Ideals of Argumentative Process and the Ethnomethodology of Scientific Work: Implications for Critical Social Theory

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Like his Frankfurt School forebears, Jürgen Habermas takes an inter-disciplinary approach to critical social theory. His discourse theory of morality, politics, and law links a normative philosophical perspective with, inter alia, empirical social science, moral psychology, and political economy. In this paper I take this interdisciplinary initiative a step further, or more optimistically, two steps further: that is, I bring the ethnomethodology of scientific work (hereafter ESW) to bear on problems that emerge when we attempt to apply Habermas's discourse theory to scientific inquiry and argument as a social process. The specific problems I have in mind arise in the attempt to contextualize Habermas's discourse theory by confronting it with real discourses in institutional settings. Because the normative theory relies on highly idealized standards of argumentation, its implications for the assessment of actual scientific discourses are often unclear or even at odds with institutional practices that have proven their epistemic worthiness. Inspired by the work of Harold Garfinkel and Harvey Sacks, ESW allows us to clarify the root difficulty in Habermas's model and to suggest some possible modifications. In doing so, however, we must also deal with a further challenge, namely, that an ethnomethodologically modified discourse theory must abandon its intention of providing standards for the critical assessment of scientific arguments. I argue that this need not be the case.

I develop the argument in the following steps. After reviewing Habermas's normative argumentation theory (section 1) and describing the problems it faces when we confront it with good scientific practice (section 2), I recall the basic moves that inform ESW and then present its specific challenge to Habermas's model (section 3). I then sketch some modifications that might allow a normative argumentation theory with critical intent to incorporate contextualist insights of ESW without losing sight of critical social assessment (section 4). The modifications I propose extend some earlier attempts to contextualize Habermas's approach by coupling it with ESW (e.g., Hoy and McCarthy 1994, chap. 3; Rehg 2001). These efforts drew on ethnomethodology as a way of concretely situating the "ideas of reason" that govern scientific

argumentation, namely, the ideas of truth, objectivity, and accounta-
bility. However, these studies largely disregarded the more specific
audience idealizations that set standards for argumentation as a social
process—idealizations such as full inclusion, or openness of discourse,
equality of participation, and freedom from coercion. Such idealizations
are my topic in what follows.[1]

Although I focus primarily on Habermas's model, the problem of
context has been an ongoing concern for philosophers of science and
social epistemologists who strive to develop contextualist but non-rela-
tivistic models of scientific rationality. Recent proposals notwithstanding
(e.g., Solomon 2001; Kitcher 2001; Longino 2002; Norton 2003), the
problem of context deserves further systematic attention from the
standpoint of argumentation theory.

1. Habermas's Theory of Argumentation

The discourse theory developed by Habermas (among others) sets forth
normative standards for the discursive justification of "validity claims" of
various sorts, that is, claims to truth, moral rightness, authenticity, and
so on.[2] Here the terms "discourse" and "discursive justification" refer to
intersubjective argumentation and dialogue; consequently, discourse
theory, in the sense I employ it here, is a normative argumentation
theory.[3] In cases involving competing claims, discourse theory implies
that a given claim C is better justified than rival views if the more cogent
or better arguments favor C rather than the alternatives. The standards
for cogent arguments go beyond those associated with deductive and
inductive logics, or what some argumentation theorists refer to as the
"logical" aspect of argumentation (Wenzel 1990; Johnson and Blair
1994). For an argument to be cogent, not only must the reasons (or
premises) sufficiently support the conclusion from a logical point of view;
the argument must also be dialectically robust, that is, it must hold up
against reasonable challenges—further questions, relevant objections,
counterarguments, and so on. A genuine test of dialectical robustness
requires in turn that the *intersubjective process* of argumentation satisfy
certain social conditions, such that the community of inquirers is
sufficiently capable of constructing and critically testing arguments for
their relative strength. Habermas's process standards contain the familiar
idea that open, free debate provides the best means of correcting
individual biases, sharing information, scrutinizing assumptions, and so
on, so that the genuinely better arguments can emerge and conclusions
are as well justified as the available state of knowledge allows
(Habermas 1984/87, 1: 21–44; 1990, 86–9).

Drawing on Alexy (1990), Habermas (1998, 44; 1990, 89) has
provided a partial list of process standards, according to which a
discourse counts as reasonable[4] insofar as it meets conditions of
openness (publicity or inclusiveness), equality, and freedom. That is,
discourse is reasonable insofar as (1) it includes everyone able to make a
relevant contribution (openness or publicity), (2) participants have equal
voice in the discourse (equality), and (3) participants are not coerced in
any way—whether by internal psychological pressures, social pressures,
or institutional rules—that would distort or block critical discussion
(freedom). Because few if any actual discourses can satisfy such highly
idealized requirements—and no discourse can guarantee their satisfac-
tion—these standards operate as "counterfactual idealizations." It is
important to understand what Habermas means here: although counter-
factual, process idealizations are not irrelevant for actual discourse. They
articulate "pragmatic presuppositions" that participants must make
insofar as they seriously engage in discourse and consider a consensual
outcome as justified by the more cogent arguments. To "make" such
presuppositions means that we consider ourselves justified, given our
experience in the actual discourse, in presuming that the outcome would
not change even if the assessment of the available arguments and
information were to become as good as it possibly could be.

Habermas developed his argumentation theory primarily for purposes
of defending the cognitive character of morality (and related forms of
practical discourse in politics and law) from the value skepticism that
dominated moral and political philosophy at mid-twentieth century (see
Habermas 1990; 1996). Nonetheless, he presents his process ideali-
zations as standards for rational argumentation in general (Habermas
1998, 43; Alexy 1990, 166). Thus we should expect them to apply not
only to practical discourses but also to empirical discourses such as occur
in the sciences. In the latter context, Habermas's theory implies that
when scientists take a given hypothesis-conclusion as well justified by
the evidence, they presuppose their actual process of inquiry and
discourse has sufficiently approximated such idealizations. Conversely, if
they later uncover serious distortions in that discourse, they have reason
to revisit the issue.

Habermas has consistently linked these idealizations with the idea of
rational consensus. Indeed, this link was crucial for his reply to value
skepticism: moral-political claims have a cognitive or epistemic character
—they represent a kind of knowledge, the outcome of a learning process
—in virtue of a counterfactual, idealizing supposition that reasonable
participants could reach consensus on those claims after engaging in a
rational discourse that sufficiently satisfied process idealizations (Haber-

mas 1990). In the area of empirical discourse, the link between rational discourse and consensus led him to a consensus theory of truth that seemed to *define* truth in terms of ideal consensus (Habermas [1971] 2001; [1972] 1986). He has since rejected this theory (Habermas 2003). On his current pragmatic-realist view, a claim does not count as true just *because* it could meet with agreement under ideal discursive conditions. Process idealizations do not define truth; rather, in conjunction with logical and dialectical standards, they define cogent argumentative justification. Nor do they project a final state or "end of inquiry"—on this point Habermas differs with Peirce. Rather, because these standards govern inquiry as an *ongoing process* of discourse, they refer to available information and arguments (for a given level of knowledge and investigative capacities); thus new methods and information may call for revision of previously accepted beliefs (Habermas 2003).

However, without further specification of the argumentative context, it remains unclear whether process idealizations as such imply a goal of consensus. Indeed, to derive his consensual principle of moral universalizability (U), Habermas must link process standards with a specific interpretation of the social function of moral norms (Habermas 1998, 39–45). For empirical discourse, universal consensus becomes a goal in virtue of the common-sense presupposition that we share a single objective world (Habermas 1984/87, 1: 13–5; Hoy and McCarthy 1994, chap. 3). Other types of discourse imply different levels and degrees of consensus. Whereas moral discourse aims at universal consensus on both the conclusion and the reasons, political discourse over national policy aims at a consensus of citizens on the conclusion, but not necessarily on the reasons (Habermas 1996, 110–8). An individual's "ethical-existential" discourse with friends and counselors over a personal decision most plausibly requires only a local consensus on the reasonableness of a decision (though Habermas 1992 takes a stronger line).

2. Real vs. Ideal Discourse: The Problem of Institutional Context

Of the various criticisms leveled against Habermas's theory of moral and political discourse (see Benhabib and Dallmayr 1990; Rosenfeld and Arato 1999), the charge of context-insensitivity is the one that concerns me here. Although much of this critical literature targets the ideal of consensus, part of the difficulty lies in the abstract, counterfactual character of the process idealizations, which make their plausible contextualization in real discourses problematic (see Blaug 1999). In relation to scientific discourse, the problem of contextualization arises

from the observation that even good science often does not adhere to process idealizations. The issue is not that scientists sometimes act in ways that subvert epistemic aims; any practice is beset by human frailty and failures. Rather, the issue resides in the fact that process standards often appear impossible to satisfy for real scientific discourse given the scarcity of discursive resources such as time, attentiveness, money, and so on. Shortcuts and satisficing measures are required, but how are these selected and justified? How do participants actually determine, in context, that they have sufficiently approximated the process idealizations—that the actual discourse warrants the counterfactual projection that the outcome would hold up under idealized conditions?

These questions become especially acute when we see that approximating measures often cannot be simultaneously optimized. Efforts to maximize informational input, include more participants, ensure equal voice, and build consensus can, under constrained circumstances, call for incompatible institutional measures over whose priority participants may disagree. For example, broadening the inclusiveness of a discourse can make it more difficult to achieve consensus.[5] In a debate over U. S. National Academy of Science expert-committee procedures we can see the tension between ideals of openness (publicity) and non-coercion of participants (see Hilgartner 2000). The procedures in force up to 1997 called for closed discussion and tight control of information available to the public before the committee felt ready to release their report. This regime of confidentiality stemmed from the fear that opening discussions to public scrutiny would expose the experts to outside pressures and hinder the free exchange of opinions. Critics of confidentiality retorted that closed procedures restricted the insights available to the committee and rendered the argumentation process opaque.

In some contexts the standards themselves are in question. For example, in debates over science funding, "best-science" elitists once defended the epistemic superiority of a distribution of grants that would privilege the better-equipped research centers, a view that seems to contradict the standard of equality (Kevles 1979, 151–2). Scientists also do not honor without qualification the ideal of inclusive, or open, discourse within the science community. Rather, it is expected that a research team may withhold results, and refuse to share data, until it feels ready to publish; attempts by non-team members to obtain data, or efforts by team members to distribute data to outsiders, have provoked ethical criticism in some cases.[6] These tensions underscore the point that idealizations are not a set of instructions for institutional design; rather they function as critical guidelines (see Blaug 1999). As such, they remain institutionally indeterminate and ambiguous until specific pro-

cedural rules and mechanisms specify their relative import and priority in context. NAS procedures, for example, prioritize the freedom of panelists over inclusiveness. But how does one reasonably bridge the gap between idealizations and specific procedural designs? How does one determine that the latter are adequate in light of the former?

Habermas's conceptual framework suggests at least three sorts of considerations that help specify abstract process ideals for institutional designs. In the previous section we have seen how specific *argument-ative goals* associated with different types of arguable claims (truth, moral rightness, authenticity, legal validity, etc.) can specify the relevant circle of participants, and the breadth and depth of consensus. Even so, goals such as empirical truth and moral rightness require further specification before they are institutionally serviceable. For example, the heavy burden of proof in criminal law serves the goal of just decisions, but specified in a way that gives priority to avoiding false convictions. Similarly, stringent review mechanisms in science do not simply serve the abstract goal of truth but in addition a goal of minimizing false claims. *Feasibility considerations* provide a second mode of constraint on ideals, in a number of ways. These include not only external constraints (cost, time) but also social-psychological realities. A courtroom procedure, for example, organizes participant roles so as to correct for the likely biases of the contending parties and create a space for rational argumentation (see Rehg 1994, 214–27; Habermas 1996, 234–7). In science, the attribution of credit is a good example of an institutional mechanism that helps render counterfactual process idealizations effective in the face of the social-psychological realities of human interaction. Credit mechanisms deserve further comment, for they help us understand how Habermas's model might explain certain departures from process ideals, in particular from the ideal of openness.

As is well known, the attribution of credit plays an enormous role in the growth of scientific knowledge. According to Hull (1988), science is epistemically successful in large measure because of such institutional mechanisms as credit, which channel self-interested behavior (of people with an intrinsic curiosity about nature) into productive modes of cooperation. As creatures seeking not simply truth but also their own career advancement—which in science means the ability to do more science—scientists have an individual, competitive interest in receiving recognition for their work. Habermas (1990, 160) acknowledges that argumentation, as a disputational competition to convince one's inter-locutors, channels self-interest into the pursuit of truth. On the other hand, his process idealizations are supposed to put every motivation out of play other than the desire for truth—which implies that this

competition is disinterested insofar as personal ambition is concerned and thus differs from the competition for credit. Consequently, his model must place credit mechanisms at the level of institutional conventions that are supposed to compensate for or redirect recalcitrant, non-ideal aspects of human interaction. Specifically, the promise of recognition and advancement motivates scientists to undertake risks and contribute to epistemic aims. But for credit to work, one must *temporarily* suspend the openness ideal, to prevent competitors from co-opting a discovery and the credit it brings. Credit mechanisms, and norms allowing for tem-porary exclusion and secrecy, therefore specify the openness ideal by situating its operation in a temporal framework. Such mechanisms are legitimate insofar as they make it feasible for self-interested actors to cooperate in a collective epistemic endeavor.

In Habermas's democratic theory we find a third kind of con-sideration. That is, the way a polity should specify the abstract basic rights necessary for a robust constitutional democracy depends partly on *historical experience*. In some cases this constrains process idealizations. For example, the National Socialist experience in Germany makes it reasonable to exclude fascist agenda from political discourse, thereby setting a limit on a process idealization connected with equal voice, the idea that each participant "is allowed to introduce any assertion whatever into the discourse" (Habermas 1990, 89). In German political discourse, some assertions are excluded from the start.

Taken together, these three contextual factors—argumentative goals, feasibility, and historical experience—provide the ballast or raw material that bring ethereal process idealizations down to institutional earth. To summarize, we might say that well-designed discursive procedures aim to optimize the substantive quality of the outcome as measured by the goal(s) of the procedure and what is socially feasible in the light of a particular historical experience. If things go well, argumentative practices within such institutions sufficiently approximate the process ideal, thus warranting the presumption that outcomes are reasonable. Thus the key question for assessing institutionalized discourse is whether a given de-sign and the discourses it supports satisfy this sufficiency criterion.

To answer this question, one must engage the services of a con-textualizing reason that delves into the substance of the three contextual factors. But notice that contextualization takes a top-down approach here, consistent with Habermas's general theoretical strategy which starts with abstract models of communicative reason and works its way down to reality through a rather tortuous process of de-idealization (see Habermas 1984/87, 1: 328–37). More precisely, the critical assessment of institutional design and execution proceeds via the critique and

defense of competing *limitations* on impossible idealizations. Here epistemic reason sides with the latter, while reconciling itself with an imperfect contextual reality. Process idealizations, that is, *define* epistemically adequate discourse: although substantive reasons justify conclusions at the logical level of argument construction, arguments count as fully adequate justifications only if they have been critically tested through procedures whose social conditions meet ideal process conditions.[7]

As ethnomethodologists have pointed out, Habermas's top-down model runs the risk of missing context-specific forms of reasonableness that elude the ideal model (Bogen 1999). ESW thus suggests an alternative strategy, one that conceives the relation between ideali-zations and context differently from Habermas's model. Like Habermas, ESW holds that to understand reasonable modes of contextualization, one must enter into that dense texture of substantive argumentation, procedures, and institutional savvy that make up specific argumentative situations. But rather than oppose ideal and context, ESW orients the very meaning of idealizations to their use in context.

3. The Demands of Social Order

Recall that ESW, like other sociologies of scientific knowledge (SSK), approaches science from the standpoint of social order (see Barnes 2003). Accordingly, to understand science as a rational process, one must show how scientists, in their practices of inquiry and discourse, solve the problem of "social order," that is, how they maintain a level of intersubjective coherence or coordination in their practices sufficient to allow them to make ongoing sense of their interactions, including conflicts, as meaningful for one another. In this context, terms like "social order" and "orderly interaction" refer more to the mutual intel-ligibility of talk and action than to irenic relations: enduring conflicts can also be "orderly." According to ESW, this process of sense-making in-volves irreducibly local, situated achievements of mutual understanding; consequently, general norms of rational method cannot capture the highly contextualized modes of rationality by which practitioners maintain an ongoing sense of their interactions (see Heritage 1984).

From an ESW perspective, Habermas's approach to scientific discourse runs into difficulties with context precisely because its top-down approach to communicative rationality is bound to miss these "endogenous rationalities" or "local management practices."[8] Starting with high-level, abstract idealizations, Habermas asks whether actual practices of inquiry can more or less live up to them. But such

idealizations must be rendered meaningful in context, and compared with ESW, Habermas's approach seems poorly equipped for analyzing this ground-level achievement of meaning. More specifically, the abstract reflection on which Habermas relies for his categorical framework of speech acts and validity claims can hardly match the sophisticated observational methods that inform the ethnomethodologist's perspicuous attempts to notice and describe the situated "procedures" by which people rationally structure their communication and actions in context. These observational methods are designed to uncover the tacit, un-noticed moves by which actors maintain the ongoing sense of doubly contingent social interactions—moves that engaged participants typically "witness but ignore." Philosophical reflection, relying mainly on an a priori philosophical framework, is unlikely to disclose such tacit, context-ually embedded procedures.

As I mentioned earlier, some initial attempts have been made to engage the services of ethnomethodology for addressing the problem of context in Habermas's discourse theory (Hoy and McCarthy 1994, chap. 3; Rehg 2001). For this project, it is important to notice that unlike rule-skeptical SSK scholars, ethnomethodologists like Michael Lynch do not reduce science to "external" social influences (see Lynch 1992; 1993). They take the normative intuitions of their subjects as not in need of unmasking or instruction; rather, the idea is to increase awareness of the situational complexity of such norms. However, in pursuing this goal ethnomethodologists adopt a kind of anti-theoretical particularism that radicalizes the Wittgensteinian point that rules are grounded in practice in a manner that resists complete articulation. This particularist stance poses a challenge for an argumentation theory of science—indeed a chal-lenge that philosophers of science and critical theorists have not yet fully confronted.

We should be clear about the nature of this challenge. In taking a particularist approach, ethnomethodologists do not simply reject general ideas of rational method as *irrelevant* to the situated practices of scientists. Instead they shift the direction and mode of analysis: rather than generate a list of abstract ideals through armchair reflection and transcendental arguments, they closely describe the various ways that scientists actually employ or invoke such process requirements in their concrete dealings with one another. If one analyzes the actual use of rational ideals in scientific discourse according to a top-down application model—scientists applying general process norms in concrete situations—one often finds the invocation of normative ideals to be rather stunted, philosophically naive, and even self-serving. A different picture emerges if one views such invocations as moves in the ongoing production of

social order among scientists. What might appear from a top-down perspective as a limited and ad hoc, perhaps even self-deluding, use of one or another ideal of scientific rationality can become, from the bottom-up perspective, a move that is epistemically rational in view of the local, existing demands of maintaining orderly interaction.

In any case, ESW revises the normative status of general norms and ideals. Abstract standards, written laboratory procedures, general rules of method and the like are formulations or "accounts" that gloss the complexities of actual practice as embodied social (and material) interaction. Thus the practical sense of such ideals remains indeterminate until actors actually employ them in practice—invoke them or allude to them, often in ways that cannot be predicted in advance by inspecting the literal meaning of the ideal. Consequently, philosophical recon- structions of rules of method and the like are relevant for practice only as simplified formulations that provide participants with *partial cues* for making ongoing sense of their situated interactions—cues that, precisely as relevant, remain malleable and potentially contestable in context. General norms have action-guiding force not as general prescriptions formulated in advance of concrete situations, but only as embedded in locally situated interactions.

By working in this particularist mode, sociologists like Lynch have generated a series of detailed case studies that describe and analyze the on-site laboratory practices and conversations of scientists as they strive to produce publishable results, as they interpret one another's argu- ments, and so on (e.g., Lynch 1985; Garfinkel et al. 1981). Indeed, on Lynch's radical interpretation, no general unified theory of scientific rationality is possible, but only a series of case studies focused on broad scientific "epistopics" such as justification, confirmation, explanation, and so on (Lynch 1993, 200–1, 306). However, this anti-theoretical stance works not only against a top-down approach such as Habermas's, but also against any normative argumentation theory that aims to provide standards for the critical assessment of claims. Although Lynch is not a rule-skeptic, his particularism could be read in an acritical, relativist fashion (but see Lynch 1997a; 1999). In the next section I propose some broad framework assumptions that allow the critical argumentation theorist to incorporate particularist insights without forfeiting critical aims.

4. A Framework for Context-Sensitive Argumentation Theory

To incorporate ethnomethodological insights into a critical argumentation theory, one must deal with the anti-theoretical, potentially relativist and

acritical implications of the particularist stance as ethnomethodologists interpret it. To address this challenge constructively, I propose a framework that (4.1) revises the status of process idealizations, (4.2) acknowledges their limits for explicating good process, yet (4.3) does not fall into relativism.

(4.1) Ethnomethodology suggests that we can incorporate greater context-sensitivity into high-level process idealizations if we conceive them as possible recurrent discursive moves whose determinate sense— the meaning for the actual practice of science in a specific locale (laboratory or research team) and context (research domain)—depends on the demands specific to the locale and context in which the ideals are actually invoked. For example, the idea of inclusiveness, that anyone capable of making a relevant contribution should not be excluded from discourse, remains uselessly indeterminate until one identifies (a) a specific individual or group that (b) in a specific discipline has been arbitrarily and systematically excluded, but that (c) is capable of making a relevant contribution. The last specification (c) in turn requires both evidence of competence and a determination of relevance.

Each of these specifications requires argumentation in its own right that goes beyond simply invoking the bare norm of inclusion. To establish a contribution as relevant, one must delve into the substance of the science itself. Similar demands arise with the norm of equality: besides identifying the subordinated group, and so on, one must provide a convincing measure of equal voice. None of these moves is given in the abstract idealization but requires further dialectical argumentation and close reflection on actual practices. Such efforts are necessary to overturn entrenched presumptions that the status quo sufficiently approximates standards of reasonableness, that existing exclusions and inequalities do not violate good process but are justified by the lack of ability in the excluded parties to make relevant contributions. To be useful for critical rather than conservative purposes, then, process standards must be explicitly invoked in a rhetorically effective manner that makes sense in the specific context (cf. Cramer 2003; Prelli 1989). As an example, consider the concern for greater participation of women in science. To a large extent, this concern issues from moral demands, or equity requirements, such as the demand for equal job opportunity (see Wylie 1997). But here I am interested in the *epistemic* function of ideas of inclusion and equality, that is, the role they play in cogent scientific argumentation. To the extent to which epistemic considerations motivate the drive for more women in science, we should find advocates of inclu- sion arguing that excluding women from science undermines epistemic goals. To make this case, one cannot simply invoke abstract ideals but

must reverse traditional exclusionary views by arguing that women are equally competent and can indeed enrich scientific understanding.[9]

Examining the literature on women in science (see Schiebinger 1997; 1999), we find that proponents of inclusion have in fact made such arguments, albeit along different lines. At one end of the spectrum are those who champion distinctive "ways of knowing" for women. For example, the undeniable association of the entry of women into primatology with the critique of masculine bias and enriched understanding of female primates has been attributed to the distinctive ability of women scientists to empathize with female primates.[10] Others have argued that positioning in a marginalized group, or explicit commitment to feminist goals, makes women (or anyone similarly positioned or committed) more sensitive to dubious background assumptions in the dominant scientific paradigm. On this type of approach, specifically feminist political-cultural perspectives, rather than gender or sex as such, constitute the epistemically relevant parameters for inclusive science (e.g., Harding 1986; Longino 1990).

More prosaic arguments are also available. At the very least, one can argue that excluding women impoverishes science simply by reducing the pool of talent. Or one might notice gender-associated tendencies. A study by Alison Wylie (1997) suggests that women reshaped archaeology not so much because they brought an explicitly feminist perspective with them, but simply because they had an interest in gender questions; this led to different research agenda rather than to a radically different kind of archaeology. Joan Gero has made a sociological argument against segregation and inequality in archaeology. Specifically, she links the poor representation of women archaeologists in research on Paleo-Indian hunting practices with a tendency of scientists in that area to disregard relevant findings in other areas (edge-wear analysis) in which women archaeologists tended to specialize (Gero 1993; see Wylie 1997, 84). Gero's analysis thus suggests that the uneven gender distribution within different areas of archaeology has fostered an impoverished understanding of paleocultures, specifically a view that associates early cultural development primarily with developments in hunting technology.

My first concern here is not so much with the tenability of the above arguments; indeed, each of these feminist approaches has been subjected to criticism. Rather, I emphasize the historical fact that the feminist call for inclusion was linked with substantive epistemic arguments—arguments that based the call for inclusion on detailed sociological analyses linking male dominance with bias in a field, that appealed to role models of outstanding women scientists (e.g., Jane

Goodall in primatology), that invoked evidence for the topical relevance of the specific contributions of women scientists, and so on.

The feminist philosophy of science suggests the general point that process idealizations have a determinate sense for actual practice only as they are *rhetorically usable and effective* in specific contexts for specific epistemic purposes, and in connection with substantive dialectical arguments that debunk existing models and establish alternatives. This has at least two implications. On the one hand, ideals of inclusiveness, equality, and noncoercion in scientific argumentation refer not to some ideal universal audience but always to specific features of a specific institutional arrangement in some particular domain or locale (cf. Tindale 1999; Crosswhite 1996). On the other hand, one cannot simply appeal to process idealizations as one would to norms with a universal legislative force that directly applies to concrete cases. Process ideals are indeed normative, but in the manner of potential rhetorical sites for critical interventions into existing practices and conventions in the science community.

To render process idealizations more context-sensitive, then, I suggest we view them as enduring sites of contest and reflection in social life—potential questions or rhetorical *topoi* that in principle remain open to contest and thus can never be disregarded by practitioners as finally settled. Who is admitted, who is excluded, what counts as equal voice, how coercion differs from legitimate constraint are questions that are always potentially up for discussion, and whose resolution requires detailed argumentation focused on the specific features of the relevant area of inquiry. Indeed, questions such as these can arise for any social order whose constitutive practices (and membership conditions) depend on shared normative expectations. Insofar as members regard a social order as legitimate, they presuppose the operative answers to such questions are adequate.[11]

Thus to refer to process idealizations as rhetorical *topoi* does not so much deny their status as pragmatic presuppositions as specify it: (a) process idealizations represent enduring (potentially normative) issues of social organization, including the social organization of scientific practices; (b) they are rhetorically available insofar as the corresponding vocabulary and cultural tradition provides thematic possibilities and historical precedents; (c) they have actual normative force for existing practices only insofar as practitioners can make convincing arguments for their context-specific relevance by linking them with substantive considerations connected with the context at issue.

(4.2) The first framework assumption above is primarily a way of thinking about process idealizations. Though inspired by the particularist

challenge, the assumption as such remains at some distance from the actual research of ESW itself, which requires detailed analysis of real conversations involving scientists who invoke process idealizations or something similar. I thus propose the assumption as a hypothesis for interdisciplinary study involving ESW and argumentation theory. The second assumption has a similar status: process idealizations do not capture all the epistemically relevant features of scientific argumentation as a social process. One must allow for further social-psychological and institutional aspects of rational sense-making in science that resist subsumption under these idealizations.

This assumption rests on an observation about the local pursuit of science. Scientists can pursue epistemic aims—can contribute to the production of reliable public knowledge—only insofar as they can satisfy the demands of social order at the local work site and within the discipline, that is, only insofar as their language and behaviors make sense to their interlocutors and their individual efforts sufficiently mesh for research to proceed.[12] These local and discipline-specific arrangements generate context-specific demands of social order—demands defined not only by local laboratory routines but also by particular individuals, with their various quirks and biases, strengths and weaknesses, institutional statuses, capacities to command resources, and so on. The pursuit of knowledge exposes these particular social arrangements to pressures of innovation, rendering them labile in ways that elude explanation from the standpoint of process ideals. Thus we should expect, at the local level, a range of discursive rationalities, strategies and tactics, tailored to the particularities of a given social constellation, on the one hand, and the emergent epistemic possibilities, on the other.

To get a sense of this particularistic dimension of argumentation, consider James D. Watson's account of his experiences as a graduate student hot on the trail of DNA (see Watson 1968). Even if the historical details of that account are open to question, the kinds of local interpersonal challenges Watson faced are familiar. His account shows that successfully mounting an effective argument in science—in this case, the argument for the double-helix structure of DNA—requires the scientist to manage the various social opportunities and obstacles presented by the actual personalities and local conventions that make up the existing social order. Thus Watson and his colleague Francis Crick had to deal with the British convention that one ought not to barge in on a problem area on which another scientist had been working. To pursue their epistemic aims, they not only had to develop their own working relationship but also had to forge alliances with sympathetic scientists in

secure institutional positions, and had to win over or circumvent opponents.

Such moves clearly form an important part of the actual practice of inquiry and argument-making, yet they do not easily square with process idealizations. Some discursive moves might serve more to cultivate a friendly relationship than advance the argument as such; in other cases, the necessary maneuvers might involve more bargaining and the trading of favors than rational discourse. Thus one might spend more time trying to convince a powerful senior scientist who is out of touch with current developments but who commands important institutional resources. Idealizations like equal voice and freedom from coercion hardly appear adequate as guides for negotiating the multifarious discursive demands of actual scientific practice at the local level.

But what might appear as anomalous from the standpoint of idealizations are, in the given context, epistemically rational, precisely because the pursuit of knowledge can proceed only as an ongoing production of social order, that is, a process in which multiple personalities, working under particular social-psychological and institutional demands, must be able to make sense of what they together are engaged in achieving. This sense-making, I suggest, includes the requirement that each scientist be able to make sense of him- or herself as an individual scientist with a given training, expertise, professional status, and career path, who can work with other individuals on common projects.[13] As a result, the demands on argumentation as an orderly social process shift with each group of individuals, often in ways that confound normative proposals and that cannot be assessed in advance of a detailed examination of the particular situation.

Using Habermas's multidimensional conception of reason, we might account for maneuvers such as Watson and Crick's in terms of a strategically rational orientation, which actors adopt when they treat one another more as obstacles or aids for realizing their individual ends than as dialogue partners in a joint venture. On this interpretation, the Watson/Crick case illustrates the strategic implementation of epistemic aims at a microsocial level. From a normative epistemic perspective, we would presumably evaluate the local history of such a case by asking whether the particular strategies and maneuvers furthered or subverted the process of inquiry.[14] If we take the Habermasian analysis of credit as a model (section 2 above), then we might regard strategic violations of ideal process as legitimate so long as these were *temporary and necessary* to advance research. (The moral acceptability of violations is also relevant, but here I focus on epistemic legitimacy.)

My remarks in section 3 indicate that a critical theory informed by ESW leads to a different kind of assessment. Rather than start with a sharp distinction between strategic and discursive reason—as though these categories enjoyed a kind of jurisdictional authority, legitimated by a priori philosophical analysis, over the case at issue—one starts with a close descriptive analysis of the various ways that the actors in the local situation go about the execution of the various tasks necessary for their research. This approach presupposes the analyst's own mastery of laboratory talk (at least to a point), but the relevant categories for analysis and critique arise, in the first instance, from the actors themselves—what their talk and interactions, as perspicuously described and closely analyzed, reveal as their practical concerns. Although we should not be surprised if actors actually invoke categories such as Habermas's, the initial descriptive analysis does not impose such categories on the data, as concepts "lying behind," or presupposed by, the actors' described behaviors. For example, in his analysis of consensus in laboratory work, Lynch (1985, chaps. 6–7) does not start by seeking to uncover an underlying or tacit consensus among the scientists he studies—an approach subject to the danger of reading into data the sociological theory one wants to test. Rather, Lynch studies transcripts of shop talk for actual methods and expressions of reaching agreement.

When ethnomethodologists bring these descriptive techniques to bear on the normative dimensions of practice, they often find a significant amount of creative personal agency in the ways that actors orient themselves to norms and ideals (see Heritage 1984, chap. 5). Rather than simply adhere to norms mechanically, actors often use one another's awareness of shared norms as something like the glue that holds a set of behaviors together as an intelligible (or "orderly") interaction. This glue connects a rich pastiche of interactive moves—justifications and objections, apologies and excuses, praise and blame—which are pasted together, often in an ad hoc manner as dictated by the contingent confluence of various individual and collective interests, goals, and needs. Thus the precise constellation of behaviors that actually transpire does not follow simply via methodical application of norms, but through goal-directed choices made by individuals who regard each other as mutually accountable for the normative adequacy of their choices.

The foregoing observation implies that strategic decision making—the instrumentally rational pursuit of ends—and normatively legitimate process interpenetrate in a way that belies the Habermasian presumption that strategic rationality poses a *problem* for discourse until proven otherwise before the court of ideal process. Rather, ESW has us ask, first of all, about what *counts for participants* as problematic strategic

behavior, and that will vary depending on the particular context and interpersonal demands on local order.

More positively, ESW opens up the analysis of local maneuvering to a richer range of considerations than we find in the idea of strategically implementing epistemic aims through means whose legitimacy is assessed in relation to process idealizations. In closing this subsection, I suggest one avenue for developing this idea further. If the pursuit of epistemic aims depends on maintaining a productive social order at the work site, then participants must satisfy a range of interpersonal demands and needs, some of which are quite specific to the particular individuals involved. This level of sense-making and social order, I propose, involves *particularistic* modes of solidarity in epistemic aims, a kind of scientific friendship that is productive of good science. Watson and Crick seem to have enjoyed such a relationship, and it proved productive to their joint inquiry; Rosalind Franklin, unfortunately, seems to have suffered from a lack of collaborative support.[15] The twin categories of strategic pursuit of individual goals, on the one hand, and the leveling equality of universal process idealizations, on the other, cannot do justice to this aspect of scientific practice. The former category is too cynical, and the latter is too optimistic.

(4.3) Habermas's approach to the evaluation of social practices draws its critical force above all from process idealizations. The two assumptions inspired by ESW, however, contextualize the operation of process ideals in a way that seems to rob them of critical power. The first assumption construes the ideals as rhetorical possibilities whose force depends on substantive features of the argumentative context; the second assumption holds that descriptive analysis of the particular case should provide the relevant categories for analyzing and evaluating that case. It seems, then, that critical theorists who take ESW to heart must hesitate to bring any set of evaluative categories into a local assessment, unless the participants themselves actually employ those categories in their rhetoric and practice. If so, then the proposed framework relativizes process norms to the point where they no longer function as critical standards that hold across different contexts of inquiry. Does each context now have its own concretely situated methods of sense-making that are insulated from outside critique?

I think not. Consider again the manner in which process ideals work rhetorically. Although the ideals must be contextualized in relation to substantive considerations if they are to be rhetorically effective, they have a general linguistic form. That is, normative idealizations such as "opportunities in science ought to be open to any competent person, regardless of race or gender" have a general semantic structure that

glosses the details of local practice. As such, these ideals are easily packaged for travel across context-boundaries. Thus the same norm or idealization—as a verbalized "account," an abbreviated history of argument—can find employment in the constitution and critical alteration of *different* practices. Ethnomethodologists have made a similar point for written laboratory procedures, disciplinary standards, mathematical proof-accounts, and the like: in working up research proposals and experimental arguments, scientists are oriented from the start toward broader audiences of potential critics (e.g., Lynch 1985; Livingston 1995 and 1999). In most of these case studies, however, the critical standards at issue tend to be technical ones specific to a disciplinary practice. More relevant for the present context are those arguments that invoke broader process ideals to justify a local argumentative procedure.

The NAS case study alluded to above provides an example. In presenting its reports as authoritative, NAS takes pains to defend its procedural regulations in terms of process idealizations that support the objectivity of its findings. Thus, in the October 1981 Status Report for its study of diet and cancer, the NAS committee appealed to the competence of members as well as to the range and balance of the committee's composition, which was to "ensure comprehensive coverage of the scientific literature and to provide a broad perspective to the committee's conclusions"; in addition, the report cited extensive consultation (quoted in Hilgartner 2000, 47). The committee clearly wanted its discourse to appear inclusive of the relevant discursive perspectives. However, as already mentioned, the discursive process remained confidential before publication of the report. When outside critics challenged confidentiality, they appealed inter alia to the possible benefits of "unexpected insights volunteered by the interested public"—in effect, to a broader notion of inclusive discourse.[16] The NAS reply emphasized another process idealization, namely, the "independence" of the discourse from outside pressures (Hilgartner 2000, 55–9). The upshot of this debate was a revised set of NAS procedures, which opened up the process in certain limited respects.

If the internal committee discussions represent a kind of local context, then we can see how process idealizations can function across contextual boundaries to open local discourses to outside criticism and change. On the one hand, the NAS procedures, in particular confidentiality requirements, protect the local production of social order among committee members. That is, confidentiality grants committee members a certain independence from outside pressures as they work through the messy interactions involved in the process of making sense of one another's positions. On the other hand, the NAS committee

realizes it must publicly justify its procedures to a broader audience, and it takes pains to do so in its Status Report. It therewith anticipates the possibility of outside procedural critique. The justification itself appealed to process ideals: inclusiveness (comprehensive coverage and balance of perspectives) and, after the debate over procedure actually broke out, freedom from coercion (independence). Interestingly, critics of confidentiality appealed to similar idealizations: not only to the benefits of a broader inclusiveness but also to the concern that a closed process would *not* be independent of influence by special interests (see Hilgartner 2000, 58).

This case shows how outsiders can legitimately and effectively invoke process standards to criticize a local scientific discourse. That does not mean that such standards enable critics to dictate in advance, from the outside, just how the participants should *receive* the criticism and draw the appropriate practical conclusions for their local context; process standards do not function as laws whose jurisdictional force over local contexts merely awaits effective enforcement. Rather, as commonly available rhetorical touchstones, such ideals open up different local contexts of argumentation to mutual criticism and exchange, the results of which cannot be deduced from the content of the idealizations alone.

In conclusion, I have attempted to indicate how ethnomethodology might inform a critical theory of scientific argumentation. Compared to Habermas's discourse model, the key shift in the proposed framework lies in the status of abstract process standards, conceived now not as rules to which all scientists everywhere and always should adhere, but as glosses on actual practices—general cues that have relevance for cogent argumentation insofar as participants can use them in the selective, context-specific ways I have described. I have also argued that such ideals cannot fully account for all the epistemic features of local processes of inquiry and argument in science. Besides such general ideals, good process at the local level depends on concrete relations of solidarity. These contextualizing shifts, however, do not entirely rob process ideals of their critical force. Rather they allow us to articulate idealizations with a closer attention to their context-specific modes of employment. As so informed by ethnomethodology, critical argumentation theory can contribute to a non-relativistic, contextually relevant understanding of scientific discourse.[17]

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Notes

1. Habermas sometimes distinguishes ideals from idealizations, but here I use the terms interchangeably, in both cases as referring to conditions that can never be empirically guaranteed to obtain, as I explain in section 1; in view of Habermas's understanding of their normative character, I also sometimes refer to these as "standards" or "norms," though as we shall see in section 4 this usage must be qualified.

2. In this context, "validity claim" does not refer to the formal-logical concept of truth-preserving argument form, but rather is a translation of the German *Geltungsanspruch*, which has a broader meaning, with connotations of what holds as true, or as intersubjectively binding, etc.

3. In relation to democratic deliberation, Bohman (1996) distinguishes Habermas's conception of discourse from the idea of dialogue, which he understands to include elements of compromise; however, if one examines dialogical theories proposed by argumentation theorists, the affinities with Habermas's approach are unmistakeable (Eemeren and Grootendorst 1992; Eemeren et al. 1993; Feteris 2003).

4. Unlike some Anglo-American philosophers, Habermas does not draw a sharp distinction between "reasonable" and "rational." I thus use these terms interchangeably in this essay.

5. For the case of political discourse, see Blaug (1999, chaps. 3–4); see also Webler et al. (2001).

6. For a controversy in which a physicist fell under ethical suspicion for allegedly sharing information with non-team members, see Staley (2004, chap. 4).

7. Habermas (1996); Habermas (1998, 44) holds that the rational acceptability of a discursive outcome is a function of the arguments in connection with properties of the argumentative process.

8. I take these phrases from Bogen (1999, chap. 2), and Lynch (1985, 58), respectively.

9. Note that exclusionary views were not automatic, but required explicit arguments when modern science was first emerging. Potter (2001) documents such arguments in the case of Robert Boyle; Schiebinger (1999, 69–72) traces such exclusionary views to the eighteenth century.

10. This particular type of argument is open to serious objections (see Schiebinger 1999, 5ff; Haraway 1986); in the specific area of primatology, the argument might also reverse the causality: as Fedigan (1994) suggests, the receptivity of primatologists to feminist critique may have fostered the entrance of women into the discipline.

11. Turner (1994) has forcefully criticized the idea of social practices based on shared presuppositions, however he is committed to a causal explanatory approach, whereas I employ a normative model in this essay; for replies to Turner, see Lynch (1997b); Schatzki (2001); Rouse (2001).

12. A further question concerns the degree of consensus required for inquiry as a process and for reliable public knowledge. Consensus is often thinner than traditionally supposed (see Gilbert and Mulkay 1984, chap. 6; Cole 1992, chaps. 4–5), and for fruitful inquiry it might not be a necessary goal (Solomon 2001). My sense, however, is that reliable public knowledge requires a higher level of consensus across a discipline, but I cannot argue the point here.

13. For the role of identity in scientific argumentation, see the literature surveyed in Golinski (1998, chap. 2).

14. Habermas (e.g., 1984/87, vol. 2; 1996) has used the concept of strategic rationality mainly to analyze action coordination in certain macrosocial structures and institutions (e.g., markets, bureaucracies,

legal systems); here I assume that a similar mode of assessment works for the microsocial level.

15. Though not awarded the Nobel Prize—which went to Watson and Crick (both working at Cavendish Laboratory), and Maurice Wilkins (working with Franklin at King's College)—Franklin provided the X-rays of DNA that proved crucial to cracking its structure. Wilkins has expressed the view that his failure to communicate with her cost them the race with Watson and Crick (see McElheny 2003, 39; see Judson 1996, Afterword I). Others have raised critical questions about possible bias against Franklin as a woman scientist (for literature, see Delamont 2003).

16. Hilgartner (2000, 58), quoting Phillip M. Boffey, *The Brain Bank of America* (New York: McGraw-Hill, 1975), 257–8.

17. For feedback on an earlier version of this paper, I thank Alison Wylie and other participants in the Sixth Annual Philosophy of Social Sciences Roundtable, St. Louis, Missouri, March 2004. I also thank Wylie for providing me with a copy of Gero (1993).