smith 1990, 112; Smith 1992, 164). The launch of Sputnik in 1957 caused Eisenhower to elevate SAC to the body initially envisioned by Golden six years earlier. Eisenhower renamed SAC the Presidential Science Advisory Committee (PSAC), and appointed

James Killian as his special assistant to the president for science and technology (Burger 1980, 7). The Presidential Science Advisory Committee then named Killian as chair. At last, the president had an official science advisor in the White House. Although PSAC would survive as an institution until the 1970s, its heyday was in its first six years. Both Eisenhower and Kennedy were interested in hearing from PSAC, and they listened conscientiously to their science advisor. Thus, 1957–63 is often called "the golden age of presidential science advising" (Smith 1992, 165).

Sputnik inspired increased interest in and concern about science from many quarters, not just the president.[12] Scientists organized the Parliament of Science, convened by the American Association for the Advancement of Science (AAAS) in March 1958 to address a range of issues relating to science and society, including the need for science advice in government. The report from the meeting concluded that "representation of science and technology at the highest levels of government where . . . national policies are formulated" was needed, and as the newly elevated PSAC and science advisor "does seem to furnish the necessary high-level representation," the report "strongly endorse[d] the continuation of such an arrangement" (Parliament of Science 1958, 855). Some observers saw the new PSAC and its chair as a crucial counterbalance to science advice from more militant scientists who had the ear of the AEC (see, for example, Lapp 1965, 135). By 1960, roughly one hundred scientists served in an advisory capacity to the government through PSAC panels (Gilpin 1964, 8n16). By 1963, that number had increased to 290, only 32 of whom were directly employed by the government (Leiserson 1965, 412). Thus, PSAC became a major avenue for civilian and university scientists' influence on federal policy.

Yet pressures for increased scientist involvement continued, even with the creation and expansion of PSAC. As the government-science institutional structure grew, PSAC was quickly overburdened. To alleviate the rapidly growing set of tasks before PSAC, the Federal Council for Science and Technology (FCST) was formed in 1959 to address issues arising from within the government research structure, such as tensions among NASA, the NSF, and the AEC (Wolfle 1959, 48–49; Leiserson 1965, 414). This allowed PSAC to focus on general policy advice and the concerns of the broader scientific community. To help coordinate the tasks of PSAC and FCST, the science advisor to the president served as chair of both. In 1962, President Kennedy added yet another science advising body, the Office of Science and Technology (OST), to be headed by the chair of PSAC and FCST (Leiserson 1965, 415). The OST was finally to provide a home for

the general coordination of research, the job the NSF was never able to take on, as well as a liaison for Congress (Lapp 1965, 199–200). Jerome Wiesner, Kennedy's official science advisor, thus had three additional hats to wear: chair of PSAC, chair of FCST, and director of OST. Some called him the "science czar" (ibid., 200).

The need for science advice was felt not just at the level of the president. In addition to this elevation and centralization of science advice in the White House, there was a growing sense that one needed good science advice for policy development across the executive branch. For example, the State Department set up the Office of Science Advisor in the late 1950s to assist the department and its embassies on matters scientific (Wolfle 1959, 31). Beyond the federal government, attempts to set up science advisory bodies at the state and local level, in imitation of the federal government, proliferated in the 1960s, with uneven success (Sapolsky 1968). Even political parties saw the need for science advice. The Democratic National Committee appointed an Advisory Committee on Science and Technology, made up primarily of prominent university scientists from a wide range of fields, to advise its Democratic Advisory Council (Lapp 1965, 181–85); the committee existed to help devise policy platforms for the 1960 election. The importance of scientists providing advice became increasingly obvious across government institutions and was viewed as something requiring serious attention. The United States had come quite far from the days of the First World War, when those in government did not bother approaching scientists for advice.

With this rise in prominence came an increased reflection by scientists on the proper role of the science advisor. According to the scientists involved with the Democratic National Committee, their role was motivated by the idea "that the citizen-scientist has a responsibility to think about the problems of science and society and to communicate his thoughts to those who can convert ideas into the fabric of national policy" (ibid., 182). But the scientist was not to become the politician, as "scientific and technological facts should not be the property of any political party." Concerns over protecting the integrity of science, echoes from the 1930s era SAB, were becoming more prominent. But the primary focus remained on being available to serve society by giving advice. This ethos of providing advice led to public service both within the government proper and in the public eye in general.[13]

As the institutional structures for science advice proliferated in the late 1950s and early 1960s, the general number of scientists serving in advisory capacities and the time commitment of those scientists also increased greatly. In addition to the formal commitment to sit on a standing advisory body, many scientists participated in the less formal summer studies programs, in which scientists would gather to discuss a particular problem and to provide targeted advice (Greenberg 1999, 28). Rather than complaining about being ignored, as they had prior to World War II, some scientists grumbled about the time advising the government took away from their research. For example, in 1962 Hans Bethe noted that he "spent about half of [his] time on things that do not have any direct connection with my science," and worried about the impact of such extra-research commitments on both the senior statesmen of science like himself and on younger scientists (quoted in Wood 1964, 41–42).

Despite the apparent quandary of being called upon too much, difficult times lay ahead for science advisors. Part of the difficulty would arise from strained relations between PSAC and President Johnson (Herken 2000, 146–64). But personal ties were not the only thing at issue. Although the prominence of science advisors had been on the rise, deep tensions in the structure of science advising lay dormant, just below the surface. One of the most important was the extent to which science advisors to the president were free to speak their thoughts publicly, including in front of Congress. This was a particular problem for the special science advisor to the president, who was also head of OST. His advice to the president was confidential, but as director of OST, he had an obligation to be forthcoming with Congress about the state of U.S. research (Leiserson 1965, 415; Price 1965, 242). How this tension over confidentiality and openness was to be resolved remained unclear. In addition, as budgets tightened with the expense of the Vietnam War, Congress increasingly demanded tangible results from research expenditures, while constraining the expansion of research budgets. Finally, many members of the public had developed antiscience views by the end of the 1960s (Brooks 1968; Price 1969). All of these factors placed strain on the advising system, strain that would become explosive by the early 1970s.

Controversy and Regulation of Science Advisors (1965–75)

By the mid-1960s, the federal advisory structure was well established, if impermanent. However, tensions arose over the proper role of the science advisor, and where such an advisor's loyalties ultimately resided. These tensions would bring about the dissolution of PSAC in the early 1970s, but not the end of science advising. Instead, institutional structures for science advising

again proliferated, and tensions that arose were mitigated (or even resolved) by the passage of the Federal Advisory Committee Act (FACA) in 1972. The need for this act indicates how central science advice for policy had become to the policy process. The importance of and difficulties with science advising warranted its own legislation.

The passage of FACA had its origins in the difficulties of PSAC under both the Johnson and Nixon administrations. As noted in the previous section, PSAC, with all of its subcommittees, involved several hundred scientists by the mid-1960s, and this wide base continued under the Nixon administration (Perl 1971, 1211). However, divisions within the scientific community undermined the effectiveness of PSAC. In particular, scientists supporting weapons test bans or opposing new armaments were often pitted against scientists supporting new weapons systems. In the 1960s, the "peace" scientists often advised the government through PSAC, whereas the "war" scientists often advised the government through the Department of Defense or the national laboratories (Smith 1992, 167). Johnson was irritated by scientists who spoke against the Vietnam War while serving in an advisory capacity (ibid., 168); Nixon was even less tolerant of criticism coming from science advisors.

Initially, it was hoped that the standing of the science advisor would improve with the change from the Johnson to the Nixon administration. But early in Nixon's first term, Lee DuBridge, Nixon's special assistant for science and technology, was frozen out of national security and budget decisions (ibid., 169–70). Although PSAC and DuBridge had other issues on which they could focus, particularly the public's increasing concern over environment and health issues, their effectiveness was uneven (ibid., 170). The downfall of PSAC began when the first of two scandals involving the advisory committee and the Nixon White House erupted in 1969. PSAC was not consulted directly on Nixon's antiballistic missile (ABM) program (having been excluded from many national security decisions); however, prominent PSAC alumni provided a steady stream of criticism in congressional testimony about the program. DuBridge, attempting to help Nixon, drafted a letter of support, but was unable to get other PSAC members to sign it (ibid., 172–73). These events did not sit well with the administration; as Bruce L. R. Smith recounts, "Nixon and Kissinger were angered at the lack of support from the scientists, and Kissinger was reinforced in his belief that the committee [PSAC] was filled with administration critics operating under the pretense of scientific neutrality." The scientists of PSAC, "who saw themselves as the essence of objectivity," viewed the episode as "proof that the administration did not want advice but merely public support" (ibid., 173). Administration officials, on the other hand, considered the behavior of the scientists to be disloyal, and believed that science advising "was becoming chaotic" (Henry Kissinger, quoted in ibid., 173).

The supersonic transport (SST) controversy, coming right on the heels of the ABM issue, exacerbated the rift between PSAC and the administration. An ad hoc PSAC panel, headed by Richard Garwin, had been put together to study supersonic transport. The Garwin report, finished in 1969, was highly critical of SST's prospects, for both economic and environmental reasons. The report was kept confidential and, in 1970, administration officials then testified in Congress that, "According to existing data and available evidence there is no evidence that SST operations will cause significant adverse effects on our atmosphere or our environment. That is the considered opinion of the scientific authorities who have counseled the government on these matters over the past five years" (quoted in Hippel and Primack 1972, 1167). This was in direct contradiction to the Garwin report, however. When Garwin was asked to testify before Congress on the issue, he made clear that the PSAC report was contrary to the administration position. Concerted effort by a member of Congress got the report released, and the administration was sorely embarrassed (Hippel and Primack 1972, 1167). After his reelection in 1972, Nixon asked for and received the resignations of all PSAC members and refused to refill the positions (Smith 1992, 175). He transferred the science advisor position to the National Science Foundation, out of the executive office. The advisory committee had been killed and the science advisor position would not be reinstated to the executive office until 1976, and then only by an act of Congress (ibid., 178).

The most obvious lesson of these controversies was the danger of confidential science advice given to the executive branch, particularly when dealing with issues not related to national security. The executive branch could either make public the detailed advisory report, or keep it confidential, and in either case make claims that the advisors supported the project or policy choice. Many observers saw this as a dangerous imbalance of power and/or an abuse of science's authority. Thus, Martin Perl lamented the limited advice scientists gave to Congress compared to the executive branch, and criticized the confidentiality of much science advice: "Large numbers of advisory reports are made public; but, unfortunately, it is just those reports which concern the most controversial and the most important technical questions that are often never made public, or only after a long delay. This is unfortunate . . . for the process of making technical decisions in a democracy" (Perl 1971, 1212–13). The problem of confidentiality led Frank von Hippel and Joel Primack (1972) to call for "public interest science," in which scientists would act to advise the public, Congress, or special interest groups directly in order to counter the authoritative but secretive weight of executive office science advice.

Congress, however, came up with a legislative response to the problem. After a lengthy development, the Federal Advisory Committee Act was made law in 1972. Among its provisions, which govern all federal advisory committees, are requirements that advisory committee meetings be publicly announced within a sufficient time frame prior to the meeting date, that meetings be open to the public (with few exceptions), that meeting minutes be made available to the public, and that committees be balanced in their composition (Jasanoff 1990, 46–48; Smith 1992, 25–31). Meetings were opened to the public to help prevent potential abuses of power. Advisory committee reports as well as deliberations were to be made public, except when national security dictated otherwise. Advice on public issues concerning the environment, consumer safety, or transportation choices could not be secret anymore. The advisory system would no longer be able to be used to create "a facade of prestige which tends to legitimize all technical decisions made by the President" (Perl 1971, 1214).

In addition to dealing with the problem of secret advice, FACA also addressed the second lesson from both the demise of PSAC and other prominent controversies of the day (such as the debates over DDT): that scientists can and would enter the political fray, often on opposing sides of an ostensibly technical issue. It was apparent that scientists could be experts and advocates simultaneously. (Indeed, Hippel and Primack called on scientists to be both advocates and experts.) If scientists could have such different views on technical topics that they naturally served as advocates for different sides, a balance of the range of scientific views was needed on advisory committees. The Federal Advisory Committee Act thus required that advisory committees be "fairly balanced in terms of points of view represented and functions to be performed" (FACA statute, quoted in Jasanoff 1990, 47). This requirement addresses the traditional concerns with getting the appropriate expertise for any given panel, that is, expertise from relevant disciplines, while also recognizing the range of scientific views one is likely to find among scientists and the need to balance them. Exactly what "balance" means, and which point of view should be considered, remains a source of controversy. Indeed, by the 1990s the problem of which scientific experts to trust (given the frequent presence of scientists on multiple sides of an issue) would blossom into the sound science–junk science debate discussed in chapter 1.

Despite these concerns over science advising and the role of scientists,[14] the need for scientists in advisory roles continued to grow in the 1970s, particularly with the expansion of the regulatory agencies. Although PSAC was dismantled by the executive office, this singular decline was more than made up for in the expansion of science advising at the agency level. Even in 1971, estimates of the number of scientists involved in the broadly construed executive advisory apparatus ran into the thousands (Perl 1971, 1212). New legislation (for example, the National Environmental Protection Act, Clean Air Act, and Clean Water Act) increased the need for science advice to inform public policy. New agencies were created to implement the regulatory laws (such as the Environmental Protection Agency [EPA], Occupational Safety and Health Administration [OSHA], and Consumer Product Safety Commission [CPSC]). To provide these agencies with independent (from their staff scientists) advice, science advisory boards were created and proliferated at an astonishing rate.[15]

While wielding no direct power over agencies (such power would be unconstitutional), these advisory boards have become a major tool for the legitimation of policy decisions. Often created initially by the agency in order to help vet their science-based policy decisions, these boards became mandated by Congress in many cases in order to ensure that science used in policymaking would be sound. A negative or highly critical review by an advisory board almost always sends a policy back to the agency for reconsideration.

However, as noted in chapter 1, these advisory boards have done little to squelch controversy over science-based policy. While it was hoped that the advisory boards might decrease the adversarial debates that were becoming the norm in science-based policymaking, they did not. Rather, they became yet another player embroiled in the debates.[16] The scientific uncertainties usually were too deep to be settled by adding another layer of scientific review. Even when multiple review panels came to the same conclusion, it was not clear that the scientific issues were settled.[17] In addition, the National Research Council's reports, considered the gold standard for science advice, often failed to produce clear consensus statements on key policy points, such as the risk saccharin poses to humans (Bazelon 1979, 278). Publicly dueling experts became the norm in high-profile policy decisions with scientific bases (see Nelkin 1975). Despite the now legislatively mandated and entrenched role for science advice in policymaking, chronic debates over

sound science and junk science still occurred. The federal science advisory apparatus, now extensive enough to be called the "fifth branch of government," did not help to resolve these difficulties (Jasanoff 1990).

Conclusion

From the broad perspective of a century, the importance of science advising has steadily grown. Although there were fits and starts in developing the relationship between government and science advisors, the role of the science advisor changed from a sporadic, occasional influence, to a strong, personal relationship, to a deeply entrenched institutional necessity. Scientists now clearly occupy influential and important roles throughout our government, providing advice on a wide range of technically based issues. While the agencies that they advise cannot be required to follow their advice, it is uncomfortable and politically risky to ignore it, particularly since FACA was enacted, requiring that the advice be made public. In this regard, scientists giving advice wield real power and influence (even if their advice is not always followed as they would like). Even when science advice is divided, and we have competing or dueling experts, simply ignoring the experts is not a viable option. Yet on which experts to ultimately rely remains a source of contention. In this climate, disputing parties have been reduced to merely attempting to tarnish unwelcome expert opinion as "junk science" or "politicized science."

One might think that given the obvious normative tensions involved with science advising, and its importance in the second half of the twentieth century, philosophers of science might have been able to assist with sorting out the issues involved with science in public policy. Yet despite the clear authority of the scientist's voice in contemporary society, this aspect of science has been largely ignored by philosophers of science.[18] This neglect, which grew out of the crucible of the cold war, was key to the adoption of the current value-free ideal for science.[19]