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Reading and Writing as Method: In Search of Trustworthy texts

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'The research presented here is based on an ethnography of …': a researcher who situates a manuscript with this phrase signals to the reader that the research being presented is part of a particular tradition of scholarship. The statement stimulates in a reader, including one acting as a reviewer, *expectations* about the logic that the research has followed – expectations developed and honed through disciplinary practices and the reader-reviewer's own research, writing, and reading experiences.

The specifics of these expectations have been discussed, analysed and, to some extent, formalized in the burgeoning methods literature dealing with the criteria and standards best suited to evaluating ethnographic and other qualitative and interpretive research (e.g., Brower et al., 2000; Erlandson et al., 1993; Golden-Biddle and Locke, 1997; Lather, 1993; Lincoln, 1995; Lincoln and Guba, 1985; Miles and Huberman, 1994). Meeting a reader-reviewer's expectations requires the researcher to be familiar with the standards and evaluative criteria that are accepted within the epistemic community of which both are members and to enact them in the research narrative in a persuasive and compelling manner. It is to this writing and reading duality that we attend, first laying out our review of what that methods literature suggests are the most widely used standards informing reader-reviewers' expectations, and then explicating, with organizational ethnography as an illustrative case, the specific textual elements that researchers can use to meet these standards and expectations.

In practice, evaluative criteria and standards commonly figure in the context of research design, anticipating most immediately the fieldwork stage of research and, to some degree as well, the 'deskwork' stage (analysis after the completion of fieldwork). Less attention has been paid to the role these criteria play in the 'textwork' phase – the crafting of a persuasive manuscript (Van Maanen, 1996; Yanow, 2000) and its dissemination to reader-reviewers of various sorts[1] – because to the extent that writing is considered at all as a method, it is often conflated with the analytic stage of research. Although parsing research into stages in some ways oversimplifies – analysis, after all, begins with the development of a research proposal long before one enters the field, and it continues as one writes – it is useful to

distinguish between deskwork and textwork in order to focus attention on this latter, neglected aspect. Of course, achieving persuasiveness in a research manuscript depends not only on the character of the writing itself, but also, importantly, on the quality of the prior execution of methods in the field. The proverbial 'silk purse' of a compelling narrative cannot easily be wrought out of 'sow's ear' field research: reading, writing, and research are, in other words, at this evaluative point, intertwined.[2]

Our own approach to ethnography falls within the community of scholarly practice that takes a constructivist-interpretive approach. Methodologists are increasingly distinguishing between 'qualitative' methods that are informed by positivist ontological and epistemological presuppositions and 'interpretive' methods that are informed by constructivist-interpretive ones. As the former increasingly encompass such things as focus groups, Chicago School-style field research is increasingly being referred to by the latter term (see discussion in Yanow and Schwartz-Shea, 2006: xv–xix). In some disciplines or topic areas, e.g., anthropology, 'qualitative' and 'interpretive' may be used inter-changeably, because 'qualitative methods' still, in practice, includes interpretive ontological and epistemological presuppositions. In others, such as organiza-tional studies, 'qualitative methods' at times still refers to both constructivist-interpretive and objectivist-realist presuppositions. For the sake of clarity, we use the terminology of 'interpretive' rather than 'qualitative'. With the possible exception of reflexivity (see note 5), all of the evaluative criteria reviewed in the next section of the chapter are currently being applied to realist ethno-graphies as well as to interpretive ones, and the writing strategies discussed later (again, with the possible exception of reflexivity) similarly hold for realist ethnographers.

Interpretive methodologies provide conceptual grounds for understanding why research, writing, and reading should be intertwined. It is today fairly common to think about a double hermeneutic (Giddens, 1984; Jackson, 2006): that researchers interpret actors' interpretations (see also Geertz, 1973: ch. 1), a tenet of both phenomenology and hermeneutics. Drawing on literary studies, we wish to elucidate a third interpretive moment. Reader-response theory (e.g., Iser, 1989) emphasizes that textual meaning is conveyed not only through the writer's intent or the elements of writing (e.g., metaphor, word choice, rhythm); it rests also on the prior knowledge, from experience and situatedness in the world, that readers bring to their readings of texts. In this way, the interpretive *act of reading* links phenomenology to hermeneutics. Recognizing the existence of a 'triple hermeneutic' (Yanow, 2009) could lead ethnographers (and other interpretive researchers) to consider how to incorporate into their written accounts the various textual elements that address and engage the criteria by which their research will be evaluated and through which they will persuade readers of the trustworthiness of their 'truth claims'.

Trust is commonly treated as part of the researcher–researched relationship: it is implicated in the much-touted rapport that researchers work to build as

part of entrée into the field setting. Here, we engage it as essential to the researcher-reader relationship, the latter established through texts rather than face to face. Such trust is central to the conduct of science. We begin our discussion of establishing researcher-reader trust from the perspective of the reader, turning afterward to the writer. One of the questions this discussion raises concerns the responsibilities of each party to this writing-reading enterprise, which we engage in the concluding section.

This chapter assumes a research community and, particularly, reader-reviewers who, at minimum, are not so hostile to qualitative or interpretive research that they reject its scientific status out of hand. Ideally, the reader-reviewer also endorses, if not practices, such research. In reality, openness to qualitative-interpretive approaches such as ethnography varies considerably by discipline and even by epistemic community within disciplines. For example, some organizational studies subfields or journals are clearly demarcated by positivist-oriented editorial positions, including in the selection of reviewers, and there, ethnographic work is likely to be held up to criteria more appropriate for positivist science and its presuppositions. We hope that our discussion of evaluative criteria, in addition to being useful to those writing up ethnographic research, also helps inform editors and reviewers of such journals as to the standards accepted within interpretive science.

reading strategies: emergent standards for interpretive research

Historically, ethnographic and other field researchers have not always been explicit about the *sine qua non* for such research, relying instead on the tacit knowledge and common practices shared within what were comparatively bounded (and perhaps even insular) research communities. As the borders of disciplinary and epistemic communities have become more porous and both qualitative and interpretive scholars have increasingly found their work being assessed by researchers outside their own community boundaries, the need to articulate forms of evaluative standards that fit the presuppositions informing those methods has developed, with the consequent growth of that literature.

It is important to note that researcher-writers and reader-reviewers are differently situated with respect to evaluative criteria. Researchers, especially novices, are instructed by both methods texts and supervisors that these criteria matter, and several of them are typically incorporated into research designs. To state the obvious, researchers must attend to these criteria from the beginning stages of planning the research, through its execution in the field, to the final stages of analysis and writing, although the prominence or relevance of particular criteria may vary across the life of a research project. By contrast, a reader-reviewer is in the position of assessing a purportedly finished manuscript, an evaluation of the evidence it presents and the research processes that produced it as manifested in the writing.

We argue that researchers writing for readers across epistemic boundaries might write more scientifically persuasive manuscripts if they recognized that reader-reviewers are likely to be asking, implicitly if not explicitly, 'What makes this ethnographic account trustworthy?' This sort of question (see also Clifford, 1988) lies at the heart of the attitude of doubt (or, in the language of the philosophy of science, 'testability') that characterizes the scientific endeavour. At the very minimum, understanding the textual mechanisms that foster a reader's judgements of the text's trustworthiness might lead to better initial manuscripts and fewer rejections (assuming journals and reviewers who are in the same ethnographic or interpretive epistemic community as the author, or who are sympathetic to it). Greater transparency with respect to methods and methodologies might also serve to educate reviewers beyond that epistemic community. But more than that, trustworthy research is important for acts that build on that research, as is common in action research (see for example, Greenwood and Levin, 1998, and Chapter 11 by Sykes and Treleaven, this volume) and central to the more engaged research increasingly called for by sociologists, anthropologists, political scientists, and others (see Burawoy, 2004; Lamphere, 2003; Monroe 2005).

But how can such trustworthiness be achieved? What do reader-reviewers expect? Situating ethnographic research within interpretive methods more broadly and summarizing across two decades of thinking with respect to evaluative criteria that are methodologically appropriate to interpretive ontological and epistemological presuppositions, Schwartz-Shea (2006) finds that six criteria seem to be most used and referenced within interpretive epistemic and methodological communities.[3] These common criteria lead reader-reviewers in those communities to expect a *thickly descriptive* manuscript that demonstrates *reflexivity* about the researcher's roles in the field. They expect to see in the manuscript, both in its methods section and, as important, woven into its substance, the results of decisions and actions taken in each research stage: the planning and fieldwork execution of the *triangulation* of evidentiary sources; the detailed *audit* recording research steps taken during fieldwork that divert from initial research design; the use of *negative case analysis* or some similar sense-making technique during analysis; and the use of *member checking* with respect to textwork drafts.[4] Based on the relative presence or absence of these six elements in a manuscript, reader-reviewers make judgements about the overall trustworthiness of the research narrative. In the more detailed discussion of these that follows, we have numbered these expectations for clarity; but reader-reviewers likely assess research manuscripts in a more holistic, rather than 'check-list,' fashion.

1 ***Thick description*** refers to the detailed descriptions of settings, events, activities, interactions, persons, language, and so forth, in such a way as to explicate the contexts of the 'lived experiences' of the people studied. The wealth of detail conveys a subtext: that the researcher was actually present

on site as an eyewitness – in the originating case of ethnography, the 'being there' that is distinctive of such research. (Historical and archival-textual 'ethnographic' research produces similar descriptive texture; see, e.g., Darnton, 1984; 2003; Jackson, 2006.)

Detail alone does not satisfy this criterion because its purpose is not the goal of complete description – an impossibility – but, rather, a nuanced portrait of the cultural layers that inform the researcher's interpretation of interactions and events. Readers look for *sufficient* detail, as relevant to the research question: to support the claim, for instance, that what the researcher saw was a 'wink' and not a 'blink', in Geertz' famous example, borrowed from Ryle (Geertz, 1973: 6–7); or, in an example of its use in document-based analysis, the claim that 'Western Civilization' (*Abendland*) had particular, strategic meanings in post-World War II German politicians' debates over reconstruction (Jackson, 2006). Sufficiency is not an absolute quality. Meaning depends on social, political, and organizational context, and thick description imparts the specificity of that context to the reader. Readers, like writers, make situation-specific judgements as to what constitutes relevant detail in 'sufficiently' thick texts. Unlike researcher-writers, readers are commonly less familiar with the research setting being described, something that researchers should bear in mind.

2   Researcher **reflexivity** shows that the researcher understands herself as the means, the instrument, through which the research (as well as its reporting) is produced. Researchers' demographic identities (gender, race, sexuality, social class, nationality, and other components), manifested in dress, accent, physiognomy, and other elements of nonverbal communication, and other aspects of their phenomenological backgrounds (education, training, upbringing, and other elements of lived experience carried internally) contribute to a 'positionality' that can affect not only the character of interactions and research questions posed, but also access to research sites and persons in them and the kinds of data co-generated with research participants.

Shehata (2006) and Pachirat (2009) show that the effects of researcher identities and organizational locations cannot be understood *a priori* because identities are negotiated in context: participants 'read' researcher identity and power in complex and, at times, unanticipated and even contradictory ways. This is why readers increasingly expect ethnographers (and other interpretive researchers) to document the role of the self in the research process and analyse aspects of their presence, persona, and location for the ways they might have affected the co-generation of data (and their analysis as well). Reflexivity, in other words, is conducted both in the field and in the text. It is increasingly acceptable, if not expected outright, that researchers not hide behind 'third person', omniscient exposition in their accounts – the so-called 'view from nowhere' or 'God's-eye view' (Haraway, 1988; Harding, 1993) – but, instead, weave their analyses of their positionality into their textual representations.[5]

3   **Triangulation** can be understood most broadly as drawing on different kinds of sources or analytic tools in trying to understand a phenomenon.[6]

Qualitative methods texts sometimes distinguish among several types of triangulation, including multiple methods of accessing data (observation, interviews, documents – this is the most commonly understood meaning), multiple data sources (e.g., persons observed and/or interviewed, locations within the research site, times of day, week or year), and multiple researchers (such as teams of ethnographers studying a single site).[7] Multi-sited ethnography (e.g., Marcus, 1995; Hannerz, 2003) might also be seen as a form of triangulation. Methodological discussions of triangulation emphasize the complexity of understanding social settings by noting not only the extent to which data from multiple methods, sources, researchers, and/or sites present possibilities for corroboration and refutation (that is, subjecting provisional inferences to testing), but also that such multiple approaches to data are likely to bring to light inconsistent and even conflicting findings (Hammersley and Atkinson, 1983; Mathison, 1988). Readers expect to find accounts of how researchers grappled with inconsistent or conflicting findings emerging from triangulation of whatever sort. As Becker (1998: 44) argues, simplicity should be 'an empirical finding rather than a theoretical commitment' (see also Law, 2004, on not eliminating the complexity of everyday life).

4   An *audit* (or 'audit trail'; Lincoln and Guba, 1985) documents, as completely as possible, while in the midst of research, changes in processes and steps used in the conduct of the research, as if those processes might be 'audited' at its conclusion – which is what reviewers are, in a sense, doing. The researcher identifies and describes any changes to the original research design made in response to situational realities – a commonplace of ethnographic (and other interpretive) research, by contrast with the normative ideal common in survey and statistical research of a fixed/*a priori* research design. The audit's purpose is to make the linkages among researcher decisions, evidence generated, and inferences drawn as transparent as they can be, serving to remind the researcher in the midst of analysing data and field notes and writing research texts of what transpired in the field from a methods perspective. Audits themselves are private records, much as fieldnotes are; readers look for statements or other indications that an audit has been kept and for the general attitude of transparency that auditing enacts.

5   *Negative case analysis* (or some similar process[8]) challenges researchers' own meaning-making processes. As an ethnographer immerses himself in data – sifting repeatedly through, say, field notes, reflexive diaries, interview transcripts, documents, and whatever other sources of data are at hand – negative case analysis is a technique of reflective inquiry designed to prevent him from settling too quickly on a pattern, answer or interpretation. Asking himself, 'How do I know what I think I know?' and 'How would I know if my analysis missed some angle?' he consciously searches for evidence that challenges or negates his initial impressions, pet theories or favoured explanations – the 'negative' case that would require their re-examination. It both improves analysis and demonstrates to reader-reviewers

that during data analysis, the researcher actively inquired into his own meaning-making processes, not looking for confirmatory evidence alone. Interrogating members' dissenting views, acquired through member checking (discussed next), is an example of negative case analysis.

6   Whatever the character of field relationships (that is, negative *or* positive), **member-checking** means going back in some fashion to the people in the setting studied for an assessment of whether the researcher did a good job of capturing *their* understandings of their own situations. This is not just the journalistic practice of 'fact-' or 'quote-checking' (which implies that there is a singular social reality that the reporter can capture). It is, instead, a fuller recognition that lived experience – for example, insider vocabularies, positioned understandings of an event, organization or policy, tacit knowledge of these – is quite complex and the researcher may or may not have fully grasped its meanings *from members' perspectives*. Member-checking focuses researchers' explicit attention on possible differences between their and members' interpretations and calls upon them also to theorize those differences.

The charge is not necessarily to take members' critical responses at face value. The researcher may have access to information not available to (some) members that contextualizes their views in ways that they may not recognize; or she may have academic insights from research literature that lead her to frame experiential data in a theoretical context unfamiliar to situational members (discussed further below). What member-checking highlights is researchers' engagement with their own and others' sense-making. As Atkinson et al. (2003: 194) advise, 'informants' accounts should neither be endorsed nor disregarded: they need to be analyzed'.

Moreover, neither the 'getting-it-right' from members' points of view nor reflective inquiry about the 'goodness' of one's analysis posits the existence of an external, objective reality that the researcher needs to capture with precision. Instead, both turn the analytical spotlight on the potential gulf in understanding and interpretation between researcher and research participants.

From a reader-reviewer's perspective, the degree to which a manuscript engages these six elements provides grounds for assessing its *trustworthiness*. Trustworthiness has become a widely invoked and accepted umbrella term or 'meta-goal' in qualitative and interpretive research communities. It is what such methodologists posit as a more appropriate criterion than 'validity', 'reliability', and the like, as these are rooted firmly in a positivist scientific methodology that rests on the presumption of a real social world that can be mirrored in social scientific studies. That presumption renders those criteria logically inappropriate for the evaluation of research rooted in phenomenological, hermeneutic, and other interpretive methodologies (Schwartz-Shea, 2006).[9] 'Trustworthiness' addresses the broadest question asked about any research project – whether the study is deserving of readers' trust in its representations, analysis, and findings. It is a comprehensive standard for assessing the overall quality of a research study, a summary judgement on the care taken by a researcher to document and justify the many steps in the research process that

produced the research report. A study judged trustworthy may not be perfect, but its findings are worthy of being taken seriously as science. What is most notable about trustworthiness as a standard for assessment is that it places scientific research squarely in a social context, recognizing the interdependence of researcher and readers as contextually embedded, sense-making actors.

These six methodological expectations discussed in the standards literature are present in various ways in research manuscripts. Writing is thickly descriptive (#1) and reflexive (#2), or not; but the primary way a reader knows if the other four have been engaged during the research process is if the researcher says so in or through the text (thereby demonstrating analytic-textual reflexivity, beyond field-site positional reflexivity). Methods statements can explicitly indicate whether triangulation (#3, planned in research design and carried out during fieldwork), an audit (#4, implemented during fieldwork and drawn on during deskwork and textwork), negative case analysis (#5, a deskwork-based analytic process), and member checking (#6, part of textwork) were used; and those uses are manifested elsewhere in the manuscript as field data are presented and discussed. Attention to an epistemic community's expectations for procedural and manuscript characteristics throughout research, analysis, and writing processes will likely enhance the overall trustworthiness of a research text (and, hence, of the research itself) because the presence of these elements in the manuscript demonstrates the researcher's methodological awareness.

It is not our intent to suggest that these standards be used by readers evaluating a manuscript (or by writers producing one) in a checklist fashion. Aside from being too constricting, such usage would be inconsistent with interpretive presuppositions which privilege context; and so we emphasize that their readerly use for evaluative purposes needs to be appropriate to study purpose and content (as does their use in the research and writing). They provide research communities with a starting point for thought and dialogue about the quality of a particular study, where judgements themselves need to be historically contextualized and contingent.[10] Thinking about reviewer-readers' expectations may help researchers improve their writing by reminding them that these various dimensions contribute to readers' assessments of textual *and*, *hence*, *research* trustworthiness.

But whereas they inform the (trustworthy) character of a text, the six elements do not instruct on the specifics of what needs to be written to make a good ethnographic case. We turn now to that level of writing. Although we focus on organizational ethnography, the strategies we discuss are relevant to other forms of interpretive writing, as well.

writing strategies: enhancing textual trustworthiness

Knowing and understanding the expectations that reader-reviewers will likely bring to a manuscript is important to textwork, but such knowledge is

insufficient: an ethnographer cannot simply declare, 'Trust my research!' Readers' trust must be earned over the course of the narrative: it is built up in myriad ways, as noted above, following the norms particular to epistemic communities. The writer's craft consists in knowing those norms and signalling that they have been met, sometimes in rather direct ways, as in the methods section of a research report, but, also, indirectly, by manifesting them in the narrative itself. In this section we deconstruct trustworthiness by reviewing the specific sorts of textual elements that contribute to its creation. The planning for several of these is commonly discussed in a research proposal, but they need to be re-engaged in the research narrative, and their presentation there is typically not treated in methods textbooks. Here, too, our intention is not to constrict the creativity of textwork by listing these items in recipe-book fashion, but instead to heighten authors' (and reviewers') awareness of what goes into producing trustworthy texts.

The presence or discussion of eight elements conveys clearly the ethno-graphic character of a text and contributes to persuading a reader not only of the trustworthiness of the text, but of the research itself and its truth claims: *access*, *place/space*, *time*, *exposure*, *researcher role*, *silences*, *data details*, and *data representation* (Yanow, 2009). The first six elements enact a key charac-teristic and strength of ethnography: that it is built on and acknowledges the material dimensions of life – the ways in which human experience is situated, time bound, embodied, and partial. The researcher moves through, in, and about the organization (or other setting) for a limited period of time, inter-acting with organizational members who themselves have bounded views and understandings of their own worlds. The final two elements engage the materiality of the research text, reflecting the central role of symbols and meaning-making in ethnographic writing. How (and whether) organizational members talk to each other, how they conceive of organizational identity, membership, and activities, and how all this is (re)presented by the ethnog-rapher are at issue in an ethnographic study. Together, these eight elements demonstrate the ways in which an ethnographic study consists of more than a set of interviews, although 'interviewing' – often described more commonly as 'talk' than as formal interviewing[11] – can be a central method in organiza-tional ethnography, along with participant-observation and document analy-sis. They also demonstrate the systematicity required of ethnographic research and writing, as well as the ways in which single setting studies constitute more than a single 'n' observation.

A. the material dimensions of organizational life: research design in the text

1   Organizations can be studied from the outside, but organizational ethnogra-phy rests on *access* to their 'insides', to the meaning-making of organizational members in particular settings and contexts, as these are manifested in relation-ships both 'outside' and 'inside' the organization: the formal organization of

hierarchies and position descriptions and relationships with the environment, as well as the water cooler conversations, corner offices, informal memos, and internal rituals that shape organizational identities and membership (see also Chapters 1, 7 and 8 in this volume). Readers want to know the details of initial access – permissions granted, limitations, personal contacts. A feature of organizational (and other forms of) ethnography is the extent to which initial access is sometimes built on the researcher's prior experience and knowledge. Researchers often select and enter their settings because they spent time there in years past or have family ties, linguistic or some other personal connection to the place or activity they are studying.

However, the proverbial 'foot in the door' is only part of what access entails, because entrée can vary within organizational spaces (for example, from one department or office to the next) and across its occupational specialties and hierarchies (see for example, Pachirat, 2009), as well as over time. There is a 'relational turn' in ethnographic methodology that recognizes that access is about establishing relationships (see Feldman, Bell, and Berger (2003) and Chapters 10 (Beech et al.) and 4 (Down and Hughes), this volume). But access also involves developing and maintaining these relationships. Readers want to know not only what measures were taken to gain access but also to sustain it – those participatory efforts such as volunteering for assignments, treating others to coffee or beer, or even babysitting the kids – as well as when access was not obtained or was truncated, and why.

In research conducted from an interpretive methodological perspective, one of whose ontological and epistemological presuppositions is that 'objectivity' – the ability of the researcher to stand outside of the subject of study – is not conceptually possible (see Bernstein, 1983; Hawkesworth, 2006; Yanow, 2006), there is no reason to hide any of the details of gaining and maintaining access. Aside from that, efforts to hide the fact that a setting was chosen because the researcher had prior knowledge of organizational activities or knew people who could facilitate entrée often leave narratives feeling 'thin' or 'inauthentic', thereby weakening the trustworthiness of the analysis. The more explicit and transparent the explication of initial and ongoing access, the more trustworthy the research is likely to be – or at least, the less it will appear as if the researcher is trying to deceive the reader. Keeping a daily record of methods choices – an audit – from the very beginning of the research project, in addition to substantive field notes, can provide detail indispensable to the textual demonstration produced later of the ethnographer's commitment to transparency.

2   Ethnographers' research settings are distinctive; and, thus, a central feature of ethnographic writing is its extensive description of the *place* or *space* in which the research was conducted, as well as its context – that is, not only what the inside of the organization looked like, but also how it is situated in its historical, national, 'industry', and/or neighborhood 'space'. Ethnographers make choices concerning place or space: what kind of industry or organization, or which department within an organization, or what set of

organizations within an organizational field, is likely to provide the best setting for exploring the theoretical matter under investigation? What is the best kind of neighborhood or community, region or state within which to address the research question? Which level of government, which agency, which section, which department? Sometimes choice of place is constrained by difficulties in obtaining access, and serendipity and coincidence may also enter the picture; but the presence in the written report of deliberations concerning the selection of settings, the rationales behind the choices made, and the circumstances of the serendipities can help to situate the analysis and, in some cases, demonstrate reflexivity about the tacit assumptions of the researcher's epistemic community (see notes 5, 10). Detailing how various organizational members enact boundaries ('inside' versus 'outside'), contents, and contexts can contribute to a thickly written text.

This thinking extends to the choice of persons for formal interviews and other talk. Here, 'space' may metaphorically mean the organizational level or the departmental location within the organization's structure or the political or communal role of the person being interviewed, although it can also refer to choices of interview settings. For example, is this person likely to be more comfortable talking with me if we meet away from her workplace or his regular 'hang-out'? Will I get different information if we meet in a club setting where I might also be eligible for membership (see for example, Wilkinson, 2008)? For documentary aspects of an ethnographic project, space would refer more to the location and choice of archives, newspaper morgues, and the like in terms of availability of certain materials, access to particular files, and lack of access to others. Detailing the rationale for choices made can help a reader evaluate the trustworthiness of the subsequent analysis and can demonstrate the researcher's efforts to triangulate the phenomenon of concern (whether by organizational location in the hierarchy and/or types of evidentiary sources).

3   Organizational ethnographers engage *time* in at least two aspects. First, length of time in the field is the most basic information expected by readers. Too often, however, researchers neglect to make clear what constitutes 'a day' in the field – was this 9–5, and then the researcher went home? Or did research continue after that at the corner bar, at dinner, at an evening party? Another neglected facet of time in the field is the 'periodicity' of observation – how often and with what regularity did the researcher interact with research partic-ipants? Knowledge claims based on ten days' immersion in the research field – say, one 8-hour day a week for 10 weeks (8 hours × 1 day × 10 weeks; i.e., 80 hours; 10 days) – are likely to be seen as less persuasive than claims based on daily encounters for the same time period (that is, 8 hours × 7 days × 10 weeks; i.e., 560 hours; 70 days), let alone for six months or a year (with whatever periodicity), but perhaps more persuasive than ten 4-hour days over the same period (4 hours × 1 day × 10 weeks; i.e., 40 hours; 10 'days').

Second, time needs also to be considered in terms of possible organizational rhythms. The 'DDT' of observation can be essential to such understanding of organizational life: over what *Dates* (seasons) of year was the project conducted,

which *Days* of the week, what *Times* of day? To see why these might be significant, imagine a field research project assessing decision-making processes in an organization whose normal work takes place during business hours on weekdays from September through July. Spending time on location in August, on weekends, or before or after customary work hours would likely produce different understandings than research conducted at other dates, other days, other times. Prior knowledge may make this point obvious to a researcher; yet what is obvious to 'insiders' may not be so to 'outsiders', so that recording DDT details (for example in an audit) can help the later crafting of a convincing methods narrative.

4   Attending to organizational rhythms can help a researcher tune into the timings of observations. Similarly, bringing time and space together – *exposure* – can reveal other aspects of organizational activity. Think of exposure to the sun: maximizing it depends not only on the time of day, but also on where one is standing (vis-à-vis shade) and located (vis-à-vis the equator). In research this can be achieved, metaphorically, by 'mapping' the organization or part of one: maximizing coverage of research-relevant managerial, professional or other occupational bases; of perspectives within a single department; of departments in an organization or a horizontal slice or regional arm of one; or of both vertical and horizontal swathes through it. In these ways, exposure to the different 'territories' of an organization or its constituent parts can reveal their varying rhythms and lived experiences: CEOs whose daily actions are affected by stock market fluctuations, whereas front-line workers' acts reflect clients' changing, perhaps seasonal, demands; state university presidents marking time by legislative schedules, whereas professors and staff mark time by the comings and goings of students and grading deadlines over the course of the academic calendar; middle managers struggling to meet the expectations and needs of both superiors and subordinates. Exposure to various organizational spaces, literally and figuratively, across time typically leads to greater, and deeper, insights and understandings than what can be achieved by 'parachute' ethnography.[12]

'Purposive exposure' or 'snowball exposure,' or a combination of the two,[13] in designing interviews and conversations can lead to 'saturation' as one begins to hear the same information in response to substantive questions or the same names in response to the question, 'With whom else should I be speaking?' (but see the discussion of silences, below). Influenced by a phenomenological position that interpretation and ensuing action reflects situated, lived experience and/or a hermeneutic position that interpretation and action reflect the unspoken, common sense, tacitly known 'rules' at work in various communities of meaning, ethnographic interviewing (Spradley, 1979; Rubin and Rubin, 2005) is commonly undertaken in ways that attempt to map the full terrain of the topic of research, as noted above, seeking to garner exposure to viewpoints around the spectrum of opinion, experience, expertise, and so forth. This exposure approach enacts the criterion of triangulation, a point that can be noted in the methods section of a research narrative.

5   As ethnographers move through their organizational sites, they should also consider various aspects of their *researcher role* – how they present who they are and what they are doing and, importantly, the extent to which they emphasize or downplay their role as researcher. Some of this may be determined by the conditions under which they gained access – that is, as a welcomed partial insider or as a reluctantly admitted stranger. Ethnography entails participant-observation, with researchers choosing to position themselves at one or another point along a continuum. At one end, the researcher participates and observes in the role of researcher alone. At the other end, the researcher participates out of a situation-specific role crafted for the research, all the while still observing others, as well as herself, in her researcher role. In organizational ethnography, a researcher may combine various points on this continuum over the life of the research project, sometimes becoming the compleat researcher-observer when interviewing, e.g., the CEO, at other times turning to a more situational participant-researcher role.

In the latter case, the ethnographer observes himself, as well as others, in a constant awareness of his researcher role and research purposes (Gans, 1976). Van Maanen's work on the making of policemen (1978) is a case in point. Having trained in the police academy with new recruits, he then rode along with 'real' police officers. In a classic example of reflexivity, he writes of one occasion in which he was faced with having to choose whether to act out of a police officer role (which, as a non-sworn, he could not legally do). In his researcher role, he reflects on his own responses in that situation, incorporating them into his interpretations of how a new officer constructs his police role.

This dual role is central to participation-focused ethnographic research.[14] The more readers know about a researcher's situational and research roles, the circumstances under which she chose or acquired or developed them, and any advantages or problems presented by a situational role (e.g., role conflict), the better able readers are to evaluate evidence and truth claims. Discussion of researcher role is also a logical textual place to display the sort of positional reflexivity that is increasingly expected, e.g., the ways in which one's personal characteristics affected the extent to which organizational members marked one as suspicious or as a person to be trusted. For example, Lin (2000) describes how being a young, Asian-American woman affected the ways she was received by the men she sought to interview in prisons; Shehata (2006) relates how shop floor workers thought him a spy working for managers at the top of the hierarchy until other workers vouched for him.

6   The systematic process of mapping the materiality of the organization often attends most strongly to *presence* – that which more active or vocal research participants have accomplished or think. But organizational ethnographers need also to attend to *absence*, to *silences*, those positions and views that may have been silenced by others or are, perhaps, silent by choice. Snowball exposure, for example, builds on relational networks, and networks, by definition, are selective in whom they include. Furthermore, the active, vocal opposition may be more easily discoverable than the voice of those who have chosen metaphoric 'exit'

(Hirschman, 1970) or who have been pressured or outright forced to be silent. 'Saturation' could be an opportunity to begin inquiry, focusing attention on what one might *not* be hearing. Researchers may have to exert effort to identify oppositional voices outside of such networks, and readers want to know that the researcher has thought about whether there might be silent, and silenced, voices and, if so, what efforts have been made to identify, include, and analyse them.[15]

Drawing attention to silences is a significant way to enact in the narrative a number of readers' evaluative criteria. It can reveal the reflexivity of the ethnographer, for reflection calls into play an introspective contemplation of whether one has been caught up, exclusively, in the voices of active organizational members.[16] It can be part of negative case analysis, demonstrating awareness of the connections between observed patterns and the tenor, tone, and strength of particular voices, opening the door to reflective inquiry concerning other organizational actors who might be articulating views that 'negate' what one has been predominantly hearing. It can be part of triangulation across methods, when, for example, a particular voice is absent in observed or reported conversations, but revealed to be powerfully present in a public relations document.

Demonstrating in the text the ethnographer's attention to *place* and *space*, *access*, *time*, *exposure*, *researcher role*, and *silences* provides grounding for a number of the evaluative criteria discussed in the previous section: *thick descriptions* of settings, persons, events, and interactions; the awareness of where evidence came from that is useful for both *triangulation* and *negative case analysis*; a sensitivity to the complexity of researcher role that is essential to *reflexivity*; the hierarchical or other location of organizational members for *member checking*; and the changes in research design – from initial access to the moment of exit from the field – that comprise an *audit*. Some of these – especially place/space and data details, as discussed below – contribute to thickly described texts; others – time and exposure – contribute to thickly done field research. Taken together in an overall gestalt, clear statements about whatever of these elements is relevant to the study provide essential context that readers need to assess a project's truth claims. The completeness and detail provided – its evident systematicity as well as the transparency with which this is done – will increase readers' confidence in the narrative and, hence, in the research reported. These elements are interactive and study-specific: a researcher reporting having based research on three years in the field, for instance, may not need to provide details on daily engagement – an example of the limitations of using these elements in a checklist fashion, rather than making research-specific judgements.

B. the materiality of the research narrative: the text itself

These first six aspects of ethnographic research – entering a collective entity, moving through organizational space and organizational time aiming for exposure, and presenting the self to others whose lives are entwined with

activities located specifically *there* – emphasize the materiality of ethnographic research into organizational life. Coexistent with this material experiential space is the material space of the research text – the words and symbols that are used, or not used, to make and (re)present meaning about organizational activities, identities, and values (on this, see also Van Maanen, 1995). The narrative crafted in textwork constitutes these sorts of organizational aspects for readers.

7   The *data details* provided in the narrative construct the thick description that has become one of the most discussed aspects of ethnographic writing. The textwork challenge here is to engage readers with the details *that matter*, and to do so in a way that presents complexity, ambiguity, and nuance without losing readers or trying their patience! (On detail run amok, see Wolcott, 1990.) The six elements discussed above contribute to 'thickly done' field research; recording them builds 'thickly written' field notes; and both of these can result in thickly described texts that help readers trust that the analyst was there and understands the organization under study. As noted in the earlier discussion, the point is to detail the context(s) of event, action, person, and so on such that a reader can follow the inferences drawn.

8   Presenting in two-dimensional narrative the variety of the 'stream of experience' that is everyday organizational life is challenging. Moreover, critics have recently argued that thick description is not well understood, implying that it is naively used (see Atkinson et al., 2003, 114). For these reasons and also to stimulate authorial and readerly imagination, organizational ethnographers would do well to consider the full range of forms of *presentation* of their descriptions and analyses – from the basic graphic layout of the manuscript page (margins, font sizes and enhancements, set-offs, 'negative' [blank] spaces, numbered or bulleted lists, etc.) to various kinds of visual presentations of data, such as charts, diagrams, maps, photographs and, with internet publishing, videos (as applicable; e.g., organizational charts depicting the hierarchical relationships of authority and responsibility; flow charts diagramming Standard Operating Procedures).[17] Researchers can reflect, in writing, on the limits presented by textual media for communicating understandings of their organizational tales. They might even ponder how triangulation in publishing could open new vistas for communication as the inter-textuality of global conversations (Weldes, 2006) increases with the spread of the internet. And, finally, further complicating the task of representation and meriting space in the research text are considerations of privacy, ethics and, increasingly, policy demands made by various national human subjects protection regimes in both the private and public sectors (Yanow and Schwartz-Shea, 2008).

member-checking redux: from a writer's perspective

Because member checking is a complex endeavour (as noted by, e.g., Miles and Huberman, 1994: 275–7; Emerson and Pollner, 2002) and because so little

has been written about how a writer might contend with members' feedback, we return for a moment to that topic and engage it from a textual perspective. Doing so requires, first, a brief analysis of members' possible reactions to a draft text, as well as researchers' possible responses to those reactions.

The relational character of field research lies at the heart of these mutual reactions. The way(s) in which members react and the way(s) those reactions are engaged in the final text will depend, at least in part, on researchers' relationships with members in the field (including whether continued access is a significant issue), as well as on researchers' purposes for engaging in the research project. As Beech et al. (Chapter 10 in this volume) observe, the character of these relationships can change over the life cycle of a research project, and these changes themselves may lead to one sort of response or another, on either side of the relationship.

The intricacies of 'research friendships' can lead to participants accusing the researcher of a range of (mis)interpretations and intentions, from 'just' 'getting it wrong' to betrayal of trust, or worse. We wish to highlight, however, how epistemological and power differences might also contribute to members' reactions. Members' reactions to an ethnographic study might vary by their understandings of what 'research', in general, is about and/or their expectations for the particular research project or researcher (ranging, for instance, from anticipations concerning who might read the report to the degree of trust placed in the researcher). Their reactions might also vary according to members' own situational or societal power. For example, members of vulnerable groups might be less likely to protest, vocally, researcher representations, fearing reprisals, for instance, whereas more socially or organizationally powerful group members might use their resources to press for changes in the text (see for instance Mosse, 2005).

The research purposes that potentially inform researchers' responses to member reactions can vary, from a commitment to 'giving voice' to a marginalized group (e.g., the homeless), as in some action research (see, for instance, Wang et al., 2000, and Sykes and Treleaven, Chapter 11, this volume); to presenting a group's distinctive worldview for policymaking purposes (e.g., medical students, as in Becker et al. 1977 [1961], or addicts, as in Burns, 1980), common in policy-analytic ethnographic research; to providing a critical, even potentially emancipatory, perspective on group meanings and practices, as in some feminist research (see Pierce, 1995, on litigation lawyers, and Kaufman, 1989, on religiously orthodox women). Each of these purposes can affect the ways in which researchers choose to treat members' reactions in the final text.

Whatever the mix of researcher purpose and the power and expectations of members of the organization under study, one possible result is seamless agreement: all members agree that the researcher adequately expressed their perspectives. Alternatively, and more likely, one or more members may protest some aspects of the researcher's representations. What should a researcher do with comments or protests he or she receives? At the most

basic level, the researcher can consult her fieldnotes and review her data, and if she discovers that she has, in point of fact, made a mistake, she can make the correction, acknowledging the member's assistance. But if, upon rechecking her notes, the researcher feels that her interpretation is still appropriate, how can this difference be engaged?

Researcher judgement about the credibility and significance of the replies can lead to quite different responses. While we are mindful that a considered judgement concerning the character and/or source of the objection might make ignoring a member's critical comments the most appropriate response in some circumstances, we do not recommend this as an automatic or constant course of action, at the very least because it implies a dismissal of members' interpretations that is inconsistent with the spirit of member checking. If not ignored, the member's comment might be placed in a footnote, discussed in an endnote, or treated in the main text. Engagements with the comment in these three instances can vary in length, according to the seriousness with which the researcher regards the critical view. Those comments judged significant by the researcher might be included in the research report and assessed against other quotations and other thickly descriptive elements. The differences in view between the researcher and the member could both be presented (much as differences among member views themselves might), along with reasons why the researcher's analysis should take precedence over that of the member (if that be, indeed, the case). In these ways, the researcher demonstrates to reviewers and other readers that she has taken members' interpretations seriously.

We note, however, that the discussion of member-checking on the whole presumes that those members are themselves honest, trustworthy, and well-intentioned and that their comments are offered in the void of politics and power (perhaps because the methodologists who developed this concept were, for the most part, studying people of equal or lesser power to themselves, rather than people in more powerful positions). Researchers need to exercise judgement about what they need to send back, to whom.

writing and reading responsibilities

What are the responsibilities of each party to the writing–reading enterprise? On the surface the answer is simple. Writers owe readers their best efforts, and that implies, importantly, truthfulness about what they have done so that readers can assess the strengths and weaknesses of the evidence, methodology, and arguments presented without worrying about authorial deception. This holds for reviewer-readers as well: a reviewer owes a writer, at a minimum, serious assessments using criteria and standards appropriate to the manuscript's epistemic presuppositions. In our discussion here, we have assumed researcher honesty and integrity because we think the conduct of science rests, in the end, on trust; and that rests on truthfulness (with public support and funding

for science resting on both). But we are not naive about the power dimensions of scientific practices. When writers and readers share the same epistemic community, communication in the review process and, with publication, among a wider public may be *relatively* smooth. Even in this scenario, however, deception is always a possibility. The career-related pressures on researchers to publish in A-list journals[18] are increasing, and this might lead some (whatever their research approach) to consider trying to mask what they have actually done – to craft a text that implies use of a different method, for instance, or greater immersion in and exposure to the field than was actually the case. But the thick character of ethnographic writing cannot easily be manufactured, and practiced reviewer-researchers develop a sixth sense concerning the truthfulness of an account, based on their own experiences conducting and writing up field research and extensive reading of others' accounts. Indeed, a not-so-hidden agenda of thick description, as noted above, is to persuade a reader that one has truly been where one claims to have been, talked to those one claims to have spoken with, done the things, seen the events, had the interactions one claims to have engaged, and so on – and such is not easy to fake.

Outright deception in the sciences at large, however, appears to be relatively rare. The widespread public attention garnered by violators of accepted scientific procedures – those accused, e.g., of falsifying lab notes or manipulating experiments[19] – can be contrasted with the vast majority of researchers who conduct their studies in good faith. (A perhaps more insidious problem, the potentially deceptive nature of state-sponsored or -generated data, has received much less attention; see, for instance, Sadiq, 2005.) The presumption of researcher honesty can be seen in a number of arenas. Professional ethics statements, such as that of the American Anthropological Association, and research regulation policies, such as Institutional Review Board (IRB) policies in the US, are written primarily to regulate the acts of researchers who would intentionally abuse *participants*, not those who would intentionally deceive *colleagues*. Likewise, IRBs presume that researchers will submit to their procedures – there are (as yet) no penalties at the postgraduate level for failure to do so – and they also presume that in the field or in the laboratory, researchers will conduct themselves in the ways described in their approved proposals.[20] Without a general presumption of researcher honesty and ethics, the research endeavour could become mired in a monitoring system that would likely stifle its vitality.

The more difficult cases of writer–reader misunderstanding occur because of the diversity of epistemic communities and the consequent variety of understandings of the purposes of research and researchers' relation to society. One of the most pointed areas of disagreement involves understandings of objectivity. Methodologically positivist, variables-oriented researchers are much more likely to assume that scholars can and should be 'objective' – a claim that positions science outside the fray of political concerns. In contrast, interpretive researchers are often more politically involved, observing that

'neutral' scientists may play the role of experts who serve the state or the corporation in value-laden ways (e.g., seamlessly producing 'better' products to serve society or 'better' policy to 'control' it; see, e.g., Salemink's (2003) discussion of the role of ethnographers in supporting the US military and federal policymakers with respect to the war in Vietnam, or the general 2007 discussion of anthropologists embedded in the US military in Afghanistan and Iraq). Rather than 'objectivity', researcher 'reflexivity' becomes the significant marker of scientific research.

It is not only researchers who should reflect on their positionality in the research process (in the field, at the desk, during dissemination of reports). Reader-reviewers should be more aware of how the interpretive act of reading shapes judgements of the trustworthiness of a manuscript. It is not simply ontological, epistemological, and/or methodological differences *in the manuscript* that are at play (although the importance of these should not be underestimated, as the discussion above highlights), but also what readers bring with them to their readings in the form of prior knowledge and expectations, of whatever sort. The positionality of *the reader-reviewer* may contribute to the assessment of the research. In assessing the trustworthiness of a particular account, reader-reviewers are often exploring whether they can base subsequent actions on those representations – whether those actions are practice-related (Can I translate this account into improving, e.g., organizational practices?) or research-related (Can I build further research on this project?). This highlights the political character of research: if its truth claims are judged credible, the research may have implications for maintaining or changing the status quo, with all the life- and other resource-affecting implications of either path. Attention to the third hermeneutic brings reader positionality into focus, thereby bringing 'reading as method' into the methodological picture. Just as reflexivity about the role of the self in research and writing has increasingly become de rigueur in interpretive and some qualitative work, greater reflexivity about the role of the self in reading and reviewing could also be salutary for research communities.

The complexity of the third hermeneutic moment should be apparent. Social science communities are far from fully cognizant, explicitly, of this moment, although science studies and feminist theory scholars are making a dent in those silences. We are disheartened, however, by two recent turns that could stymie further developments. First, researchers in many fields conducting qualitative research of an interpretive presuppositional character are being increasingly pressured by those who embrace narrow understandings of science to conform to the characteristics of and criteria for positivist-informed quantitative research (e.g., King, Keohane and Verba, 1994; Pfeffer, 1993). In some disciplines and/or countries in which interpretive research has been a longstanding tradition, researchers find themselves 'under siege' (e.g., Freeman et al., 2007: 25) from those who would mandate a unitary conception of natural and social science along the

lines of positivist presuppositions, a position which threatens the existence of ethnographic research.

Second, some scholars (for example, Sutton, 1997) advise qualitative researchers to hide what they are doing or downplay the role of their qualitative evidence in producing research findings. We hope Sutton's advice, another form of conformity, is outdated; we find it to be unenlightened, at best, and seriously misguided, at worst. It is unenlightened in that qualitative methods of an interpretive sort have long been a part of the organizational studies world and methodologists have worked to articulate and promote the standards and criteria that are appropriate for evaluating such manuscripts and the research they present. This work supports an argument that 'validity' and 'reliability' are no longer universally applicable for such research. The advice is misguided because interpretive researchers have moved far beyond defensive modes to exploring new methodological terrain that re-positions that research firmly within the canons of science (see, e.g., Prasad, 2005; Yanow and Schwartz-Shea, 2006). By granting legitimacy only to positivist-informed research, those who would follow such advice would deny the historical standing and significance of the contributions of qualitative-interpretive science. Moreover, a qualitative-interpretive science that tries to conceal the character of its evidence can only be a weak version of quantitative-positivist work (and vice versa): the presuppositions informing the conduct of each are simply too different for such a masquerade to be successful. From this perspective we might just as well give up the interpretive and ethnographic projects altogether.

Given this complex writer–reader terrain, the old adage directed at writers – 'Know your audience!' – is still relevant, but insufficient, advice. Because organizational studies and other cross-disciplinary fields mix epistemic communities, and communication across sub-field boundaries is often necessary and desirable, such 'knowing' is neither easy nor simple. Rather than conforming to outside pressures to adopt positivist criteria or disguising what they do, interpretive researchers should join in a proactive effort to educate members of other epistemic communities – journal editors and reviewers, as well as colleagues – in the methodological requirements, including evaluative criteria, of qualitative-interpretive research. That is one goal of this chapter – to articulate the once tacit criteria of these research communities so that the standards may be more effectively written into ethnographic texts, thereby supporting the standing of ethnography as science.

acknowledgements

_____ **notes** _____

[1] Researchers – especially those just starting out – are often advised to share their draft manuscripts with a handful of colleagues to get feedback before submitting them to journals. Here, we have these 'informal' reviewers in mind as much as journal reviewers.

[2] Although this discussion raises issues concerning the power of rhetoric to persuade, including, at times, the possibility of producing a convincing, yet counterfeit, account (see note 19), for reasons of space, we cannot give this the full attention it deserves. On writing as method, see Richardson, 1994. On writing in ethnography, see Clifford and Marcus, 1986; Geertz, 1988; Golden-Biddle and Locke, 1993; Hammersley, 1990; Marcus and Fisher, 1999; Van Maanen, 1988.

[3] In contrast to most of the criteria literature, Schwartz-Shea (2006) proceeded inductively, starting with methods texts and the textual practices of interpretive researchers, to yield this historically contingent set of standards for evaluating the quality of particular research projects at this point in time. Turning from an *a priori*, theoretical, top-down enumeration of criteria to an inductive approach based in researcher practices has two advantages. It demonstrates a common methodological – presuppositional – grounding across all those research undertakings that share a constructivist ontology and interpretive epistemology, a commonality masked by the cacophony that characterizes the criteria literature. More important, a temporally-contingent set of standards allows for their evolution as research questions and approaches change in response to changing societal conditions. As Smith and Deemer (2003) argue, a definitive list of criteria – a universal, unchanging taxonomy – is inconsistent with interpretive presuppositions that all research efforts are historically contingent. This contextualized, historical flexibility is what an inductive, practice-based approach to standards affords.

[4] A sharp reader may find the ghosts of realism haunting some of these terms. That is not surprising given the hegemony of variables-based research against which the discussion of 'alternative' criteria has developed. For purposes of this discussion, we have chosen to retain the categories from that literature, rather than include a critique of their lingering realism by offering new terms, in order to capture aspects of the debate as it is presently constituted and unfolding and also because those terms help us better to theorize the research process and suggest replies to common criticisms of both qualitative and interpretive research.

[5] Counter to some common misperceptions, such accounts need not be 'navel-gazing', tell-all revelations. Moreover, reflexivity seems to be increasingly expected even of those ethnographic projects that are realist, rather than interpretive, in their ontological and epistemological presuppositions. This contributes to different understandings of what reflexivity entails, potentially introducing a tension between realist researchers' methodological positions and readers' more interpretive evaluative criteria. The point warrants further discussion that we are unable to take up here for reasons of space.

At another level of analysis, reflexivity asks researchers to reflect on the ways in which their research communities are historically constituted such that particular socio-political contexts shape, in unarticulated ways, the research questions asked or the very concepts used to investigate phenomenon (for trenchant examples, see Oren, 2006 and Lynch, 2006, as well as Chapters 11 (Sykes and Treleaven) and 12 (Ghorashi and Wels) in this volume). Such assumptions might fruitfully be brought to light in research narratives (see note 10).

[6] The term has its origins in the use of trigonometry for the purpose of surveying, whether on land or at sea. In this usage, it is a method of locating an unknown point using two known points at the vertices of a triangle. It is notable as one of the few techniques endorsed by both positivist and interpretive social science methodologists. Within variables-oriented methods, it has the status of a supplementary technique, commonly referring to the use of multiple indicators in operationalizing a complex concept (Neuman, 1997), although it also increasingly refers simply to the use of different methods for accessing and generating data in the same study (Jones and Olson, 1996).

[7] Triangulation may remind some of 'multi-method research', referring commonly to the combination of quantitative and qualitative research methods (see for example the new *Journal of Mixed Methods Research*, Volume 1, 2007). Our own take on this is that using multiple methods is fine, and, indeed, interpretive research has long drawn on multiple types and sources of evidence. But multiple methodologies or paradigms in a single research project (for an example, see Papa et al., 1995), with their contradictory ontological and/or epistemological premises, re-cast the research such that it is no longer engaging the same research question, even when the various approaches may be poking at a similar topic or concern (see also Jackson, 2008, on this). For this reason we do not include here multiple theories as a kind of triangulation.

[8] Methodologists have used various terms for techniques intended to accomplish the same goal. For example, Miles and Huberman (1994: 262) refer to this idea as 'checking the meaning of outliers', 'using extreme cases', 'following up surprises', 'ruling out spurious relations', and 'checking out rival explanations'. Becker (1998: 192–4) discusses 'deviant cases'. Erlandson et al. (1993) and others use 'member checking' to mean much the same thing, whereas Lincoln and Guba (1985) also add 'peer debriefing' – having a colleague in the same research field critique one's preliminary analysis. Brower et al. (2000: 391) include 'weighing competing interpretations' and 'recognizing and examining competing views or voices' as ways of meeting the criterion they call 'criticality' (different from what Golden-Biddle and Locke (1993) meant by the same term).

[9] Looking at organizational ethnography, Golden-Biddle and Locke (1993) argued for three evaluative criteria: *authenticity*, *plausibility*, and *criticality*, the first two of which are requisite, in their view, for convincing readers of ethnographic texts. We suggest that those three are, in a sense, 'meta-goals' similar in status to *trustworthiness*.

[10] Such a 'dialogical' conception of social science is important for understanding how some research may be disregarded by its contemporaries and then seem particularly prescient to readers of a later era. For an example, see Brandwein's (2006) discussion of the reception of Crosskey's legal analysis of the history of the Fourteenth Amendment to the US Constitution. Similarly, Keller (1983) discusses how cytogeneticist Barbara McClintock's work, later deemed path-breaking, was ignored by her male contemporaries. As feminist philosophers of science have shown, who is part of the dialogue is critical to what 'counts' as knowledge.

[11] Anthropologists have always talked at length to people in the field – about kinship, tools, language, etc. – rarely, if ever, calling it 'interviewing'. The need to designate such talk with that word seems to have been driven by other social scientists working at the survey end of the 'talk' continuum who needed, or felt they needed, to locate what they were doing more firmly in 'scientific' space. In organizational ethnography, the decision-makers we sometimes want to talk to are often sitting in their offices rather than hanging out on street corners or in pool halls, which means we have to set up appointments to meet them. Hence, these talks become designated 'interviews'. Thanks to Merlijn van Hulst for helping clarify this formulation of the point. Organizational anthropologist Malcolm Chapman (2001: 23) includes a humorous, but nonetheless illustrative, story about the distinction, drawing on his experience conducting a field research project jointly with a survey researcher: 'I think my colleague at the time viewed [the unstructured interview that characterized their research] as a subset of "interviews"; I regarded it as a subset of "talking to people"'.

[12] Paul Bate (1997) writes, critically, about business ethnographers who 'fly in' and 'fly out' of their research sites. We share this critique of a recent development in the world of 'applied' ethnography – the so-called 'quick ethnography' argued as suitable for studying organizational settings (businesses, schools, government agencies), public policy processes, and the like (see, for example, Handwerker, 2001). In our view, such analyses can miss the kinds of insights that are key to grasping from their perspectives why organizations and their members do what they do – insights that can often be developed only through dwelling in a place more intensively and 'deeply', over a longer period of time.

[13] Despite the ubiquity of the terms purposive and snowball 'sampling' in qualitative-interpretive research, we prefer the concept of 'exposure' to 'sampling'. The argument that ethnographic methods do not constitute 'sampling' is based on the term's close association with statistical science, where it is used to approximate the characteristics of the population under study. What interpretive research does is not sampling, in the sense that researchers make no claim for statistically scientific approximation, and the term's usage in an interpretive context glosses this distinction, as well as the ways in which 'representativeness' means different things in these different contexts. Moreover, the notion of 'sampling' implies a research design in which the researcher is 'outside' of the context and thus able to control the selection of information to be studied. Aside from the initial choice of location, ethnographic researchers' ability to 'sample' in this way is limited. The concept of exposure better reflects the epistemological assumptions of openness that are part of ethnographic research design.

[14] Humphreys (2005) and other auto-ethnographers position themselves at the end of the continuum where the researcher is the situational participant, raising other sorts of concerns that we cannot engage here.

[15] Which is not to say that this is always possible, but at least one ought to put some thought and effort into it.

[16] This is part, at least, of what ethnographic methodologists intended by the phrase 'going native'. We think there is more to it, and we also think that the phrase itself is erroneous and misleading, predicated on some earlier, colonial-era understandings of the relationship between researcher and researched; but we do not have the space to expound on this further.

[17] Within anthropology, visual ethnography has a longstanding history, back to the work of Margaret Mead and, later, Timothy Ash. Within organizational studies, its range has been much more limited (see for example Dougherty and Kunda 1992 on the use of photographs as data). On the visual (re)presentation of data, see Tufte (1997).

[18] We think these pressures are more pronounced in the UK and Europe these days, where departmental funding and even existence is tied to quantified publication output measures, than they ever have been in the US. We are also mindful of Sokal's hoax (1996), in which he carefully adopted a 'post-modern' writing style and successfully achieved publication – in a hoax that allowed him to make a point about that kind of writing; but hoaxes are only successful when the deception is, in the end, unmasked.

[19] Researcher dishonesty typically draws widespread public attention in both print and audio-visual news media and even in fictionalized novels (see, e.g., the coverage of the accusations of data falsification in Whitehead Institute Director David Baltimore's lab, discussed, e.g., in Kevles, 1998; see also LaFollette, 1992; Goodman, 2006). We know of no such violations in organizational studies research.

[20] We thank Michael Musheno (seminar comment, 10 September 2007) for drawing our attention to this point. Whether this presumption will continue is uncertain. One of us heard an IRB administrator discuss the possibility of post hoc random sampling of completed projects to assess compliance with their IRB-approved research designs. We should also note that US graduate students may be denied their degrees if it is discovered that they have failed to submit their proposals for research involving human participants to their university IRB. For further discussion see Yanow and Schwartz-Shea (2007).

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