Peerless Science
Peer Review and U.S. Science Policy
Daryl E. Chubin and Edward J. Hackett

"Peer review is the principle on which the internal governance system of science has traditionally depended. During the past ten years there has been a great deal of evidence suggesting that that assumption is no longer valid (if it ever was), and that many of the strains in the science–government relationship in the U.S. are traceable either to the assumption itself or to the ways it is implemented. Chubin and Hackett examine the assumption and its implementation from different perspectives and explore its role in the science–government relationship.

"The authors display the ability to take peer review and use it as a springboard to illuminate a central issue of our time: the relationship between the conduct of science and the larger society in the U.S. in the waning years of this century. The writing itself—colorful, interesting, heartfelt—is surprising and refreshing."

—William A. Blanpied, National Science Foundation

This book examines the structure and operation of peer review as a family of quality control mechanisms and looks at the burdens placed on the various forms of peer review. Assuming that peer review is central to the functioning of U.S. science policy, Chubin and Hackett explore the symbolic and practical value of peer review in the making, implementing, and analysis of this policy.

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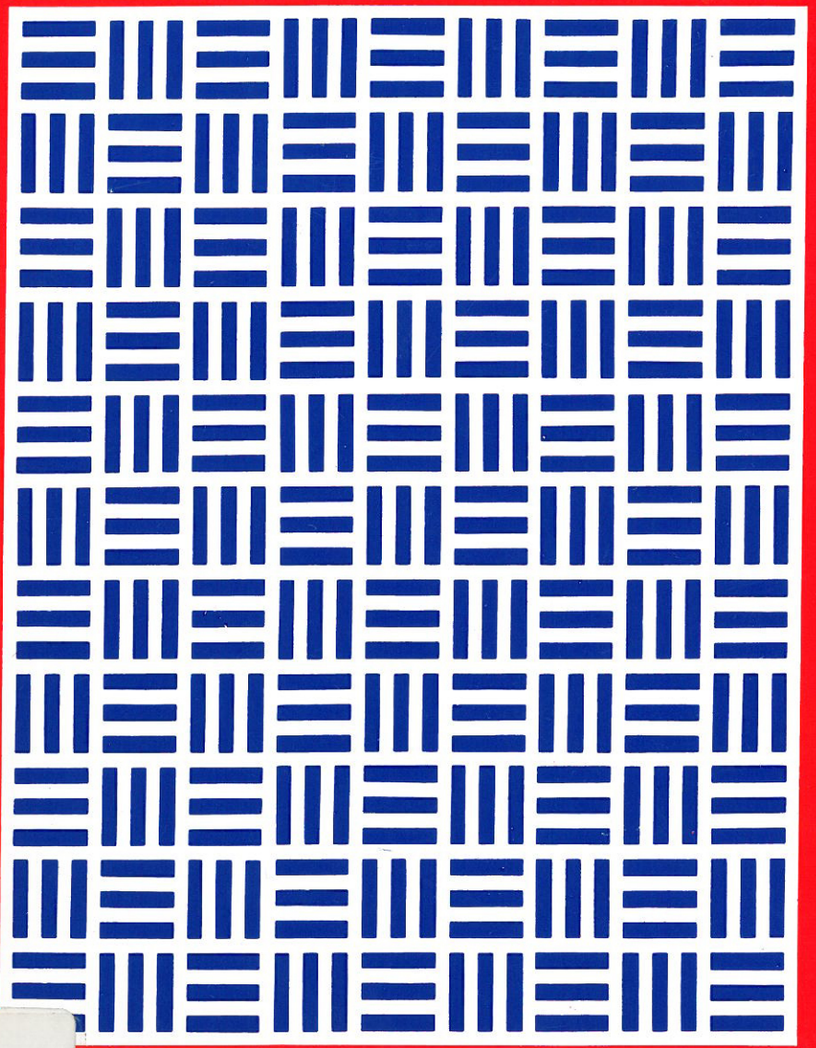

Daryl E. Chubin
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Preface and Acknowledgments

Peer review is not a popular subject. Scientists, federal program mangers, journal editors, academic administrators, and even our social science colleagues become uneasy when it is discussed. This occurs because the study of peer review challenges the current state of affairs. Most prefer not to question the way things are done—even if those ways at times appear illogical, unfair, and detrimental to the collective life of science and the prospects of one's own career. Instead, it is more comfortable to defer to tradition, place faith in collective wisdom, and hope that all shall be well.

Because peer review is such a sensitive subject, we feel it is especially important to thank all those who sustained us—intellectually and otherwise—during our mounting obsession with science as a social enterprise and peer review in particular. The work on peer review began innocently a decade ago, when Chubin coauthored with Ian Mitroff an analysis of peer review at the National Science Foundation that called into question how it was done and how it was studied. Two years ago, Chubin approached Hackett with the invitation to collaborate on the manuscript in progress that became the book in your hand. Fresh from a study of scientists' reactions to the research funding system, Hackett was eager to think more comprehensively about allocative mechanisms and science policy. We first worked together in the mid-1970s at Cornell, and had kept in touch through a dizzying succession of job changes. Looking backward, we thank Bob McGinnis for his support in those early years of science studies and for introducing us to one another and to the friends we respect for their minds and cherish for their hearts. To us, they will forever be associated with Cornell because history, not current affiliation, makes it so: Carl B. Backman, the late Dick Campbell, Helen Hofer Gee, Gerry Gordon, Scott Long, the late Nick Mullins, Dot Nelkin, the late Derek Price, and Robin M. Williams, Jr.

Chubin's sabbatical, in 1983–4, at Cornell's Science, Technology, and Society Program and Department of Sociology, expanded this circle to include others who contributed to the development of the manuscript: Gil Gillespie, who took charge of the National Institutes of Health survey data reported in chapter 3; Mike Brown, who was the most indulgent office-mate any visiting professor could hope for, and especially Sheila Jasanoff, with whom Chubin co-edited a 1985 special issue of *Science, Technology, and Human Values* on "peer review and public policy." Chewing over many issues with her was hearty preparation for this volume.

Hackett's work on this volume was greatly facilitated by the time, resources, and patient encouragement provided him by the chair of his department, Shirley S. Gorenstein.

Many others have provided one or both of us the kind of stimulation that makes scholarship such a satisfying process. At Georgia Tech, these people included Stan Carpenter, Jeff Franklin, Jon Johnston, Mel Kranzberg, Alan Porter, Dave Roessner, and Fred Rossini. At RPI, this role was played by Linnda Caporael, Tom Carroll, Susan Cozzens, Teri Harrison, Deborah Johnson, and Ned Woodhouse. Deserving special mention is a valuable resource on numerous issues for both of us: Sal Restivo, who founded and edits the series in which this book appears.

Others' writings and, more important, their conversations over the years have seeped into the pages that follow. We thank them for—wittingly or not—enriching our effort: John Andelin, Bill Blanpied, Larry Busch, Nancy Carson, Chris Caswill, Ellen Chu, the late Gene Frankel, Tom Gieryn, Peter Healey, Lisa Heinz, Chris Hill, David Hull, Rachelle Hollander, Carlos Kruytbosch, Karin Knorr-Cetina, Marcel LaFollette, Bill Lacy, Wil Lepkowski, Jim McCartney, Jim McCullough, Willie Pearson, Jr., Arie Rip, Rusty Roy, Henry Small, Samantha Solomon, Tom Stossel, Al Teich, Steve Turner, Ron Westrum, Pat Woolf, the late Christopher Wright, and John Ziman.

Thanks for material support of various kinds go to George Kurzon for allowing Chubin to participate in his legal appeal, recounted in chapter 3 (some of which appeared in the *Bulletin of Science, Technology, and Society* (vol. 2, 1982, pp. 423–432, c. Pergamon Press), which led to the survey described in that chapter; to David Edge and Roy MacLeod who provided access, via Nigel Gilbert, to their editorial files at *Social Studies of Science* (the fruits of this access appear in chapter 4); to the National Science Foundation which, through Work Order No. 83–GB–0032G (and the scheming of Chuck Herz and Alex Morin) supported Chubin's

project to synthesize what was known "back then" about misconduct in scientific research (revisited in chapter 5), and to the Office of Technology Assessment's Contract No. 633–1310, which permitted Chubin (*before* he became a federal employee) to draft what was first excerpted in the 1986 OTA technical memorandum, *Research Funding as an Investment: Can We Measure the Returns?* and later revised as "Research Evaluation and the Generation of Big Science Policy," published in *Knowledge: Creation, Diffusion, Utilization* (vol. 9, 1987, pp. 254–277, c. Sage Publications) before being overhauled for use in chapter 6. The Office of Technology Assessment provoked and supported some of Hackett's thinking on the issues presented in this book through its Contract No. H3–4075.1, which led to his report, "Science in the Steady State: The Changing Research University"; the National Science Foundation also supported Hackett's writing through Grant No. BBS 87–11341.

A blanket acknowledgment is also owed to the editors, program managers, and anonymous referees we have encountered over a combined professional life of nearly thirty years. They have given us ample opportunity to try our hands at the roles of reviewer and reviewee. They have also heightened our sensitivity to the process of proposal and manuscript appraisal. Most of all, they have fed us a rich diet of participant observation in outrage, provocation, and political maneuvering. Our files are engorged with the products of their sharp minds and sharper tongues. Although neither of us systematically examined this personal documentation of "peer" behavior, we will carry its benefits and scars for many years. We have tried to incorporate the distilled wisdom of this experience in these pages. While we owe debts of gratitude and more to the above mentioned people and organizations, we alone are responsible for the views recorded here.

Rosalie Robertson, our editor at the Press, was patient beyond words with a manuscript that was always on its way but seemed never to arrive.

Finally, our spouses and children have had to accommodate our preoccupations and immersions in this venture. We love them for those and other reasons and, as small thanks, we dedicate this book to them.

1

The Centrality of Peer Review

> A profession which seeks the truth must consider whether silence about motives and restraint in expression serve, on balance, to enhance or suppress it.
>
> Harold Orlans (1975)

The special properties of scientific knowledge are often attributed to the special circumstances of scientific work. Good science is predicated on a self-regulating community of experts, some contend, and peer review is the mechanism of self-regulation in science. Indeed, peer review has been well institutionalized: it is strongly bound into the structure and operations of science and supported by a network of values, beliefs, and myths. Yet there is mounting evidence that peer review in the United States is not functioning well, and there is growing concern among scientists and policymakers about the soundness of the peer review system.

What Is "Peer Review"?

The synonyms are many: peer advice, peer evaluation, peer

judgment, quality control, peer censorship, merit review, refereeing, and so forth. We will use "peer review" as a generic term encompassing all these, recognizing key differences between particular implementations of the idea where the need arises. Briefly defined, peer review is an organized method for evaluating scientific work which is used by scientists to certify the correctness of procedures, establish the plausibility of results, and allocate scarce resources (such as journal space, research funds, recognition, and special honor).

The practice of peer review is familiar but not benign. Scientists are well acquainted with it, frequently alternating between the roles of performer and evaluator, of defendant and juror. Peer review is not only a routine component of the scientific role, but it is also fundamental to the institution of science, defended as symbol and guarantor of the autonomy of science. Thus peer review is built so deeply into the brickwork of science that many refuse to examine and improve it, fearing that any significant change would weaken the entire edifice. In some minds, to question peer review is to question science itself, and to question science is to challenge deeply held values about progress and the prospects for society.

As we approach the final decade of the twentieth century, there are murmurings from many quarters—federal research agencies, journal editors, scientific societies, and Congress—that peer review is in need of repair or rebuilding. Perhaps it now bears far more weight than was ever intended. The burden placed on grants peer review, for example, may have become unreasonable as the importance of obtaining research support, the competition for support, and the sheer volume of proposals have increased in recent decades. Perhaps the peer review process has been pressed to serve so many distinct purposes that it serves none well. In various forms, peer review has been employed to allocate discipline-based and interdisciplinary research grants to individuals and groups (such as centers and programs), to judge the publishability of manuscripts, to award fellowships and other support to individuals, to confer honors, and, most recently, to adjudicate cases of scientific misconduct. Perhaps peer review has been overextended, applied to so many dissimilar procedures that it has lost meaning or, in what amounts to the same thing, has acquired a range of diverse meanings for diverse speakers and audiences. Some "peer groups" are composed of expert scientists, while others include leaders from outside the scientific community. In some cases these peers decide; in others, they advise or endorse.

Whatever the cause of these strains on peer review, they have the consequence of making the organizational components of science seem deficient in mutual understanding and unity of purpose. Thus the "community of science" exhibits far less solidarity than many suppose, with internal competition for resources and power rising to the surface. The attentive public then may recognize that scientific research is an uncertain process with indefinite outcomes that must be packaged skillfully to retain public commitment and the investment of federal and state funds. And with this recognition may come reluctance to invest money or moral energy in science.

Peer review simultaneously serves several values that are not entirely in harmony. As a *process*, peer review is expected to operate according to values of fairness and expediency, yet its *product* is to be trustworthy, high-quality, innovative knowledge. There is no assurance that the process will yield the product; to the contrary, the process may interfere with efforts to secure the product. Other values may intervene as well, imposed by the institutional context of science. For example, accountability and due process are bureaucratic requirements of a particular governance system that may envelope peer review. Or national needs may be asserted: in different fields of science at different times research has been supported to provide solutions to specific problems in space exploration, health, social welfare, and economic competitiveness. Finally, these values are not always clearly expressed. For example, at the same time that the federal government invests so much in basic science, it also expresses through its funding policy a preference for short-term "mission-oriented" science that serves specific public purposes.

Peer review is paradoxical: as a decision-making process within science it allocates resources, monitors ongoing work, and validates products, preserving the professional autonomy of science through apparently rigorous self-regulation. The imprimatur of peer review labels the products of science "new," "important," and "useful." But at the same time that peer review functions to preserve professional autonomy it serves as a conduit for forces in the social environment that make the profession accountable to a larger constituency. Thus peer review forms a bridge between the mysterious and esoteric content of science and the mundane world of resource allocation. As Marcel La Follette writes,

> A common fiction is that science is one thing and science policy another. According to this interpretation, what scientists

> do to maintain the quality and reliability of scientific knowledge is independent from influence by or on science policy; only the funding (input) or the knowledge (output) can be said to interact with political values. . . . Such a position, however, represents beliefs that are not just inaccurate but naive. And this is particularly true for the peer review system.[1]

Symbolism and Chauvinism

The symbolism of peer review is also a powerful social lubricant. It deflects criticism by asserting the autonomy and authority of science. It also makes new knowledge claims more credible to the nonscientist because those claims bear the approval of the scientific community. But peer review drives a wedge between nonscientists and the process of claims-making, for scientists jealously guard their power to accept or reject the findings of their peers. Moreover, they are implacable in sequestering this process from the public view, as the legal proceedings described in chapter 3 make clear. In this sense scientists are chauvinistic, forcefully asserting their special prerogatives to produce and evaluate new knowledge.

Such enclaves of expertise are not unusual in societies characterized by a complex division of labor. In fact, we usually delegate to experts the authority for making decisions in areas we do not understand or have not been trained to know. We trust the expert to bear our best interests in mind. We hope that if our trust is misplaced the expert's own profession will take swift and decisive corrective action on our behalf. But is this an appropriate relationship between science and a democratic society? Is it flexible enough to serve the times of rapid change in science? Is it sturdy enough for a society increasingly dependent on science?

According to Prewitt, "democratic culture" features public control and accountability instead of peer control and autonomy; public scrutiny in place of internalized standards of conduct; checks and balances and critical public opinion supplant self-regulation and self-evaluation.[2] Yet peer review intervenes in this process, at once serving as a mechanism of scientific self-regulation that preserves the autonomy of science and as a symbol of professional accountability that insures democratic control of science. Thus our decisionmakers frequently use

> peer review as an indicator of the quality and reliability of published information. On the strength of that stamp of legitimacy, policy-makers use scientific information to support decision-making—in regulation, in funding decisions, in promotion or tenure cases.[3]

These symbolic uses of peer review reinforce scientists' chauvinism and increase the distance between science and society. As experts pass judgment on one another's ideas, guided by disciplinary criteria of importance and quality, the intrinsic "rightness" of peer review is sustained and the self-assurance of professional self-regulation verges on self-deception. Alternative allocation mechanisms are viewed as threats to the autonomy of the profession and the integrity of its products. In this sense, peer review is the flywheel of science, lending stability to an enterprise that is buffeted by shifting external demands, variable resources, and strong competitive pressures.

In a larger sense science is not an independent institution but, as research costs have grown, one that has become increasingly dependent on society for resources. At the start of every budget cycle the case for supporting science is argued, connecting scientific success with national objectives for health, economic competitiveness, defense, transportation, agriculture, and the like. On the one hand, science is firmly in service of other societal goals, but on the other it systematically denies this dependence. Under such circumstances is it possible to remain assured of financing the best science? And can peer review, at the center of the profession's claim to autonomy and the society's demand for accountability, continue to serve as a mechanism of allocation and control?

Peer review is the focus of tensions between science and other social institutions.[4] When the public doubts science or resents the risks created by science, it is the process of peer review that is called into question. Scientists invoke peer review in their own defense. The media and the Congress scrutinize and investigate it. Peer review is often under siege and yet, remarkably, while the peer review *system* may absorb severe damage, the peer review *concept* emerges with renewed support from all parties. When the disputatious moment has passed, the system returns to business as usual.

This book will argue that U.S. science cannot afford to conduct "business as usual." To do so would yield an ordinary science guided by safe policies financed in unexceptional ways—hardly the prescription for the research excellence or the economic "competitiveness" so vigorously sought by national policymakers. The

complacency of U.S. science, we contend, is revealed in the methods through which it disburses its scarce resources and in the idiosyncratic way it defends those methods.

U.S. science has generally been decentralized, despite early and recurring proposals to create a single organizational home for all basic research. Unlike their European counterparts, U.S. scientists both benefit and suffer from the competition and opportunity afforded by multiple funding sources, each with its own rules and strategies for research success. But as growth in the national science budget is pinched by fiscal constraints, and as other nations attain prominence in areas of scientific research once viewed as our private preserve, U.S. scientists and science policymakers ask again: Who gets supported and who does not? How do we evaluate research productivity? How do we recognize and reward quality? These are old questions that require new perspectives, new thoughts, new criteria; perhaps new answers will follow.

Approach to a Dialogue

Does this mean we advocate dismantling the systems that administer "grants peer review?" No, but we are certain that those systems can and must be made more responsive, and less cumbersome, burdensome, and risk-averse. Do we doubt the efficacy of "journal peer review?" At times we do. Scholarly journals serve a variety of purposes: insuring the accuracy of results, providing rapid communication of new findings, disseminating new knowledge to a wide audience, and informing public policy, among others. It is unlikely that a single review mechanism would serve every purpose and every audience equally well.

We are also populists at heart. Demystify what professionals do and the public can understand and participate intelligently in decisions that affect the common good. People need not understand the minutiae of space science, particle physics, or human molecular genetics to participate responsibly in public debate about the relative merits of these high-ticket science initiatives.

Instead, people must understand the basic prospects and purposes of these areas of research. They also must become "science-literate"—knowledgeable about how modern science operates as a social enterprise, including awareness of life in the laboratory, the career patterns of scientists, the role of instruments in research, the institutional processes that favor funding one sort of science over another, and the myriad uncertainties that stand between scientific research and consumer benefit.

People should be given more opportunity to learn about science as an intellectual and social enterprise and to exercise this knowledge through participation in decisions. Science must be taken down from its pedestal and placed in a social context constructed (some might say *controlled*) by journalists, politicians, and an informed, active citizenry. The call for a moratorium on recombinant DNA research in Cambridge, Massachusetts, and the community responses to environmental hazards in Woburn, Massachusetts, Love Canal, New York, and in eastern Pennsylvania, near the Three Mile Island nuclear power plant, are examples of these groups' power when they choose to exercise it. But why must public participation be born of crisis? Is it possible willingly to recognize the public as a legitimate stakeholder in such matters?[5] We view these not as romantic or radical notions but as prerequisites for a healthy participatory democracy.

In the following chapters we will probe the relationship of peer review to science, policy, funding, publishing, and democratic principles in the United States. We have chosen to place ourselves intrusively in the narrative: separating the third-person analyst from the first-person actor is a chore hardly worth doing any more. It is artificial and incomplete; it shrouds analysis in jargon and the pretentions of "value-neutral" discourse, yet provides no assurance of value neutrality. Some even say that the appearance of value neutrality provides a convenient cloak for partisanship.

In our view, if one rejects the proposition that there is a single, objective, definitive account of an issue, then one must be prepared to entertain multiple and sometimes competing accounts.[6] This is not as radical as it may first appear, for one must always ask why one account prevails over the others. In effect, this is the day-to-day work of policy analysts and policymakers, who must sort and evaluate competing accounts and then act, often with unwarranted decisiveness, in the face of uncertainty. Our approach thus recognizes that analysts, policymakers, and scientists inhabit different cultures and view the world through culturally determined perspectives.

Five Axioms about the Culture of Science

The perspective on science that underlies the argument we shall present is grounded in the social studies of science literature. To avoid a lengthy review of that research and its relationship to other views of science, we shall instead present its essential principles as a set of five axioms.[7]

1. Scientific research is a social act. It is not a solitary struggle between "nature" and the human mind, as accounts of the heroic scientist would lead us to believe, but instead entails relations within a community of scientists and a community of minds seeking recognition and consensus.
2. Science is done in a nested set of contexts—countries, cultures, disciplines, organizations, laboratories, and so forth—and characteristics of the contexts shape the direction and content of scientific work.
3. Scientific work is performed by individuals whose efforts are influenced by culture, specialized training, diverse motivations, varied intellectual skills and interests, values, biases, and prejudices. Science is not performed by white coats in an asceptic environment—a sort of intellectual clean room—free of human society and human failings. To the contrary, it is an intensely human and social activity, and bears the marks of its context and performers.
4. In the process of doing research, scientists do not merely operate on reality, they *construct* it, trying to persuade others to accept these constructions.
5. Science can be seen as the product of multiple realities, only one of which is generally accepted at a given time among a particular community of researchers. (In those rare instances where competing constructions are held by competing communities of scientists, memorable controversies erupt.) The prevalence of a certain socially constructed reality is called "consensus," "authority," "knowledge," or perhaps even "nature" or "truth." Thus scientific truths are, at bottom, widely accepted social agreements about what is "real," arrived at through a distinctively "scientific" process of negotiation.

The Policy Context

Throughout the history of institutionalized science, which began in the seventeenth century, the concept of peer evaluation has been invoked by the scientific community as a mechanism of quality control.[8] In practice, peer review is presumed to distinguish inferior, misguided, flawed, or bogus research reports or proposals from sound, innovative, meritorious ideas. But this is hotly disputed by at least some vocal scientists. Fragmented as the scientific community may be, most of its members are likely to agree that peer review serves three functions: (1) insuring that scientists are accountable for the public funds they receive; (2) preserving the professional autonomy of the scientific community; and (3) certifying the soundness of new developments in science and technology.[9]

Today the social mechanisms that authorize and channel peer review (as it is practiced in federal agencies, universities, or journals) are shaped as much by considerations of public policy as by the technical criteria of the scientific community. The importance of policy considerations is openly acknowledged in many ways. For example, an NSF advisory committee coined the term "merit review" to recognize that "technical excellence is a necessary but not fully sufficient criterion for research funding. To reach such goals as increasing the practical relevance of research results, or improving the nation's infrastructure for science and engineering, additional criteria are needed."[10] Or consider the two-stage review process at NIH, which begins with an assessment of scientific merit by an Initial Review Group of scientists (also called a Study Section, described in the next chapter), then proceeds to a level of programmatic and policy review by a National Advisory Council (which includes nonscientists). In practice the second level of review typically makes awards in line with the technical ratings, with only a small proportion of proposals funded "out of order" (or "specialed"). Nonetheless, the National Advisory Councils have the authority to act more independently if they choose.

Such criteria of utility make great sense in these pragmatic times, but they reflect a sharp change from the original "contract" between science and society envisioned by Vannevar Bush in 1945. In an oft-quoted passage, Bush asserted that

> Scientific progress on a broad front results from the free play of free intellects, working on subjects of their own choice, in the manner dictated by their curiosity for the exploration of the unknown.[11]

Bush thought that colleges, universities, and research institutes were

> uniquely qualified by tradition and by their special characteristics to carry on basic research . . . [because these institu-

> tions offer scientists] . . . an atmosphere which is relatively free from the adverse pressure of convention, prejudice, or commercial necessity. At their best they provide the scientific worker with a strong sense of solidarity and security, as well as a substantial degree of personal intellectual freedom. All of these factors are of great importance in the development of new knowledge, since much of new knowledge is certain to arouse opposition because of its tendency to challenge current beliefs or practices.[12]

Perhaps times have changed, or perhaps free intellects were never so freely at play in well-funded laboratories. However that may be, today's free intellects do not play freely, but instead find themselves tethered to national goals for health, defense, economic competitiveness, and the like. Colleges, universities, and research institutes have come to depend on federal research support, a dependence that is transmitted (and perhaps amplified along the way) to the scientists and scholars they employ, further limiting intellectual "free play." New ideas must pass through the filter of peer review, which stimulates opposition and encourages applicants to be cautious, if not conservative, in their proposals.

Thus peer review is a chimera, a powerful and somewhat frightening creature composed of incongruous parts, that affords scientists both freedom and accountability, simultaneously insulating them from social pressures and expressing those very pressures. As a tool of public policy, peer review justifies the flow of public funds and the establishment of collective priorities:

> In the long-standing relationship between government and science in the United States, major responsibility for funding basic scientific research has settled upon the government partner. For its part, the scientific community has accepted primary responsibility for defining research needs and opportunities and providing assurance that public funds are allocated on a priority basis, through peer review. For either partner to breach its responsibility carries serious risk to the solidarity of what has proved an extraordinarily effective partnership.[13]

But breaches do occur, precisely because peer review is burdened with inconsistent responsibilities. And when they do, the ambivalent rhetoric of scientists and nonscientists alike clouds our understanding of peer review practices. Commentators are torn by

their ambivalence, simultaneously supporting and criticizing the peer review system. Advocates invoke peer review to defend the scientific integrity of policy decisions:

> The genius of the approach of the National Institutes of Health and the National Science Foundation to federal research is that awards are based on merit and that proposals are given fair consideration by acknowledged peers in the field.[14]

Or consider the Food Safety Modernization Act of 1983, which provides that

> the Secretary shall by regulation establish procedures for receiving advice from a scientifically qualified staff of individuals . . . in cases in which the Secretary determines with respect to the safety of a substance in food that there is a substantial scientific issue the resolution of which may be materially facilitated by independent scientific peer review.[15]

But others are less sanguine, questioning whether "good" and "bad" science can be distinguished, whether science and values can be separated in the course of public decision-making, whether peers can be identified for most scientific work, and whether gatekeepers exercise good faith in their use of peer review. Some critics claim that peer-reviewed decisions are based mainly on an ideology which protects the "old boy network" from scrutiny by peers.[16] In many cases these reviews are merely input to decisions that are made behind closed doors.

Some commentators propose that different funding mechanisms are suited to different types of science. Harvey Brooks contends that peer review may be helpful in selecting research designed to discover "truths" about nature but is ill suited to make judgments about the utility of research.[17] Deborah Shapley and Rustum Roy assert that our society's dependence on peer review, which has endured for thirty years, resulted from a combination of arrogance, inertia, and fear. A generation of scientists has experienced no other means of obtaining research funding and thus feel their careers at risk whenever questions about peer review are raised.[18] Shapley and Roy's observations demand careful consideration. Perhaps their claim that grants peer review is not working seems harsh because for many the rituals of peer review are taken as axiomatic, habitual, and beyond debate. Yet this degree of un-

critical commitment to peer review resembles the mystique that surrounds a sacred ritual or icon and prevents believers from examining it analytically. As Shapley and Roy explain:

> The term "peer review" in the context of science policy has acquired a deep symbolism within the science community. It is repeated like a mantra or used as a talisman to shield any activity, put it above reproach, so to speak.[19]

They go on to recite the litany of peer review's evils: it is a ritual that impedes good science, wastes time, and diffuses responsibility; it is doomed to fail because "peers" cannot be identified and, if identified, have conflicts of interest that hopelessly bias their judgments; it sometimes depends on judgments about work not yet performed, ignoring the important roles of chance and serendipity and giving free rein to conservatism and groupthink; it is demoralizing.[20]

Finally, some commentators are critical *and* supportive of peer review in the same breath. Early in the course of a scathing article about peer review one scientist writes:

> I can scarcely find words to describe such a questionable, dastardly, and potentially libelous process. The issue before us is not peer review. The issue is one particular system of peer review applied to that tiny promissory segment of a scientist's portfolio called "the proposal," with a heavy-handed impact that can cripple his morale and career. . . . we must have a system in which human frailties and their evil consequences are checked more closely. . . . We must now strive for superior application of the noble principle of peer review.[21]

Sociologists have heard all this before. The problem, according to the scientist quoted above, lies not in our systems but in ourselves. Yet sociologists are trained to be skeptical of analyses that center on the "evil consequences" of "human frailties," particularly analyses that call for a powerful system to remedy matters. Rather than lament human frailties, let us examine social systems.

Studying Grants Peer Review

Evidence is sorely lacking on peer review practices, largely because the reviews themselves, which reside in the files of journals and funding agencies, were obtained under assurances of strict confidentiality and are not readily available for analysis. Studies of grants peer review at NIH and NSF have generally concluded that peer review operates fairly to identify and support the best science.[22] This result, however, is easy to anticipate, and much that matters has been obscured or ignored in the studies cited above. In a commentary on such research, Arie Rip observes that "because everybody [involved in the science funding system] is so concerned about fairness, the system will be reasonably fair, and the studies commissioned to check its fairness will come up with results showing just that."[23] And Roy points out that the form of peer review varies as much across programs within an agency as it does across agencies. Most programs use "*ad hoc* mail reviewers"; some use those reviews as data for panels that undertake a second level of review; still others confine decision-making to program managers and a few in-house agency advisors. Thus the peer review system is more accurately viewed as a family of closely related procedures that have some similarities and marked differences.[24]

Other studies have discerned in grants peer review an unmistakable bias toward conservatism in the name of quality control.[25] Reviewers' tolerance for innovativeness is bounded: unorthodox ideas and techniques are more welcome from those with impressive credentials, such as a prestigious academic background and an extensive track record. But sometimes established scientists who reach beyond "conventional wisdom" or propose to work outside their areas of acknowledged competence are rebuffed (for example, Luis Alvarez, Richard Muller, and Albert Szent-Gyorgi).

The inherent difficulties of grants peer review have been exacerbated in recent years by budget constraints and by the further bureaucratization of science.[26] Some fear the "incipient dismantling of the peer review system" brought about by research fraud, university lobbying for "pork-barrel" grants, disputes over intellectual property, and increased secrecy in scientific research.[27] These phenomena, discussed at greater length in chapter 5, remind us that peer review is neither a scientific procedure insulated from environmental conditions nor a mysterious rite shrouded in secrecy and ceremony. It is instead a social and political process that turns on issues of privacy, efficiency, safety, and fairness.

Despite the problems enumerated above, few alternatives to current procedures or criteria for grants peer review have been advanced. When they are, they inevitably become entangled in value conflicts: for example, processing submissions more efficiently versus choosing more carefully; continuing established investigations

versus promoting promising but risky new work.[28] Suffice it to say here that an innovation such as formula funding might streamline the review process, but it would operate chiefly by narrowing the field of scientists eligible for such awards. Considering the array of difficulties that confound peer evaluations of proposals and manuscripts, the most likely outcome of formula funding is to increase the magnitude of errors by increasing the magnitude of the "prize" awarded by the process and decreasing the number of awards made and competitors allowed to participate.

In the absence of data, how does one examine these criticisms? How does one argue that block grants to labs and centers, set-asides based on track record alone, or lengthening of the standard funding period from three to five years will give us better research or less bureaucratized science? How does one overthrow the oppressive burden of tradition—the arrogance, inertia, and fear noted above—and begin to address the more fundamental questions about the fit between science and society raised by Prewitt and by LaFollette? Richard Atkinson and William Blanpied pose these larger issues precisely:

> Should peer review operate only to evaluate merit or should it also help establish priorities? Can it or should it be effective in changing the direction of a program, in allocating resources among programs within agencies themselves? These questions are significant because they challenge the assumption that peer review is the best possible way to allocate resources in the best overall interests of both science and society.[29]

Summary: A Study of Policy and Practice

This book could be read as substantiating and articulating the challenge to the assumption that peer review is the best way to allocate resources and express diverse interests. It is also a manifestation of the continuing tension between science and other social institutions over matters of resources, quality, and the direction of scientific work. To quote Atkinson and Blanpied again:

> The assumption that research is a sacrosanct activity that government must continue to support adequately has lulled much of the scientific community into a state of political apathy and has allowed government to treat science as if it were, in fact, just another special interest.[30]

There is a role for social and policy scientists in jarring the apathy out of scientists, a need for research on scientific rituals, and an imperative to translate professional practice into terms amenable to social intelligence and science policy. While peer review remains a concern in Washington, initiatives to "study" and "fine-tune" the system need not emanate solely from the National Science Foundation, the National Academy of Sciences, and the Congress.[31] Indeed, we intend to inform such initiatives by offering new perspectives on peer review as a form of professional self-governance. We wish to contribute to the ongoing dialogue about peer review and, more ambitiously, to help shape an action agenda that clarifies the multiple meanings of peer review and leads to changes in its practice.

Response rate

Overall, 336 (47%) of the scientists surveyed returned usable questionnaires. This is a good response rate, as only a single mailing (with no follow-up reminders) had been sent in order to preserve respondents' anonymity. The response rate did vary by sampling stratum, with higher rates among the more successful applicants. Of those who were uniformly successful, 52% (205 people) completed and returned the survey. Those who experienced mixed success had a response rate of 47% (25 responses); uniformly unsuccessful investigators had a response rate of 39% (106 responses). These differences in response rate, while modest, are noteworthy because the cover letter stated that, "If we do not receive your completed questionnaire, we will assume that your satisfaction with the present peer review system precluded it." That provocative statement was expected to elicit responses from dissatisfied scientists, perhaps at some risk of under-representing those who were satisfied.

These response rates may be better understood within the context of similar studies conducted by others. A recent NSF survey of prospective principal investigators had a 67% response rate, with completed questionnaires returned by 88% of the funded applicants, but only by 52% of those not funded.[1] Similarly, in an interview study performed under contract to the NIH, 85% of those whose applications were approved but *not* funded agreed to participate in the study, whereas 68% of those whose applications were disapproved chose to participate.[2] It is not surprising that studies under the official sponsorship of a funding agency elicit higher response rates than do studies conducted by academic social scientists: such surveys have an aura of authority and offer respondents an opportunity to "speak" directly to policymakers.

More puzzling is the universal tendency for less successful scientists not to respond. One would expect them to have much to say, perhaps in an effort to alter the system, send a message, or offer a quasi-public explanation for their failure. Instead, successful scientists—those most likely to be supportive to the status quo—are also most likely to respond. For this reason, when reading averages and aggregated percentages (such as, "X% of all respondents were enthusiastic about Y") one should bear in mind that such figures combine the responses of two quite different subpopulations which are generally not equally represented in the sample.[3]

Notes

CHAPTER 1

1. Marcel C. La Follette, "Journal Peer Review and Public Policy," *Science, Technology, and Human Values* 10 (Winter 1985): 3–5.

2. Kenneth Prewitt, "The Public and Science Policy," *Science, Technology, and Human Values* 7 (Spring 1982): 5–14.

3. La Follette, op. cit., p. 4.

4. Two reports issued by the Twentieth Century Fund highlight these disputes and how they compromise the autonomy of science: Task Force on Communication of Scientific Risk, *Science in the Streets,* background paper by Dorothy Nelkin (New York: Priority Press, 1984); Task Force on the Commercialization of Scientific Research, *The Science Business,* background paper by Nicholas Wade (New York: Priority, 1984).

5. James C. Petersen, ed., *Citizen Participation in Science Policy* (Amherst: University of Massachusetts Press, 1984).

6. Daryl E. Chubin, "Values, Controversy, and the Sociology of Science," *Bulletin of Science, Technology, and Society* 1 (1981): 427–37; Daryl E. Chubin and Sal Restivo, "The 'Mooting' of Science Studies: Research Programs and Science Policy," in Karen D. Knorr-Cetina and Michael J. Mulkay, eds., *Science Observed* (London and Beverly Hills: Sage, 1983), pp. 54–83.

7. Daryl E. Chubin and Ellen W. Chu, eds., *Science Off the Pedestal: Social Perspectives on Science and Technology* (Belmont, CA: Wadsworth, 1989); Bruno Latour, *Science in Action* (Cambridge, MA: Harvard University Press, 1987).

8. Harriet Zuckerman and Robert K. Merton, "Institutionalized Patterns of Evaluation in Science," in Robert K. Merton, *The Sociology of Science* (Chicago: University of Chicago Press, 1973) pp. 460–96; D. A. Kronick, "Authorship and Authority in the Scientific Publications of the Seventeenth Century," *Library Quarterly* 48 (1978): 255–75.

9. On points (1) and (2) see General Accounting Office, *Better Accountability Procedures Needed in NSF and NIH Research Grant Systems,* Report to the Congress (Washington, D.C., 1981); and Howard J. Sanders, "Peer Review: How Well is it Working?" *Chemical & Engineering News,* March 15, 1982, pp. 32–43. On the more general point (3) of public understanding of science as public policy, see Dorothy Nelkin, "Science and Technology Policy and the Democratic Process," in *The Five-Year Outlook: Problems, Opportunities, and Constraints in Science and Technology,* vol. 2 (Washington, D.C.: National Science Foundation, 1980), pp. 483–92.

10. National Science Foundation Advisory Committee on Merit Review, *Final Report* (1986), p. 2.

11. Vannevar Bush, *Science: The Endless Frontier* (U.S. Office of Scientific Research and Development, July 1945, rpt. NSF, 1960), p. 12.

12. Ibid., p. 19.

13. American Association for the Advancement of Science, "Board Statement on Politics and Science," *Science* 224 (January 6, 1984): 27.

14. Henry B. Gonzalez, "Scientists and Congress," *Science* 224 (April 13, 1984): 127–29.

15. Orrin Hatch, "Amendment of Title IV of the Federal Food, Drug, and Cosmetic Act," U.S. Congress, Senate, *Congressional Record,* 98th Cong., 1st sess., October 6, 1983, p. S13789.

16. Michael J. Mahoney, "Psychology of the Scientist: An Evaluative Review," *Social Studies of Science* 9 (1979): 349–75; Ian I. Mitroff and Daryl E. Chubin, "Peer Review at NSF: A Dialectical Policy Analysis," *Social Studies of Science* 9 (1979): 199–232.

17. Gerald Holton and Robert S. Morison, eds., *Limits of Scientific Inquiry,* (Cambridge: American Academy of Arts and Sciences, 1978) pp. 171–90.

18. Deborah Shapley and Rustum Roy, *Lost at the Frontier* (Philadelphia: ISI Press, 1985).

19. Ibid., pp. 102–3.

20. Ibid., pp. 99, 54, 103–4.

21. D. H. Osmond, "Malice's Wonderland: Research Funding and Peer Review," *Journal of Neurobiology* 14 (1983): 95–112.

22. For example, see Grace M. Carter, *What We Know and Do Not Know about the Peer Review System,* Rand Corporation report N-1878-RC/NIH, June 1982; Stephen Cole, Leonard Rubin, and Jonathan R. Cole, *Peer Review in the National Science Foundation: Phase I of a Study,* (Washington, D.C.: National Academy of Sciences, 1978).

23. Arie Rip, "Peer Review Is Alive and Well in the United States," *Science, Technology, and Human Values* 10 (3) (Summer 1985): 82–86.

24. Rustum Roy, "Peer Review of Proposals—Rationale, Practice, and Performance," *Bulletin of Science, Technology, and Society* 2 (1982): 402–22.

25. National Commission on Research, *Reviewing Processes: Assessing the Quality of Research Proposals,* (Washington, D.C.: 1980).

26. Erich Bloch, "Science Policy and Tight Budgets," *Science* 227 (March 1, 1985): 991; Barbara J. Culliton, "NIH Proposes Extending Life of Grants," *Science* 226 (December 21, 1984): 1400–1402.

27. Donald Kennedy, "Government Policies and the Cost of Doing Research," *Science* 227 (February 1, 1985): 480–84; Daryl E. Chubin, "Research Malpractice," *Bioscience* 35 (February 1985): 80–89; John Maddox, "Privacy and the Peer-Review System," *Nature* 312 (December 6, 1984): 497; "Secrecy in University-Based Research: Who Controls? Who Tells?" *Science, Technology, and Human Values* 10 (special issue, Spring 1985): 3–114.

28. Rustum Roy, "An Alternative Funding Mechanism," *Science* 211 (March 21, 1981): 1377.

29. Richard C. Atkinson and William A. Blanpied, "Peer Review and the Public Interest," *Issues in Science and Technology* 2 (Summer 1985): 101–14.

30. Ibid, p. 110.

31. U.S. Congress, House, Committee on Science and Technology, Task Force on Science Policy, *An Agenda for a Study of Government Science Policy,* 98th Cong., 2d sess., December 1984 (Washington, D.C.: USGPO, 1985); Barbara J. Culliton, "Fine-Tuning Peer Review," *Science* 226 (December 21, 1984): 1401.

CHAPTER 2

1. *Grants Peer Review: Report to the Director, NIH Phase I,* NIH Grants Peer Review Study Team, (Washington, D.C.: December, 1976), pp. 3, 4.

2. Ibid., p. 4, emphases added.

3. Rustum Roy, "Alternatives to Review by Peers: A Contribution to the Theory of Scientific Choice," *Minerva* 22 (3, 4) (Autumn-Winter 1984): 316–28.