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New Lexicon, Old Language

Negotiating the “Global” at the National Science Foundation

Donald Brenneis

Bureaucratization of the imaginative...“Bureaucratization” is an unwieldy word, perhaps even an onomatopoeia, since it sounds as bungling as the situation it would characterize. “Imaginative” suggests pliancy, liquidity, the vernal. And with it we couple the incongruously bulky and almost unpronounceable.

—*Kenneth Burke*

In his characterization of bureaucracies as “bungling...bulky and almost unpronounceable,” Kenneth Burke (1964:76) captures a widespread opinion. In few other contexts, to his view, are innovation, creativity, and fluid adaptation to a changing world less likely to flourish. Indeed, Burke drew upon the seemingly contradictory juxtaposition of bureaucracy and imagination as a prime topic for the analytical method he termed “perspective by incongruity” (Burke 1964:76). Pace Burke, in this article I suggest that federal bureaucracies provide arenas within which imagination is crucial: New knowledge is produced, new problems are recognized, new languages sought for their definition, and new options pursued, foreclosed, or not even considered for their comprehension and, occasionally, remedy. At the same time, and with the irony to which Burke’s comment points, such agencies are also sites in which imagination must be disciplined—translated, routinized, compared, and evaluated—to succeed, that is, to lead to new understanding. Although frequently characterized by considerable institutional and conceptual inertia, such bureaucracies have also been

exceptionally significant in shaping our sense of crisis and change. This essay presents a primarily ethnographic exploration of one such bureaucratic locus in which questions of power, knowledge, and novelty are implicated.

The National Science Foundation (NSF) provides a particularly telling case study in this regard. Charged with funding and thereby guiding innovative basic research in the natural, behavioral, and social sciences (despite recent legislative attempts to shift its focus to more immediately relevant and applied work), NSF is consistently faced with pursuing new knowledge—and new kinds of knowledge—through established disciplinary and organizational frameworks. NSF constitutes a critical if often unrecognized nexus for the institutionally inflected negotiation of new understandings.

This essay focuses on the development of a new NSF funding initiative in the social sciences. The initiative, “Global Perspectives on Sociolegal Studies,” is an offshoot of the standing program on Law and Social Science, a multidisciplinary program within the social and behavioral sciences at NSF. At the core of the Global Initiative was the recognition that many of the issues central to sociolegal studies could no longer be comprehended within the framework of one state’s legal and social control systems, a framework which had defined the broadest appropriate ambit of most previously funded research. Such diverse “problems” as international labor migration, cross-border environmental crises, multinational corporations, and human rights were seen as demanding that formerly assumed boundaries, both political and intellectual, be rethought.

The genesis of the Global review panel in some ways represents the recognition of new and problematic phenomena, legal and social control issues no longer considered comprehensible in terms of “the state.” Characterizing these issues as “global” defined both a new class of research issue and a newly legitimated topic for research funding. It also gave this new category a label, as well as introducing to NSF funding discussions a host of related terms such as “transnational,” “regional,” and “internationalization.” At the same time, the broader range of evaluative practices through which funding decisions were made under this new rubric, the ways in which the imaginative was dis-

ciplined, remained much the same as they had long been for other NSF panels. New concepts—or at least new labels for them—were introduced, but the institutional communicative patterns within which they came to figure remained relatively unchanged.

My purpose here is to examine the tension between the novel, in this instance the topicalization of the global as a current and pressing issue, and the routine, that is, those ways of comparatively evaluating research applications with an eye to the concerns of both “science” and fairness. After locating my approach vis-à-vis some of the intellectual perspectives informing this volume as a whole, I will turn to a brief historical account of the genesis of the Global Initiative and a discussion of the “Belmont Report,” a planning conference paper which became its foundational text (National Science Foundation 1990b). Central to the Belmont Report is the recognition of a dramatically transformed world and of the necessity for new, imaginative, and unorthodox ways of defining research problems and of pursuing them. The ethnographic core of the paper focuses on an analysis of funding panel meetings as reading events. In this part of the discussion, I will draw primarily on my own experience as a knowledge worker, a member of the Global panel during its first three years. What ways of reading and of talking about reading shaped our recommendations, and how did we bring our sense of the explicitly innovative goals of the initiative together with our negotiated rankings of particular proposals? Finally, I will return to the Burkean tension between routine practices and novel ideas.

READING THE READINGS

As Marcus noted in his original proposal for this seminar, the topic “power/knowledge shifts” represents in several ways a reappraisal of the issues at the heart of *Writing Culture* (Clifford and Marcus 1986) and subsequent related approaches to the intersections of text, power, and cultural criticism. I want to draw on one part of the complex set of arguments in that volume, specifically the introduction and essay by James Clifford (1986a, 1986b), as a starting point here. At the core of Clifford’s analysis is an examination of the production of particular forms of scholarship and the knowledges they implicate. The relationships between formal and representational conventions, on the one

hand, and what we take to be fact, on the other, are clearly and suggestively explored. In his illuminating focus on the complex interactions of author, generic form, and contingent knowledge—and more markedly in some of the less-nuanced studies that these essays inspired in others—however, three consequential and complementary issues remain unaddressed.

First, the focus of analysis remains primarily the completed, published text. Articles, monographs, and published research reports provide the raw material and are where the interpretive action lies. Embedded here is, to my view, something of a fiction that the published product is indeed a finished one. Linked to this focus on finished texts is a central concern for the interplay of generic assumptions and authorial strategies. The writer is at the heart of such work, and it is primarily through a community of writers that the joint production of such generic conventions is effected. Surprisingly little attention is paid to the role of editorial and review processes, interventions, and negotiation, for example, although such considerations might well strengthen the underlying critical argument.

As the apparently finished text is the focus, informed “reading” in the classical literary critical sense often serves as a principal methodology. What is left as unproblematic is the reception of these texts; their effects and implications are usually assumed rather than pursued. Clifford briefly hints at the importance of a (perhaps) imagined audience, noting that among the ways in which ethnographic writing is “determined” is its institutional character; in his words, “one writes within, and against, specific traditions, disciplines, *audiences* (my emphasis)” (Clifford 1986a:6). He does not, however, pursue the issue of reception further, other than to cite Asad’s comment that “Allegories are secured . . . by teaching people to read in certain ways” (Clifford 1986b:119). What ways of reading there are and how readers are taught to use them—and by whom—are crucial and unasked questions. The neglected audience is, I should note, a recurring problem in a wide range of rhetorical analyses. Detailed, insightful, and compelling accounts of what writers—and speakers—are up to are fortunately becoming more common (as in the contemporary renaissance of rhetorical approaches to scholarly writing by such scholars as Bazerman

1988; McCloskey 1985; Nelson, Megill, and McCloskey 1987), but equally effective accounts of how, why, and to what extent such texts “succeed” with audiences, whether of readers or listeners, are infrequent. (For examples of several different audience-focused approaches see Boyarin 1993; Brenneis 1987; Duranti and Brenneis 1986; Radway 1984; Schieffelin 1985.)

At the heart of my broader research project lurk some particular forms of scholarly writing and reading, ones in which the three concerns noted above cannot be elided. These forms of writing are the social science research funding proposal (and some cognate forms in the humanities) and related fellowship support applications (Brenneis 1988, 1994; see also Swales 1990). Before “writing culture,” many of us must also “write money”; that is, compete and obtain support for ourselves and our research projects. Our grant proposals can be seen neither as finished products nor as texts with inconsequential and unproblematic readers. An analysis that focuses solely on the writing practices and textual features characteristic of such writing, while potentially revelatory, clearly cannot tell the whole story. Any account of the “writing machine” behind grant proposals must be complemented by an investigation of the “reading machine” that is brought to bear on them.

How we as authors craft such proposals and how we as reviewers read, interpret, and evaluate them are crucial and connected processes in shaping our scholarly discourses—in the defining and policing of disciplinary boundaries, in the support of some kinds of research at the expense of others, and, more subtly, in the ongoing negotiation of the complex links between language and epistemology: links between what we can know, how we can come to know it, and how it can be represented and conveyed. Grant writing, reading, and rewriting have also come to occupy increasingly larger portions of our scholarly work time. And for many, the funded proposal has replaced the refereed acceptance of a manuscript as the definitive mark of serious scholarly accomplishment. As a colleague once commented to me, “Anyone can publish an article somewhere or another these days, but getting funded by NSF or NIH [National Institutes of Health] means it’s good science.” In writing money we also write status.

At the same time a quotidian practical concern for academics and an analytically invisible cluster of social practices, the funding nexus is one through which many of us as scholars are engaged in producing knowledge, not only as funded researchers but also as active readers and reviewers of the proposed work of others. Such agencies as NSF are clearly bureaucratic institutions, but they are remarkably porous ones, as they depend absolutely on the intermittent but consequential participation of scholars from outside the foundation. In this instance, to paraphrase Pogo, we have met the bureaucrats, and they are us.

THE GLOBAL INITIATIVE

In this section I briefly trace the development of the “Global Perspectives on Sociolegal Studies” initiative within NSF. I am primarily concerned with the foundational text for the initiative, a document that is both rhetorically positioned and substantively significant in shaping subsequent definitions and discussions on the panel.

The initiative emerged within a particular institutional context in which a catalytic role was played by Felice Levine, then program officer for the Law and Social Science panel, an interdisciplinary program within what was then the Social and Economic Sciences (SES) Division of NSF. The broader NSF context is significant, as this was the second such interdisciplinary initiative within SES. The first initiative, “Human Dimensions of Global Environmental Change,” involved program officers from economics, geography and regional science, law and social science, political science, sociology, and other programs. In setting up the “Human Dimensions” initiative, SES was following the lead of programs in biological and ecological science at NSF and drawing directly on the perception of widespread environmental risk as a new problem and topic. Levine has noted to me that the “Global Perspectives” initiative would not have worked without “Human Dimensions” as a prototype, in large part because the earlier initiative essentially foregrounded human agency and response within an already well-established scientific discourse about the environment, one in which themes of interdependence, inflected at times by metaphors drawn from chaos theory, were salient. “Human Dimensions” both established a rhetoric of interdependence (along the lines of more purely biologi-

cal theories) and legitimated the enterprise as “science,” a critical consideration within NSF.

Heartened by the success of the “Human Dimensions” initiative, as measured by both its funding level and the number of applications received, Levine organized a conference, held in Belmont, Maryland, in mid-1990. The purpose of the meeting was twofold: “(1) to examine how a global perspective extends or alters traditional ways of conceptualizing and studying law and law-related processes and behaviors; and (2) to provide guidance on the implications of a transnational perspective for research strategies and support” (National Science Foundation 1990a:1). In short, Levine asked the twenty participants, drawn from a wide range of disciplines and intentionally including a number of scholars who had not worked extensively in sociolegal studies, to consider “both the global dimensions of sociolegal phenomena and sociolegal dimensions of global phenomena” (National Science Foundation 1990a:1).

At the core of their discussions was an assumption of increased interaction and interdependence in the world—among people and across institutions, organizations, and economies. Participants saw the character of global life as undergoing a major substantive transformation: “Even with issues that seem to be local, processes of international diffusion and interaction have become so common that it is no longer useful to think of events as independent. Not only are legal agreements, treaties, and innovations diffusing...but so too is culture being transmitted across national boundaries by the movement of people or the rapid transmission of ideas” (National Science Foundation 1990b:3). Such a change required recognition as a consequential new phenomenon; it also demanded an appropriate epistemological transformation on the part of researchers. As the Belmont Report argued, “Events around the globe demonstrate the importance of thinking transnationally if our understanding of sociolegal phenomena is not to become limited and parochial” (National Science Foundation 1990b:3).

While the “Human Dimensions” panel fit relatively well within a global environmental paradigm, “Global Perspectives” suited the underlying ecological model less comfortably. Interdependence was a recurrent theme, but much of the impetus behind discussions in

Belmont had to do with the somewhat different recognition that earlier units of analysis were insufficient for dealing with current issues. The states—or statelike entities—which had historically been central to sociolegal inquiry had become problematic, acting more frequently not as the arenas for and forces of social control but as parties in broader conflicts. Multinational organizations, international institutions, and new forms of transnational governance and social control all were taken to challenge the nation-state as the appropriate primary unit for description and analysis. The interdependence metaphor, however necessary initially for in-house funding arguments, became a bit strained when applied to questions of conflict.

Situational factors such as the transformation of Eastern Europe and the former Soviet Union and “uncertainty in the Middle East” were also offered as further significant incentives for the timeliness of such funding. These discussions were located within a context in which a dramatically changing world was a recurring theme.

In preparing for the Belmont Conference, Levine identified five themes and asked each of the participants to prepare a memorandum concerning a particular theme. These topics, chosen to represent domains where “globalization” seemed especially marked and of particular sociolegal consequence, complemented the broader theoretical perspective of the Report. Although these themes were explicitly not intended to constitute an exhaustive inventory, for subsequent readers they became particularly important signposts, staking out much of the territory of the “global.” The core of the Report proper provides a fairly detailed account of group discussions of these five themes, which included (National Science Foundation 1990b:9) the following:

1. Commercial and economic transactions
2. Immigration and population shifts
3. Social and ethnic conflict
4. Regulation of the environment
5. Transnational governance

The Report also provided a discussion of theoretical opportunities suggested by the recognition of the “global interdependence of sociolegal phenomena,” among them the “reinvigoration of extant con-

cepts, expansion and generalization of theories and models, the development of new theories and models, and the reassessment of some of our basic assumptions” (National Science Foundation 1990b:15). Again, the summary of conference discussions called strongly for new methods to suit a transformed world situation: “A revolution in research must follow the revolution in world events and the increasing globalization” (National Science Foundation 1990b:16).

Central to these discussions was the development of a new vocabulary within which apparently new problems could be conceptualized, explored, and interpreted. Much of this vocabulary was borrowed from other scholarly and institutional contexts, but new and frequently contested, or at least implicitly contradictory, meanings also emerged, both in the Belmont Report and in its subsequent interpretations over the ensuing three years of funding discussions.

Due in part to the success of the Belmont Conference and Report and in part to Levine’s remarkable intellectual vision, political acuity, and organizational energy, a new funding initiative¹ was approved under the aegis of the Law and Social Science program. The initiative was announced in the winter of 1990 with a submission deadline of March 15, 1991; it has been continued on an annual basis every subsequent year. The language of the announcement drew heavily on the Belmont Report. In specifying the range of appropriate topics for funding, the announcement noted that

Proposals are welcome that advance fundamental knowledge about legal interactions, processes, relations, and diffusions that extend beyond any single nation as well as about how local and national legal institutions, systems, and cultures affect or are affected by transnational or international phenomena. Thus, proposals may locate the research within a single nation or between or across legal systems as long as they illuminate or are informed by global perspectives. (NSF 1990)

The Belmont Report and the initiative announcement jointly framed a broad understanding of globalization as a process and provided a range of exempla for how it was transforming the sociolegal

realm. How this wide-ranging and innovative perspective was to be put into practice, however, fell to the peer review panel appointed to fund research sponsored by the initiative.

REVIEWING PEER REVIEW

NSF review procedures are built around the notion of peer review, an institution associated not only with research funding but also with the refereeing of manuscripts for journal and scholarly press publication, ethical self-regulation within disciplines, and a range of other professional activities. Some types of grant review rely almost solely on ad hoc mail reviewers. Program officers request commentary and recommendations on specific proposals from up to ten scholars, but the reviewers never meet to discuss the proposals, and few individual reviewers will have read more than at most a handful of them. The most usual practice within SES is for a standing panel with a number of members serving multiyear terms to meet twice a year. Ad hoc mail reviews are solicited from specialists on the subject of each proposal, and at least two of the panel members read, write evaluations of, and take responsibility for shaping the panel's discussion of each proposal. The entire panel participates in comparative discussion and, most critically, the relative ranking of the full array. Finally, in some instances, as on the Global panel, there are no mail reviewers. Each proposal is read and reviewed in writing by three or four panelists, who also start off discussion. The entire group, again, usually joins in, and all collaboratively construct priority rankings for all the proposals.

In this essay I am concerned primarily with the practices of the Global panel, but several general points from the peer review literature are worth noting. First, while peer review is central to much civilian federal research funding, both the military and private foundations often rely on quite different methods of decision making, usually involving a "strong manager" who may be advised by outside scientists but is not limited by their opinions. The two most thorough historical studies of how research funding has affected the development of particular scientific fields, in both instances focusing on the guided rise of molecular biology (Kay 1993; Kohler 1991), have concentrated on prewar private foundations in which particular individual administrators acquired—

and then used—enormous discretionary power. Both are exceptionally compelling and perceptive studies, but the stories they tell are in many ways classic hero tales with brilliant and determined if at times problematic protagonists. Perhaps not surprisingly, no comparable account exists for NSF, apart from discussions of debates at the time of its founding. Influenced by the overtly powerful personalities who figure centrally in his own narrative, Kohler has suggested that, in peer review–dominated institutions such as NSF, program officers may end up as little more than “secretaries for the panelists” (Kohler 1991:404). As my brief description of review practices above hints, and as my consideration of panel meetings stresses, program officers are far from mere recorders of scholarly discussion; their power, however, is subtly exercised and, without careful observation, can be easily overlooked.

Peer review at federal agencies has been the subject of considerable critical study. Some literature concentrates on particularly flagrant ethical abuses (for example, Bell 1992), but most scholarship is concerned with more everyday structural and procedural problems within the system (Brooks 1978; Chubin and Hackett 1990; Chubin and Jasanoff 1985; Cole and Cole 1981; Cole, Cole, and Simon 1981; Cole, Rubin, and Cole 1978; Gillespie, Chubin, and Kurzon 1985; Harnad 1985; Porter and Rossini 1985; Rip 1985; Roy 1985; Salter 1985). Several themes recur in these critiques, including a concern that there is no way of predicting from a proposal the quality of the scientific knowledge to be gained (a focus in the work of the Coles and their colleagues) and the sense that review by scholarly peers, rather than elected representatives, is antidemocratic.

Two other themes are particularly helpful for thinking about the Global panel. First, Rustum Roy, an especially outspoken critic, has argued that “the system is intrinsically inimical to innovation; this situation occurs because few leading scientists are willing to expose their best ideas to their competitors, and because it often takes between 9 and 18 months to get funds, and because radically new ideas will always be viewed critically by the majority” (Roy 1985:74). While Roy does not provide much evidence to support these claims, it is clear that he sees intense competition driving the production of knowledge; exposure, delay, and resistance make the process unattractive to the best competi-

tors. A somewhat more sober and empirically grounded appraisal (Porter and Rossini 1985) suggests that interdisciplinary proposals have had a significantly lower rate of support than those clearly identified with one discipline; they attribute this finding to the discomfort of reviewers in evaluating proposals beyond their immediate expertise.

A second recurrent concern is for the fairness of the peer review process. A recent General Accounting Office report, for example, focused on questions of bias in the “selection of peer reviewers, the scoring of proposals by reviewers, and the final funding decisions of agencies” (GAO 1994:2), examining the work of NSF, NIH, and the National Endowment for the Humanities with an eye to disproportionate patterns of representation along racial, ethnic, gender, regional, or institutional lines. The results were generally positive, although relatively few younger scholars were used as reviewers. Apart from issues of general group bias, critics have often considered the question of whether it is possible for proposals to be evaluated solely on their own merits and compared equitably with each other, a concern often shared by panelists themselves.

Chubin and Hackett (1990:43–48) provide a thoughtful analysis of these critical themes, noting that the stated desiderata of rationality, fairness, validity, and reliability are often competing and incompatible goals and that frustration with peer review derives in part from a lack of clarity and agreement as to the purposes of funding.

All these studies deal with aggregate characterizations of peer review practices, whether more or less anecdotally (as in Roy 1985) or through more systematic consideration. One striking consequence of this approach is that, as the peer reviewers themselves and their behavior hold center stage, the program officers vanish from view, just as in the historical accounts discussed above. There has, furthermore, been little attention to what actually goes on in panel discussions. Although peer reviewers are treated in terms of aggregate features, they are also implicitly regarded as more or less independent actors. Chubin and Hackett (1990:48) have suggested that a serious study of the “group dynamics” of such meetings would be very helpful, but apparently no one has taken up the challenge of taking panel interaction seriously. In turning now to my own fieldwork, I will provide a somewhat fuller

account, not of “group dynamics” per se, but of the discussions as “reading events,” jointly structured and accomplished occasions within which shared understandings are disputed and negotiated.

TABLE TALK

NSF review panels take place within a bureaucracy of a particularly porous sort. At NSF a professional program officer or a scholar on a two-year visiting position, usually assisted and guided by an administrative associate, coordinates the review process and writes up the final recommendations. Most participants in the evaluation, however, are academics engaged elsewhere in their own teaching and research. If serving as ad hoc reviewers, faculty might read one or two proposals a year, providing a written evaluation for consideration by the standing panel. As a panel member, one reads and writes up considerably more proposals (often forty for NSF and more at NEH) and spends several days a year in Washington in face-to-face discussions of the entire range of applications. In short, NSF is staffed primarily, though not permanently, by what I have elsewhere (Brenneis 1994) called “nonce bureaucrats,” a shifting cast of visiting academics. Although panel discussions and final decisions about funding take place in Washington, a comprehensive understanding of the entire process calls for truly multisited ethnography (Marcus 1995). Many of the participants are never together in the same place, but all are linked through their shared though scattered engagements as authors, readers, critics, gatekeepers, and researchers. The process coalesces, taking form and acquiring visible participants, in recurrent but relatively ephemeral events such as panel meetings.²

My own research is based on considerable time spent as one of these nonce bureaucrats, that is, as an active participant in such critical reading events. To return to Marcus’s original vision for this seminar, I represent, at least in part, an insider in the institution that I’m discussing. While I am far from a full-time and fully initiated practitioner, one of the striking characteristics of such sporadic participation is its intensity and the surprisingly strong identification that it occasionally catalyzes. This sense of serious engagement in the review process (or perhaps the suspicion of one’s own complicitous coperformance of

institutional agendas) is critical in shaping both the tenor and the responsiveness of reading panel discussions.

I began serving on such review panels six or so years ago (and I've now logged three years on the Global Initiative at NSF, four years on the college and independent scholars panel for anthropology at NEH, and five years evaluating NSF graduate fellowship applications). I was invited to join the NSF panel primarily because I am one of a quite small number of legal anthropologists of my cohort who has worked extensively outside the United States. I had never applied for NSF funding, but had evaluated numerous proposals for the Law and Social Science panel as a mail reviewer. Most of my fellow panel members were chosen in part because they had received NSF funding in the past.

Given the global dimensions of our subject matter, panels included a variety of disciplines. In 1991, the first year, eleven panelists (including two deputized from the Law and Social Science panel) evaluated fifty-five proposals. The panelists' disciplines ranged from demography and economics to anthropology and legal history, with three political scientists constituting the single largest group. Subsequent panels were somewhat smaller but included a similarly diverse group. In 1991 three women and eight men served on the panel; a similar gender ratio continued in following years. The first year Felice Levine, a sociologist, long-term professional NSF staff member, and the catalyst for the Global Initiative, served as our program officer. Levine left to become Executive Director of the American Sociological Association and was succeeded by two Visiting Scholar/Administrators with extensive sociolegal research experience, Michael Musheno and Susan White.

Early on it became clear to me that panels such as ours constituted consequential audiences indeed, not only for the funding of specific research but in the broader shaping of what we take knowledge to be. It also became evident that the audiences we imagine when "writing culture" are quite different from those we must address in "writing money." Many of us in anthropology imagine our extradisciplinary audience to be involved in literary, historical, and cultural studies, a range of interpretive communities (cf. Rabinow 1986) demanding a particular range of styles and strategies. For the funding that makes our

work possible, however, we often must turn to quite different kinds of interlocutors and draw on very different forms of discourse.³ At times, in fact, our interlocutors may be the same individuals, but how they read and what they expect can change with their roles. The speed and relative ease with which such transformation often occurs speak both to the compelling qualities of the evaluation process itself and to deeper underlying notions of coherence, method, fairness, and the “studyable” that we as readers discover we share, at times to our considerable surprise.

One of the rhetorical consequences of my dual roles as active participant and critical observer is the pronominal limbo in which I’ve found myself while writing this essay. At times I write about what “we” do, at times of what “they” are up to. Such deictic shifts are not intended to assign culpability to the third person, however strong the temptation. They primarily reflect the extent to which I’ve had to rely on introspection as an empirical strategy.

I should also note here that I’ve been quite open with both full-time administrators and my panel colleagues about my dual roles as reader and researcher. My fellow nonce bureaucrats found my interest generally antic but harmless; many have been quite generous with their time and thoughts. Program officers have been exceptionally supportive and forthcoming, in large part because they are often looking for strategies for adjusting and improving the present system of review, ameliorative moves which I’m not sure my research can suggest.

This section focuses on those reading events in which I was both active participant and ethnographer, that is, the three two-day meetings of the “Global Perspectives” panel from 1991 to 1993. These sessions were consequential in part because of the individual funding decisions made during them. They also provided a recurring context in which we actively negotiated the meanings of the “global” and its implications for focus, method, and interpretation⁴—and quite directly shaped the forms and topics through which “global” knowledge was to be produced. We were actively engaged in taking and translating several foundational texts—the Belmont Report and the Call for Proposals—in terms of specific cases. Every year, for example, the question of how strictly we should interpret the Belmont document arose: Would com-

parative studies, ones in which the units remained the nation-state, be fundable under the global rubric, or could we only support proposals that truly embodied the new “transnationalism”? This was a recurrent source of amiable but spirited disagreement, concerning which each of the three different program officers with whom we worked also had different views. One program officer in fact confessed to some irritation with the panel because it seemed we had “sacralized” the Belmont Report. One reason some of us adopted such a “strict constructionist” perspective might well be not so much a substantive commitment to the idea as the response of readers driven to make decisions among too many good proposals. Relying on external criteria, in this instance goodness of fit with particular programmatic goals, was often exceptionally useful.

Beyond these formative documents, however, we were also engaged in interpreting individual proposals—couched, not surprisingly, in the jargons of a dozen different disciplines—and in trying to translate them into the terms of the Belmont Report. A critical conundrum for us was the dual mandate to take each proposal on its own terms and to compare it with all the others, at times an internally incompatible enterprise.

A further issue is the nature of funding panel discussions as communicative events.⁵ I want here to address two critical dimensions: first, the role of the program officer, and, second, the general characteristics of panelist participation. In marked contrast to the weak or almost invisible figure other studies have suggested, the program officer is pivotal. She recruits panel members and determines who will read which proposals. In a multidisciplinary panel in which at most a very few members will be specialists in the proposer’s discipline, such decisions are particularly consequential. The program officer also controls the agenda. Although there is a general trajectory from report and discussion to comparison and ranked recommendation, lots of variation is possible. Some program officers start with what they think are *prima facie* the strongest proposals and then move on to less promising ones. Other officers will move back and forth among proposals of variable perceived quality. Only the program officer has had the opportunity to read all panelists’ comments on each proposal and therefore has some

sense of initial rankings. Within the discussion of a proposal, the program officer may choose to have several more positive reviews read, followed by more negative ones, or she might choose to begin with the most positive and the most negative reviewers, calling upon other respondents only if necessary. This latter approach often led to the creation of particular antiphonal roles within the discussions more generally. I recurrently found myself, for example, paired with—and speaking amiably against—one particular colleague as we jointly staked out the theoretical and epistemological antipodes of a particular research idea. Such disclosure strategies clearly influenced the “group dynamics” of our discussions.

The program officer controlled a great deal of information to which panelists had no direct access. As noted above, only she knew beforehand what each respondent thought of every proposal and could play those cards as she saw fit. She also was familiar with the budgetary possibilities. Exact dollar figures were rarely discussed in the panel; we pursued financial questions only in the case of exceptionally expensive proposals, where we might recommend cuts. Generally, however, attempts on panelists’ part to learn how many proposals might actually be fundable were discouraged; we were to compare and rank in terms of intrinsic merit and promise and to keep money out of the picture.

It is important to point out that panels in themselves do not make funding decisions; they rather make recommendations to the program officer as to priorities for funding. A composite list placing each proposal into one of a number of priority groupings, usually organized in terms of “Fundable,” “Possible,” “Deferred,” and “Declined,” is advisory rather than binding on the program officer. She is our principal interlocutor and audience. The program officer actually makes decisions as to final recommendations for funding, but she also has a further audience in mind, here comprising both more senior administrators and the implicit audience of congressional oversight committees.⁶

After the panel meeting, two documents concerning each proposal are prepared. One, “Form 7,” is written by the program officer for future audiences within NSF and would be seen by panel members only in subsequent years if a previously rejected proposal has been resubmitted. These paragraph-long reports address various aspects of each

proposal to provide reasons for particular recommendations: the number and quality of proposals reviewed that year, the importance of the issues, investigators' track records, the clarity of the proposal, how well it fit with the topical definition of the panel, the "ripeness" of the proposed work for funding, and the nature of the panel's recommendation. In these documents the program officer translates talk into text, abstracting a clear and concise message from often lengthy and complex conversations. The power to clarify and articulate here is considerable and highly consequential. At no time during my panel experiences did I think program officers were doing anything other than trying to represent our discussions; at the same time, completing Form 7 does inherently shape the outcome of our deliberations, especially in a funding context where only about a third of the proposals could be funded.

The other documents prepared after panel meetings consist of the "panel summaries," a series of individual accounts of the written and oral responses to each proposal which are to be sent to the respective Principal Investigators. Some program officers ask individual panel members to write these, while other program officers write the summaries themselves. To quote one program officer, each such summary should provide "a simple description of what was said," one that necessarily highlights particular points of praise and criticism in the written reviews and discussions as salient and leaves others unaddressed. Panel summaries are to focus solely on the merits and limitations of each individual proposal, rather than explicitly considering the broader comparative framework, which is often in fact quite consequential. These panel summaries are particularly critical documents for those applicants whose proposals have failed; the ideal summary should provide candid feedback and give positive indicators of the potential merit of the project.

A second crucial dimension of panel discussions as communicative practice has to do with the tenor of our talk with each other. In marked contrast to the image suggested by Roy (1985) and others—and to the picture some of us might have had in mind before the panels actually began—our conversations were marked not by explicit and antagonistic competition but by remarkable amity and cooperation. One way of

thinking about this is to return to the polysemic notion of peer review. We were both reviewing the work of others as peers and, in our discussions, concerned with being peers. We became, in the context of the meeting, “nonce peers”⁷ as well as nonce bureaucrats. Participation in such decision making made one, for the moment at least, an “equal.” In peer review we jointly constituted an ephemeral peership among ourselves as reviewers as well as vis-à-vis those whom we were evaluating.

Such civility is clearly not singular to the Global panel. Shapin’s recent (1994) detailed examination of the influence of gentlemanly codes of conduct among scientists in seventeenth-century England is exemplary in this respect; what was taken as “true” information depended in large part on who presented it and how they did so. Mutual trust was considered indispensable for creating scientific knowledge. It inhered in particular relationships—that is, among those who were equals as “gentlemen”—and became attenuated as scientists were forced to rely on the findings of individuals who were not their peers. In contemporary scholarly discourse, where “gentle” origins are no longer in explicit play, there may in fact be an even greater premium on enacting equality and the trust it both engenders and reflects.

The importance of sustaining amiable discussion at panel meetings was evident. In those instances where disagreement was present, panelists often relied on humor, and particularly on the strategy of exaggeratedly embracing those roles which might have been suggested for them by the program officer. Making such role parody work required more than one performer; I could have played the defender of highly qualitative if methodologically underspecified research successfully only if my opposite took on an archpositivist persona. In Bateson’s terms (1972), argument was often reframed in terms of play; potential “bites” were prophylactically performed as “nips.” Such reframings required copformance, as well as an attuned audience that knew what was going on. Hedging and disclaimers were also common in introducing our written comments; for example, “Well, you know what I’m going to say,” or “I don’t really know much about this.”

As striking as the general commitment to civility was the degree of interdisciplinary deference shown. In part this reflected the necessarily interdisciplinary definition of sociolegal studies as a general field and

of the particular call for innovative methods issued for the Global Initiative. We also had no highly specialized external reviewers on whose opinions we could count for knowledgeable advice and were constantly consulting with each other, again, with frequent disclaimers of our own expertise. A further element muting potential overt competition was the fact that we were explicitly discouraged from talking directly about money, with the exception of some egregiously expensive proposals.

In short, our discussions were generally characterized by amiability and a willingness to listen to what others had to say.⁸ Ironically, this willingness to listen often served to limit what one might say or how strongly one might be willing to say it. My sense is that it was much easier to raise doubts about proposals—through questions, brief and often indirect critiques, or humorous comments—than it was to make an impassioned argument for them. Panelists rarely went out on a limb on which they might find themselves alone; intellectual passion often conflicted with our commitment to civil conversation. While Roy and others see individual competitiveness as a core limitation on the efficacy of peer review, especially in regard to innovative work, I'd suggest the opposite: that the highly collaborative nature of panel work makes disagreement difficult.

One of the most striking features of panel sessions has to do with how the discussion of individual papers and our comparative discussion would come to an end. Quite frequently the panel officer would say something like, "I hear a consensus developing," or "The sense of the meeting seems to be..." Usually what she would literally be hearing would be silence rather than any overtly articulated panelist commentary. She was responding to the end of open disagreement and the apparent unwillingness of panelists to push the discussion any further. Silence is clearly polysemic here; often motivated by the topic having been exhausted, it could also be "heard" as a sign of panel agreement. Panelists could respond to the program officer by claiming they'd heard no such agreement, usually with tongue patently in cheek to mark the comment as play, but such cavils were very rare. Coming to closure through silence and the open-ended interpretive possibilities it engendered were critical features of our discussions—and left a great deal of play for the program officer.

I have elsewhere (Brenneis 1994) explored some of the processes of socialization and self-discipline that lie at the heart of becoming a bureaucratic reader and a participant in such panel discussions. I want here to note one further feature of our table talk, the development of a putatively common language for comparison and evaluation. Transparency of goals and means was considered important, and proposals that might be thought-provoking but “noisy” (to borrow a term from communications theory) were often considered incomparable and therefore difficult to fund in good conscience. A frequent comment at panels ran along these lines: “It’s a terrific idea, but I don’t really know how to compare it to the other proposals.” Such engaging if troubling proposals were frequently tabled for later discussion but rarely were returned to with enthusiasm. At the same time, we were expecting applicants to be working in an uncommon, or at least highly innovative, language as they struggled to get an empirical handle on what were seen as pressing and novel problems. How we used a common language to discuss proposals which were, at their best, likely to be fairly uncommon, was a constant if usually unspoken tension in our discussions.

This notion that both the language of proposals themselves and that of the evaluative talk about them should be referentially transparent—that is, that they should carry meaning assumed to be independent of rhetorical or idiosyncratic baggage—has to do with views of both science and fairness. Informing the peer review system is what I see as the heuristic fiction of interrater reliability, of decisions not being the result of a majority view but the closest possible approximation of some external, objective standard. In peer review, we are not agreeing with each other but, through disciplined reading and discussion, coming into agreement with and about the phenomenon at hand.

Concerns about fairness also figured critically here. Procedural regularity, that is, treating each proposal equally, was seen as critical. Such an “equal protection” procedural model also presupposed the commensurability of proposals. We often assumed that the ability to make sense of a particular proposal vis-à-vis other proposals was indispensable for a fair determination of comparable merit. Innovation, truly novel approaches, and those that are not clearly argued—or, particularly, those for which methodological strategies are left underspeci-

fied—were unlikely to be read as comparable and therefore unlikely to be recommended for funding.

A final issue in this section has to do with the outcomes of our discussions over the three years of Global panels. Perhaps most surprising is how unsurprising the recommended projects were, a comment not on their scientific merit but on the novelty of their topics (a situation that is, by report, changing for the better as potential applicants get a stronger sense of the possibilities the initiative affords). While some proposals clearly focused on the new initiative, for example, a study of specific problems of policing across borders, or one studying through survey research the legitimacy of the Court of Justice in the European Community, many represented “older” issues: the effects of race in eyewitness testimony, internal ethnic conflict, and classical comparative studies. A secondary goal of the initiative had been to encourage joint research with non-US scholars, but very few such proposals were funded—a clear instance, I think, of the application of assumedly universalistic criteria for evaluation across quite disparate intellectual communities.

CONCLUSIONS: GLOBAL ISSUES AND GLOBALIZING DISCOURSE

I want here briefly to consider lexical innovation as only one part of the broader discursive activity associated with research funding discussions. Language is more than lexicon (Brenneis 1995). While the vocabulary may be new, in short, the ways in which that vocabulary is used are consistently under negotiation and, more signally, are embedded in usually much more conservative linguistic practices. How we talk about these issues and how institutional concerns and constraints shape and discipline the style as well as the terms of our discussion has proved much less susceptible to transformation. How we think about language, and particularly the quite specific ways in which we turn to assumptions of the importance of referential transparency in NSF discussions, has not changed markedly, although the subjects to which we turned our discussions are innovative. In part this is because the “global” as topic is considered within an already “globalizing” discourse, one in which fairness, science, and notions of universal and objective criteria figure critically.

In regard to one of Marcus's framing questions for this volume, that of the response of institutions to a possibly radically transformed world, the NSF provides a complex but basically limiting case. The new vocabulary is there, in large part due to the real intellectual vision of several critical actors, most notably Felice Levine, who is clearly the hero figure in this account. And, of even more consequence, so is the money. How these new terms and the conceptualizations they imply are deployed and how they actually affect outcomes (and especially, how their innovative implications are frequently muted by the weight of ongoing panel discursive practices) remain central and quite open questions. To return to Burke's incongruity, How and to what extent can the imaginative flourish? New words have been found, but they are often used as part of a much older story.

Notes

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1. Within NSF, the term "initiative" indicates a somewhat exploratory funding opportunity, often renewed on an annual or biennial basis. It is distinguished from standing programs (such as the Law and Social Sciences Program), which have ongoing organizational and funding status within NSF. Appointments to Program panels are usually made for a three-year term, while participation in

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initiative funding is on an annual basis. Some initiatives represent a coalition of standing Programs and Program Officers; the Global Initiative is much more clearly linked to one Program.

2. In a germinal article, De Solla Price and Beaver (1966) characterize such dispersed scholarly “communities” as the “invisible college.” While their perspective is, in my view, overly optimistic and was confined to a much smaller range of characters, i.e., “researchers,” than more recent accounts, it remains a useful heuristic notion.

3. For stimulating considerations of the implications of funding audiences for the focus and definition of suitable research projects see Rafael (1994) and several of the essays in Gupta and Ferguson (1997a and 1997b).

4. Kearney (1995) provides a particularly thought-provoking account of just how diverse, contradictory, and indeterminate current uses of notions such as “global” are.

5. While there have been at least two interesting analyses of the language of written peer evaluations (He 1993; Johnson and Roen 1992), I know of no consideration of oral evaluatory discussions. Grimshaw (1989) and some of the articles in Drew and Heritage (1992) touch on some kinds of related practices.

6. The congressional audience was taken very seriously. One program officer noted to me that the most difficult part of the job was working with successful applicants to rewrite their titles and proposal abstracts to make them intelligible and compelling for a lay audience. While Senator Proxmire’s “Golden Fleece Awards” have been discontinued, the fear that what to scholarly eyes was an excellent proposal might seem to others a senseless boondoggle remained.

7. I am indebted to Craig Calhoun for suggesting the phrase “nonce peers.”

8. Stuart Plattner, NSF program officer for cultural anthropology, confirmed this interpretation for me by noting that he and his colleagues select potential participants with an eye to their capacity for working well together, and that panelists who are other than amiable in meetings are rarely asked to return.