

# Fairness as Appropriateness

## Negotiating Epistemological Differences in Peer Review

Grégoire Mallard  
*Northwestern University*

Michèle Lamont  
*Harvard University*

Joshua Guetzkow  
*University of Arizona*

Epistemological differences fuel continuous and frequently divisive debates in the social sciences and the humanities. Sociologists have yet to consider how such differences affect peer evaluation. The empirical literature has studied distributive fairness, but neglected how epistemological differences affect perception of fairness in decision making. The normative literature suggests that evaluators should overcome their epistemological differences by “translating” their preferred standards into general criteria of evaluation. However, little is known about how procedural fairness actually operates. Drawing on eighty-one interviews with panelists serving on five multidisciplinary fellowship competitions in the social sciences and the humanities, we show that (1) Evaluators generally draw on four epistemological styles to make arguments in favor of and against proposals. These are the constructivist, comprehensive, positivist, and utilitarian styles; and (2) Peer reviewers define a fair decision-making process as one in which panelists engage in “cognitive contextualization,” that is, use epistemological styles most appropriate to the field or discipline of the proposal under review.

**Keywords:** *peer review; fairness; pluralism; ethics; epistemology; interdisciplinary*

Theoretical and methodological approaches are objects of continuous and frequently divisive debates in the social sciences and the humanities (Bourdieu, Chamboredon, and Passeron 1968; Merton 1972; Somers 1996; Steinmetz 2005). When textbooks and specialized publications refer

to differences in theoretical and methodological approaches, they often frame them as irreconcilable epistemological styles, stressing incompatible elements. By “epistemological styles,” we refer to scholars’ preferences for particular theoretical styles (ways of understanding how to build theories and how to accumulate knowledge) and methodological styles (methods of proving, and belief in the very possibility of proving, theories [Knorr-Cetina 1999]).<sup>1</sup>

The diversity of theoretical styles ranges from the view that authors should acknowledge how the formulation of their theoretical orientation is shaped by their own social location, identity, and political orientation (Smith 1990; DeVault 1999) to the view that theories emerge from the observation of new evidence in light of existing explanations, without being affected by who the researcher is or how she apprehends her object (Nagel 1961). Regarding methodological styles, authors such as Nagel (1961), Ragin (1987), Singleton and Straits (1999), Stinchcombe (2005), and Tilly (1994) have developed compatible views on the procedures best suited for hypothesis testing and on the privileged role of formal models for proving theories, while other authors, such as Clifford and Marcus (1986), vehemently reject such approaches in favor of a contextual methodological approach. Clifford and Marcus (1986) even criticize the very idea that social scientific methods can prove or disprove theories. In their quest for a monopoly on truth or science, scholars often use a

---

**Authors’ Note:** An earlier version of this article was presented at the annual meetings of the American Sociological Association, Chicago, August 2002. Grégoire Mallard acknowledges the support of a graduate research fellowship from the Lurcy Foundation, which funded his work at Princeton University in 2002-2003. Michèle Lamont acknowledges a generous grant from the National Science Foundation (grant no. SES-0096880), which made this research possible, as well as a fellowship from the Radcliffe Institute for Advanced Studies and the Center for Advanced Study in the Behavioral Sciences, with the support of the Andrew W. Mellon Foundation (grant no. 29800639). Joshua Guetzkow acknowledges the support of the Robert Wood Johnson Foundation Scholars in Health Policy Research Program. We thank the following organizations for authorizing access to their funding panels: the American Council of Learned Societies, the Social Science Research Council, the Woodrow Wilson National Fellowship Foundation, and two anonymous fellowship competitions. We are particularly indebted to Craig Calhoun, the late John D’Arms, Judith Pynch, Stanley Katz, Robert Weisbuch, and all the participants of this study for making it possible. We thank John Bowen, Don Brenneis, Nina Eliasoph, David Frank, Howard Gardner, Patricia Gumport, Stanley Hegginbotham, Warren Ilchman, Eléonore Lépinard, Paul Lichterman, Alexandra Kalev, Doug McAdam, John Meyer, Christine Musselin, Catherine Paradeise, and Mitchell Stevens for their helpful comments. We thank Kathleen Much and Lynn Gale. Please address correspondence to Michèle Lamont, Department of Sociology, Harvard University, William James Hall, 33 Kirkland St., Cambridge, MA 02138; e-mail: mlamont@wjh.harvard.edu.

polarizing style of argument that suggests that there is only one correct approach (Merton 1972; Weber 1913; Bourdieu 1988; Abbott 2001).

Despite such differences, decisions concerning the allocation of funds for research, the publication of articles, and the granting of awards are routinely made by funding agencies, editorial boards, and professional associations in the social sciences and the humanities. Moreover, decisions that are perceived as “fair” are made notwithstanding differences of opinion about epistemological matters. This article draws on eighty-one interviews conducted with panelists serving on twelve funding panels attached to five multidisciplinary fellowship competitions in the humanities and the social sciences. Almost without exception, the panelists we interviewed believe that the deliberations in which they participated were fair and that their panel was able to identify the best proposals. However, if epistemological styles divide scholars, under what conditions do they believe they are able to identify the best proposals and make fair decisions?

Studies of peer review and evaluation in academia have largely eschewed the challenges that epistemological differences pose to “fair” collective decision making, focusing instead on *distributive fairness*, defined as fairness in the distribution of awards. They adopt an a priori normative definition of fairness as conformity to the norm of universalism, which if properly applied ensures that allocations are proportional to the intellectual deservingness of recipients rather than to their group identity or status or their particularistic relationship with the panelists (Cole 1992; Merton 1942). As such, research on peer review focuses on the relationship between the distribution of awards and the social characteristics (e.g., age, race, gender, prestige) of the individuals whose work is evaluated. By showing that particularistic criteria do not affect distributional outcomes, researchers have provided evidence that peer reviewers typically adopt the norm of universalism (Zuckerman and Merton 1971; Cole 1978; Cole, Rubin, and Cole 1978; Cole and Cole 1981; G.A.O. 1994).

This literature has yet to systematically consider *procedural fairness*, which “is concerned with procedures used to arrive at those outcomes” (Beersma and De Breu 2003, 220). Where procedural fairness is concerned, a few recent studies have speculated that fair decisions are made by applying to a wide range of proposals the same set of general criteria agreed upon by evaluators during the decision-making process (Callon 1994; Callon, Lascoumes, and Barthe 2001; Collins and Evans 2002; Pillutla and Murnighan 2003, 242). This understanding of procedural fairness requires “that panel readers can understand each document in terms comparable to those in which they understand the others” (Brenneis 1994, 31).

Our empirical study challenges this view of procedural fairness and suggests another approach to producing fair evaluation that has not been documented by the literature on peer review. The panelists we interviewed linked fair evaluation less to the application of *general* criteria than to the application of *appropriate* criteria, that is, to “cognitive contextualization,” defined as the application of the epistemological styles most relevant to the field or discipline from which a research proposal emanates. Rather than demonstrating the broad applicability of their favorite epistemological style, evaluators describe themselves as making fair judgments when they use standards that fit best with the discipline or field of the proposal.

The article proceeds as follows. The first part locates our argument within the literature on decision making in peer review and spells out the nature of our theoretical and empirical contribution. The second part presents our data and methods. The third part extends studies of peer review by describing the content of the epistemological styles found in evaluation: the “constructivist,” “comprehensive,” “utilitarian,” and “positivist” styles. It also documents the epistemological diversity involved in evaluation, that is, the distributions of these styles across disciplinary clusters and competitions. The fourth part shows that panelists stress cognitive contextualization in their account of how to reach fair decisions. It also buttresses our argument about the importance of cognitive contextualization by analyzing three exceptional moments in the collective deliberations when panelists breached this rule.

## Theories of Procedural Fairness

In this section, we describe how the literature on peer evaluation addresses the issue of fairness by distinguishing theories and findings that concern two analytically distinct forms of fairness: distributive and procedural fairness.

The existing literature on peer review has focused almost exclusively on *distributive fairness*, that is, the values and norms that ensure that the allocation of rewards is based on the deservingness of recipients rather than on particularistic consideration pertaining to personal characteristics or subjective factors (Zuckerman and Merton 1971; Cole 1978; Cole, Rubin, and Cole 1978; Cole and Cole 1981; Liebert 1982; Roy 1985; Bakanic, McPhail, and Simon 1987; Chubin and Hackett 1990; G.A.O. 1994; Armstrong 1997). The respect of the norm of universalism (defined in opposition to particularism), along with those of disinterestedness,

communalism of results, and organized skepticism, ensures the legitimacy of science as an institution (Merton 1942). The empirical literature has found that reviewers follow universalistic norms more often than not, and hence that evaluations are fair in terms of outcomes (Zuckerman and Merton 1971; Cole 1978; Cole, Rubin, and Cole 1978; Cole and Cole 1981; G.A.O. 1994).

In contrast, the literature on *procedural fairness* in peer review should be “concerned with procedures used to arrive at [fair] outcomes” (Beersma and De Breu 2003, 220). Authors observe that “distributive and procedural fairness may be positively correlated, but they do not need to be” (Beersma and De Breu 2003, 220). Noting that evaluators focus on the intellectual merits of proposals or articles provides little leverage for analyzing procedural fairness when conflicting criteria are used to define intellectual merit, as is generally the case in the social sciences and humanities.

To date, experts on peer review have developed only limited operational measures of procedural fairness, for example, indices of degree of consensus within panels, based on ratings of proposals by reviewers (Cole, Cole, and Simon 1981) or rejection rates (Hargens 1987). Although they tell us that consensus among reviewers is the exception rather than the norm, they also have yet to explore precisely what reviewers disagree about when they disagree and how they reconcile their differences (Zuckerman and Merton 1971; Cole 1978; Cole, Rubin, and Cole 1978; Cole, Cole, and Simon 1981; Hargens 1987);<sup>2</sup> Authors can only “speculate that the great bulk of reviewer disagreement observed is probably a result of real and legitimate differences of opinion among experts about what good science is or should be” (Cole, Cole, and Simon 1981, 885).

A few recent studies have considered procedural fairness and more specifically how evaluators come to an intersubjective understanding of their evaluation as fair (Callon 1994; Callon, Lascoumes, and Barthe 2001; Collins and Evans 2002). However, these authors speculate about (rather than demonstrate) the normative conditions under which fairness is produced; they discuss the ability of panelists to make their idiosyncratic standards general by demonstrating their broad applicability to a wide range of objects (called a “*montée* (or escalation) *en généralité*”). This is accomplished through an intersubjective process of translation, defined as the “special ability to take on the style of the ‘other,’ to alternate between different social worlds and translate between them” (Brenneis 1994, 31; Callon 1994; Collins and Evans 2002, 262). For these authors, singular standards must be generalizable for evaluation to be fair, meaning that panelists have to apply the same criteria to a broad class of objects for procedural fairness to ensue. This pathway to the production of fairness corresponds to the

normative framework that undergirds recent writings on “cosmopolitan ethics” (e.g., Hollinger 1995), where fairness and justice are understood as resulting from a capacity to value diversity and to creatively build pluralistic “we-perspectives”—this normative stance extends that of liberal political philosophers who associate fairness with the ability to uphold general standards that transcend incommensurate individual or group-based preferences (Rawls 1985, 1995; Solum 1989; Habermas 1990, 1995).

An alternative pathway to the production of procedural fairness, which was revealed by our case study, is the adherence to the rule of “cognitive contextualization.” Reviewers evaluate fairly when they use standards that are most appropriate to the object of evaluation. Rather than applying a single universal criterion indiscriminately, they specify which criteria, or lenses, are most appropriate to assess the strengths and weaknesses of the object under evaluation. This requires locating the object, which in this case are grant proposals, within specific fields of expertise, including within the intellectual conventions and epistemological styles that prevail within these fields. The fairest criteria are thus not the most widely shared criteria but the standards deemed most appropriate given the distinctive features of a particular object. This pathway to fairness ensures that the hierarchies between and within different disciplines and research fields do not lead to the epistemological dominance of specific epistemological styles (Bourdieu 1988). The contextualization of evaluation lessens potential tensions between panelists as it privileges respect for difference over specific disciplinary (or methodological) hegemonies, contextual fitness over universal validity, and appropriateness over consistency. This pathway to the production of fairness has not been identified nor discussed in the literature on peer review. However, it is compatible with a pluralist vision of fairness, also developed by political philosophers—a “pluralist vision of the world [understood] as an expanse of private exclusive clubs, interacting with as much civility as they could, but each defined, animated, and sustained by a vivid sense of the difference between ‘we’ and ‘they’” (Hollinger 1995, 67). One of the contributions made by this article is to identify and describe cognitive contextualization as a privileged pathway to procedural fairness in peer review.

## Data and Methods

We examine academic evaluation in the specific context of multidisciplinary panels that distribute fellowships to graduate students and faculty

members in the social sciences and the humanities. This setting presents some distinctive characteristics as compared with the peer review of journal submissions or the departmental evaluation of faculty members for promotion. Most importantly, it brings together scholars who come from different disciplinary horizons, and who therefore often have to make explicit their theoretical and methodological preferences, given the low level of disciplinary knowledge they can count on sharing with other evaluators.

We conducted interviews with panelists serving on five different multidisciplinary fellowship panels and twelve funding panels in the social sciences and the humanities. We studied each panel in two successive years. The funding competitions were held by the Social Science Research Council (SSRC), the American Council of Learned Societies (ACLS), the Woodrow Wilson National Fellowship Foundation (WWNFF), a Society of Fellows at a top research university, and an anonymous foundation in the social sciences.<sup>3</sup> These competitions were chosen because they cover a wide range of disciplines and because they are all highly prestigious. Although the SSRC and the WWNFF competitions are open to the social sciences and the humanities, the ACLS supports research in the humanities and in humanities-related social sciences. The Society of Fellows supports work across a range of fields, whereas the anonymous foundation only supports work in the social sciences. The SSRC and the WWNFF programs provide support for graduate students, whereas the ACLS holds distinct competitions for assistant, associate, and full professors. The Society of Fellows provides fellowships to recent PhD's, and the anonymous social science foundation supports research at all ranks.

A total of eighty-one interviews with panel members in charge of final deliberations were conducted.<sup>4</sup> This total includes sixty-six interviews with forty-nine different panel members (nineteen panelists were interviewed twice, because they served on panels for the two years that the study lasted). Interviews lasted approximately ninety minutes and were conducted as soon as possible after the conclusion of panel deliberations, typically within a few days or at most within a few weeks. Interviews were conducted over the phone or, where possible, in person. Fifteen additional interviews were conducted with relevant program officers and panel chairpersons for each panel, who provided details about what had happened during the panel deliberation.<sup>5</sup> Program officers are not included in our analysis of epistemological styles. The five interviews we conducted with three different panel chairs are included, because they also served as peer reviewers and were asked about their criteria of evaluation.

Drawing on interview data was the best research strategy available given that issues of confidentiality create enormous hurdles to accessing peer review panels, a reality which has resulted in a paucity of research on peer review and in a focus on final rankings, as opposed to the meanings given to criteria of evaluation.<sup>6</sup> We were able to observe three panels and drew on field notes in the interviews we conducted with their members. We approach panelists as informants who have intimate knowledge of the process of deliberation. We use their description to reconstruct this process. We also consider the epistemological styles they mobilized in the context of the interviews, and those they attributed to other panelists a posteriori, to express the repertoires of epistemological styles mobilized by respondents. We are confident that the responses of our panelists are an adequate basis to understand a great many, if not all, of the conditions that make it possible to believe that the process is fair. We do not give primacy to observations over interviews because, just as is the case for interviews, ethnographic descriptions are reconstructed representations (Latour and Wolgar 1979).<sup>7</sup>

A first battery of questions concerned how panelists evaluated and ranked proposals prior to and during the meetings (i.e., both in isolation and in interaction). Respondents were asked to describe what they appreciated in the proposals they judged to be the best and the worst prior to the deliberations.<sup>8</sup> They were also asked to describe the process by which proposals that had a high ranking prior to deliberation ended up not being funded and how some low-ranked proposals were funded.<sup>9</sup> The general strategy consisted of asking panelists to specify their own criteria of evaluation by producing “boundary work,” that is, by contrasting their evaluative standards with those of others (Lamont and Molnár 2002); panelists were asked to describe how they perceived themselves to be similar to or different from other panelists.

Interviews also concerned how decisions were reached, what factors facilitated the production of consensus and fostered fairness, and what factors led to the occasional breakdown of deliberations. Asking panelists to describe exchanges surrounding especially controversial proposals was particularly fruitful in revealing the diversity of arguments used by panelists, what styles they privileged and considered most appealing to others, and how they believed preferences for specific styles should be expressed. We also asked panel chairs and program officers to comment on debates surrounding controversial proposals to learn their understanding of how fair outcomes are produced. We focused on the only three conflictual cases that involved, respectively, ten panelists and the panel officer in competition 2, 14 panelists in year 1 of competition 4, and 11 panelists in year 2 of the

same competition. We build a typology (see next section) inductively based on responses of the seventy-one panelists interviewed.

To analyze the interview data, we developed inductively and through an iterative process a comprehensive yet parsimonious list, or typology, of the epistemological styles used by panelists. The units of analysis are specific statements panelists made regarding the use of theory and the choice of methods. These statements were typically elicited when respondents were asked to describe what they appreciated or disliked about proposals or panelists; but, they also came up in other contexts in the interview, such as during descriptions of controversial decisions. These descriptions therefore show the explicit epistemological criteria that are most valued by panelists when judging proposals. Each statement about theory and method was coded according to one of the four epistemological styles that emerged inductively from our analysis of the interviews. For instance, we coded as “constructivist” the epistemological style of a panelist who admitted that she disliked a particularly positivist and quantitative proposal because of her own preference for the reflexive attention to the positionality of the author and for the qualitative and fine-grained analysis of social processes. In the next section, we present ample illustrations of each style in different disciplines and give many examples of the diversity of substantive contents captured by each style. Once the coding scheme was finalized, we content-analyzed the interviews with the assistance of Atlas.ti (Kelle, Prein, and Beird 1995). This software package increases intercoder reliability by standardizing the set of codes, tracking the codes assigned by each coder, and allowing each transcript to be coded by one coder and then checked by another.

To establish the degree of diversity in epistemological styles within each competition and across disciplines, we produced frequencies of panelists’ mentions of each epistemological style and aggregated the results for disciplines and competitions. Panelists who mentioned a given epistemological style at least once during the course of the interviews were given a code of “1”; otherwise they received a code of “0.” Hence, the analysis takes into consideration the use of multiple codes by panelists. For instance, a panelist can be coded as using a comprehensive style to assess one proposal and a positivist style to assess another. However, the analysis does not take into account the number of times a panelist mobilized each style (which could be a measure of the strength of the adherence to that style), because interviews did not cover all the proposals discussed because of time constraints.

To aggregate these frequencies by discipline, and because of the relatively small number of panelists (forty-nine) and the relatively large number of disciplines under consideration (eleven), we created two

**Table 1**  
**Most Important Epistemological Styles Found in Interviews**

Epistemological Styles	Positive Evaluation	
	Theoretical Style	Methodological Style
Constructivist	When the proposal presents personal, political, and social elements as relevant to research	When the proposal shows attention to details and to the complexity of the empirical object
Comprehensive	When the proposal emphasizes a substantially informed rationale for research and theoretically informed agenda	When the proposal shows attention to details and to the complexity of the empirical object
Positivist	When the proposal aims to generalize empirical findings, disprove theories, and solve a theoretical puzzle	When the proposal seeks to test alternative hypotheses using a formal model enclosing the world in a defined set of variables
Utilitarian	When the proposal seeks to generalize findings, disprove theories, and solve puzzle related to “real-world” problems	When the proposal seeks to test alternative hypotheses using a formal model enclosing the world in a defined set of variables

disciplinary clusters to analyze the distribution of epistemological styles across disciplines.<sup>10</sup> Each cluster includes at least fourteen panelists. The first cluster, the humanities, includes art history, English, musicology, and philosophy. The second cluster, the social sciences, includes anthropology, economics, geography, political science, and sociology. This cluster also includes the one natural scientist among our interviewees (described as “a scientist” to protect his anonymity). We consider the discipline of history separately from these two clusters because historians alternatively describe themselves and are described by others as belonging to each of the two clusters.

### **The Content of Epistemological Styles**

We identified four types of epistemological styles that panelists used in evaluating proposals: the “constructivist,” “comprehensive,” “positivist,” and “utilitarian” styles.<sup>11</sup> Table 1 presents the main styles that panelists

referred to in the course of the interviews, which can be delineated and contrasted according to the elements they consider to be positive and negative.

These styles vary with respect to what the panelists view as valid ways to address theory and what they regard as valid methodological approaches. Regarding how panelists understand the role of personal (and social) characteristics of the researcher in the process of theory making, differences primarily concern whether panelists appreciate the use of personal, political, or social rationales in the justification of a topic and/or the presentation of expected findings or, alternatively, the use of purely intellectual or scientific motivations for a project that aims to advance theory. Regarding methodology, the differences concern appreciation for approaches that either prioritize respect for the complexity of the empirical object and the singularity of cases or, alternatively, break down objects into simple and commensurable elements with the goal of maximizing the generalizability of research results. The opposition between “reductionist” or “antireductionist” (McCartney 1970; Latour 1993) parallels the opposition between idiographic approaches and nomothetic approaches leading to the identification of generalizable propositions. Even though some of these styles echo specific positions found in debates opposing methodological and theoretical perspectives, this typology does not reflect the totality and nuances of all possible epistemological styles found in the social science literature (Abbott 2004). It is indeed derived from the main lines of opposition that appear in the interview material. Below, we describe the four styles in some detail, drawing on the interviews with panelists.

### **The Constructivist Style—Reflexivity and “Giving Voice”**

Respondents who express a constructivist style favor extraintellectual rationales for the presentation of findings and antireductionist claims. In particular, they appreciate applicants who refer to their identity in explaining their choice of perspective or who adopt a reflexive stance concerning their relationship with their object. Reflexivity is conceived as a necessary part of the construction of the object in a way that results in better scholarship. This reflexive stance is often concomitant with an antireductionist style in the choice of methodological approach; recognizing that the researcher “constructs” her object and that her own identity shapes how she apprehends is antithetical to simplifying and decontextualizing it for the sake of producing generalizable knowledge. This is illustrated by an English scholar who suggests that personal reflexivity is an important part of scholarship. Discussing an applicant, he says

There is something that I liked in her work: she includes in her discussions her very personal responses to [sculptures], but it always turns out that her personal responses open up something about the art work that seems important to me.

Hence, discussing feminist scholarship as “consciousness-raising,” an English scholar links her personal commitment to the study of women with paying attention to the contextual singularity and complexity of women as an empirical object. Tying her own intellectual trajectory to the evaluation of proposals, she says

[By studying women’s issues] I had my consciousness raised, which is a phrase we used to use all the time. Throughout all of my teaching and all of my research, Women’s Studies have been a key component, and taking into account one’s circumstance and situation, literally, what it means to be a woman in a particular place and time [was vital]. You know, *you can’t just say “a woman,” you’ve got to think about what kind of woman, what class are you of, all of that stuff* [our italics] . . . . At one point I said, this [particular] proposal is very much about women, but doesn’t seem very concerned with them, and I noticed [two other panelists] were like, “Yes!” [Being “concerned” means] that you’re really thinking about what it means, whether we’re talking about a woman writer, however you’re thinking about women as subjects or objects of study, that you’re really thinking about their positions, or their positions as women.

This scholar favors paying attention to the particular as a methodological principle, just as she values consciousness-raising for the researcher. This reflexive attention to the personal situation of the knowing subject is part of the construction of the object of knowledge.<sup>12</sup>

Working in development studies, another panelist, a geographer, links her concern with contextual distinctiveness to her personal intellectual commitment to promoting cultural diversity. Discussing a proposal on Africa, she says

Oh, I love [. . .] thinking about issues of the subaltern, the disadvantaged, and sort of trying to be a medium of communication in their situation and plight, and to also work with concepts of indigenous knowledge [. . .] and understanding life from their point of view. I’m very much politically committed to diversity of lived experience on this planet. There is so much racism attached, and barbarism attached, to conceptions of Africa that I really feel that looking at Africa is a way of looking at ourselves, as well as at the history of the West in the last 500 years.

Attention to specificity and complexity (the use of “indigenous concepts”) will help the researcher improve self-reflexivity (“a way of looking at ourselves”). Being a “medium of communication” is made possible by the interaction between knowing subjects and known objects. In contrast, adherents of other theoretical and methodological styles often deride this idea as self-indulgence, conformity to intellectual trends, and narcissism (with the researcher being more concerned with his relation with his object than with the object itself).

### **The Comprehensive Style—*Verstehen* and Attention to Detail**

Unlike the constructivist style, the comprehensive style dismisses references to the identity or personal, social, or political motives of the researcher in the choice of topic and promotes the use of more exclusively intellectual rationales, while praising an antireductionist methodological approach. Hence, we coded statements made by panelists as expressing the comprehensive style when they coupled intellectual rationales for the choice of perspective with attention to the complexity of the object of study. Borrowing from Weber (1905, 1911), we call this epistemological stance “comprehensive,” because it resembles closely a style that he used in his writings promoting *Verstehen* as well as in his work on value neutrality.

An English scholar also ties attention to complexity with mastery (as opposed to reflexivity), when he praises an applicant’s “literary skills” defined as “an attention to complexity, ambiguity, mixed signals, self-contradiction, which aren’t ordinarily in the interest of scientists; noticing things really closely and picking up details, and then speculating about what’s going on in those details.” In addition, proponents of the comprehensive style in history and in the social sciences give low priority to the production of law-like statements that reduce the complexity of empirical cases. Promoting “theoretically informed, but not theoretically driven” proposals, this historian says about a proposal he likes:

Theory is in the background . . . theory [should] not [be] what comes to the fore. There was one that I [evaluated] this time [and it was] the only one I actively disliked. I thought this was a person who really had nothing except the theory.

An anthropologist praises proposals that “represent complexity,” meaning proposals that do not “refine a highly complicated social situation in a one-sentence synthesis, or that’s going to draw a highly refined theoretical

conclusion.” In her mind, theory cannot “drive” the proposal and be reduced to alternative hypotheses derived from the literature. Summarizing his style, this anthropologist says he values research that requires one to be “immersed in a great deal of particularity and detail” and to “connect one range of particularities and details to another.” A political scientist provides a clear illustration of this opposition when he criticizes panelists who both favor reductionism and appreciate extraintellectual rationales for the choice of topic:

In one debate, [the other panelists would ask] “is this person contributing to a generalizable theory of politics, or nomothetic laws of politics, or a universal theory?” [They] argu[ed] that this person should be supported because he or she is making a theoretical contribution. I don’t take that argument very seriously. . . . Because you can’t do it! And, in fact, most people now understand that they can’t do it, but there are things about the discipline that reward it anyway. So most smart people understand, for instance, that you’re not getting to a science of politics, but since they win out in battles where you can demonstrate a certain capacity to do a formal theory or use methods that make it look more like that, they continue to articulate around those questions. I’ll tell you one other thing I wouldn’t support. There’s a way of framing what are important questions in political science today and it came out sometimes in the discussions, which is this—what is your analytic puzzle? And that completely drives me crazy.

This scholar also adds that he does not assess proposals based on whether their authors want to change the world, or to change theory, because of their personal or political views. He adds: “These scholars are [ . . . ] describing situations and trying to construct theories about them! I also don’t think it’s more significant that someone wants to work in refugee camps in Angola than work on French feudalism, because they really are social scientists who are defining themselves in a particular career path, which is going to be about theory and teaching in universities.”

### **The Positivist Style—Generalizability and Hypothesis Testing**

Panelists who favor proposals that provide an intellectual account of the choice of perspective and that draw on a method that can lead to generalization are coded as adopting a positivist style. Panelists who prefer a positivist style value research projects that test hypotheses and methods appropriate for achieving this goal. From this perspective, formalism and deductive models can go hand in hand with empiricism. Reality and data

are adduced to close scientific debates. A political scientist values proposals that claim to produce falsifiable knowledge:

You recognize [excellence] first of all by the willingness of someone to stick their neck out seriously to produce disconfirmable knowledge . . . [A] good theory is one that maximizes the ratio between the information that is captured in the independent variable and the information that is captured in the prediction, in the dependent variable. [You have] to be systematic, because otherwise what you're doing is not replicable. Otherwise, what one is doing is a personal expression.

Referring explicitly to Popper during the interview, he separates science from nonsense, while praising the production of replicable knowledge. "Giving voice" to dominated groups has no room in his understanding of how to do research, in contrast with those who espouse the constructivist style. In his view, adopting a systematic methodological approach that spells out variables and causal links to do theory building is generally a characteristic of top proposals. Panelists who promote the positivist style appreciate case studies only if they offer a compelling puzzle that allows for the testing of competing theories. This perspective is illustrated by a political scientist who says

Many [proposals] were single case [studies], which was fine, but I wanted a clear sense of what it was this was a case of. . . . I was looking for the applicant to situate the work and to identify what broader class of events or class of phenomena this was a part of and therefore an understanding of how this would illuminate that broader range of issues . . . . In other words, when you start with a set of suppositions of how things work, or a set of hypotheses if you want to put it that way, then you need to know what evidence would disconfirm that, you need to confront evidence that could prove you wrong . . . . [I value] that type of standards of evidence, going after data that would prove you wrong. Not just articulating your preferred explanation, but also thinking through and articulating other possible explanations.

Another political scientist offers a clear statement about how the positivist approach is superior to its interpretive alternative in providing a better empirical test. When asked how he assesses research proposals, he says

I think what I look for is: first, there would be a research design that's fairly explicit about the nature of the kinds of calls and claims that are being made. [And this for] all of them, even the ones that are saying, "I'm trying to

generate understandings and have more humanistic claims.” You know, even those that in some sense pull on what we might call a “descriptive understanding of causality,” or less positive notions of causality. I want to know . . . what are the exact relationships they’re trying to map out? I want to know something about the alternative explanations: Which ones are being considered? Which ones have already been rejected? And that I think is also part of a research design so that you know how it is that you’re wrong at the end of the day, if you are wrong. It’s not as if an anthropologist just randomly finds a field site. You’ll get anthropologists who I think pretend to that almost. They pretend that their field sites are found objects, as if they stumbled across them. And when somebody pushes that too much, yeah, I find them naive and frustrating and phony. Yeah, you’re like “Wait a minute, you had three years of graduate school, you’ve read all this social theory, you knew which questions you wanted to ask, you did have implicit hypotheses.”

As an economist puts it, attention to the complexity of reality is associated with a sterile attention to “details,” an “appreciation for the particular,” that is valued by disciplines such as anthropology and history that “are consumed by the local.”

### **The Utilitarian Style—Generalizability and Social Utility**

Like the positivist style, the utilitarian style favors hypothesis testing. These two styles differ in that the utilitarian style values extraintellectual rationales for assessing the significance of a topic and particularly favors research having some sort of “social utility” or “social significance.” For advocates of the utilitarian style, asking pertinent questions about the real world is much more desirable than exploring abstract questions as a theoretical end sufficient in itself. A political scientist expresses this concern well when describing proposals he rated most highly:

I think that the [proposals’] strengths are that they do interesting, comparative work that’s relevant to the real world, [that] has some sort of social utility value associated with it. They tend not to be afraid to tackle difficult problems . . . I also like to know particularly if these are policy-relevant kinds of proposals, that that’s made clear so that I know what the implications are for the people who study the policy-making process, and perhaps even for people who make the policies.

Those who adopt this utilitarian style value adjudicating contemporary public debates through scientific inquiry.

Adjudicating debates of social importance is made possible by the use of modeling techniques that exist independently of the researcher's reflexive relationship with the object of study—this is in part what distinguishes the utilitarian style from the constructivist style. Similarly, a historian explains that he values projects that have a clear social utility, and that knowledge is gained cumulatively, as a result of the efforts of the scholarly community as a whole. He says that he tends to evaluate proposals in this way:

In my own estimation, the best quality work is not narrow, in that it can at least take account of what's going on around it in some way. Well, again, in terms of urgency, in terms of deciding who has an excellent project and might be of the most value, when I think of value I think in part in terms of utility . . . Well, for instance, I see the study of [racism] as something akin to the kind of work that people who study destructive viruses do. If we can understand the dynamics of how this arises and how it is preserved, it seems to me that at a time when the world really needs better mutual understanding of cultures that it could be valuable. And I don't think any one individual is going to come up with the answers to this. So you have to have a lot of experiments going on out there, like the attempt to find the cure for AIDS, they'll do thousands and thousands of experiments, most of them have no results. But eventually, collectively, you come up with something.

“Narrow work” consists in work that lacks “generalizability” and that also lacks potential impact for real-world actors. This utilitarian ideal is realized in the collective pursuit of answers to urgent social problems, which inexorably leads to the accumulation of knowledge.<sup>13</sup>

## **The Distribution of Styles across Disciplines and Competitions**

Our interviews show that although panelists often disagree over the ranking of specific proposals,<sup>14</sup> they largely privileged the comprehensive style in their account of the deliberations. This style predominates in all the competitions but one, competition 1 (see Table 2). Seventy-eight percent of the panelists serving on competition 2 use this style, compared to eighty-three percent of those serving on competition 3, eighty-two percent of those serving on competition 4, eighty-seven percent of those serving on competition 5, and only twenty-five percent of those serving on competition 1.<sup>15</sup> More than three-quarters of panelists across all disciplines used the

**Table 2**  
**Distribution of Epistemological Styles by Competitions**

Epistemological Styles	Competitions											
	1		2		3		4		5			
	Total <sup>a</sup>	% <sup>b</sup>	Total	%	Total	%	Total	%	Total	%		
Constructivist	3	75	3	21	2	33	3	18	0	0	11	22
Comprehensive	1	25	11	78	5	83	14	82	7	87	38	78
Positivist	0	0	0	0	1	17	8	47	5	63	14	29
Utilitarian	0	0	0	0	1	17	1	6	3	38	5	10
Total	4	100	14	100	6	100	17	100	8	100	49	100

<sup>a</sup> The total in these columns is the number of panelists using each different epistemological style in each competition (here, the first one).

<sup>b</sup> The percentage in this column represents the percentage of interviewees from each competition (here, the first one) using each epistemological style.

**Table 3**  
**The Frequency Distribution of Epistemological Styles by Disciplinary Clusters**

Epistemological Styles	Disciplinary Clusters							
	Humanities		History		Social Sciences		Total	%
	Total	% <sup>a</sup>	Total	%	Total	%		
Constructivist	4	28	4	29	3	14	11	22
Comprehensive	12	86	11	78	15	71	38	78
Positivist	0	0	3	23	11	57	14	29
Utilitarian	0	0	1	4	4	19	5	10
Total	14	100	14	100	21	100	49	100

<sup>a</sup> The percentage in this column represents the percentage of interviewees from this cluster of disciplines (here the humanities) using each epistemological style. Because each interviewee may use various styles, columns do not sum up to 100 percent.

comprehensive style to describe how they evaluated proposals (Table 3): it is used by eighty-six percent of the humanists, seventy-eight percent of the historians, and seventy-one percent of the social scientists.<sup>16</sup>

Disciplinary differences exist in the use of epistemological styles. Social scientists use the comprehensive, the positivist, and the utilitarian style in

declining order of frequency. Although some panelists used consistently only one style while accounting for his or her evaluation of proposals, others used two. When they do so, they used styles that are close to one another: the constructivist and comprehensive styles, which both value interpretive methods to show the complexity of an object of study; the comprehensive and positivist styles, which both value purely intellectual or scientific motivations for a project that aims to advance theory; and the positivist and utilitarian styles, which both value the use of deductive reasoning to formalize generalizable hypotheses that cases can confirm or invalidate. The positivist style is used in our accounts by more than half of the social scientists, approximately one-fifth of the historians, and none of the humanists (see Table 4).<sup>17</sup> The constructivist style is more favored by humanists and historians than by social scientists—it is used by one-third of humanists and historians, and by fourteen percent of the social scientists only.<sup>18</sup> Panelists serving on the three more humanistic competitions use the comprehensive style most frequently when they describe their evaluations, followed by the constructivist style—the styles that share a concern with attention to detail and complexity. For instance, in competition 2, seventy-eight percent of the panelists use the comprehensive style and twenty-one percent use the constructivist style (see Table 3).

Panelists serving on competitions that fund mostly social science research also use the comprehensive style most frequently, followed by the positivist style. In competition 4, eighty-two percent of the interviewees use the comprehensive style and forty-seven percent use the positivist style (see Table 3).<sup>19</sup> These distributions show that panelists do not take on all the styles represented in their panel, much less all the styles available to them, when they assess the merits of a proposal. More importantly, they also show that all competitions and disciplinary clusters have a significant degree of epistemological diversity. This diversity could generate conflict and impede the making of awards. Thus, our question: how is agreement reached given this potentially volatile situation?

## **Cognitive Contextualization**

### **Achieving Procedural Fairness**

Instead of emphasizing translation processes aimed at demonstrating the general appropriateness of their favored criteria (Collins and Evans 2002), panelists state that they are concerned with using the most appropriate criteria to the field or discipline of the proposal under consideration. This is

**Table 4**  
**The Distribution of Epistemological Styles Used by Each Panelist by Disciplinary Clusters and Disciplines<sup>a</sup>**

Epistemological Styles	Disciplinary Clusters											Total		
	Humanities				History	Social Sciences								
	English	Musicology	Art History	Philosophy	History	Sociology	Political Science	Anthropology	Economics	Geography	Science			
Constructivist	█	█			█			█			█			11
Comprehensive	█	█	█	█	█	█	█	█			█			38
Positivist					█	█	█	█	█		█	█	█	14
Utilitarian					█									5
Total	7	3	2	2	14	6	6	5	2	1	1		49	

<sup>a</sup> Each column represents the epistemological styles that each panelist uses. A black cell represents an epistemological style used by a panelist. Although some panelists used consistently only one style while accounting for his or her evaluation of proposals, others used two. The competitions that support a larger number of humanities proposals are to the left of the table, and those that support a larger number of social science proposals are to the right. We compare styles across competitions, as opposed to panels, because panelists often serve on panels for two consecutive years. Hence, there is often a great deal of similarity within a competition over two cycles of funding. Aggregating across competitions, as opposed to panels, further maximizes the anonymity of panel members.

illustrated by an anthropologist who described “one proposal in the group we reviewed, which was by a political scientist who was doing a project which was very anthropological.” She rated this proposal highly, “although if the person had been an anthropologist, [I] would not have rated [it] as highly because there wasn’t enough fieldwork.” She accounted for giving a high rating to the proposal by noting that “for a political scientist it was an enormous amount [of fieldwork].” Similarly, describing the group dynamic on his panel, a political scientist says

The differences that I noted were, for instance, the difference between people who work with large data sets and do quantitative research, and then, their polar opposite, I suppose, folks doing community level studies in anthropology. They are such different methodologies that it’s hard to say that there’s a generalizable standard that applies to both of them. We were all, I think, willing and able to understand the projects in their own terms, fortunately, and not try to impose a more general standard, because it would have been extremely difficult.

Sticking to one epistemological style does not mean that evaluators are applying their own preferred criteria to all proposals. Quite the contrary, out of a concern for fairness, in many cases, they abstain from expressing an opinion on proposals that they do not feel competent to judge. Rather than engaging in a heated epistemological controversy or attempting to impose their privileged style of research, they almost always prefer silence. A positivist political scientist explains that

It’s one of these things where historians are on Mars and political scientists are on Venus. At some level, listening to them, I just don’t understand. And I count myself as someone who does history or historical analysis. But there’s such an appreciation for the particular that it’s real difficult for me to judge proposals. Those anthropologists that are consumed by the local, whatever that means for the moment, I have a harder time judging their merits. But when [a panelist said] “we don’t have really very many histories of 16th century [prostitutes] in [China],” well that very well may be true, but I’m not sure that that’s necessarily the reason why our committee should be funding it. Again, I just often have a harder time judging history, and I know that, which is why I tend to shut up on the history ones, because I just sort of feel like on some level I’m not a good evaluator.

This understanding helps explain that, in the large majority of accounts (31 of 49), evaluators only use one epistemological style in their accounts of the deliberations.

Program officers also promote respect for disciplinary differences. They take great pains to appoint panelists who can contextualize their evaluation by taking into consideration discipline-specific standards. As the program officer of competition 4 explains:

I think the key is to find people who are able to judge a proposal from within the criteria that that a particular proposal sets for itself for excellence. What I mean by that is, people who have multiple definitions for what excellence is, and can read a cultural studies proposal and determine a good one from a not so good one, read a political science proposal and determine a good one from a not so good one. The other is, of course, people who can bring to bear their own disciplinary expertise in ways that enable them to make very informed judgments about the degree to which proposals succeed in demonstrating the proper preparation and the potential for contribution to a disciplinary interest. And [do this] without performing as a gatekeeper, without setting themselves up as spokespeople or representatives of the discipline.

If panelists perceive the outcome of their deliberation as fair, it is therefore clearly not because they engage in translation of the type emphasized by Callon (1994). Panelists are expected to use the rule of cognitive contextualization and are perceived as not performing their role if they insist on imposing their own disciplinary standards to other disciplines and fields.

## **Breaching the Rule**

Three highly controversial cases emerged in the context of the twelve panels we studied. These cases provide additional evidence of the existence of the rule contextualization in peer review, and they also reveal the role of program officers in managing breaches through emotion work.

The three controversial cases examined in this section were described by interviewees as “our bad scene,” a “moment of decisional dead end,” a “culture war replayed,” a “polarizing debate,” and a “moment where there was a lot of emotional current in the room.” They are important for our purpose because they are instances where concerns about the procedural fairness of the deliberations were explicitly voiced. In each case, panelists and program officers succeeded in reestablishing the “normal” process after the breach—the “normal process” requiring that the rule of cognitive contextualization be respected and that “nobody feels excluded,” as was stressed by program officers leading competitions 2, 4, and 5.

*Case 1.* In competition 2, a political theorist argued in favor of a philosophy proposal against solid opposition from other panelists—an anthropologist, a historian, and a scholar in romance languages and literatures (RLL)—who considered themselves equally qualified to evaluate the proposal. The political theorist was aware that other evaluators could perceive the proposal as too “universal,” too “general,” and not attentive enough to “details.” Indeed, the RLL scholar criticized the proposal for adopting “a philosophical way” of looking at the topic “as a kind of universal phenomenon, [which] looked to [me] to be intellectually flawed [...] even going beyond normative.” Responding to this objection, the political theorist reported that he emphasized the applicant’s “awareness of the dangers of universalizing” and his ability to “historicize the literature he was reading.” Instead of praising the applicant’s concern with universality, he stressed his interest in the particular, for example, his “immersion in the literature” (the way an anthropologist would stress “immersion in the field”), his awareness of the “dangers of drawing analogies,” and his “attention to complexity” (all qualities valued in the widely shared comprehensive style). He argued that, compared to other philosophy proposals, the proposal under consideration shared much with standards central to the comprehensive style. He not only professed sharing elements of the same comprehensive style valued by the other panelists but also argued that these elements should be applied more loosely in the case of the proposal under consideration.

This argument, far from appeasing the opposition, almost led to a decisional dead end. The program officer had to call for a vote on the award, a highly unusual procedure, which most see as undesirable. One of the most outspoken opponent (described by the program officer as a “constructionist”) explains: “while I don’t feel at all that I am competent to evaluate all proposals, I always defer to the people who do have some kind of expertise in that field; where[as] this one, I thought, was stepping outside of his field and into mine.” This panelist and others accounted for the conflict in disciplinary terms; they thought they had to hold this proposal to the standard of their own disciplines, which required much more detailed analysis of microprocesses. They perceived the political theorist as nonresponsive to their objections and as disrespectful of their opinions. As one of them put it: “The one thing that bothered me was that he did not hear the criticisms that we were offering and they were quite substantive in detail: he felt like we didn’t know what we were talking about, that we were non-specialists and were out of our league, and that actually bothered me, that there was a kind of an undertone that we really weren’t up to his speed on this.”

Feeling that their expertise was not acknowledged, these panelists became more reluctant to express their differences of opinion in the self-effacing, understated, and courteous tone customarily used on funding panels. The debate became more heated. In the words of the political theorist,

Two other people just said, “OK, I’m prejudiced against [this topic].” But I said, “no, this is critical examination of [the topic], or based upon critical examination.” The [French literature] scholar said something that just blew me away, that, “well ordinarily I’m prejudiced against political theory,” or “I don’t like political theory, but if it’s concrete it’s OK.” I mean how would it have been if I started the day by saying, “I don’t like [literary studies], but if it’s abstract I can live with it.” I mean, I thought that was amazing!

For the program officer, because panelists disagreed concerning which epistemological style and disciplinary expertise were most relevant to the evaluation of the proposal, the deliberations turned into “a kind of culture war replayed . . . where the political theorist thought others are basically being really partisan,” which made him “very upset. [I] knew that he felt that this was dogmatism at its worst.” Recognizing a breach, the program officer engaged in “emotion work” (Hochschild 1979) to repair the situation during lunch—the vexed panelist had sat at a table by himself to eat. He talked to the political theorist, respectfully listening once again to his argument and assuring him that this would not happen again. Although he acknowledged that other panelists might have been unfair to him, the program officer also stated that he had heard the constructivist scholars “make real arguments against it,” and that the issue was not an attack against him or political theory. This allowed the embattled panelist to feel that the fair deliberation remained possible, despite the fact that in his view the wrong decision had been made about the controversial proposal.

This case demonstrates the existence of the rule of cognitive contextualization; in line with the classical breaching experiments that demonstrate the existence of social rules (Garfinkel 1967), breaches produce feelings of unfairness and emotions of “anger,” “distrust,” and “disrespect.” Program officers engage in backstage emotion work to restore the intersubjective conditions needed for continued collaboration—a shared definition of the situation as fair.

*Case 2.* In year 1 of competition 2, a controversy emerged around a geography proposal. An economist describes the situation thus:

The applicant kept saying she wanted to “measure” the change, which seems like a reasonable thing to do, that we’re interested in outcomes and these outcomes have changed over time. And the problem was that there was no follow through, there was no research design, there was no method that would give, at least me the reviewer, confidence that I knew how she was going to measure these changes. This was sort of great in spirit, but bad in execution. On those grounds then I think you had to say this proposal was short. But after that was said, I was basically being accused of being a “positivist,” you know, normally no one would ever say that because obviously that’s like calling somebody a “communist”! But there was a sense that I was imposing my disciplinary bias inappropriately on another discipline! And my response as I recall was: “no, I’m holding her to her own standards and I’m not trying to be hegemonic on this, but if she’s going to make these kinds of claims, she needs to be able to say what basis she will use to assess these claims, so that you know whether you’re right or wrong at the end of the day.”

This panelist insisted on holding the applicant to her own methodological standards, which he assumed to be positivist because of her use of the word “measure” in her proposal. A historian objected to this:

I was disturbed by this kind of epistemological exchange. And it was because of the use of words like, “I will measure the impact of,” in the proposal, which for [an economist] meant there had to be quantitative measures involved. Whereas, it was really about a qualitative assessment for me. We had very different readings concerning what the proposal was about with seven minutes to talk about it, and it wasn’t possible to clarify that. It finally got funded, but it wouldn’t have, I think, if I hadn’t thrown a fit.

Interestingly, the historian believed he convinced other panelists to support this proposal in part because he argued that voting for the proposal would be better than an escalation in epistemological conflict. The proposal got funded despite the fact that no consensus emerged around the proposal. On the contrary, the economist thought that his attempts to find a common ground between different epistemological styles had led other panelists to accuse him of being “hegemonic” and “unfair.” As he did not share their understanding of the situation, he felt disrespected by the historian, which in his view challenged his very presence on the panel.

In this case also, the program officer engaged in backstage emotion work to repair the breach. He reminded the panelists that the “committee always needs fairly tough-minded, empiricist, scientific social scientists who can hold up that banner and explain why their standards are what they are . . .

I think it's perfectly fair to hold the candidate responsible for them." Thus, he reaffirmed that panelists should value a wide range of epistemological styles and apply positivist standards when appropriate.

*Case 3.* In year 2 of competition 4, another conflict emerged, pitting proponents of the constructivist and positivist styles against one another. The case is described by a historian:

That was our bad scene. This [proposal] was one of those ones where there were really quite different sensibilities around the table that were-or could not-be made commensurable in the usual ways . . . . Now, [a political scientist] objected, fundamentally, deeply to the proposal for a couple of reasons. One, he said, "This is a proposal that is ideological and not objective." Now, in a sense that represents a kind of paradigm breaking assertion, because there are people in the room that operate with the presumption that in fact a sense of political commitment is a plus, not a minus. And for him, it clearly represented a fundamental failure on some level . . . . We went around for a while without finding a compromise.

During the break, the panel chair asked two panelists who had not read the proposal to assess it and make a recommendation. Then, he engaged in backstage emotional work and asked the political scientist and historian who had argued opposite positions to respect their differences. When the additional readers communicated their own evaluation, the historian reconsidered his own stance: "The point had come where if we're going to try and reach consensus, I will concede to the no's," because "by this time we had actually asked [two panelists who were not the original evaluators] to read it as well and they both came back and said that they didn't like it either."

In each of the three cases, panelists were critical that one of the evaluators had attempted to impose their favored criteria of evaluation, which they judged inappropriate. They disagreed about which epistemological style was most relevant. This situation led to feelings of exclusion and disrespect that made panelists view the procedure as unfair. These examples clearly demonstrate that contextualization of evaluation is essential to perceptions of procedural fairness.

These three breaches were resolved by backstage emotional work. Each case shows the importance of program officers as enforcers of the rule of cognitive contextualization. Program officers also recognize this role. They engage in backstage emotion work and prevent breaches from happening by choosing panelists known for their interpersonal skills. They also start the

deliberations with the “easiest” decisions (the cases around which there exists a strong consensus in favor of or against funding prior to the deliberations). This procedure is adopted “so that people come to more or less an idea of how the group process is working and have a group sense of what a winning proposal is. So, by the time [the panelists] get to that middle section [where the most difficult proposals are], there is a very clear idea of what the committee was looking for and how their process worked. By then, people in the room tend to respect each other and say, ‘OK, I’ve worked with you long enough to say that I’m going to go along with this.’”

## **Conclusion**

This article makes several contributions to the literature on peer evaluation. First, as Travis and Collins (1991) write, even though “many scientists are most concerned about the process of evaluation,” the literature on peer evaluation has overwhelmingly been concerned with the roadblocks to distributional “fairness caused by nonscientific influences such as politics, friendship networks, or common institutional positions” (324). By developing a typology of epistemological styles used by panelists to evaluate proposals, and by paying attention to how panelists believe fairness in deliberations is achieved, we hope to help social scientists better understand how epistemological styles affect not only funding decisions but also departmental deliberations, academic careers, and, more generally, evaluations in a wide range of settings.<sup>20</sup>

Second, this article sheds light on procedural fairness in peer evaluation. Authors such as Callon, Lascoumes, and Barthes (2001), or Collins and Evans (2002), who have proposed a “third wave” in science studies to focus on normative issues intrinsic to evaluation, speculate that intersubjective generalization and translation is the rule that best ensures the fairness of an evaluation. In contrast, our empirical study of how panelists understand the production of fair deliberations shows that they tie procedural fairness to respect for the rule of contextualization (and not to generalization). When panelists use inappropriate criteria and breach the rule of contextualization, they impede the decision-making process, and make it necessary for panel officers to intervene in the deliberations from outside.

This last finding suggests that even in multidisciplinary panels, discipline-specific ways of producing theory and methods are still the bedrock of peer evaluation. Respect for disciplinary autonomy and specificity

of epistemological styles within specific fields explains why culture wars do not dominate funding panels in the social sciences and the humanities. A local and situational epistemological truce appears to play out in these multidisciplinary panels because panelists avoid generalizing their preferred disciplinary criteria and because enforcers of that truce can act on a disintegrating situation. Whether this state is to be deplored or praised is not our concern. Instead, we proposed an empirical investigation of how panelists understand the conditions that lead to fair evaluation. Additional research is needed to assess whether this holds true in disciplinary competitions where more panelists may claim expertise about a narrower set of topics and where the epistemological autonomy of subfields might be less respected.<sup>21</sup>

Another topic for future inquiry is whether and how this rule of contextualization is also influenced by respect for the status hierarchy of the institutional affiliation of panelists, their social characteristics, and their gender in particular. Several female interviewees mentioned that gender plays a large role in the definition of appropriate forms of self-presentation and in how panelists make credibility claims. One of them read support for a constructivist epistemological style as indicative of support for gender diversity and for feminist scholarship, because feminist scholars have played an important role in questioning the notion of objectivity and in advocating feminist epistemology (Smith 1990). Indeed, among our panelists, women were much more likely to favor the constructivist epistemological style than men (appendix). We hope that this study will generate future research on the impact of gender and racial diversity in academia on the adoption of epistemological styles and procedural fairness.

Finally, comparisons of how procedural fairness is defined and of the relative popularity of epistemological styles across national academic communities are also topics with great potential for comparisons of academic markets (Musselin 1996). The definition of “fairness as appropriateness” by panelists that we observed here may result from larger cultural trends that have favored “respect for diversity” in the American academic context (Hollinger 1995).<sup>22</sup> This understanding may be a condition for a richer and better cross-Atlantic, not to mention global, intellectual engagement (Burawoy 2005, 20).

## Notes

1. The term “epistemological” can be used in a narrower sense. Other available terms, such as “paradigmatic,” are equally problematic (Masterman 1970).

2. Highly restricted access to accounts of collective deliberations, difficulties inherent in measuring meanings, and concerns with uncovering the impact of personal connections at work in the evaluation of science contributes to the neglect cognitive aspects of peer review in the literature (Mitroff and Chubin 1979).

3. The specific competitions studied were the International Dissertation Field Research Fellowship program of the Social Science Research Council and the American Council of Learned Societies; the Women's Studies Dissertation Grant Program at the Woodrow Wilson National Fellowship Foundation; and the Fellowship Program in the Humanities of the American Council of Learned Societies.

4. The evaluative process adopted by most funding organizations proceeds in two steps: at the first stage, individual screeners eliminate a large number of proposals; then, panels of evaluators discuss proposals in face-to-face meetings and select awardees (Guetzkow, Lamont, Mallard 2004). This article concerns only the second stage of evaluation.

5. The competitions we have studied have a program officer, a panel chair, or both. Program officers are PhD holders who may or may not have had an academic career and who are full-time employees of the funding agency. Panel chairs are generally established academics, and they preside over the panel for a few years only.

6. The fact that the interviewer is a scholar who has served on a number of evaluation panels was essential in facilitating openness among interviewees. All respondents were guaranteed anonymity, and we made a commitment to the participating organizations to disguise all information potentially leading to the identification of panelists or applicants.

7. When allowed, observations were useful for assessing the reliability of this method of interviewing.

8. For this purpose, we used the formal ranking of applicants produced by panelists prior to deliberations, which they provided to the program officer or chair.

9. Panelists also were asked to describe their criteria of evaluation beyond the context of the panel—for instance, how they recognize excellence in their graduate students, among their colleagues, and in their own work (Lamont 2008).

10. The creation of clusters was needed also to protect the anonymity of respondents.

11. To stress the strategic variability of evaluative contents across contexts, authors in science studies use the concepts of epistemological "repertoire" (Gilbert and Mulkey 1984) or "rhetoric" (Latour 1987) when describing the epistemological dimension of peer evaluation. In contrast, we use the term "style" when referring to epistemological styles, to parallel Eliasoph and Lichterman's (2003) analysis of group style and of the place of rules of fairness in the latter. We thereby associate the variations in the use of epistemological styles privileged by panelists to the interaction rather than to their strategic and intentional ability to switch codes, in the manner of Goffman (1959), Garfinkel (1967), and Collins (2005). This analysis of cultural styles puts less emphasis on people's strategic intentions than does the "culture as tool-kit" perspective developed by Swidler (1986) and others—see DiMaggio (1997) and Zerubavel (1997) for reviews.

12. It is defended for instance by Smith (1990) or Clifford and Marcus (1986).

13. In sociology, this style is illustrated for instance by Coleman (1992).

14. We do not analyze here how epistemological styles relate to variances in ranking, which are captured by measures of consensus (Cole, Cole, and Simon 1981; Cole, Simon and Cole 1987).

15. After observing that this style largely predominates in competitions, we explored whether it captures not one, but diverse styles. This was achieved by breaking down the comprehensive style into two or three different styles (for instance, a Weberian style, whereby

interpretation of complexity is intrinsically related to its explanation, and a Geertzian style, which focuses on interpretation only). The analysis did not generate any conclusive results.

16. Other authors have found that positivism dominates broadly in the social sciences (Camic and Xie 1994; McCartney 1970; see Passeron 1991 for criticism), but they have not studied interdisciplinary competitions.

17. Because each interviewee may use various styles, the frequencies of the use of each style do not sum up to 100 percent. The majority of panelists appear to use only one style. Only eighteen of forty-nine panelists (thirty-seven percent) used more than one epistemological style in their account of their evaluations. Twenty-four of the thirty-eight panelists who use the comprehensive style use only this style, while fourteen of them use the latter in combination with the constructivist or positivist styles (Table 4). Thus, a majority of panelists use only their most favored styles.

18. One should not infer from this relatively small number of panelists a strong correlation between disciplines and epistemological styles, because the study does not consider intradisciplinary competitions.

19. Additional analyses available from the authors indicate that these patterns are stable within competitions across the two years we studied, even though more than a third of panelists usually rotate every year and the disciplinary composition of panels fluctuates as well.

20. A note on the authors' own favored epistemological style can be helpful to frame future research on the topic, which might draw on other styles than the one adopted by the authors. This article adopts a comprehensive epistemological style; we identified inductively four epistemological styles and provide a (somewhat) thick description of cases that illustrate them. We also adopt a purely intellectual rationale for our work, as we aim to advance theory. Future research on the topic could draw on diverse epistemological styles when discussing or elaborating our results.

21. Hoping to provide such a comparison, we had secured permission to study disciplinary panels from the division of the Social and Behavioral Sciences at the National Science Foundation. However, invoking the Privacy Act, access was ultimately denied by NSF's General Counsel's office. For a comparative perspective on peer review, see Lamont and Mallard (2005).

22. For a comparative perspective on peer review, see Lamont and Mallard (2005).

## References

- Abbott, Andrew. 2001. *Chaos of disciplines*. Chicago: University of Chicago Press.
- . 2004. *Methods of discovery. Heuristics for the social sciences*. New York: W.W. Norton.
- Armstrong, J. Scott. 1997. Peer review for journals: Evidence on quality control, fairness and innovation. *Science and Engineering Ethics* 3 (1): 63-84.
- Bakanic, Von, Clark McPhail, and Rita J. Simon. 1987. The manuscript review and decision-making process. *American Sociological Review* 52 (5): 631-42.
- Beersma, Bianca, and Carsten K. W. De Breu. 2003. Social motives and integrative negotiation: The mediating influence of procedural fairness. *Social Justice Research* 16 (3): 217-40.
- Bourdieu, Pierre. 1988. *Homo academicus*. Trans. Peter Collier. Cambridge, UK: Polity Press.

- Bourdieu, Pierre, Jean-Claude Chamboredon, and Jean-Claude Passeron. 1968. *Le métier de sociologue* [The craft of Sociology]. Paris: EHESS Editions.
- Brenneis, Donald. 1994. Discourse and discipline at the national research council: A bureaucratic bildungsroman. *Cultural Anthropology* 9 (1): 23-36.
- Burawoy, Michael. 2005. For public sociology. *American Sociological Review* 70 (1): 4-28.
- Callon, Michel. 1994. Is science a public good? *Science, Technology and Human Values* 19 (4): 395-424.
- Callon, Michel, Pierre Lascoumes, and Yan Barthe. 2001. *Agir dans un monde incertain, essai sur la démocratie technique* [Action in an Uncertain World: Essays on Technical Democracy]. Paris: Seuil.
- Camic, Charles, and Yu Xie. 1994. The advent of statistical methodology in American social science—Columbia University, 1880-1915: A study in the sociology of statistics. *American Sociological Review* 59:773-805.
- Chubin, Daryl, and Edward Hackett. 1990. *Peerless science: Peer review and U.S. science policy*. Albany, NY: State University of New York Press.
- Clifford, James, and George Marcus, eds. 1986. *Writing culture: The politics and poetics of ethnography*. Berkeley, CA: University of California Press.
- Cole, Jonathan, and Stephen Cole. 1981. *Peer review in the National Science Foundation: Phase two of a study*. Washington, DC: National Academy Press.
- Cole, Stephen. 1978. Scientific reward systems: A comparative analysis. *Research in the sociology of knowledge, science and art*, ed. R. A. Jones, 167-90. Greenwich, CT: JAI.
- . 1992. *Making science: Between nature and society*. Cambridge, MA: Harvard University Press.
- Cole, Stephen, Jonathan Cole, and Gary Simon. 1981. Chance and consensus in peer review. *Science* 214:881-6.
- Cole, Stephen, Leonard Rubin, and Jonathan Cole. 1978. *Peer review in the National Science Foundation: Phase one of a study*. Washington, DC: National Academy of Sciences.
- Cole, Stephen, Gary Simon, and Jonathan Cole. 1987. Do journal rejection rates index consensus? *American Sociological Review* 53:152-6.
- Coleman, James S. 1992. The rational reconstruction of society. Presidential Address. Annual Meetings of the American Sociological Association. Pittsburgh, PA.
- Collins, Harry H., and Robert Evans. 2002. The third wave of science studies: Studies of expertise and experience. *Social Studies of Science* 32 (2): 235-96.
- Collins, Randall. 2005. *Interaction ritual chains*. Princeton, NJ: Princeton University Press.
- DeVault, Marjorie L. 1999. *Liberating methods: Feminism and social research*. Philadelphia: Temple University Press.
- DiMaggio, Paul. 1997. Culture and cognition. *Annual Review of Sociology* 23:263-87.
- Eliasoph, Nina, and Paul Lichterman. 2003. Culture in interaction. *American Journal of Sociology* 108 (4): 735-94.
- G.A.O. (General Accounting Office). 1994. *Peer review: Reforms needed to ensure fairness in federal agency grant selection: Report to the Chairman, Committee on Governmental Activities, US Senate*. Washington, DC: G.A.O.
- Garfinkel, H. 1967. Studies of the routine grounds of everyday activities. In *Studies in ethno-methodology*, 35-75. Oxford: Polity Press, 1987.
- Gilbert, Nigel, and Michael Mulkay. 1984. *Opening Pandora's box. A sociological analysis of scientists' discourse*. Cambridge, UK: Cambridge University Press.
- Goffman, Erving. 1959. *The presentation of self in everyday life*. Garden City, NY: Doubleday.

- Guetzkow, Joshua, Michèle Lamont, and Grégoire Mallard. 2004. What is originality in the social sciences and the humanities? *American Sociological Review* 69:190-212.
- Habermas, Jurgen. *Moral consciousness and communicative action*. Trans. Christian Lenhardt and Shierry Weber Nicholsen. Cambridge, Massachusetts: The MIT Press.
- . 1995. Reconciliation through the public use of reason: Remarks on John Rawls's political liberalism. *Journal of Philosophy* 92 (3): 109-31.
- Hargens, Lowell. 1987. Scholarly consensus and journal selection rates. *American Sociological Review* 53: 174-96.
- Hochschild, Arlie. 1979. Emotion work, feeling rules and social structure. *American Journal of Sociology* 85 (3): 551-75.
- Hollinger, David A. 1995. *Post-ethnic America: Beyond multiculturalism*. New York: Basic Books.
- Kelle, Udo, Gerald Prein, and Catherine Beird. 1995. *Computer-aided qualitative data analysis: Theory, methods and practice*. Thousand Oaks, CA: Sage Publications.
- Knorr-Cetina, Karin. 1999. *Epistemic cultures: How the sciences make knowledge*. Cambridge, MA: Harvard University Press.
- Lamont, Michèle. 2008. *Cream rising: Finding and defining excellence in the social sciences and the humanities*. Cambridge, MA: Harvard University Press.
- Lamont, Michèle, and Grégoire Mallard. 2005. *Peer review in international perspectives: US, UK and France*. Report commissioned by the Social Sciences and Humanities Research Council of Canada.
- Lamont, Michèle, and Virág Molnár. 2002. The study of boundaries across the social sciences. *Annual Review of Sociology* 28:167-95.
- Latour, Bruno. 1987. *Science in Action: How to follow scientists and engineers through society*. Cambridge, MA: Harvard University Press.
- . 1993a. Portrait d'un biologiste en capitaliste sauvage. *Petites leçons de sociologie des sciences* [Portrait of a Scientist as a Capitalist, In Lessons of Sociology of Science], 100-30. Paris: Editions Points Seuil Sciences.
- Latour, Bruno, and Steven Wolgar. 1979. *Laboratory life: The construction of scientific facts*. Second edition prepared in 1986 with a new postscript. Princeton, NJ: Princeton University Press.
- Liebert, Roland. 1982. Productivity, favor, and grants among scholars. *American Journal of Sociology* 82 (3): 664-73.
- Masterman, Margaret. 1970. The nature of a paradigm. *Criticism and the growth of knowledge*, eds. I. Latakos, and A. Musgrave. Cambridge: Cambridge University Press.
- McCartney, John. 1970. On being scientific: Changing styles of presentation of sociological research. *The American Sociologist* 5 (1): 30-5.
- Merton, Robert K. 1972. Insiders and outsiders: A chapter in the sociology of knowledge. *American Journal of Sociology* 78 (1): 9-47.
- . 1942. The normative structure of science. In *The sociology of science*, ed. Norman W. Storer, 267-78. Chicago: University of Chicago Press, 1973.
- Mitroff, Ian I., and Daryl E. Chubbin. 1979. Peer review at the NSF: A dialectical policy analysis. *Social Studies of Science* 9 (2): 199-232.
- Musselin, Christine. 1996. Les marchés du travail universitaires, comme économie de la qualité. *Revue Française de Sociologie* 37 (2): 189-208.
- Nagel, Ernest. 1961. *The structure of science: Problems in the logic of scientific explanation*. New York: Harcourt, Brace and World.
- Passeron, Jean Claude. 1991. *Le raisonnement sociologique: L'espace non-poppérien du raisonnement naturel* [Sociological Reasoning: The Non-Popperian Space of Natural Reason]. Paris: Nathan.

- Pillutla, M. Madam, and J. Keith Murnighan. 2003. Fairness in bargaining. *Social Justice Research* 16 (3): 241-62.
- Ragin, Charles C. 1987. *The comparative method: Moving beyond qualitative and quantitative strategies*. Berkeley, CA: University of California Press.
- Rawls, John. 1985. Justice as fairness: Political not metaphysical. *Philosophy and Public Affairs* 14 (3): 223-51.
- . 1995. Political liberalism: Reply to Habermas. *Journal of Philosophy* 92 (3): 132-80.
- Roy, Rustum. 1985. Funding science: The real defects of peer review and an alternative to it. *Science, Technology, and Human Values* 10 (3): 73-81.
- Singleton, Royce A., and Bruce C. Straits. 1999. *Approaches to social research*. 3rd ed. New York: Oxford University Press.
- Smith, Dorothy E. 1990. Women's experience as a radical critique of sociology and The ideological practice of sociology. In *The conceptual practices of power. A feminist sociology of knowledge*, 1-57. Boston, MA: Northeastern University Press.
- Solum, Lawrence B. 1989. Freedom of communicative action: A theory of the first amendment freedom of speech. *Northwestern University Law Review* 83 (1 and 2): 54-135.
- Somers, Margaret. 1996. Where is sociology after the historic turn? Knowledge cultures, narrativity, and historical epistemologies. In *The historic turn in the human sciences*, ed. McDonald. Terrence, 53-90. Ann Arbor: University of Michigan Press.
- Steinmetz, George, ed. 2005. *The politics of method in the human sciences*. Durham, NC: Duke University Press.
- Stinchcombe, Arthur L. 2005. *The logic of social research*. Chicago: University of Chicago Press.
- Swidler, Ann. 1986. Culture in action: Symbols and strategies. *American Sociological Review* 51: 273-86.
- Travis, G. D. L., and Harry M. Collins. 1991. New light on old boys: Cognitive and institutional particularism in the peer review system. *Science, Technology and Human Values* 16 (3): 322-41.
- Weber, Max. 1905, 1911. *Max Weber on the methodology of the social sciences*. Trans. and ed. Edward A. Shils and Henry A. Finch. Glencoe, IL: Free Press, 1949.
- . 1913. *Confucianism and taoism*. Abridged M. Morishima; trans. M. Alter and J. Hunter. London: London School of Economics and Political Science, 1984.
- Zerubavel, E. 1997. *Social mindscapes: An invitation to cognitive sociology*. Cambridge: Harvard University Press.
- Zuckerman, Harriet, and Robert K. Merton. 1971. Patterns of evaluation in science: Institutionalization, structure and functions of the referee system. *Minerva* 9:66-100.

## Appendix

### The Frequency Distribution of Epistemological Styles by Sex of Panelists

Epistemological Styles	Gender of Panelists				Total
	Men		Women		
	Total	% <sup>a</sup>	Total	%	
Constructivist	1	9	10	91	11
Comprehensive	21	55	17	45	38
Positivist	10	71	4	29	14
Utilitarian	5	1	0	0	5
Total	28	57	21	43	49

<sup>a</sup> The percentage in this column represents the percentage of panelists using a particular epistemological style who are men.

**Grégoire Mallard** (Ph.D., Princeton University, June 2008) is Assistant Professor in the Department of Sociology at Northwestern University. His dissertation analyzed how issues of sovereignty and scientific development are related to questions of national security in the field of nuclear proliferation. He co-edited with Catherine Paradeise and Ashveen Peerbaye, *Global Science and National Sovereignty: Studies in Historical Sociology of Science* (Routledge, 2008). He has also published articles on peer evaluation and knowledge practices in the humanities, in the *American Sociological Review*, *Research Evaluation* and *Sociologie du Travail*.

**Michèle Lamont** is Robert I. Goldman Professor of European Studies and Professor of Sociology and African and African American Studies at Harvard University. Her recent publications include *How Professors Think: Inside the Curious World of Academic Judgment* (Harvard University Press, 2009). She is a fellow of the Canadian Institute for Advanced Research and is co-director of its research program on Successful Societies. Together with Peter A. Hall, she is the co-editor of *Successful Societies: How Culture and Institutions Affect Health* (forthcoming, Cambridge University Press). Lamont is currently serving as Chair of the Council for European Studies, the learned society of American social scientists and historians working on Europe.

**Joshua Guetzkow** is Assistant Professor in the Department of Sociology at the University of Arizona. He received his PhD in sociology from Princeton University. His dissertation employed a range of methods to examine the connections between welfare and criminal justice policy in the United States over the last 40 years. He recently completed a postdoctoral fellowship with the Robert Wood Johnson Scholars in Health Policy program at Harvard University, where he studied the interpenetration of the criminal justice and mental health systems through a case study of a mental health court. His research interests include cultural sociology, crime and punishment, the sociology of knowledge, public policy, the law and psychiatry, and the cultural analysis of policy making.