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# Open Science and Closed Science: Tradeoffs in a Democracy

Daryl E. Chubin

In 1942, when Robert Merton published his classic essay on the norms of science, the shadow of war extended across the world. The threat of fascism—and its concomitant commitment to state ideology—appeared the antithesis of free and open scientific inquiry. Science, like nature, could not be shackled to a political system; instead, science served a higher rationality to which properly socialized scientists paid homage by contributing to the extension of certified knowledge. Merton argued that a set of identifiable norms prescribed behavior that is both functional for the advance of knowledge and morally binding on the professional scientist.<sup>1</sup> But, to this day, the status of the norms as descriptive of scientific practice or, alternatively, as an ideal at best tacitly transmitted and seldom enforced, remains unclear.<sup>2</sup>

No such ambiguity, however, surrounds the values that Merton (and later Bernard Barber) posited as underlying the institution of science. If communism, universalism, disinterestedness, and organized skepticism were the prerequisites for the production of objective knowledge in Merton's scheme, then a "liberal-democratic" society was the obvious environment in which science flourished.<sup>3</sup> The concordance of values in the external culture and the norms operative within the internal social system of science was assumed.

This axiom has had a significant effect on how

social scientists and policy analysts have investigated science. It has, by and large, resulted in an analytical focus on relations *within* the scientific community and not on influences from without. Such emphasis is tantamount to declaring the autonomous social system of science closed to intrusions of "lesser" rationality and "non-scientific" actors. The democracy that guaranteed free inquiry among credentialed scientists precluded the need for such intercourse with other social institutions. When such intercourse did occur, the congruence among basic political values and science rendered the latter nonproblematic.<sup>4</sup>

Democracy, of course, is problematic. So, too, is Merton's formulation of its meliorative role vis-à-vis science. Nevertheless, we can use his interpretation as a plausible theoretical framework for discussing the openness of science, and thereby help to clarify the empirical status of the norms. The norm "communism," for example, speaks directly to the communal character of scientific knowledge. Unlike those interested in the accumulation of property, scientists are said to earn "profits"—in the form of recognition, prestige, and research opportunities—as they share their intellectual property by publishing in scientific journals. Communication allows research findings to be scrutinized by those who will eventually deem or deny it to be reliable knowledge.

For Merton, in 1942, open communication was an imperative for scientific integrity. For science in 1985, secrecy is a negotiable behavior—an article of faith for some, a deplorable condition for others. What has changed in these 40 years in scientific precept and practice? And how does the democracy that Merton postulated as supportive (and therefore analytically unproblematic) function as an arbiter of scientific openness and secrecy?

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One answer can be found in the institutions within which science occurs. Corporate science has blossomed and it now interacts with many university research programs; likewise, government patronage, especially in biomedicine, has been tied more closely to several mechanisms of accountability.<sup>5</sup> The era of biotechnology, microelectronics, and artificial intelligence appears to have redefined the norms of science and the relevant actors who judge conformity to and deviance from them. For certain, there have been tremendous changes in the justifications for imposing new and sometimes competing imperatives on those once expected only to extend certified knowledge.

Put another way, liberal-democratic society has complicated the production, communication, and utilization of science. It has politicized scientists by discipline and by employment sector; it has caught science in a torrent of tradeoffs. Democracy has made "openness" and "secrecy" the subjects of policymaking and conjecture; it has forced consideration of how science can be closed by special interests and closed off to the public good. Majority rule—minority rights politics often draws scientists into the thick of controversy, forcing them to defend the costs and benefits of limiting or expanding research and its applications.<sup>6</sup>

The stakeholders have also mobilized. Science does not speak with one voice, and the public is no longer surprised when experts disagree. Science can represent—and lend credibility to—a range of interests that extend beyond a single community, scientific or otherwise.<sup>7</sup> Is it sensible, then, to talk of openness and secrecy as primarily a communication problem within science? I think not. Rather, the dilemma for post-Mertonian (and, for that matter, post-Kuhnian) analysts of scientific practice is how democracy is used to open or to close science (where "openness" refers to the full range of sequenced processes involved in science—from patronage of a research program to the application of knowledge in public policymaking).

Unfortunately, the data base on how scientific research is conducted and communicated narrows as one approaches the "closed" end of the continuum.<sup>8</sup> The lack of documentation makes it all the more likely that the analyst's *values* will dominate interpretation of sketchy and impressionistic data. Even knowledge of such internal rituals as "peer review" are closed to scrutiny by analysts of scientific behavior.<sup>9</sup>

Democracy, however, is a door that swings both

ways. Once the democratic context is seen as a source of individual, corporate, and government power, then the scientist's search for "truth" appears as a campaign driven by interests and material resources. The institutional norms of science assume certain meaning according to local organizational needs—be they intellectual problem-solving or the financial sustenance of team research. Some commentators see this meaning as facilitating the extension of certified knowledge, others as subverting it altogether. And, within an organization, each scientist may experience individually these contradictory impulses (a "sociological ambivalence" that Merton also diagnosed<sup>10</sup>).

Organizational demands, then, can create many types of science, some of which represent closure of traditionally open inquiry and exchange. The challenge is to document such closure and to identify gaps in data that might be useful for future research. To do this, we must be sensitive to the contextual "texture" of science: private, public, planned, basic. To speak of the "scientific community," "democratic control," and the like is to ignore the subtleties that characterize practice and defy ideal-types.

## Definitions

A surfeit of "motherhood" concepts—university, policy, objective knowledge—plague the discussion of science in a democracy. Moreover, there is no common terminology for discussing the topic of openness in science. Because discourse, like analysis, depends on shared definitions, I offer descriptions of three terms central to discussion of the tradeoffs surrounding scientific choices in the 1980s.

*Closed science* is defined as research which, in its production, communication, or utilization, is inaccessible to potential consumers. The grounds for such closure are always political, in the sense that certain interests, fortified by legitimate power, can exercise democratic control. The information denied to interested parties becomes the focus of a dispute or controversy which includes the means of control and ways of opening it.

*Participation* is defined as the opportunity for interested parties—those within a research community, scientists outside a particular community, and non-scientists—to consume and evaluate new

claims to knowledge. Participation in the decision-making process is typically either mandated by the government or springs from the interests and actions of citizens.<sup>11</sup> Credentials and location in an organization can endow each participant with a degree of credibility that will affect the extent and impact of his or her participation. Participants evaluate science with partial knowledge and unequal power to control the opening or closing of the dispute.

*Peer review* is defined as a generic term for various procedures, designed and executed—largely by scientists—for evaluating the quality of scientific work. That evaluation may be one of two kinds: evaluation of the work promised in applications for research funding (“grants peer review”) and evaluation of claims of new knowledge resulting from ongoing or completed projects (“journal peer review”). Two dimensions of peer review have been distinguished in the literature, each suggesting as the criteria of choice the “truth” or the “utility” of knowledge claims.<sup>12</sup> For the purposes of this article, the common element of peer review as competition for a scarce resource is a reminder of how scientists themselves may impose secrecy during the process of science.

## Secrecy as “Normal” Scientific Practice

### *The Centrality of Peer Review*

Peer review comes in many forms: solicited advice, written reports, roundtable discussion. All are shrouded in confidentiality and anonymity. Conventional wisdom holds that to disclose the referee’s identity to the scientist whose work is under review will compromise the candor, and thus the quality, of the review. It is further assumed that such disclosure will lead to rancor between peers. Several variants on the confidentiality-anonymity interlock indicate the extent to which quality control in science depends on barriers erected between peers:

1) One member of the peer pair—the referee—knows the identity of the scientist that he or she is evaluating, even if “blind reviewing” is the procedure. If claims must be substantiated by the claimant’s previous work, then the article’s bibliography may be a surefire pointer to the author’s identity. To the more cynical observer, then, credibility has its costs.

2) One-sided anonymity precludes the possibility of recourse in the event that evaluations are less than arduous or to the point. The referee is protected by the veil of anonymity; the proposer/claimant is suspended in a state of prolonged ignorance—unless he or she can discern the referee’s style and lift the veil through direct inquiry. Do irate scientists confront one another over a confidential matter, or do they rely on the review system and “third parties” to buffer their differences?

3) Because peers are usually defined by substantive competence, those playing the above roles are competitors. Every incentive exists to retain information that might benefit the other. Whereas the communal spirit would suggest “we’re all in this together,” peers are just as apt to set aside altruism and act “selfishly,” i.e., to withhold information, at least temporarily, so that some competitive advantage, however transitory, is not lost.

4) The dichotomy between the face-to-face civility that peers are expected to maintain and the behind-the-scenes savagery that peer review protects creates noticeable strains—and sometimes outright cleavages—in a research community. Peer review thus disarms and factionalizes while shielding strong personalities from any timely cross-examination. The public and private exchanges between researchers may differ significantly in style and tone.<sup>13</sup>

5) A procedure which delays the flow of information is secretive insofar as “peers” (denoting “equal”) are cast in unequal roles—the disparity of power clearly favors the referee. The norm of “organized skepticism” would seem to justify the disparity, except that there is nothing “organized” about it. The referee is simply acting as an agent of the research community, given license by an editor or program manager. The referee’s skepticism about a claim is sometimes sufficient justification to postpone its disclosure to other peers, e.g., in the literature. We call such agents “gatekeepers” and invest trust—for the sake of efficiency—in their judgment of claims that merit no, or no rapid, dissemination.

### *Barriers to Analysis*

Because reviews are solicited with assurance that the referee’s confidentiality will be preserved, the later use of these written reports as data in



studying the effects of secrecy, delay, and rejection (or similarly, of premature disclosure) is almost precluded. I say "almost" because cleansing a report of a referee's identity is easy enough but such action washes away information on a host of "status" variables that lend credibility to the report and, therefore, may have been used to justify the recommended action. As a consequence, the researcher's ability to understand a principal mechanism of quality control in science is curbed by operation of the system itself.

Only through the benevolence of risk-taking editors will access be gained. If one regards referee reports as pivotal in the process of evaluation and communication of research results—a kind of diary of scientific practice—then analysis of them is imperative.<sup>14</sup> But examination of the data base can be done only by adjusting the ground rules under which reviews are solicited, not by retroactively revoking the assurances once provided. Journals will be required to separate the guarantee to confidentiality from that anonymity. The former can be sacrificed while the latter is maintained. Opening scientists' practices to scrutiny by other scientists should be welcome, and certainly not controversial.

Consider, for example, the informal review procedures that precede the formal peer evaluations outlined above. Every scientist and organization within which he or she works belongs to a network of research specialists which provides, on request, assessments of draft papers and other "work in progress." Membership in such networks is self-selecting: one's name is added to a "preprint distribution" list; by dint of publishing, one is defined as a peer within an "invisible college" or "scientific specialty;" or ties to mentors and fellow students in graduate school identify one as a "trusted assessor."<sup>15</sup> All of these roles are less than public; they utilize behind-the-scenes communication. They form a secret subculture within a research community. Their purpose is to check and double-check, to test prevailing opinion through reliable community representatives. The result of this semi-public practice is restricted communication that is not only tolerated, but also endorsed and expected by participants in the community.

The presumption is that delay serves to sharpen the soundness of claims, i.e., the community benefits from an informal review prior to submission of claims for formal peer evaluation. Again, analysts rationalize this process as "organized skepticism" and thereby imply that the delay is in-

tellectually defensible. But what if there are other reasons for limited or delayed disclosure? Local organizations, through custom or contractual agreement, may impose restraints unrelated to the claim per se. Profits expected to follow from the knowledge claim or penalties associated with legal or regulatory conditions will override the rush to disclosure which the "extension of certified knowledge" prescribes.

Competing and superordinate norms guide the behavior of scientific organizations. Knowledge becomes more than a shared commodity; it is valuable property that must repay its owner before it enhances the stock of community knowledge. Secrecy thus symbolizes how the norm of disinterestedness is merely part of a hierarchy that is observed, but only at the discretion of the knowledge-holder or -originator. One who controls knowledge has the power, in effect, to determine the timetable for its disclosure and consumption.

This description will be no revelation to the members of research communities in which the commercialization of results has become a regular occurrence; but it does thrust the analysis of scientific communication into a context where the production, dissemination, and evaluation of new findings take on extreme local colorations. Too often, the analyst is left only with rational reconstructions of the "ethos of science." We know what scientists say they do; we do not observe them "doing" science. Moreover, the issue is not whether norms are breached, but rather how they are redefined or stretched to accommodate various constraints—some in the environment external to the organization, others home-grown. Three examples will show how closure has come to be accepted as "normal" practice.

## Redefining "Normal" Practice

### *University-Industry Partnerships*

The current new partnerships between universities and corporations naturally bring differing traditions, practices, and motives into a hybrid nether land where new rules must emerge to handle what had been routine in the respective organizations. It is difficult to make explicit how, in the new joint knowledge production cycle, every communication should be channeled and every

circumstance anticipated. And so this is a time of re-creation of norms, a testing of practices, and perhaps even a reallocation of priorities within science. Extending certified knowledge has a place, but not necessarily the central place, in the emergent hierarchy of needs and objectives.

In this area, too, the analyst of science confronts a deficiency of data where access to organizations, and especially to the research practices within them, is limited. Has secrecy connected with patent potential changed the way researchers act at the bench, or when they talk with peers in other labs, or when they draft their research reports? Do their published products appear different—in style, content, and attribution of support—than others in the same journal? Has negotiation with editors changed? Are some journals no longer receptive to their work? Has their network of trusted assessors shifted and, correspondingly, are they no longer the intellectual asset to peers whose research has remained commercially unexploitable (“pure”)? The testing of a full agenda of hypotheses awaits development of a data base.

As the support of academic research by private industry increased five-fold between 1970 and 1984 to \$400 million, the number and complexity of formal partnerships between universities and corporations grew.<sup>16</sup> Even though these relationships presumably benefit both partners, they tend to strain institutional operations—especially in the university—by redefining rights and responsibilities if not actual behavior. Many people see such redefinition as constricting ordinary practice, i.e., as a change that narrows behavior by providing incentives for certain research, typically that eminently patentable, over others. If such change were self-generated, it would be tolerated as an evolutionary inevitability. As it is, the change is sudden, wrenching, and purposeful—money talks and the university becomes the site of a research business deal.<sup>17</sup> The openness of the deal, and the ensuing operation, masks the alternatives foreclosed. As Leonard Minsky, head of Ralph Nader’s Center for Universities and the Public Interest, has written:

When university researchers are *en bloc* consultants, when funds are specifically tied to the generation of commercially useful products, and when corporate scientists effectively choose the topics of research for university scientists, we have reached an entirely new level of corporate control. The profit motive must now necessarily dominate

the consciousness of professors and students alike.<sup>18</sup>

Zoologist Alexander Fabergé has also observed a transformation in research style that he believes signals a motivational change. He writes that research in classical genetics was marked by extreme openness. Morgan, for example, wanted “to do everything to get other biologists to share [his] experiences;” but “with the recent advent of gene cloning for commercial use,” molecular biology is “the most secretive of all biological sciences today.”<sup>19</sup> (Fabergé is speaking here of university scientists, not those in private labs.)

The difference between open and closed corporate science is whether the sponsor penetrates the research site to dictate what is open and what is proprietary. The tradeoff for sponsorship is the scientist’s surrender of decisionmaking authority for development of his or her research program, a process that is not unilateral but one of negotiation. Nonetheless, sponsors do delineate a programmatic focus and try to determine a timetable for progress.<sup>20</sup> The nature and extent of the scientist’s input to these decisions can be truncated by the prerogatives of the patron.

### *Pork Barrel Funding*

In the last few years, a number of U.S. universities have flouted the normal process of peer review and sought to receive funding directly from Congress.<sup>21</sup> Typically, the politics of “pork barreling” teams members of Congress with university representatives, including professional lobbyists, to secure funds for the building of new laboratories and centers, library additions, and even dental schools. The authorizations are usually tucked away as amendments to pending bills. Many of the efforts have failed; but the successes have spurred other attempts. About thirteen “science” buildings under construction in 1984 were the result not of regular peer-review grant funding but of such extraordinary efforts. The cost to the Federal government is estimated to be \$131 million to date.<sup>22</sup>

The attempts to circumvent peer review have been deplored by such organizations as the National Academy of Sciences (NAS), the Association of American Universities (AAU), and the National Science Board.<sup>23</sup> Their objections, however, are ironic. The critics appear to be berating the “pris-

tine" universities for engaging in activities that violate the regular procedures of peer evaluation—which scientists control. Academic science becomes beholden to a public patron while retaining no claim to quality control or recourse over decisionmaking.

The curious triangle created by pork barrel politics—the scientific community, universities (both public and private), and the political institution—demonstrates how the judgment of scientific merit, and resource allocations that follow from it, can be subordinated, if not subverted to political ends. The victim of cooptation and control is the scientific community. It is closed off from a political process that supersedes peer review. As a 1983 editorial in *Science* stated: "If . . . government money is awarded to universities as a result of success in a competition for political influence instead of as a reward for success in an open competition for scientific merit, the independence and preeminence of American science could be eroded."<sup>24</sup>

Pork barreling is commonplace in Washington. Until recently, science had not resorted to such blatant political means to funding. In doing so, universities have circumvented the quality control mechanism by which they purportedly live. Paradoxically, the universities who do so opt out of conventional competitions for resources and limit their participation instead to the political process. The risk is twofold: They may fail in the political arena and draw the ire of institutional peers for forsaking the conventional process.

The cost to the universities is similarly complex. For the scientific community, the errant way of one of its institutional members sets a precedent and breaks ranks. It merely shows that the NAS, AAU, and other community guardians may not represent the best interests of all its members. Consider, for example, this exchange between journalist Dan Greenberg and Robert Rosenzweig, President of the AAU:

[Greenberg]: Since good science usually gets done in these buildings, regardless of their political origins, why does it matter if the net effect is beneficial for the country's scientific enterprise?

[Rosenzweig]: It matters if it has the effect of changing substantially and systematically the way in which decisions are made. Because I don't think there is any other decisionmaking system that's going to produce results as good, on balance, as the one we've used for the last 30 years.<sup>25</sup>

The disjunction between rhetoric and action may suggest that, in these cases, openness is an expedient principle—a resource deployed as part of a "vocabulary of justification"<sup>26</sup> to defend self-interest—but not binding on practice. In pork barreling, accountability to the scientific community is traded for accountability to Congress and the public: The opinions of the relevant research community on the allocation of Federal funds to science are made irrelevant.

A research community may thus invoke "autonomy" when it is politically useful; autonomy becomes a euphemism for closed science. But, as pork barreling illustrates, the greed of one institution can compromise the autonomy of all. Depending on the audience, the issue, and the stakes, scientists will pay lip-service in the name of openness to the ideology of autonomy.

### *Research Misconduct*

The recurring assertion of scientific autonomy is also observed in cases of research misconduct (or what is often termed "academic fraud"). On what occasions do scientists refrain from hanging out their dirty laundry for public inspection? Is secretiveness about what "really" goes on in science perhaps associated with attempts to deflect attention from professional wrongdoing, e.g., the fabrication of data in a Federally-funded project? Is such closed science effective in defending the integrity and authority of the profession, as well as justifying continued public support of its research?

Allegations of fraud in science are most often challenged for two reasons. First, the accusers are often journalists for whom muckraking is a job.<sup>27</sup> They seek "good stories" and, we are told, have neither concern for nor comprehension of systematic data "at the bench." Second, the steady stream of reported cases of "tainted" science, especially over the last decade, reveals how unprepared research universities, Federal agencies, and professional societies were to investigate suspected fabrication or thievery.<sup>28</sup>

When intellectual property rights are ill-defined, then definitions of "due process" and "full disclosure" may also be unclear. For example, when should a university announce that a researcher is under investigation for misconduct? At what point should the sponsoring organization (e.g., government agency, private foundation) be informed?



Some anonymity and confidentiality are, of course, essential for due process. To ignore them is to invite charges that the accused's character has been defamed or his or her privacy invaded. And aside from the legality of such action, not to receive such consideration would be tantamount to desertion by one's employer and profession.

As such issues are being negotiated and codified, the struggle over the costs of procedures attests to an erosion of science's autonomy and an increase in accountability.<sup>29</sup> And many critics believe that this change will exact a high professional cost—a re-examination of basic notions of publication, allocation of credit, and career advancement.

Would a more open research process deter misconduct? Would this very openness—enhanced knowledge of research practices—decrease the autonomy of science to its detriment? A monopoly of control can undermine the societal trust that all professions strive to maintain as a condition of their legitimacy. The scientific community cannot plead for openness to defend its autonomous control over the communication of technical information, while at the same time denying access to those they define as outside the professional community. Yet when is access appropriate—and to what end? On this question turn such issues as whistleblowing, censure, and the counter-norm of secrecy.

## The Impositions of Basic Science

Proprietary, pork barrel, and tainted science all suggest a shifting context for the conduct of science. The closure of open scientific inquiry by discretionary means cannot be dismissed willy-nilly as a perversion of democracy in the name of greed, fame, or excellence, for the road to scientific virtue is paved with diverse motives. The norms which sanction and shield those motives may not be "best" for all research communities nor for all time. Scientists, like other professionals, have their own interests and values. They must defend their autonomy while eliciting investments in research from public and private patrons. Thus, they cannot be relied upon to guarantee an open, universalistic science: There is no such thing. Instead, the health of the scientific community is maximized by closing it off—for example, from scrutiny of peer review processes and bench procedures. In short, what is best for science may not be best for society, and vice versa.

In the process of weighing the costs and the benefits of observing certain norms and ignoring others, the tradeoffs are clarified. A measure of autonomy is compromised to heighten accountability, candor is abridged to preserve confidentiality, and dissemination of research results delayed by the prospect of commercial profitability. Impatience with and intolerance for tradeoffs similarly engenders conflicts both within science and between science and other social institutions. These conflicts often resist systematic study because they are shrouded in the trappings of the scientific profession.<sup>30</sup> To expose disagreements between experts erodes some of the distance that professions purposely build; it demystifies what, since the time of Merton's writings on the scientific ethos, has been

a popular ideology of science, largely positivist in origin, that distorted the understanding of science and the scientific method. This ideology held that science offered a superior path to truth, perhaps even the exclusive path. But what was meant by science was not what scientists actually did, but a highly reductionist and mechanical image of science as an impersonal, objective process unaffected by ordinary human passions or modes of perception.<sup>31</sup>

Perhaps not science but our perceptions of it have changed—much in the same way that Merton's norms have come to represent the official ideology of scientists, but only a crude indicator of their practices. The contrasts between open science and closed science help to clarify, much like the norms, how constituent communities of science adapt their behavior to ideology on the one hand and local organizational constraints on the other. Inevitably, the behavior will redefine expectations of "normal" scientific practice. In this way, autonomy—individual and institutional—is retained, candor displayed, and results communicated to the satisfaction, and seeming progress, of the research community.

In general, we might all prefer open to closed science, but it is clear that such a normative choice neither preempts nor protects the research process. Science marches on prescribed trails with few signposts. Some trails are never open; others open only to a select few. Participation in science is theoretically open, especially to those who are selected as peers, gatekeepers, and other agents of social control in science. The imposition of



such selection, however, serves to close science to many, at least to deliberations over fundamental matters of quality of knowledge claims and professional comportment. In the end, "openness" is an interest-bearing idea; it cannot be settled with recourse to facts or logic. It is a matter for political debate, not scientific judgment alone.

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## Notes

1. The essay, first published in 1942, is reprinted as Chapter 13 in Robert K. Merton, *The Sociology of Science* (Chicago, IL: University of Chicago Press, 1973), pp. 267–278.
2. Excellent reviews of the empirical and programmatic status of the Mertonian norms are found in Nico Stehr, "The Ethos of Science Revisited," in Jerry Gaston, ed., *The Sociology of Science* (San Francisco, CA: Jossey-Bass, 1978), pp. 172–195; Michael Mulkey, "Sociology of Science in the West," *Current Sociology*, Volume 28 (Winter 1980): 1–184; Thomas Gieryn, ed., *Science and Social Structure: A Festschrift for Robert Merton* (Transactions of the New York Academy of Sciences, Series II, Volume 39, 1980).
3. Bernard Barber, *Science and the Social Order* (New York: Free Press, 1952).
4. In the preface to Barber's book, Merton predicted that only when science was viewed as a social problem would it become a subject for systematic study. He anticipated a lack of congruence and, simultaneously, growing interest in what is known today as "social studies of science."
5. For examples, see Judith P. Swazey and Stephen R. Scher, eds., *Whistleblowing in Biomedical Research* (Washington, DC: U.S. Government Printing Office, 1982); and Kenneth Prewitt, "The Public and Science Policy," *Science, Technology, & Human Values*, Volume 7 (Spring 1982): 5–14.
6. Gerald Holton and Robert S. Morison, eds., Special Issue on "Limits of Scientific Inquiry," *Daedalus*, Volume 107 (Spring 1978).
7. Dorothy Nelkin, "The Political Impact of Technical Expertise," *Social Studies of Science*, Volume 5 (February 1975): 35–54; Dorothy Nelkin, ed., *Controversy: Politics of Technical Decisions*, second edition (Beverly Hills, CA: Sage, 1983).
8. A small, but intriguing literature on ethnography of science now affords us accounts of "laboratory life;" see especially Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (Beverly Hills, CA: Sage, 1979); Karin D. Knorr et al., eds., *The Social Process of Scientific Investigation*, *Sociology of the Sciences Yearbook* 1980 (Dordrecht The Netherlands: D. Reidel, 1980); Karin D. Knorr-Cetina, *The Manufacture of Knowledge* (Oxford, England: Pergamon Press, 1981).
9. Daryl E. Chubin, "Peer Review and the Courts: Notes of a Participant Scientist," *Bulletin of Science, Technology & Society*, Volume 2 (1982): 423–432.
10. "The Ambivalence of Scientists" (1963), pp. 383–412 in Merton, *op. cit.*
11. James C. Petersen, "Citizen Participation in Science Policy," in James C. Petersen, ed., *Citizen Participation in Science Policy* (Amherst, MA: University of Massachusetts Press, 1984), pp. 1–17.
12. Harvey Brooks, "The Problem of Research Priorities," *Daedalus*, Volume 107 (Spring 1978): 171–190. For summaries of findings on grants peer review studies, see Grace M. Carter, *What We Know and Do Not Know about the NIH Peer Review System* (Santa Monica, CA: Rand Corporation, 1982); Jonathan R. Cole and Stephen Cole, *Peer Review in the National Science Foundation*, Phase Two of a Study (Washington, DC: National Academy Press, 1981); U.S. General Accounting Office, *Better Accountability Procedures Needed in NSF and NIH Research Grant Systems*. Report to the Congress (Washington, DC: U.S. Government Printing Office, 1981); and National Commission on Research, *Review Processes: Assessing the Quality of Research Proposals* (Washington, DC, 1980).
13. A lucid portrayal of the differences among scientists chosen to analyze the first specimens gathered from the Apollo moon missions is found in Ian I. Mitroff, *The Subjective Side of Science* (New York: Elsevier, 1974). Mitroff also elaborates Merton's notion of "counternorms" as an outgrowth of ambivalence.
14. The "open peer commentary" of *The Behavioral and Brain Sciences* is a refreshing departure in editorial policy. Whether or not it has intensified animosities—personal and intellectual—in the multidisciplinary cognitive science community is unknown. For a pertinent example of "open" peer review at work, see Stevan Harnad, ed., *Peer Commentary on Peer Review: A Case Study in Scientific Quality Control* (Cambridge, England: Cambridge University Press, 1982).
15. For a classic statement, see Derek de Solla Price and Donald de B. Beaver, "Collaboration in an Invisible College," *American Psychologist*, Volume

- 2 (November 1966): 1011–1018; a recent literature review may be found in Daryl E. Chubin, *Sociology of Sciences: An Annotated Bibliography on Invisible Colleges, 1972–1981* (New York: Garland, 1983); see also Daryl E. Chubin, "Trusted Assessorship in Science: A Relation in Need of Data," *Social Studies of Science*, Volume 5 (1975): 362–368.
16. Wil Lepkowski, "University/Industry Research Ties Still Viewed with Concern," *C&E News* (25 June 1984): 7–11.
  17. Nicholas A. Ashford, "A Framework for Examining the Effects of Industrial Funding on Academic Freedom and the Integrity of the University," *Science, Technology, & Human Values*, Volume 8 (Spring 1983): 16–23.
  18. Lepkowski, *op. cit.*, p. 8.
  19. Alexander C. Faberge, "Open Information and Secrecy in Research," *Perspectives in Biology and Medicine*, Volume 25 (Winter 1982): 263–278.
  20. An example is the U.S. "war on cancer." See Kenneth E. Studer and Daryl E. Chubin, *The Cancer Mission: Social Contexts of Biomedical Research* (Beverly Hills, CA: Sage, 1980), Chapter 3.
  21. A series of reports in *Science*, all by Colin Norman, chronicle some of the recent developments in academic pork barreling: "Lobbying Pays Off for Catholic U. and Columbia," *Science*, Volume 220 (24 June 1983): 1358; "More Tales from the Academic Pork Barrel," *Science*, Volume 221 (1 July 1983): 36–37; "Need a New Lab? Just Ask Your Senator," *Science*, Volume 222 (28 October 1983): 396; "Universities Lose Pork," *Science*, Volume 222 (4 November 1983): 483; "Pork Barrel Funding Deemed Not Kosher," *Science*, Volume 222 (11 November 1983): 592; "Pork Barrel Funds Decried," *Science*, Volume 222 (2 December 1983): 997; "How to Win Buildings and Influence Congress," *Science*, Volume 222 (16 December 1983): 1211–1213; "Congress Votes \$19 Million for Boston University," *Science*, Volume 225 (31 August 1984): 910; and "Pork Barrel Scorecard," *Science*, Volume 226 (2 November 1984): 519.
  22. Kim McDonald, "NSF Seeks Ways to Prevent Universities from Bypassing 'Peer Review' System," *Chronicle of Higher Education* (28 November 1984): 25–26.
  23. Kim McDonald, "U.S. Science Officials Ask Congress to Stop Bypassing Peer Review of Research Grants," *Chronicle of Higher Education* (3 October 1984): 13–14.
  24. Ernestine Friedl, "Bypassing Peer Review for Scientific Facilities," *Science*, Volume 222 (9 December 1983): 1079.
  25. "Q&A: Big Academe's Washington Representative," *Science & Government Report* (1 October 1984): 3–4, 6–7.
  26. Michael J. Mulkay, "Norms and Ideology in Science," *Social Science Information*, Volume 15 (1976): 637–656.
  27. The most recent case in point is William J. Broad and Nicholas Wade, *Betrayers of the Truth: Fraud and Deceit in the Halls of Science* (New York: Simon & Schuster, 1982). For a discussion, see Daryl E. Chubin, "They Blinded Us with 'Science'?" *Science, Technology, & Human Values*, Volume 8 (Fall 1983): 23–29.
  28. Daryl E. Chubin, "Research Misconduct: An Issue of Science Policy and Practice," Final Report to EVIST Program, National Science Foundation (Atlanta, GA, July 1984). An excerpt of this report appears as "Research Malpractice," *BioScience*, Volume 35 (February 1985): 80–89.
  29. Nicholas H. Steneck, "Commentary: The University and Research Ethics," *Science, Technology, & Human Values*, Volume 9 (Fall 1984): 6–15.
  30. As Merton wrote in 1963, "... when sociological analysis is stripped bare of sentiment, it often leaves the sociologist shivering in the cold. Since his own sentiments and allegiances are involved, it becomes all the more difficult to examine the hot conflicts of associates with required detachment. The sociological and psychology study of multiples and priorities, with all it could tell us about the cultural and social behavior of scientists, accordingly tends to remain undeveloped." [Merton, *op. cit.*, p. 398.]
  31. Daniel Yankelovich, "Science and the Public Process: Why the Gap Must Close," *Issues in Science and Technology*, Volume 1 (Fall 1984), p. 9.