CHAPTER 11

SHEILA JASANOFF

JUDGMENT UNDER SIEGE: THE THREE-BODY PROBLEM OF EXPERT LEGITIMACY

The 2004 U.S. presidential election will be remembered for many things: the close margin of George W. Bush's victory in the electoral vote (he would have lost to the Democratic candidate, John Kerry, if only the state of Ohio had swung the other way); renewed questions about the viability of the electoral college; the inaccuracies of exit polling; and the stark division of the country's voting map into the "red" states of America's heartland and the "blue" states of its more cosmopolitan periphery. More curiously, it was also an election that pitted one perception of the relationship of science and government against another. On Kerry's side were multiple Nobel laureates and other leaders of the scientific community, vocally asserting that the Bush administration had betrayed science in the pursuit of crass political objectives.[1] These advocates cited the administration's lack of support for embryonic stem cell research, which many saw as the next great frontier in biomedicine; they also pointed to a series of White House actions manipulating or suppressing scientific data – on environment, public health, and defense – that the government had deemed inconsistent with its overall political strategy.[2] Against these charges, Republican representatives either issued denials or claimed a superior ethical sensibility, most explicitly so in George Bush' statement in the second presidential debate, "We've got to be very careful in balancing the ethics and the science…because science is important, but so is ethics, so is balancing life."[3]

This was not the way relations between science and government were scripted to work in mature democracies. For more than fifty years, cooperation, not friction, has been the order of the day in dealings between science and the state in technologically advanced nations. Indeed, the political scientist Etel Solingen predicted that there would be "happy convergence" between the goals of the state and its scientific communities, when there is "a high degree of consensus between state structures and scientists, who enjoy internal freedom of inquiry and relatively comfortable material rewards" (Solingen 1993: 43). More empirically minded researchers have shown that it is in the state's interest to sponsor scientists as a separate "estate" to assist in matters of policy formulation and implementation (Price 1965), a "brain bank" to draw on for policy legitimation (Boffey 1975), or a skilled and specialized labor force available to lend its authority to the state in times of national need (Mukerji 1989).

Sabine Maasen and Peter Weingart (eds.) (2005), Democratization of Expertise? Exploring Novel Forms of Scientific Advice in Political Decision-Making – Sociology of the Sciences, vol. 24, 1–16.

These findings are consistent with the vision of a new social contract between science and the state put forward by presidential adviser Vannevar Bush at the end of the Second World War: in exchange for continued governmental support and freedom to define their research priorities and methods, scientists would provide the public with beneficial discoveries and a trained workforce (Bush 1945). Put succinctly, the contract provided money and liberty in exchange for knowledge and technical skills. In reality, the liberty offered to science was never complete; state support always came with strings attached, and the strings have both multiplied and tightened over the years, so that science today operates within a thick web of social constraints. Vannevar Bush's hope of weaning American science from dependence on military aims, and so liberating scientists from national security controls, for example, turned out to be illusory (Dennis 1994, 2004). Other state priorities, from environmental protection to enhanced university-industry collaboration, have shaped both the content and structure of governmental funding programs. And ethical concerns have led to varied restrictions on the use of federal funds for animal, human and biotechnological research, as well as a host of accounting and reporting mechanisms to force science to explain itself better to its public sponsors (Stokes 1997; see also Kevles 1998; Guston 2000).

Yet in a liberal democratic order, in which the state must continually expose itself to "attestive witnessing" by citizens (Ezrahi 1990), scientists' cooperation in national projects remains an invaluable resource, and states for the most part have been unwilling to risk serious breaks with organized science for the sake of short-term political gains. Rancorous partisan politics of the sort that surfaced in the 2004 presidential election is therefore unprecedented in the annals of recent science and seems contrary to the spirit of the postwar social contract. If scientists and their expertise are of such immense value, then mere party politics ought not to disrupt the peaceful coexistence of science and the state. Why, then, have relations between science and the party in power have soured of late? Why, more specifically, have tensions arisen around biomedical funding, for decades one of the most pampered and cosseted areas of U.S. science policy?

In addressing these questions, I argue that the implicit contract between science and the state has subtly shifted focus in recent decades. Although public support for science remains of paramount concern to researchers and research institutions, the politics of science no longer centers solely on the size of appropriations. Only by continually reaffirming its utility in expanding domains of application can science assert sustained claims on the public till. At stake, therefore, is a deeper right to define how, when, by whom, and to what extent science will be integrated into the solution of public problems, and who, indeed, will frame those problems in the first place. These questions straddle the line between science and politics, or truth and power, and attempts to answer them entail inevitable boundary conflicts over where the role of science ends and that of politics or policy begins (on boundary conflicts involving science see Gieryn 1999). Precisely this sort of boundary struggle can be discerned in George Bush's desire to locate the stem cell controversy in the domain of "ethics" and "balancing life" – areas of acknowledged political supremacy – rather than in "science."

As the stakes have shifted, so too has the content of the decisions for which the state relies on science. Across a wide range of contemporary policy issues, uncertainty and ignorance militate against the design of unambiguous technical solutions. Broadly characterized by the label of "risk" (Beck 1992), the threats that states are asked to mitigate on behalf of their citizens require the assessment of complex trajectories of social, technological and environmental change. There is typically no single, universally agreed upon, correct outcome to these sorts of assessments. Incoherence, not consensus, is the normal epistemological condition in many domains of policy-relevant knowledge.

In offering opinions on such contested and indeterminate issues, scientists can no longer stand on firmly secured platforms of knowledge. The questions contemporary policymakers ask of science are rarely of a kind that can be answered by scientists from within the parameters of their home disciplines. Scientists instead are expected to function as experts, that is, as persons possessing analytic skills grounded in practice and experience, rather than as truth-tellers with unmediated access to ascertainable facts. Accordingly, the technical expert's attributes often include, but are rarely limited to, mastery of a particular area of knowledge. What politicians and society increasingly expect from experts in decisionmaking processes is the ability to size up heterogeneous bodies of knowledge and to offer balanced opinions, based on less than perfect understanding, on issues that lie within nobody's precise disciplinary competence. Judgment in the face of uncertainty, and the capacity to exercise that judgment in the public interest, are the chief qualifications sought today from experts asked to inform policymaking. In these circumstances, the central question is no longer which scientific assessments are right, or even more technically defensible, but whose recommendations the public should accept as credible and authoritative. That question leads immediately to a second-order query: whose judgment should we trust, and on what basis?

All this has important consequences for democracy. So long as scientists were called upon mainly to provide specialized information – or, in the familiar phrase, to "speak truth to power" – there was no need to worry unduly about their political accountability. Peer pressure, it was assumed, would keep scientists honest; deviations from standards of professional rectitude would be uncovered and corrected by communities whose central function was to discover the truth and make it public. The shift from science to expertise, and from knowledge to judgment, confounds this easy expectation. Holding persons accountable for speaking the truth is different from holding them accountable for exercising judgment. And yet, as I show below, the discourses and practices of accountability have not yet caught up with the changing role of experts in the political process. Accountability measures in many societies still focus on one or possibly two of the three bodies that are relevant to the effective integration of science and politics: the bodies of knowledge that experts represent ("good science"); the bodies of the experts themselves ("unbiased experts"); and the bodies through which experts offer judgment in policy domains. The democratization of expertise demands, I suggest, renewed attention to the third of these bodies – namely, the institutions of advice-giving. It is this neglected level of analysis that I foreground in this paper, arguing that attempts to ensure data quality and lack of bias

are not alone enough to serve the needs of democratic governance; they must be coupled with measures for securing the legitimacy of expert advisory bodies.

To this end, I begin by briefly discussing the disjunction between the rhetoric of scientific disinterestedness in U.S. science policy and the reality of science's thickening ties to society. I then use two phases of the American debate on the peer review of regulatory science to show how a reductionist rhetoric of "good science" – encompassing only the first of the three relevant bodies – continues to dominate the U.S. framing of the problem of expert legitimacy. That framing, I show, is deeply resistant to counter-discourses emanating both from academic research in science and technology studies (STS) and from national regulatory practice. One consequence of that framing, in turn, is to blur the lines of expert accountability, drawing attention away from the institutional setting of advice-giving and concealing the need for public review of expert judgments.

Contrasting the American approach with that of Britain and Germany, I next illustrate how partial vision is not unique to the United States: these political cultures have also dealt selectively with the three-body problem, each highlighting one body at the expense of the others. I conclude by discussing the need for a richer theorization of the authority of policy-related expertise. Through that work we can begin to supplement, and compensate for, the weaknesses of accountability systems that reduce the three-body problem of expert legitimation to one or another of its constitutive elements.

THE DISINTERESTEDNESS OF SCIENCE: RHETORIC AND REALITY

It is tempting to dismiss the scientific community's opposition to the Bush administration in 2004 as the complaints of a disappointed suitor. As the veteran science journalist Daniel Greenberg has documented, scientists dependent on the state for research support now constitute a powerful lobby, no less insistent in their demand for public funds than the beneficiaries of any other entitlement program (Greenberg 2001). This dependence, according to Greenberg, has bred a variety of deplorable behaviors in the scientific community, ranging from overselling the promises of research to outright fraud. Scientists, on this account, have lost faith in an administration that has not simply poured funds into new research frontiers identified by their communities, from climate change to embryonic stem cells. Political success has eroded what Greenberg sees as science's historically pristine ethical position – a position famously characterized by the sociologist Robert Merton as including the virtues of openness, communal sharing of results, and lack of interest in the financial or political consequences of inquiry (Merton 1973).

The overt political positioning of prominent scientists and scientific organizations in the 2004 U.S. presidential campaign was certainly a stark reminder that the years of ivory-tower science, guided by the Mertonian norms, are definitively over. With active state encouragement,[4] scientists in the United States and around the world have become avid entrepreneurs, not only in the search for nature's secrets but also in tirelessly seeking support for their work before and after the phase of discovery. The resulting multi-level engagement of scientists with politicians, venture capitalists, journalists, the mass media, patent lawyers, the courts, and the public renders almost

fantastic any residual notions of science's disinterestedness and detachment from society.

But the messiness of today's interactions between science and society is not news to academic observers of that relationship. At no point in the growth of modern science was detachment from society the norm (see, for instance, Shapin and Schaffer 1985; Golinski 1992; Jardine 1999; and for the modern period, Kevles 1987). Rather, science and other powerful social institutions – church, state, corporations, the media – have long engaged in negotiations about the nature and limits of the patronage that scientists enjoy, and the associated constraints on their liberty. Science's vaunted detachment, in other words, is a partial thing, achieved through societal interactions that are necessarily political. Galileo had to submit his beliefs formally to the strictures of the Catholic Church. Today, the controls on science are more subtle, if more pervasive: they relate, for the most part, not to scientists' substantive beliefs on particular issues, but to the means with which they are allowed to pursue certain lines of inquiry, the conditions under which their advice is sought, and the extent to which research trajectories are subordinated to political imperatives such as war or national security, environmental protection, or finding cures for life-threatening disease.

Clearly, then, it is both simplistic and ahistorical to claim that science became politicized for the first time at the turn of the 21st century, for arguably there never has been a time when the work of science was wholly distinct from the work of politics.[5] To be sure, substantial qualitative and quantitative changes have occurred in the performance of science and in its social, political, and economic links to society. Some have argued that the increased density of science-society interactions, particularly in the conduct of research, constitutes in and of itself a break with the past. European science policy scholars, in particular, have suggested that purely curiosity-driven, basic, or "Mode 1" research is a thing of the past. Instead, they say, we have entered the era of "Mode 2" science, characterized by wide-ranging interdisciplinarity, growing public-private collaboration, the rise of application-driven sciences, and increased demands for social accountability (Gibbons et al. 1994; Nowotny et al. 2001). These observations have rightly been seen as significant for the organization and funding of science, but their implications go further. Thoroughgoing changes in the production of science cannot but affect the foundations of scientific authority. As long as scientists could claim objective access to nature's laws, on the basis of observations unbiased by personal or political interests, that alone was sufficient to underwrite their expertise. With science more and more being produced in the service of social ends, the possibility of bias is far more evident, and the grounds of expert authority correspondingly in greater need of rearticulation.

Yet if the practices of science have evolved in the ways that scholars have documented, the political rhetoric around science has not kept pace, particularly in the United States. One looks in vain for explicit acknowledgment that expert deliberations are a site of hybrid judgment, combining technical and normative considerations. Instead, virtually all public pronouncements on the role of science in policy home in on the need for untainted science and the associated need to defend science from the corrupting encroachments of money and politics. Thus, the United States charged the European Union with maintaining an illegal and *unscientific* moratorium against the importation of genetically modified crops and foods in its 2004 case in

the World Trade Organization (Winickoff et al., in press). In a related vein, Europe's commitment to the precautionary principle has been widely decried by U.S. critics as a politically motivated opt-out from the intellectual rigor of *scientific* risk assessment – not taken on board as a valid normative response to uncertainty. U.S. scientists for their part have also tended to frame disputes over policy-relevant science in the black and white language of purity and deviance, whose logic is to represent scientists as accountable only to their own specialist peers. The Union of Concerned Scientists, for example, focused its February 2004 pre-election campaign on the need to restore scientific integrity in policymaking.

This lag between reality and rhetoric does not advance the cause of democracy. If science has always been in some deep sense political, then it is not the *fact* of science's embeddedness in politics that should any longer be of primary concern, but rather the *nature* of that embedding and its implications for accountable governance. When an American administration withholds research funds from a promising area of biomedicine, or denies the validity of the scientific consensus on climate change, the problem is not the threat that is thereby posed to the mythic purity of science. Of greater importance is the tacit change that such disagreements signal in the rules of the game by which science and politics have previously ordered their relations vis-à-vis each other. There is an apparent retreat from politicians' earlier deference to scientists' judgments on basic elements of science policy: when is it in the public's best interests to fund a promising line of research; and when is contested knowledge robust enough to justify policy action? Put differently, what seems to have eroded in the Bush era is not so much the integrity of science itself as scientists' influence over decisions at the nexus of science and politics – above all, over how to deliberate and how to act when knowledge and understanding are incomplete. It is that shift in the seat of judgment that calls for analysis.

Occurring largely outside the purview of formal legal and political institutions, such struggles over the institutional division of power between science and politics raise important questions for governance and political theory. At a time when the vast majority of public decisions involve sizeable components of technical analysis, any change in the relative positions of scientific and political judgment carries with it a displacement in the exercise of power, with possible consequences for participation, deliberation and accountability. Now no less than in 1960s, when Yale University political theorist Robert Dahl used it as the title of his seminal treatment of democracy, the question at the heart of politics remains, "Who governs?" (Dahl 1961). A difference, however, is that technical decisionmaking is now more visibly and continuously a part of the playing field of politics. Consequently, there is a need to enlarge the scope of political analysis to take on board, or retheorize, the role of experts in processes of governance. A look at two episodes in some 25 years of debate on the quality of regulatory science in the United States underscores the need for conceptual advances.

THE RECURSIVE POLITICS OF REGULATORY PEER REVIEW

The quality and reliability of science for public policy have been recurrent themes in the United States for more than a quarter-century (see particularly Jasanoff 1990).

Critics of policy-relevant science have sought to ensure its robustness, and a favorite device has been the review of the government's findings and conclusions by other, appropriately trained eyes. This demand supplements the more general requirement of public justification, minimally through notice and comment provisions, that has been a part of the U.S. administrative process since the mid-1940s. On the assumption that policymakers' judgments on science as on other matters will be mission-oriented, and hence potentially biased, critics have demanded that those judgments be submitted to validation by experts, or peer review. Ongoing controversy over the forms of peer review in U.S. regulatory decisionmaking offers an ideal site for reconsidering the rules of accountability that secure expert legitimacy in that country. Two moments in the peer review debate are of particular interest, the first occurring in the 1980s and the second in 2003 and 2004. Together, they illustrate the power of a framing of policy-relevant science that persistently denies its hybridity and normative content.

An issue that captured the attention of U.S. policymakers perhaps more than any other in the late 1970s was what to do about cancer-causing substances in the environment (for a detailed account of these developments, see Brickman et al. 1985). In 1971, President Richard Nixon declared a "war on cancer," which resonated with public fears of an insidious and irreversible disease that had become, with heart disease, one of the country's two biggest killers. Federal agencies responsible for regulating the environment, pesticides, food and drugs, cosmetics, consumer products, and worker health and safety took up the challenge of working out principles for assessing and controlling the risks of carcinogens. Operating under newly precautionary legislation, these agencies were charged with preventing harms to public health and the environment before they materialized. In the case of carcinogens, this meant identifying the hazardous substances, if possible, before they entered the commercial pipeline or were dispersed into the environment. To carry out that preventive mandate, regulators felt they had to make many conservative assumptions: about the mechanisms of cancer causation (e.g., no safe threshold of exposure); dose-response relationships (e.g., that cancer incidence at high exposure doses should be linearly extrapolated to low doses); and the relationship between humans and test animals (e.g., that humans should be assumed to be similar to the most sensitive test animals). Affected industries argued, for their part, that these assumptions were scientifically untenable and led to irrational, economically burdensome regulation. Agency risk assessments, critics charged, would not hold up to scrutiny if they were peer reviewed by impartial experts with no ties to the agencies' regulatory mission.

It emerged in the ensuing debate that the term "peer review" was highly malleable and functioned effectively as an instrument of boundary maintenance between science and politics, as well as between regulators and their critics (Jasanoff 1987). Virtually all interested parties agreed that the science underlying regulatory decisions ought to be reviewed in some fashion, but there the consensus ended. There were disagreements about who the reviewers should be, what should be reviewed, and how review processes should be structured and organized. In my 1990 study of these developments, I concluded that "peer review," had fallen together with the more general function of expert advice-giving (Jasanoff 1990). Scientific advisory committees had become what I termed a "fifth branch" of government, and they functioned

best when they conformed to standards of political legitimacy as well as technical rationality. Advisory processes produced the highest levels of participant satisfaction when they permitted the joint negotiation of technical and normative concerns and when expert advisers remained answerable to the publics affected by their judgments.

The peer review debate of the 1980s ended pragmatically in a victory for agency discretion and decentralized decisionmaking. An influential 1983 report by the National Research Council (NRC), the advisory arm of the National Academies, concluded, against industry advocacy to the contrary, that risk assessment functions should not be located within a single expert body but should rather be carried out separately by each relevant agency, consistent with its particular statutory mandate (National Research Council 1983). Called the Red Book because of its cover color, the report defined risk assessment as a purely technical activity, as distinct from risk management, a process taking account of economic and social factors. Yet background studies commissioned for the Red Book affirmed that risk assessment, too, was a hybrid process, calling for value judgments as well as technical analysis. Those findings buttressed the report's conclusion that risk assessment should remain within the control of authorized regulatory bodies – and, by extension, their legislative missions. Implicitly, the Red Book concluded that process and substance legitimately influence each other in regulatory analysis. While not cognizant of the academic literature in science and technology studies, the NRC report was in this respect compatible with emerging STS insights about the co-production of knowledge and norms (Jasanoff 2004).

In retrospect, we can say that the Red Book's practice was more sophisticated than its rhetoric, but – unreflexively adopted and with no theoretical underpinnings – the practice proved less influential than the rhetoric. Discursively, the report gave strong support to the characterization of risk assessment as a science, a view that powerfully informs regulatory discourse to this day. In terms of practice, the report offered a far more subtle view of the weaving together of analysis and judgment. In effect, the Red Book contained within its covers two contradictory views of risk

Table 1: Two Discourses of Risk Analysis

Dominant Discourse	Insights from Regulatory Practice
Risk assessment (RA) should be separate from risk management (RM).	Judgment enters into both RA and RM; there can be no clear separation.
RA should not include economic, social, and political concerns.	RA occurs within particular frames which reflect social and political values and may differ across cultures.
RA can be and should be science-based.	RA is limited by uncertainty and ignorance.
There is a clear boundary between science and politics; there exist pre-established criteria by which we can decide whether an analysis is science-based.	The boundary between science and policy is not given in advance; criteria are established by negotiation and convention.

assessment and regulatory science that would come into clearer focus over subsequent years (see Table 1).[6] Politically, however, it was the less nuanced and more easily instrumentalized view that proved more durable.

As if to illustrate this point, a second major episode in the politics of U.S. peer review began unfolding in the summer of 2003. On August 29 of that year, the Office of Information and Regulatory Affairs (OIRA) of the Office of Management and Budget (OMB), the economic arm of the executive branch, issued a *Proposed Bulletin on Peer Review and Information Quality*. The *Bulletin*'s stated purpose was to ensure "meaningful peer review" of science pertaining to regulation, as part of an "ongoing effort to improve the quality, objectivity, utility, and integrity of information disseminated by the federal government."[7] Specifically targeted was the category of "significant regulatory information," that is, information that could have "a clear and substantial impact on important public policies or important private sector decisions with a possible impact of more than $100 million in any year." The proposal, it was estimated, would have far-reaching influence across the federal agencies, requiring 200 or more draft technical documents to be subjected annually to OMB-supervised "formal, independent, external" peer review (Anderson 2003).

The *Bulletin*'s principal intellectual justification was that the quality of science crucially depends on peer review. As the text observed,

> A "peer review," as used in this document for scientific and technical information relevant to regulatory policies, is a scientifically rigorous review and critique of a study's methods, results, and findings by others in the field with requisite training and expertise. Independent, objective peer review has long been regarded as a critical element in ensuring the reliability of scientific analyses. For decades, the American academic and scientific communities have withheld acknowledgment of scientific studies that have not been subject to rigorous independent peer review (*Bulletin*, Supplementary Information, 68 *Federal Register* 54024).

These statements, and indeed the entire thrust of the *Bulletin*, assumed that science is a unitary form of activity, that peer review likewise is a singular, well-defined process, and that the application of peer review to all forms of science – including regulatory science – can therefore be viewed as unproblematic. Peer review was advanced as a kind of objective audit mechanism for policy-relevant science, to be applied as a backstop to studies conducted by and for regulatory agencies. This characterization downplayed the political implications of removing ultimate control of the review process from the jurisdiction of the regulatory agencies to the OMB and thereby the White House.

The *Bulletin* appeared to turn the clock back on years of policy learning. Not only was it oblivious to research findings on the interpretive flexibility of peer review, but it also went against the grain of the 1983 NRC Red Book in calling for a single, uniform process of validation, approved by OMB, for all types of regulatory science. The impulse toward standardization, overriding cross-agency differences in practice, was visible at many points in the proposal text, as exemplified by the following quotations:[8]:

> 54024: "Existing agency peer review mechanisms have not always been sufficient to ensure the reliability of regulatory information disseminated or relied upon by federal agencies."

> 54024: "Even when agencies do conduct timely peer reviews, such reviews are sometimes undertaken by people who are not independent of the agencies."
>
> 54025: "When an agency does initiate a program to select outside peer reviewers for regulatory science, it sometimes selects the same reviewers for all or nearly all of its peer reviews on a particular topic."
>
> 54025: "it is also essential to grant the peer reviewers access to sufficient information…"
>
> 54025: "the results are not always available for public scrutiny or comment."
>
> 54025: "experience has shown that they are not always followed by all of the federal agencies, and that actual practice has not always lived up to the ideals underlying the various agencies' manuals."[9]

Not surprisingly, the OMB proposal came under severe criticism from many quarters, including the highest reaches of organized science, where the move to draw regulatory peer review within the supervisory ambit of an already suspect executive branch was immediately perceived as political. In November 2003, the National Academy of Sciences hosted a public workshop at which were aired many research and practice-based objections to the proposal. By mid-December, the end of the official comment period on the proposed *Bulletin*, 187 written responses had been filed, some two-thirds critical of the proposal. At its February 2004 annual meeting, the American Association for the Advancement of Science (AAAS) adopted a resolution calling on OMB to withdraw the proposal. Reasons offered by AAAS and other opponents included fears of political interference, unnecessary bureaucratic hurdles, asymmetric treatment of experts funded by agencies and corporations (the proposal initially identified only the former as having a potential conflict of interest), and the rigidity of a "one size fits all" approach to review (see, for example, Steinbrook 2004; *Philadelphia Inquirer*, January 25, 2004).

For me personally these developments posed particular intellectual challenges. As an STS scholar whose work had specifically addressed the topic of regulatory peer review, I had a stake in opposing a policy initiative that seemed inconsistent with the basic findings of my and my colleagues' work. I was also aware that my own study of advisory committees could be, and had been, uncritically read as an endorsement of more stringent peer review, with little attention to my observations about the constructedness of policy-relevant knowledge.[10] Breaking a lifetime habit of standing apart from current controversies, I therefore participated in the National Academy workshop and, more exceptionally, submitted written comments to OMB urging that the proposal be retracted. My conclusions that regulatory science is different in context and content from research science, and that "peer review" therefore cannot be uncritically translated from one domain to the other, were referenced in the AAAS resolution and to some extent reported in the media. Their impact on OMB, however, proved slight.

On April 15, 2004, OMB issued a substantially revised proposal, taking note of many of the submitted comments.[11] The new version narrowed the scope of the most stringent peer review requirement to a newly defined category of "influential scientific information" containing, as a subset, "highly influential scientific assessments"; it also granted more flexibility to agencies to design their peer review procedures, and it removed the one-sided restriction on experts whose research was funded by

regulatory agencies. At the core, however, the proposal continued to embrace the notion of an autonomous science whose quality and objectivity could be improved in a straightforward way through critical scrutiny by "peers." Instructively, the revised proposal cited my work on advisory committees only to support the propositions that peer review practices are varied and that fair and rigorous review can build consensus around agency actions based on science. That regulatory science is, by its very nature, a site of politics was evidently inconsistent with the deeply entrenched Mertonian discourse of science's integrity, independence, quality and rigor. In this case, as we have seen, the discourse of scientific integrity masked a profoundly political institutional realignment between regulators and the White House. Neither scholarship nor practical wisdom was able to undermine a discourse that offered such substantial instrumental benefits to the ruling interests of the moment.

CULTURAL PRACTICES OF EXPERT LEGITIMATION

As in the United States. regulators in Britain and Germany accepted risk assessment as a principled approach to ordering knowledge and weighing policy alternatives, and risk analysis occupies a central place in both countries' practices for coping with the consequences of technological change.[12] Yet in neither European national setting has the methodological robustness of risk assessment received nearly the same attention as in the United States, and nowhere else have political battle lines been drawn around the design of regulatory peer review. Tacitly, at least, decisionmaking in both European countries takes on board the hybrid picture of risk judgments that represented one face of the 1983 NRC Red Book report (see Table 1). That hybridity, in turn, demands accountability to wider interests than those of relevant technical communities – forcing consideration of more than simply the body of policy-related knowledge. Accordingly, political representation remains part and parcel of the process of risk analysis in both countries, consciously built into the design of expert committees and consultative processes.

But even though the hybridity of risk judgments is generally conceded, practices for ensuring lack of bias remain partial and untheorized, reflecting different cultural traditions for the construction of public knowledge – traditions that I have elsewhere termed "civic epistemology" (Jasanoff 2005: chapter 10). On the whole, the focus in British regulatory circles is on the body of the expert: accountable judgment is sought through consultation with persons whose capacity to exercise judgment on the public's behalf is regarded as superior, even privileged. Though members of British expert panels can and do represent both technical specialties and social interests, ultimately it is the excellence of each person's individual discernment that the state most crucially relies on. To a remarkable extent the legitimacy of British expertise remains tied to the person of the individual expert, who achieves standing not only through knowledge and competence, but through a demonstrated record of service to society. It is as if the expert's function is as much to discern the public's needs and to define the public good as to provide appropriate technical knowledge and information for resolving the matter at hand.

Needless to say, this faith in individuals' power to see for the people could hardly exist in a more diverse or less empiricist cultural context, where common norms of

judging and assessing facts were felt to be lacking. A cost of the British stress on virtuous expert bodies has been to protect the assumption of common vision itself from critical examination. Consequently, a narrow group of experts can with the best will in the world make erroneous judgments on matters that were too complex for their collective reckoning. Britain's infamous "mad cow" disaster of the 1990s illustrated the hazards of blind faith in embodied expertise at the expense of due consideration to what experts know, or can know, and the institutional context in which they exercise their expertise.[13]

In Germany, by contrast, expert committees are usually constituted as microcosms of the potentially interested segment of society; judgments produced in such settings are seen as unbiased not only by virtue of the participants' individual qualifications, but even more so by the incorporation of all relevant viewpoints into a collective output. Reliance on personal credentials is rare in Germany unless it is also backed by powerful institutional supports. To be an acknowledged expert in Germany, one ideally has to stand for a field of experience larger than one's own particular domain of technical mastery. And it is ultimately the institutional context for forming communal expert judgments that matters most to producing social robustness.

The constitution of such bodies reflects something important about what counts as right reason in the German public sphere. The painstakingly representative character of German expert advisory bodies, their membership often specified in detail by legislation, encodes a belief that it is possible to map the terrain of reason completely; an accurately configured map can then be translated into an institutionalized instrument of decisionmaking. An expert within such an institution functions almost an ambassador for a recognized region or place from among the allowable enclaves of reason. Rationality, the ultimate foundation of political legitimacy in Germany, flows from the collective reasoning produced by authoritatively constituted expert bodies. A paradoxical consequence of this map-making approach to public reasoning is that expert bodies, once constituted, leave no further room for *ad hoc* citizen intervention. They become perfectly enclosed systems, places for a rational micro-politics of pure reason, with no further need for external accountability to a wider, potentially excluded, and potentially irrational, public.

These contrasts help throw the cultural specificity of U.S. legitimation practices, and their solution to the three-body problem, into sharper relief. Professional skills and standing count for more in the United States than the intangible qualities of individual judgment (as in Britain) or institutional representation and balance (as in Germany). In a meritocracy that prides itself on individualism and objective markers of intelligence (Carson 2004), the surest way to become an expert is by climbing the ladder of professional recognition. What an expert stands for or has achieved outside the spheres of method and knowledge is of lesser consequence. Civic virtue is not a prime desideratum in the appointment of experts, although the capacity for team play obviously plays a part in the nomination and selection of experts for important advisory positions.

Of course, U.S. policy is not wholly insensitive to possible imbalances in the constitution of expert groups. The Federal Advisory Committee Act seeks to correct for just this eventuality through its requirement that committees be balanced in terms of

the views they represent. Nonetheless, the dominant discourse of policy-relevant science remains unwaveringly committed to Mertonian ideals of purity and detachment, despite all scholarly demonstrations of hybridity and co-production. It is the perceived deviation from the transcendent objectivity of science that most often threatens expert legitimacy in the United States. Allegations that experts have been captured by political interests or by politically motivated research programs erupt in U.S. policy debates with a regularity unheard of in other modern democracies.

None of the three solutions to the problem of expert legitimacy provides for systematic lines of accountability running from experts to wider publics. Intensely political choices of individual experts and groupings remain concealed behind divergent national rhetorics and practices of accountability.

THEORY AS INTERVENTION: REGROUNDING THE LEGITIMACY OF EXPERTISE

Experts have become indispensable to the politics of nations, and indeed to transnational and global politics. Experts manage the ignorance and uncertainty that are endemic conditions of contemporary life and pose major challenges to the managerial pretensions and political legitimacy of democratically accountable governments. Faced with ever-changing arrays of issues and questions – based on shifting facts, untested technologies, incomplete understandings of social behavior, and unforeseen environmental externalities – governments need the backing of experts to assure citizens that they are acting responsibly, in good faith, and with adequate knowledge and foresight. The weight of political legitimation therefore rests increasingly on the shoulders of experts, and yet they occupy at best a shadowy place in the evolving discourse of democratic theory.

I have suggested that expert legitimacy should be reconceptualized as a three-body problem that pays explicit attention to each of the three bodies involved in producing expert judgments: the body of knowledge that experts concededly bring to decisionmaking; the individual bodies of the experts themselves; and the institutionalized bodies through which they offer judgment and policy advice. A brief study of the peer review debate in the United States illustrates the political hazards of too great an emphasis on the first body: the knowledge component of expert judgments. Coupled to an outmoded and uncritically accepted discourse of scientific purity, that emphasis has impeded wide debate by American scholars and publics on the credibility of experts and institutional foundations of their legitimacy.

A brief contrast with two European political systems shows that the U.S. approach, while possibly unique in its commitment to a transcendental notion of scientific integrity, is not unique in the partiality of its understanding of expert legitimacy. The U.K. emphasis on the embodied expert and the German preoccupation with rational expert collectives each militates against deeper questioning of the constituents of expert authority. More specifically, no national decisionmaking system has as yet taken on board the fundamental STS insight that experts *construct* – they do not simply *find* – the knowledge base on which they rest their hybrid analytic-deliberative judgments. In each democratic society, then, an imperfect framing of the problem of expertise has foreclosed the continuous dialogue between expert and

critical lay judgment that is imperative under contemporary conditions of ignorance and uncertainty.

Addressing this deficit in democratic practice requires us to recast the role of experts in terms that better lend themselves to political critique. Key to this move, as I have argued elsewhere, is to import notions of delegation and representation into the analysis of expert decisionmaking (Jasanoff 2003). Under a theory of delegation, experts can be seen as acting not only in furtherance of technical rationality, but also on behalf of their public constituencies, under cognitive and normative assumptions that are continually open to wider review. Equally, citizens need to recognize that governmental experts are there to make judgments on behalf of the common good rather than as spokespersons for the impersonal and unquestionable authority of science. In turn, this means that a full-fledged political accountability – looking not only inward to specialist peers but also outward to engaged publics – must become integral to the practices of expert deliberation.

We come, finally, to a concluding word on the role of scholarship and the relations of theory to practice. The history of expertise as a public problem in the United States and elsewhere suggests that deep reform – aimed not just at current policy practice but at its entrenched ideological foundations – cannot be effectively mounted at the surfaces of already framed debates and controversies. The long U.S. conversation on regulatory peer review illustrates the impediments to making critical voices heard within the press of politics as usual. To challenge, let alone change, deep-seated habits of mind and thought, embedded in resistant institutional practices, requires the would-be critic of expert rule to step out and away from the four corners of ongoing disputes. It calls for the tacit assumptions of the workaday political world to be made explicit, and new language may have to be found to describe previously unseen or taken-for-granted realities. Scholarship provides the platform for such intervention, and the power of the word stands ready to be embraced in the project of rejuvenating democracy.

Harvard University, Cambridge, MA, USA

NOTES

1 For a summary of these charges, see the statement on "Restoring Scientific Integrity in Policymaking" issued by the Union of Concerned Scientists on February 18, 2004, http://www.ucsusa.org/ (visited January 2005). See also US House of Representatives, Committee on Government Reform (Minority Report), *Politics and Science in the Bush Administration*, http://www.house.gov/reform/min/politicsandscience/pdfs/pdf_politics_and_science_rep.pdf (visited April 2004).

2 The Republican strategy included placating the religious right on issues relating to abortion (hence, by extension, stem cell research), as well as industrial special interests opposed to stringent controls on carbon emissions and other forms of environmental regulation.

3 CBS News.com, Text of Bush-Kerry Debate II, St, Louis, Missouri, October 8, 2004, http://www.cbsnews.com/stories/2004/10/08/politics/main648311.shtml (visited November 2004).

4 A notable example of such encouragement in the United States was the 1980 Bayh-Dole Act, which in effect required publicly funded researchers to seek commercial returns from their work. For critical accounts of the consequences of that legislation, see Press and Washburn (2000); Krimsky (2003).

5 For more on the deep linkages between the construction of scientific and political power, see particularly Jasanoff (2004).

6 Not all of the insights in the right-hand column, to be sure, were apparent to the authors of the Red Book. In particular, issues of framing and cross-cultural variation in risk assessment surfaced in these terms only in subsequent scholarly research, some of which took the Red Book and its assumptions as primary data for analysis. See, for example, Jasanoff 1986; Krimsky and Golding 1992).

7 *Proposed Bulletin on Peer Review and Information Quality* (hereafter cited as *Bulletin*), Summary, 68 *Federal Register* 54023, September 15, 2003.

8 All page citations are to the *Federal Register*, vol. 68, no. 178 (September 15, 2003).

9 I am indebted to John Mathew and John Price for identifying these extracts.

10 It was not the first time my work had been misread in the policy domain as affirming rather than critiquing dominant conceptions of the science-policy relationship. Other similar episodes included a misinterpretation of my work on science advice in a U.S. Supreme Court decision on the admissibility of expert evidence. See Jasanoff (1996).

11 http://www.whitehouse.gov/omb/inforeg/peer_review041404.pdf (visited January 2005).

12 The regulation of biotechnology provides an especially instructive site for observing national practices of regulatory practice and expert legitimation in action. See Jasanoff 2005.

13 In April 2000, the U.K. government estimated that the total cost of the BSE crisis to the public sector would be 3.7 billion pounds by the end of the 2001-2002 fiscal year. *The Inquiry into BSE and variant CJD in the United kingdom* [hereafter cited as *The Phillips Inquiry*] (2000), Volume 10, Economic Impact and International Trade, http://www.bseinquiry.gov.uk/report/volume10/chapter1.htm#258548 (visited April 2004).

REFERENCES

Anderson, F.R. (2003), "Peer review of data", *The National Law Journal*, September 29, 2003.

Beck, U. (1992), *Risk Society: Towards a New Modernity*, London: Sage.

Boffey, P.M. (1975), *The Brain Bank of America: An Inquiry into the Politics of Science*, New York: McGraw Hill.

Brickman, R., S. Jasanoff, and T. Ilgen (1985), *Controlling Chemicals: The Politics of Regulation in Europe and the U.S.*, Ithaca, NY: Cornell University Press.

Bulletin, *Supplementary Information*, 68 Federal Register 54024.

Proposed Bulletin on Peer Review and Information Quality (September 15, 2003), *Summary*, 68 Federal Register 54023.

Bush, V. (1945), *Science – The Endless Frontier*, Washington, DC: US Government Printing Office.

Carson, J. (2004), "The merit of science and the science of merit". in S. Jasanoff (ed.), *States of Knowledge: The Co-Production of Science and Social Order*, London: Routledge, pp. 181–205.

Dahl, R,A. (1961), *Who Governs?*, New Haven, CON: Yale University Press.

Dennis, M.A. (1994), "'Our first line of defense': Two university laboratories in the postwar American State", *Isis* **85**(3): 427–55.

Dennis, M.A. (2004), "Reconstructing sociotechnical order: Vannevar Bush and US Science Policy", in S. Jasanoff (ed.), *States of Knowledge: The Co-Production of Science and Social Order*, London: Routledge.

Ezrahi, Y. (1990), *The Descent of Icarus: Science and the Transformation of Contemporary Democracy*, Cambridge, MA: Harvard University Press.

Gibbons, M., C., Limoges, H, Nowotny, S. Schwartzman, P. Scott, and M, Trow (1994), *The New Production of Knowledge*, London: Sage Publications.

Gieryn, T. (1999), *Cultural Boundaries of Science: Credibility on the Line*, Chicago: University of Chicago Press.

Golinski, J. (1992), *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760-1820*, Cambridge, MA: Cambridge University Press.

Greenberg, D.S. (2001), *Science, Money, and Politics: Political Triumph and Ethical Erosion*, Chicago: University of Chicago Press.
Guston, D.H. (2000), *Between Politics and Science: Assuring the Integrity and Productivity of Research*, New York: Cambridge University Press.
Jardine, L. (1999), *Ingenious Pursuits: Building the Scientific Revolution*, London: Little, Brown.
Jasanoff, S. (1986), *Risk Management and Political Culture*, New York: Russell Sage Foundation.
Jasanoff, S. (1987), "Contested boundaries in policy-relevant science", *Social Studies of Science* **17**: 195–230.
Jasanoff, S. (1990), *The Fifth Branch: Science Advisers as Policymakers*, Cambridge, MA: Harvard University Press.
Jasanoff, S. (1990), *The Fifth Branch: Science Advisers as Policymakers*, Cambridge, MA: Harvard University Press.
Jasanoff, S. (1996), "Beyond epistemology: Relativism and engagement in the politics of science," *Social Studies of Science* **26**(2): 393–418.
Jasanoff, S. (2003), "(No) Accounting for expertise?", *Science and Public Policy* **30**(3): 157–62.
Jasanoff, S. (ed.) (2004), *States of Knowledge: The Co-Production of Science and Social Order*, London: Routledge.
Jasanoff, S. (2005), *Designs on Nature: Science and Democracy in Europe and the United States*, Princeton, NJ: Princeton University Press.
Kevles, D. (1987), *The Physicists: The History of a Scientific Community in Modern America*, Cambridge, MA: Harvard University Press.
Kevles, D.J. (1998), *The Baltimore Case: A Trial of Politics, Science, and Character*, New York: W.W. Norton.
Krimsky, S. (2003), *Science in the Private Interest: How the Lure of Profits Has Corrupted the Virtue of Biomedical Research*, Lanham, MD: Rowman-Littlefield, 2003).
Krimsky, S. and D. Golding (eds.) (1992), *Social Theories of Risk*, London: Praeger.
Merton, R.K. (1973), "The normative structure of science," in R.K. Merton (ed.), *The Sociology of Science: Theoretical and Empirical Investigations*, Chicago: University of Chicago Press, pp. 267–78.
Mukerji, C. (1989), *A Fragile Power: Scientists and the State*, Princeton, NJ: Princeton University Press.
National Research Council (1983), *Risk Assessment in the Federal Government: Managing the Process*, Washington, DC: National Academy Press.
Nowotny, H., P. Scott, and M. Gibbons (2001), *Re-Thinking Science: Knowledge and the Public in an Age of Uncertainty*, Cambridge, MA: Polity.
Philadelphia Inquirer (January 25, 2004), Editorial, "The White House vs. Science."
Press, E. and J. Washburn (2000), "The kept university", *Atlantic Monthly*, March 2000: 39–54.
Price, D.K. (1965), *The Scientific Estate*, Cambridge, MA: Harvard University Press.
Shapin, S. and S. Schaffer (1985), *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*, Princeton, NJ: Princeton University Press.
Solingen, E. (1993),"Between markets and the state: Scientists in comparative perspective," *Comparative Politics* **26**: 31–51, at p. 43.
Steinbrook, R. (2004), "Peer review and federal regulations", *New England Journal of Medicine* **350**(2): 103–4 (January 8, 2004).
Stokes, D.E. (1997), *Pasteur's Quadrant: Basic Science and Technological Innovation*, Washington, DC: Brookings Institution.
Winickoff, D., S. Jasanoff, L. Busch, R. Grove-White, and B. Wynne (in press), "Adjudicating the GM Food Wars: Science, risk, and democracy in world trade law," *Yale Journal of International Law*.