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Discourse and Discipline at the National Research Council: A Bureaucratic *Bildungsroman*

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In a sense, virtue has become normalized; the virtuous individual can delude himself or herself into believing that he or she acts out of a sense of duty while in reality simply making his or her behavior conform to a particular norm. [Ewald 1991:140]

François Ewald has recently illuminated some critical dimensions of Foucault's (1980) analysis of the increasing control of the body and the individual throughout the modern era. Ewald argues that a transformed notion of the "norm" lies at the heart of this extension of nonjuridical control; for him, "modernity [lies in] partaking of the logic of the norm" (1991:153). Once associated primarily with moral rectitude, the "essential reference [of the norm] . . . is to the average; the norm now refers to the play of oppositions between the normal and the abnormal or pathological" (Ewald 1991:140). An abstraction from the aggregate, the norm enables the comprehension of complex social and economic worlds. It shapes policy and practice, as well as individuals' understandings of their own identity and experience.

Ewald's analysis focuses on industrial standardization and on insurance and "risk management" as two institutional arenas within which the rise of the norm can be clearly documented. Central to industrial standardization, the development of a shared system of measures and models, is communicative normalization. This makes possible the "institution of the perfect common language of pure communication by an industrial society, a language of precision and certainty, a language without puns, stylistic features or interference" (1991:151), in short, a language of apparent pure reference. (Cf. Silverstein's 1987 consideration of the ideology of monoglot "standard" English in the contemporary United States.)

Ewald's consideration of insurance, risk management, and the consequent transformation of liability and law hinges upon the rise of statistics as interpre-

tive and predictive practice. The world is, for Ewald, increasingly comprehended through the numerical average rather than the morally exemplary; populations not only become known but are initially recognized through being numbered (see also Donzelot 1989). The accidental is mitigated through probabilistic reasoning. At the same time, such mitigation also increases our sense of the world as fortuitous rather than causally coherent (a theme also pursued in some detail by Hacking 1990). Within anthropology, Urla has documented a telling case of the role of statistics in Basque rhetoric and self-definition, arguing that a consideration of statistics figures crucially in two central questions in contemporary theory, the “historical construction of identities and the politics of representation” (1993:819).

At the heart of such studies lies a concern for how *groups* are defined, compared, and controlled. Urla’s work is significant as it points to the possible use of statistics for resistance, but its focus remains a contested population. Individual practice, particularly that of bureaucratic and other actors engaged in such normalization, is taken as relatively unproblematic or mentioned only in passing. For Ewald, for example, the “virtuous individual” is characterized only in terms of unreflective conformity; Urla’s more subtle consideration of Basque leaders’ intentions assumes overtly political motivations. My intention in this paper is to treat processes of normalization from a different starting point. Turning to the biographical construction of identity and the politics of personal experience as underlying concerns, I will provide a complementary account of “the norm” and its implications for individuals in a range of very specific institutional events. While my materials support the broad outlines of Ewald’s argument, they also reflect an empirically and ethically much more complicated situation than he suggests.

This article draws upon my current research project, a long-term, participant/observation-based ethnography of the grant evaluation process in several federal agencies. The grant proposal as genre and the reading practices of evaluators and funding panels represent a critical and directly consequential nexus. In such events, texts (as well as the conventions that shape them and the understandings of knowledge and truth to which they lead) can and must be considered together with those social practices through which they are produced, received, and given force.

My ethnographic materials derive from time spent evaluating proposals, both as an outside evaluator and as a member of various funding panels for the National Institutes of Health (NIH), National Science Foundation (NSF), National Endowment for the Humanities (NEH), and the National Research Council (NRC). The last panel, that for NRC, made recommendations regarding NSF graduate student fellowships; the others considered research proposals of various sorts.

Several features of the larger project should be noted. First, I think that participants in grant-review procedures, whether professional administrators or scholarly evaluators, are making intelligent, good-faith efforts to arrive at decisions in complex and difficult contexts. Indeed, much of the theoretical interest

of the larger study comes from the ongoing negotiation of notions of quality and equality. Central to this negotiation are democratic values of due process and equal treatment, and a stress on clarity and comparability which such values entail. In any case, I am not being cynical or muckraking here, which leads me to a second general point.

I am concerned not with those egregious cases of unethical behavior which have been extensively documented in the popular and scholarly press. I am rather specifically concerned with the routine—with how large numbers of proposals are designed, written, read, evaluated, and compared on an ongoing basis. Finally, in this paper I am particularly concerned with how one learns to do this, that is, with how one acquires, at least temporarily, a bureaucratic self, or at least becomes a good “nonce bureaucrat” and derives some satisfaction from it. When I presented some of these ideas in an earlier paper, Vincent Crapanzano characterized the performance as fitting the classic genre of “Protestant public confessional” (Crapanzano, personal communication, October 11, 1991). While there is indeed some generic resonance, my concern here is not with vindicating virtue but with exploring my own ambivalence about it. I am convinced that the fairly detailed consideration of evaluatory practices helps illumine our ongoing shared construction of anthropology as a discipline and of those assumptions which define and shape our subject.

Some Theoretical Issues

Before moving on to some aspects of proposals and panel reading practices, I want briefly to note three clusters of relevant theoretical issues somewhat schematically. When we are thinking about the constitution of “selves” through discourse, three heuristic elements seem useful. First is the question of how selves are “represented,” that is, how such discourses are “about” selves, shared or otherwise. A second and complementary strategy is to consider the media and styles through which the discourse is accomplished, a strategy more compatible with the linguists’ view of discourse without necessarily being as socially impoverished as they often make theirs. Finally, it is critical to consider the interaction and possibilities for coperformance that such topics invite and such stylistic features shape and sustain. As in earlier work I have done on gossip (Brenneis 1984), the pleasures and social effects lie in topic, form, *and* the sociability that the intersection of these make possible in particular kinds of events.

A second range of questions has to do with what is accomplished “politically,” in both overt and less immediately perceptible terms. Such effectiveness goes beyond “outcomes” in some specific sense. These evaluative events have direct and consequential results for applicants; we do, for example, clearly function as gatekeepers. At the same time, power is not solely a matter of decisions. As Fred Myers and I have pointed out in regard to political discourse in the Pacific (Myers and Brenneis 1984; Myers 1986) and as Helen Schwartzman has observed in her study of meetings in a large-scale American bureaucracy (1989), much “political” discourse works because it naturalizes relationships

even as it may reshape them. Here the seductive power of such semipublic discourse is particularly critical. While significant anthropological attention has been paid to processes of resistance, we have been less likely to look at moments of accommodation, complicity, and seduction—in effect, at those moments in which what we end up talking about as “political structure,” whatever the organization or society may be, is constituted. To play with usual meanings of the terms, both ends and means are consequential.

If the first theoretical cluster I mentioned has to do with what “public discourse” might analytically entail and the second with what political dimensions it might help us understand, the third has to do with how such discourse works at the level of experience, identification, understanding, and affect. Here, I think, lies a critical but usually neglected link, that is, processes of ongoing socialization; it is not just kid stuff. If we were to see early childhood training as the only context in which we acquired our “selves,” with the remainder of our lives being, at least implicitly, the working out of such primordially acquired qualities, we could indeed find ourselves in the pickle in which earlier work in psychological anthropology often left us. If, on the other hand, we take seriously the interactively based approaches put forth by Ochs (1988), Miller et al. (1990), and Schieffelin (1990), both the significance of ongoing, often shared socialization and ways of thinking fairly rigorously about it become issues with which to reckon. If seduction is an issue, as suggested above, learning has a lot to do with it.

Into the Panopticon: Application Reading at the NRC

In this section I want to outline review procedures for National Science Foundation Graduate Fellowships. NSF is the funding agency, but at the time of my participation, it contracted the processing of applications out to the National Research Council (NRC), a division of the National Academy of Sciences. The NRC panels involve very large numbers of applicants and panel members (510 folders and 20 reviewers for the panel I served on in 1991); they also follow a clearly organized and articulated set of reading and discussion procedures. While NRC practices are more overtly regimented than procedures in other agencies, they cast a strong light on characteristics common to all the panels, which I will discuss in the next section.

Once application materials have been received at NRC, a three-stage evaluation procedure ensues. First, in what I call the “pre-reading” phase, each applicant’s file is assembled, assigned to a specific disciplinary panel, and given a preliminary assessment. The “readings” proper take place over 2½ days in early February, when several hundred scientists and social scientists gather at the NRC offices in Georgetown. Finally, there is a “post-reading” phase in which many but not all award decisions are made.

Pre-reading

Applicants may be college seniors, recent graduates (both classified as level-1 applicants) or first-year graduate students (level 9). A completed appli-

cation folder includes a number of materials: the application itself, which includes two essays on future professional and scholarly plans; undergraduate transcripts; three reference reports; reports on analytical, quantitative, and verbal Graduate Record Examination scores, as well as those for any appropriate advanced subject tests; and other supplementary information. We receive a great deal of quantitative and qualitative information, materials through which applicants and others present accounts of both past development and future professional trajectories. In making those judgments of scientific promise central to the panels' purpose, we are clearly observing, evaluating, and comparing candidates in considerable detail.

Applications are assigned to specific disciplinary panels. Usually only one discipline is represented on a panel, for example, biology or computer science. In the three years in which I was involved, anthropology, archaeology, sociology, linguistics, and "social studies" constituted the Social Sciences B (SocSciB) panel. There were between 18 and 20 of us on the panel, our disciplinary representation roughly proportional to the distribution of applicants' fields. In 1991, which is the ethnographic present for my account, 510 applications were assigned to SocSciB.

Before the readings begin, the number of awards that each panel is likely to be able to make is also determined. Part of this total is fixed beforehand. SocSciB was allotted 35 slots for what is known as Quality Group I (QGI); this represented approximately 7 percent of our total applicants. Applicants in this group are assured NSF fellowships, barring hidden problems. We were also told that approximately 21 additional applicants to SocSciB would be funded (an additional 5 percent). These fellows would be chosen from a group of 42 nominees from our panel (QGII); they would be competing directly with nominees from all the other panels, with the final decision to be made by NSF.

Each year, NRC staff prepares a multiple linear regression for each disciplinary panel (National Research Council 1991a:2). This formula includes a range of quantifiable measures as the independent variables and average panel rating as the dependent variable. The measures include GRE scores (verbal, quantitative, analytical, and advanced), standardized undergraduate grade-point averages, average referee scores (drawn from boxes checked on the reference form), and an index of the quality of undergraduate school. The proxy for institutional quality is the average combined SAT score of entering freshmen from, usually, ten years earlier. More qualitative parts of the folder, including the two application essays and the texts of referees' letters, are not reflected in the derived score.

Each panel's formula reflects the weights given to these various measures over the preceding five years. For SocSciB, for example, the average referee score figured most heavily, followed by verbal GRE, quantitative GRE, and undergraduate GPA. Analytical GRE and undergraduate institution quality were relatively inconsequential for our panel. My own sense is that the weight given to referees' scores is indicative of the more general attention we paid to qualitative and, therefore, previously unscored parts of the applications.

These panel-specific formulas are then used to compute a derived score for each applicant. A scorer enters all the relevant quantitative information into a computer, runs the weighted equation, and prepares a list with the derived scores. The first year in which I served on the panel, these derived scores were provided in a column on the applicant rosters that we were all given. In subsequent years, the scores were available in the panel room but not listed on the rosters.¹

Readings

The readings proper begin with the introduction of the panel's assigned staff member (who is defined as being present to help us in our deliberations) and of the chair (a returning participant chosen by staff). The chair then describes the procedures to be followed by the panel. Usually about one-half of the panel has served previously, as members may be invited back for up to three years' service.

The rating system is then described. We are asked to rate applicants on a scale of 1 to 6, with 1 being the highest score. While we are told, for example, that a score between 1 and 1.99 *means* that a candidate is "truly exceptional or outstanding" (National Research Council 1991b:1), we are also advised that ideally only 5 percent of applicants will receive such a score. A certain tension between absolute and relative standards for quality is evident—and is often resisted by individual panelists. A calibration exercise then takes place. The chair and staff have chosen three folders, selected to represent higher-, lower-, and middling-quality applications. We each rate all three folders, discuss our scores, and are encouraged to work towards a high degree of interrater reliability.

For the remainder of the first day, panelists read folders. The day ends when the last application has received its first reading. Throughout the day staff members have been entering scores on the computer. That evening the computer averages the first panel reader's score with the derived score for each applicant. Those applications in the lower half of average scores are not read again. For these candidates, only one panelist has read the prose sections of the applications and references. The computer also provides feedback for panelists; the number of folders read by each panelist, the average scores that we gave, and the spread among our scores are all displayed on the board, in suitably but not unfathomably anonymous form, at the beginning of the second day's meeting.

The remaining 50 percent of the applications is given a second reading by different panel members on the second morning. During lunch a further 50 percent cut is made, this time reflecting the average of the two panelists' scores; the derived score drops out of the averaging. We also again are given the chance to monitor our own—and each other's—performance. That afternoon we do a third reading.

By the morning of the third day, another 50 percent cut has been made, this time reflecting the average scorings for three panelists. The computer also notes all cases where there is a difference of more than 1.5 points (on the 6-point scale) among panelists for any particular folder. We are told that the computer will not

accept as final any score with that degree of disagreement. NRC's concern here is not panelist amity. It derives from the underlying assumption that, as an objective phenomenon, applicant quality should be represented by more closely clustered scores. The outlying readers have to negotiate their differences and come up with a final score that can be accepted as sufficiently close to consensual. Only rarely are these negotiations lengthy, and even less frequently are they contentious. It is also critical to note that, while there has been a great deal of sociable talk around the table throughout the preceding days, only rarely have applicants been discussed. Only on the last morning and only when our aggregate evaluations are signaled as impermissible, do we talk directly about particular cases. The computer not only makes critical cuts in the applicant pool along the way and provides us with information for self-monitoring; it also tells us when to get down to brass tacks.²

Our final task is to make fine adjustments in the ranked list of candidates. While only members of Quality Groups I and II are eligible for awards, there are six quality groups altogether. Those candidates in QGII who do not receive awards and all in QGIII will receive honorable mentions. The 35 candidates with the highest average scores constitute QGI; they are guaranteed awards. Approximately half of the 42 applicants in QGII (the next cluster of averaged scores) will be Fellows, although the decision is not in our hands but in those of NSF. Here panelists' anticipation of those criteria that NSF will use in making these decisions becomes significant. Particularly for those of us on SocSciB, a concern that our QGII candidates stand up well vis-à-vis applicants in the sciences was critical. It led at times to slight adjustments at the lower end of the Quality Group II ranking to favor those applicants with somewhat better quantitative profiles. We anticipate our subsequent audience in determining the final membership of QGII. The final decisions made, we part amiably and return home.

Post-reading

The numerical results of our deliberations are forwarded to NSF for final checking and for the determination of which QGII members will receive awards. The NSF Graduate Fellowship Office has developed a sequential algorithm that makes the computer processing of these decisions possible. The computer first surveys all the QGI members to determine if each state is represented among the awardees. If, for example, no one who has graduated from high school in Alabama is in QGI and one is available in QGII, he or she will be chosen. Just as panelists tend to anticipate their subsequent audience at NSF, so also does the fellowship office at NSF take its own audience, and particularly the Congress, into consideration.

If the state search does not fill the available slots, a second pass is done for gender balance, followed by disciplinary balance and proportional equivalence between level-1 and level-9 applicants. Only at this point are GRE verbal and quantitative scores consequential; staff members have assured me that the awards have almost always been fully allocated before GREs come into play.

Some Common Features of Reading Panels

In this section I will briefly outline several characteristics common to the whole range of reading events with which I am familiar. The NRC procedures differ in some ways from research proposal reviews for more senior scholars, most markedly in terms of the explicit normalization practices in which NRC panelists take part. At the same time, the NRC case illustrates widely shared features.

One critical aspect is that the “event” does not begin when we gather in Washington, DC, or end when we leave. In all cases other than the NRC, proposals have been sent to the provinces, often by the boxload. Expectations as to how many proposals will be read by each reader, the ways in which readers will respond, and by what time, that is, before the meeting or by its start, vary. What is fairly invariant is that the number of proposals and the expected degree of attention and response to them always come close to exceeding readers’ capacities. Only infrequently have reviewers been able to attend to proposals with all the time and care they would prefer; more frequently we are working close to the limits of wit and energy. While graduate fellowship folders are read in Washington rather than beforehand, the same sense of too little time, too much to read, is evident.

Often, proposals will be read by outside, nonpanel reviewers as well. In any case, it is critical to see the Washington panels as only one stage in an ongoing set of conversations, one both influenced by earlier events and subject to subsequent appraisals by program officers, funding councils, NSF and NEH officials, and Congress.

This latter factor leads to a first aspect of discourse in such panels. It is *conditionally definitive* discourse, that is, apparently decisive but also rarely the final word. The certainty of a subsequent and powerful audience has a real effect on one’s conduct in the panel: What do you think will fly with later readers? What might not? How much of your panel-member credit will you expend on the unlikely candidate, however intriguing? And how many times can you go out on a limb? While expectations, which at times are actually quite off-base but nonetheless firmly held, might vary dramatically among agencies, the fact of later evaluation does not. My sense is that this conditional authoritativeness is true of a very wide range of bureaucratic discourses.

A second feature, one that I think is also characteristic of much bureaucratic discourse, is that such reading events occur at the intersection of written text and talk. This is an important point. The intersection of text and talk is within an only occasionally face-to-face community, and the nexus itself helps constitute that community. Evaluative events provide a particularly telling way of understanding *how* literacy is socially mobilized and mobilizing.

The talk around panel tables is of a particular type, and its sequence, tone, and style are ordered in particular ways. In part this reflects the usually overwhelming number of proposals, a critical contextual fact. At a less frequently articulated level, I think it also derives from a concern for fairness and something like due process in treating the proposals, a concern that places such textual as-

pects of the proposals themselves as clarity and comparability at a premium, a point I have addressed elsewhere (Brenneis 1988).

Several further characteristics of panel discourse are worth mentioning very briefly. First, much of what we are asked to evaluate is cast in a quantitative idiom, although not generally for NEH. I think that this in part reflects once central understandings of what constitutes “science.” More significantly, if because more obscurely, I think this quantitative tone also reflects the search for a common language for appraisal and comparative ranking. A critical concern is that proposals and applications be read and evaluated “fairly,” that is, that panel readers can understand each document in terms comparable to those in which they understand the others. Michael Herzfeld has written recently of bureaucracies in terms of the “social production of indifference” (1992). Central to these evaluation panels is the social production of *impartiality*. The highly innovative project or, more particularly, the proposal that changes the terms of research formulation and design, is often, even if engaging and provocative, difficult to fit into such a common comparative language. Here lies one of the costs of what I think of as the “fairness-through-apparent-clarity” model of proposal review. This *apparent clarity* approach is inherently biased toward an understanding of scholarly progress as incremental and therefore often leads to the favoring of more or less “normal” social-science features that are clearly linked to a sense of how “science” works (cf. Markos 1987; Traweek 1988; Zabusky 1992). Proposals that promise to break new conceptual ground or to challenge and refigure dominant paradigms are viewed not so much as “bad” proposals but as difficult to evaluate and compare with other contenders.

Second, the professional program officer (or short-term rotating scholar) who coordinates the meeting plays several significant roles. Agenda setting is particularly consequential. The program officer is central in selecting outside evaluators and panelists, in setting the order in which proposals will be considered, in sequencing the oral reports in panel sessions and choosing who will report at all, and, most important, in developing what I think of as the *standardized interlocutor* with which we all interact as well as with each other. What I mean to suggest by the somewhat ungainly phrase *standard interlocutor* is the fact that, in each panel, some means of monitoring and calibrating one’s conduct is provided. In some cases, as with the NRC panels, our initial conversational partner is a computer-generated score for each applicant, the derived score. At NEH panel meetings, where each panelist reads every file, we read our numerical scores for each proposal out loud. A score sheet is provided on which we list all the scores, and the totals for each proposal determine the order in which they will be discussed. The score sheet further allows us to anticipate, think about, and revise our own evaluations before getting to the discussion of particular cases. Finally, on the NSF panel on which I have served for three years, the standardized interlocutor is less clearcut, in large part because it is the province of the program officer, who alone is privy to the rankings that we have provided and whose intentions we often try to understand and anticipate.

These standardized interlocutors also provide personal feedback to panelists and program officers. Such feedback makes self-monitoring not only possible but almost inescapable. We are often given the numbers by which to judge our own performances, as well as those of applicants, and to shape our ongoing participation. Such feedback contributes to a further recurrent feature of these reading events, that is, to the strong, shared commitment to sociability and amiable disagreement, if there is to be any disagreement at all. Few participants are comfortable as consistent outliers, those who are repeatedly more positive or negative than the middling evaluation. A set of expectations as to socially feasible disagreement, as to framing such disagreement in Batesonian terms as nips rather than bites, and as to negotiating differences in, frequently, a more choreographic than substantive manner are central to panel dynamics, as well as to the criteria by which program officers select those panelists who will be invited to serve again.

Finally, it is worth pointing out again that our decisions are recommendations—to the program officer, to a further scholarly committee, or to higher-level administrators. The anticipation of subsequent audiences shapes our discourse and our decisions.

Standardization, Socialization, and Seduction

I want to note briefly three broader theoretical implications of this ethnography. The first has to do with the practices of self-monitoring and discipline so clearly evident in our panel meetings. I want to return to Ewald's notion of the norm as a "principle of communication, a highly specific way of reducing the problems of intersubjectivity" (1991:154). Clearly, participation in such panel reading events plunges one into such a normalization process—or "norming," as it is sometimes referred to in in-house dialect. We participate through self-discipline, not through force or even, that frequently, through a conventional sense of self-interest. We acquire a new language in different ways: by attempting to standardize those criteria by which we evaluate proposals; by participating in a fiction of objectifiability engendered, at least in part, by the negotiation of what constitutes "information;" and by the adoption of fungible categories of discussion and comparison. Presumed subsequent audiences also shape the normalization process; we are often concerned both that our recommendations and the proposals they concern be represented in translatable language and that they do well in such translation.

While the language of panel discussions is seen as transparently referential, in reality our goals as readers are often other than referential. We value some proposals and approaches more highly than others and seek rhetorically, within the somewhat odd constraints of the discourse, to help effect particular outcomes. The process, however, often washes out more programmatic and innovative proposals, as does the sheer volume of materials that require decisions. At the same time, it is necessary to remember that panel reading events are situations, not life courses. One enacts what could be called *situational participation* rather than necessarily being irreversibly transformed into an agent of the

state. Instead of becoming a bureaucrat, one learns, with striking rapidity, to do bureaucratic things.

The structured character of and expectations for our participation in these events leads easily to my second point, the fact that we are encouraged to train ourselves for complicity in the process. Here the perspective on socialization I mentioned above is of signal importance. Following an interactive Vygotskian perspective, Miller, Ochs, Schieffelin, and others working on language socialization have noted that, first, one learns not only new things but new discourses and, second, that one does not become a preprogrammed, replicative product of what one is taught. In her Kaluli study, Schieffelin argues that "one acquires a set of practices that enable one to live in a culture" (1990:15). Critical here, for me at least, is the notion of repertoire. In the Kaluli case, children learn not so much "culture" as they do a range of ways of operating and negotiating, locating themselves within interaction and orienting themselves emotionally as well as cognitively. Similarly, I would see my own situation as that of becoming a "nonce bureaucrat" or, more clearly, of being socialized through negotiation with both standardized and more human interlocutors in how to carry out certain types of social practice. It has become part of my repertoire, but I hope that it has not become a new "self" in the usual psychological sense. I have acquired dialogically some new discourses but have not, I trust, become totally a "bureaucrat." This practice-and-repertoire model implicit in socialization makes any notion of an invariant, wholly individualized self somewhat problematic.

Finally, what drives our socialization? Partly it is the fact that we develop both cognitive and experiential capacities, a point suggested by Ochs's (1988) and Schieffelin's (1990) stress on the relationship of affect to learning language and learning culture. Here is a brief, mildly reflexive anecdote: on the plane home from Washington after the first of my NRC sessions, I had the nearly Durkheimian sensation of having really "done" social science. It was not quite effervescence perhaps, but both the interactions of the panel and the subsequent rememberings occasioned considerable social pleasure, pleasure of a type defined in terms of professional self-definition and satisfaction. Similarly, the scientific bureaucrats with whom Zabusky worked spoke to her of something akin to the Durkheimian "moral cooperation" inherent in the complex division of labor as, "in large measure, what motivates them to keep doing the work, despite the complexities, compromises and frustrations which are *also* part of the work" (Zabusky, personal communication, December 21, 1992).

The roots of such experiences undoubtedly lie in part, as Craig Calhoun has suggested to me (Calhoun, personal communication, December 3, 1992), in the fact that we become "nonce peers" as well as nonce bureaucrats. Participation in such decision making makes one, for the moment at least, an "equal." In peer review we jointly constitute an ephemeral peership, among ourselves as reviewers as well as vis-à-vis those whom we are evaluating.

While critical, this achievement of social parity does not provide a comprehensive explanation, nor does Ewald's terse claim of unquestioning conformity. We are often seduced into acquiescence or active complicity with these normal-

izing practices, procedures that we may well not countenance in our own scholarly work. The rewards for such seduction certainly include status, however transitory, as well as some degree of power. The relationships between these social achievements and the experience of satisfaction, however, are linked in quite complex ways. The enjoyment of technique, a sense not so much of responsibility as of successfully negotiating complex exchange relationships, and the shared production of social pleasure also need to be reckoned with.

Notes

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1. A very interesting debate on the relative predictive value of the derived score and of panel evaluation took place on the 1990 NRC panel for psychology. Based on that debate, McCauley argues, following literature demonstrating the more consistent predictive value of actuarial rather than clinical judgment, that panelists should focus on developing ways of quantifying nonnumerical parts of the application, thereby refining rather than trying to second-guess the more successful derived score (1991). Neuringer, the chair of the panel that year, provides an interesting counterargument (1993). Both pieces also provide helpful accounts of the terms and implications of panel practices.

2. In the first of my three years on the NRC panel, I discovered to my surprise that I was not required to negotiate with anyone during the final morning's score adjustments. I had so successfully normalized my own evaluations that I was the norm-setting reviewer for all of the proposals I read. Particularly given my sense of my own somewhat idiosyncratic approach to the field, I found this fairly troubling, in part as I was initially rather proud of myself. In subsequent years I worked quite consciously to resist being quite so normal (in Ewald's sense).

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