Dear Reviewer, Dear Author:
Looking for Reflexivity and Other Hallmarks of Interpretive Research

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Abstract

Readers judge authors' claims on the basis of 'signaling devices' written into manuscripts. What 'devices' should authors of ethnographic and other interpretive research manuscripts be including in their methods discussions? By what criteria should reviewers and other readers of these manuscripts be judging them? Such matters are usually engaged during the research design phase, but there is reason to re-engage them at the end of a research project from the perspective of the reader of the research report.

Turning a reflexive eye on my own participant-observer role as a reviewer of such manuscripts, I have attempted to identify those elements of interpretive methods that are, I posit, the *sine qua non* of such writing. Their absence leaves me, as a reviewer, wondering about the character of the evidence presented and how trustworthy the "truth claims" of the research are; and so my hope is that this reflection might help authors of such research write better – more cogently argued – manuscripts prior to submitting them to journals. But I also hope this discussion will be useful to editors and reviewers who may not themselves possess interpretive methodological expertise and who may, therefore, not be aware that such methods follow different evaluative criteria from those deriving from quantitative-positivist methodologies.

Dear Author, Dear Reviewer:
Looking for Reflexivity and Other Hallmarks of Interpretive Research

> "Researchers...produce *claims* in which the author figures more as a claimant than judge. That is, each scientific article functions as a judgment passed on claims made by colleagues...."
> – Bruno Latour (2004, 78)

Over the last couple of years an increasing number of manuscripts submitted to various journals have crossed my desk for review, all purporting to do ethnographic or some other form of interpretive research. As a consequence, my readerly antennae have become ever more sensitized to the elements of such writing that need to be present in order to signal to a reader – reviewer or otherwise – that she is in the presence of an interpretive research project. In establishing its procedural and presuppositional contours, these elements – dare I say, the *sine qua non* of this sort of writing – shape the reader's experience of such a manuscript, as Schwartz-Shea (2006) so vividly notes. Their presence – or, in the event, absence – affects the perceived trustworthiness of the research report, the extent to which a reader will be persuaded as to the evidentiary character of the "truth" claims made by the analysis. Reflecting on my own experience as a participant-observer in this aspect of disciplinary science, I essay here to make explicit what these elements are, in the hope that this might help

authors of such research establish the bona fides of their manuscripts sooner rather than later, to the benefit of the writing-reading-reviewing process.

This effort stems from my concern that as an epistemic community, those of us who practice interpretive methods of various sorts have, with rare exception, not done as good a job as we could, or should, in educating others as to their entailments. Resulting manuscripts – including published ones – have often not been as clear as they might in laying out what such work entails, nor have methods textbooks, by and large, been of help in this (Schwartz-Shea and Yanow 2002). This lack of clarity, in turn, has led to a mystification of interpretive research processes, on the one hand, and a sense among those proficient in other forms of research, on the other, that these methods have no system – that they are impressionistic, rather than 'rigorous.'[0]

And so I write for a second sort of reader as well: potential reviewers of such manuscripts who may be relatively unfamiliar with the kind of 'rigor' that is particular to interpretive methods' systematicity. Perhaps because research methods courses are typically taught without attention to underlying ontological and epistemological presuppositions – Statistics I courses, for example, tend to start right off with descriptive stats rather than with a more philosophical treatment of the concept of probability and Popperian (or other) arguments concerning the possibility of certain

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0Because interpretive methods do not possess rigor in the ways that quantitative methods, for instance, do, I put the term in quotation marks. See Yanow 2006 for an extended discussion of the meanings of rigor in research methods.

knowledge – many scholars from non-interpretive epistemic communities are unaware that “reliability” and “validity,” while standard indices for variables research, are inapplicable measures for interpretive research, methodologically speaking (on this, see Schwartz-Shea 2006). It would be unfair to expect interpretive researchers to rehearse these arguments in every manuscript submitted for publication: they typically do not have the page-length space to flesh out these arguments in addition to the substance of their research; they may themselves not be schooled in the methodological argumentation; and they should not be singled out to do so when, after all, we do not expect researchers using regression analyses and other statistical tools to detail the philosophical grounding for those methods every time they use them. But editors who invite submissions of “qualitative” or interpretive research should not be holding those manuscripts to the same methodological rules that apply to quantitative methods, not understanding that these two methodologies build on considerably different ontological and epistemological groundings, with attendant differences in criteria for producing sound research. Each plays, as it were, by different rules; neither can ever measure up to the standards of the other; and both editors and reviewers need to know that such differences exist, with substantial philosophical underpinnings, so that different types of research are held to their own criteria for “goodness.”

Discussions of these “rules” are typically written from the perspective

of someone in the design phase of a research project (e.g., Erlandson et al. 1993, esp. the summary table on p. 133). It is much less common to find such discussions from the vantage point of the written presentation of the research.[0] What follows takes that perspective, although I begin with elements that are commonly entertained in research design and then move to more "esoteric" ones.

The basics: Research design elements

Dear Author:

I am sure you have thought about the elements discussed in this section in designing your research project, as they are typically presented in methods textbooks as parts of qualitative (interpretive) research design. They also belong, however, in one way or another in your manuscript, typically in the methods narrative. It can be helpful to think about your writing from the perspective of an imagined reader whom you have to persuade, rather indirectly, of the character and quality of your work. The presence of these elements gives your reader a fuller understanding of what it is that you have done in this research. They serve, in a sense, as signaling devices, helping that reader assess the believability of what you will claim, later in your manuscript, to have seen and/or heard and/or read, as well as the trustworthiness of the "truth claims" of your analysis.

0For exceptions that seek to identify the central characteristics of good qualitative-interpretive research as these appear in written work, see, e.g., Brower, Abolafia, and Carr (2000) or Golden-Biddle and Locke (1993, 1997).

Conversely, their absence weakens your claims to have “insider” knowledge of or evidence from the setting or event and/or people and/or documents under study of sufficient strength to support your analytic claims.

For those trained in ethnographic or participant-observer methods, these elements may be second nature. With ever more researchers coming to these methods without such background, however, it seems that they increasingly need spelling out.

Time: “DDT.” One of the first things I look for as I read interpretive research reports drawing on participant observer-ethnographic methods is the timing – the “DDT” – of the project: over what *Dates* or times of year was it conducted, which *Days* of the week, what *Times* of day? To see why these might be significant, imagine a field research project assessing student life in a university whose normal sessions take place from September to June or July. Spending time on location in August, or on Saturdays or Sundays, or at 5 a.m. or 11 p.m. would likely produce different understandings than research conducted at other dates, days, and times. Even for research not framed in terms of variables, DDT focuses on the variability of timing in shaping the kinds and character of data that observation would generate. Which among the three elements is significant depends on the research question.

For documentary (archival, newspaper morgue, library or other

source) research, DDT is likely to be less important: the character of the documents or of the sources is unlikely to be affected by calendrical changes in day, date or time of the researcher's presence (although one might read with greater attention in the morning than in the late afternoon, but this is not, typically, the sort of variability we attend to in reporting on methods, trusting researchers to monitor their own wakefulness). Differences in historical timing are, of course, central to the work; and those trained in historical methods are commonly highly attuned to such variability, taking it into account in research design and discussing it in written reports.

In interview-based research, DDT might conceivably have some bearing on the trustworthiness of the data generated (such as chronological distance from the event discussed), depending on aspects of the research question. I would expect a researcher to let me know if this is the case.

Duration. I also want to be told, clearly, how long the researcher was present in the field, with what periodicity. Knowledge claims based on 10 days' immersion in the research field, say, one 8-hour day a week for 10 weeks, are likely to be seen as less persuasive than those based on daily encounter for the same time period (that is, 8 hours x 7 days x 10 weeks; 70 days, 560 hours), let alone for six months or a year (with whatever periodicity), and more persuasive than 10 4-hour days over the same period – but these deliberations and choices are typically (or should be) made in

designing the research project. Many methods reports also do not make clear what constitutes “a day” in the field – was this 9 to 5, and then you went home? Or did you continue after that to the corner bar, or to dinner, or to a meeting, etc.? What I want to read in the manuscript is a clear statement of the duration of the research along with an explanation for why that made sense in terms of the specific study.

Again, this is much more central to participant observer-ethnographic research than it is to documentary or interview research. For those, trustworthiness is established more through indicators of exposure to the variety of perspectives on the research topic (and requirements of exposure often drive duration); I turn to this after the next section.

Space: Research settings. Interpretive researchers commonly make choices concerning not only time but also space. What kind of organization, or which department within an organization, is likely to provide illustrations of the matter under investigation? What is the best kind of neighborhood or community within which to address the research question? Which level of government, which agency, which section, which department? The presence in the written report of these deliberations and the rationales behind the choices made also situate the analysis.

For interview research, “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, more than the physical space itself, although it can also refer to the 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"? For documentary research, space would refer more to the location and choice of archives, in terms of availability of certain materials, access to particular files, and lack of access to others. In either event, providing the rationale for choices made can help a reader evaluate the trustworthiness of the subsequent analysis.

Exposure. Textbooks often note that "prolonged exposure" over time is one of the hallmarks of interpretive research. What is missing is the understanding that prolonged exposure refers to space as well. Time and space interact in interesting ways in interpretive research.

Again, to begin with field-based research of a participant observer-ethnographer kind, the reader wants to know that you have "mapped" your territory. In an organizational setting, this can mean covering all the (research-relevant) occupational bases; all the possible perspectives in a department; all the departments in an organization or a horizontal slice or regional arm of one; or both vertical and horizontal swathes through it, much as an archaeologist might cut trenches in various locations within a single site. In a community or policy study, one might map one's territory

by interest groups or race-ethnic or class or gender or age groupings (or some combination of these). Which of these, specifically, will feature in a given study depends on the research question.[0]

In interviewing, exposure is achieved through what is commonly referred to as "purposive sampling" or "snowball sampling," or a combination of the two, leading 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?" 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, conversational interviewing is commonly undertaken in ways that attempt to map the full terrain of the topic of research, as noted above, seeking to garner viewpoints around the spectrum of opinion, experience, expertise, and so forth with respect to the topic of research. (See the discussions in Yanow

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0Jerry Murphy (1980) used the analogy to wagon trains in the US "old West" circling the campfire at night, the subject of study being the campfire, the wagons being the various perspectives on it. I have often used the image of a cubist portrait, e.g., by Picasso (e.g., Yanow 2000). The difficulty with these images, I now understand, is that they imply, to some, that garnering all these viewpoints will provide complete understanding of the topic (much as cubism was seen as a more naturalistic portrayal of its subjects than earlier figural painting; Hatch and Yanow 2006). Yet such an understanding of the analogy ignores the fact that there are gaps – areas of darkness – between the wagons, not to mention a whole world beyond that circle, such that complete knowledge is not possible (see the section on silences, below). The wagon train analogy also may be taken to imply that positioned knowledge is external – objective. This is close to a critical realist position that claims the possibility of objective knowledge of a constructed reality. It is a perspective possible only, I think, if one does not focus on the meaning of the campfire to its observers, in which case one is perforce in the realm of subjectivity and intersubjective knowledge.

and Schwartz-Shea 2006. The main exception would be research based on a single person's experience; see, e.g., Behar 1993 or Rudolph and Rudolph 2000, although each of these studies draws on more sources of data than provided by just one person, whether in interviews or in diary form.) Documentary research similarly seeks to garner views from various "positions" surrounding the topic of study.[0]

All of these methods build on the epistemological presupposition that situational understanding will develop through the researcher's exposure to a variety of perspectives or points of view, reflecting different lived experiences deriving from different education and/or training, different communal or familial or social-political-economic-cultural backgrounds, and so on. As a reader, I want to see these issues discussed, along with the deliberations and choices that led to the development of exposure (including those improvised along the way as the research design was altered; I return to improvisation below).

Silences. What is much harder to access are those positions and views that have been silenced or are perhaps silent by choice. Snowball exposure, for example, builds on relational networks, and networks, by definition, do not

0I do think we need to be careful when using the two terms referring to sampling. Neither is a sample, or sampling, as those terms are used in statistics to mean "a set of elements drawn from and analyzed to estimate the characteristics of a population" (the second definition in the *American Heritage Dictionary of the English Language*, 2000). Interpretive research is not involved with estimates: we are not engaged with the logic of probabilities as the basis for our evidentiary claims, but instead, ground our research on lived experience and situated knowledge and knowers.

include everybody. Given the various manifestations of power, oppositional voices may be more easily discoverable than voices not on the "mattering map," in Rebecca Goldstein's felicitous phrase (1983), of those involved vocally in the issue under study. As a reader, I 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 and include them.

Researcher's role. Where are *you*, dear Author, in this research project? In reading documents and in interviewing topic-relevant actors, you are – we assume – acting out of your role as researcher. But in field research that is more participatory than just observational, researchers often adopt a situation-specific role in addition to their researcher-role, acting in keeping with the demands of that role when necessary (see Gans 1976). The more readers know about your role, the circumstances under which you chose or acquired or developed it, and any advantages or problems presented by a situational role (e.g., role conflict), the better able we are to evaluate your evidence and truth claims.

One particular feature of interpretive research is the extent to which choices of setting and role build on the researcher's prior experience and knowledge. There is no reason to hide this: one of the ontological and epistemological presuppositions of interpretive research is that "objectivity" – standing outside of the subject of study – is not possible, conceptually (so,

also, it is not possible to avoid "interviewer effects"; on objectivity see Bernstein 1983, Hawkesworth 2005, Yanow 2006). Aside from that, the more you try to hide the fact that you chose that setting because you had prior knowledge of the language or the city or had connections who could facilitate entree, or some such, the less believable your narrative will be and, hence, the less trustworthy your analysis. It is not that difficult to infer that someone with a Hungarian name writing a dissertation in an English university comparing cases in Budapest and London has made those choices for certain reasons, whether having to do with language or familiarity or connections. The more explicit and transparent you can make your explication of role and setting choices, the more trustworthy your research is likely to be – or at least, the less it will appear as if you are trying to deceive your reader.

Access: Getting in and maintaining relationships. How did you make contact with the people you interviewed? With the organization or other setting in which you conducted field research? If not self-evident, with the archive(s) in which you worked? Access is not a matter of getting a foot in a door one time only; it is about establishing relationships and maintaining them (see Feldman, Bell, and Berger 2003). I want to know about the measures you undertook to accomplish this (e.g., bribes you paid to keep information flowing in your direction – just kidding, unless you want to consider treating

others to coffee or beer or babysitting the kids to be methodological bribery, an objectification of others that is one of the carryovers from the colonial heritage of fieldwork, and discuss this as part of research ethics).[0]

"Being there": Data details. It has become increasingly common for researchers to write that they have used "thick description" as a method, referencing the work of Clifford Geertz (1973). This is an interesting acknowledgement of the methodological argument that writing – the writing up of field, interview or archival notes into a research report – is itself a method of knowledge creation about the topic of study (see Richardson 1994). But much like Glaser and Strauss's grounded theory (see Locke 1996), thick description is often invoked as a signaling device, nodding toward desired membership in a particular epistemic community, but without writing practices that give substance to the signal.

What a reader wants to see is the layering of situational detail that contextualizes action and nuances it, so that, to take Ryle's example discussed by Geertz, the reader can discern whether the muscle movement in an actor's eye is involuntary twitching or intentional blinking. The same sort of layering can be provided using interview or documentary material. From a writing perspective, however, there is a subtext to this provision of

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0Kidding aside, there is an aspect of instrumentality, captured in the now-discredited term "informant" as well as in common notions of "access," in the relationship between researcher and "situational members" that is still not commonly engaged, despite all the awareness of the colonial heritage of much of this social research or of the state's role in statistical research. Seeing access as relational is a modest step toward addressing this.

data detail that goes beyond the substantive purpose of elucidating situated meaning. The density of data detail demonstrates that you really were there, saw what you claim to have seen, heard what you claim to have heard, read what you claim to have read. Interview- and document-based research writing achieves this through the details of direct quotations of key phrases, full sentences, and lengthier excerpts. In field research, it is done through the details of setting and event descriptions, of acts and interactions (who said or did what, to or with whom, when, where, and how), of persons and places.

Beyond the basics: Aspects of writing genres and other matters

Dear Reviewer:

I turn my attention now more squarely to you. In addition to those features of interpretive research writing that are commonly attended to in crafting the design of an interpretive research project are features that are manifested primarily through the writing itself. Although writing as a way of world-making (to use Nelson Goodman's term, 1978) has drawn relatively little attention in policy or political and other studies, in anthropological and ethnographic circles, including organizational ethnography, it has attracted significant attention (e.g., Clifford and Marcus 1986, Geertz 1988, Golden-Biddle and Locke 1993, 1997, Van Maanen 1986). I take such a writing-focused perspective here.

Unaware of these elements as hallmarks of such writing, you may find them to be misplaced or even unscientific. I assure you that, on the contrary, they are every bit as much scientific as hallmarks of survey research. I hope here to show why that is the case.

I. School children and college students are taught not to write in the first person. The rationale rests on accepting the possibility of objectivity: the distanced "I" renders the subject matter at arm's length, positioning the author outside of the work – or at least, this is the promise that it holds out. The ultimate in this writerly conceit is the use of the passive voice, which denies agency altogether, assigning responsibility for action to some invisible force acting upon the object in question.

Henry David Thoreau, writing *Walden* in 1854, already perceived the limitations of such writing (1854, ch. 1):

> In most books, the I, or first person, is omitted; in this it will be retained.... We commonly do not remember that it is, after all, always the first person that is speaking. I should not talk so much about myself if there were anybody else whom I knew as well. Unfortunately, I am confined to this theme by the narrowness of my experience. Moreover, I, on my side, require of every writer, first or last, a simple and sincere account of his own life, and not merely what he has heard of other men's lives; some such account as he would send to his kindred from a distant land; for if he has lived sincerely, it must have been in a distant land to me.

Interpretive researchers claim that objectivity is not possible. That is the heart of a hermeneutic phenomenology that presupposes that social realities

are socially, intersubjectively constructed; that knowledge of them rests on prior knowledge, which is both personal (coming, as it does, out of the individual knower's contextualized experience, education, and so on) and collective (the rules for sensemaking having standing within epistemic communities – communities of meaning, of interpretation, of discourse, of practice; in short, paradigmatic communities inhabiting shared, paradigmatic, hermeneutic circles). If knowledge is situated knowledge, produced by situated knowers, why not use "I"?

Reflexivity. The personal "I" puts the researcher-author into the picture where s/he belongs. But it does not tell us anything about that person's experience with the research, not so much concerning the elements itemized in the previous section but, rather, with respect to how that person's presence – how aspects of her personality, his clothing and tone of voice, her class and/or race-ethnic and/or religious identity, his gender, and so on – shaped the research. Samer Shehata (2006), for example, describes in detail how being a male, Moslem, Alexandria-born, UK- and US-educated, Egyptian-American researcher both enabled his access to some kinds and sources of information about shop-floor work and workers and denied him access to others.

Reflexive writing, then, takes the objectivity argument one step further. It is not only that objectivity in social research is not possible, but

that inter-subjectivity presupposes the involved presence of someone – here, the researcher – in co-generating knowledge, in an interactive way, together with those being observed, with whom one participates in whatever degree, whom one interviews. The double hermeneutic (Giddens 1984) at play – the interpretations of interpretations (Geertz 1973) – takes place also in analyzing documents, whether historical or contemporary (see Jackson 2006). Not only is there room for the "I" in interpretive writing (although it is not required, and not all authors are comfortable using it); what is increasingly *de rigueur* is a reflective consideration of how the person – the "positionality" – of the researcher (might have) shaped the research process.

The improvisational character of interpretive research. Interpretive research requires improvising (Yanow 2001). The best designed research project can only sketch out the broadest parameters of the project – the sorts of things itemized in the previous section, such as setting, overall time period, some idea of the types of people the research intends to try to talk to, and so on. But interpretive research engages human beings, and it is not possible to know ahead of time precisely how they will act or react – or how the researcher will be called on to respond. In talking with people (whether in formal interviews or in curbside chats), the researcher needs to engage what she has just been told. In observing (with whatever degree of participating), the researcher often needs to take on-the-spot action that was not, and

could not have been, foreseen. Even documents require situational responses, as one phrase points to an unanticipated new source, possibly a cache of letters in a distant archive or, in other uses of archival materials, an individual to interview who resides in another country.

But improvisational research does not mean impressionistic research. There is systematicity to improv, as there is to interpretive methods. Whether in jazz, in medieval or Renaissance "Early Music," or in theater, improvisation builds on extended periods of practice and preparation, solo, but also, importantly, with other people. One may not know ahead of time what one will do in the moment, but one draws *in that moment* on a repertoire of moves practiced with one's band- or troupe-mates. And improv does require being "in the moment": an intense focus on the matter at hand and on those with whom one is interacting. It requires a "yes, and" attitude – the orientation toward building on what one has just been told, on an invitation or challenge just extended, on an unexpected word just encountered – rather than "blocking" (negating) it. So it is that interpretive researchers practice their craft, learning how to observe or converse with "in the moment" focus in order to remember the details afterwards; how to "yes, and" in a conversation in order to keep it going and focused toward the research topic. As with all practices, the repertoire of observational and conversational "moves" builds up. Starting a conversation with a stranger gets easier the more practiced one is at it, as does remembering an hour-

long conversation without benefit of a tape recorder. (And yes, it is easy to fall out of practice.)

Those who do not practice improvisational music-making or theater tend to misunderstand it – to construe improvising as entirely invented on the spot, rather than as a practiced craft. This may be one of the reasons that interpretive research methods are denigrated: they are seen as something that anyone can do, without preparation or practice; they are perceived as unskilled make-it-up-out-of-your-head commonplaces rather than as skill-based, "advanced" research tools. Such improvising is not a moral failure of research design or execution, Dear Reviewer (especially of research funding proposals). It is characteristic of the nature of human interaction and meaning-making, which is the central focus of interpretive research questions and methods.

"Alternative" writing styles. I had a long fight with my dissertation advisor. Wanting my writing to trace my own sensemaking as it unfolded as my research progressed, I began my draft with the case study story, leading from that to the theoretical frame. She argued that without that frame, there was nothing to guide the reading of the story – the theoretical material needed to come first. I lost. And she was right.

That said, it is, I think, possible to find a middle path that can make sense, in which the article (or book chapter) begins with a bit of the case

story before turning to the theoretical frame. The immediacy of dialogue or quoted text or event description can draw the reader in, setting the stage on which to ground the theorizing that follows. As a literary device, the turn to the theoretical framing can be akin to the fiction writer's turning back in historical time, breaking with the present-day narrative to narrate the background that will lead up to current events. More postmodernist scholars might attempt writing that has no narrative thread, enacting that part of their theorizing.[0]

As noted above, interpretive methodologists increasingly consider the "writing up" to be itself a form of method (as it generates knowing for the researcher while writing and later for the reader). Dear Reviewer: Please consider that other forms of writing may be intentional efforts to explore methodological alternatives, rather than signs of unscientific or unprepared work.

Graphics. While interpretive researchers cannot use graphs and tables for presentation of data analyses as much as quantitative researchers can – words do not lend themselves to such condensed summarizing – we can use maps, photographs, descriptive stats graphs (bar and pie charts), organizational charts, flow charts, diagrams of neighborhoods or communities, etc. For reviewers, this means reading more words than one

0My own feeling on this is akin to my advisor's absolute "Don't! It doesn't work." But perhaps I just haven't yet read the right article.

might find in quantitative writing – these are the data of such research; but please help writers think creatively about ways to present "reams" of data (see, e.g., Tufte 1990; Dougherty and Kunda 1990; Kunter and Bell 2006).[0] Writers: please consider, also, that tables, charts, etc. do not stand on their own. If you use them in the text, refer to them (that is, what point(s) in the text are illuminated by which specific graphic; provide a title for the graphic; and talk your reader through its significant elements, showing how they link and contribute to the argument at hand).

Confusions of methods. The discussion of several of the issues in both sections of this paper sheds light on one particular confusion of methods that has popped up increasingly in the last few years in conference papers and in the manuscripts I have reviewed: researchers increasingly claim to have done "ethnographic" research, yet their methods discussions do not support such a claim. This may be explained in part by contemporary research developments and challenges: ethnographic research is increasingly moving out of the bush and into the boardroom or City Council room or hospital room. With such moves, the character of the work is changing.

But whatever the location, central to participant-observer research is the feeling of "being there" – developed both by the researcher in the midst of doing the research and by the reader in the midst of reading the account

0For that matter, the use of representations in science and other practices is itself increasingly a subject for research; see, e.g., Ewenstein and Whyte (2004), Muttanen (2004), Latour (1999), Lynch and Woolgar (1990).

of the research. The elements discussed above comprise some of the ways in which the researcher writes "being there" into the narrative, having been *there* – in extended space as well as time – in the field. One certainly is "there" when one conducts an interview; but observing the outer office and its workings prior to the interview and interacting with the secretary on one's way out the door does not add up to the sense of twitches and winks and the differences and similarities between them that contextualizes the sensemaking reported and analyzed, that makes "being" be.

One may "shadow" an individual – a governmental minister, e.g., or a CEO. This is certainly a legitimate form of interpretive-qualitative research; but even though it has a certain quality of "being there," it is neither ethnographic nor participant-observer research. Part of the confusion may be explained from the perspective that "ethnography" refers both to a set of research tools and to a mode of writing. One can use ethnographic tools – observing, with whatever degree of participating; talking, with whatever degree of formality and advanced scheduling; even reading, perhaps using diaries or newspaper accounts as surrogate interview texts; and, indeed, these are increasingly being used – without writing an ethnographic account. Ethnographic research is characterized by attention to "place-ness" as well as to the actors peopling those places and what they said, thought, and did, and why, as reported by them and in written materials; and this "place-ness" is one of the hallmarks of ethnographic writing. A collection of formal

interviews does not an "ethnographic" research project make – even if talking to people is an ethnographic tool. Neither does archival research – although I could imagine making use of the term metaphorically, claiming to have done an "ethnography" of a set of documents and meaning to have treated them as if they were surrogate notes on one's own observations and interviews, with appropriate argumentation for such usage. That said, reviewers, no less than readers, need to be attuned to these methodological differences.

Concluding thoughts

My point of departure in this essay presumes a political dimension to scientific research and writing, in the Aristotelean sense: that written reports are designed to persuade, and they draw on the rhetorical devices accepted within their epistemic communities to convince readers of the trustworthiness of their evidence and their claims to knowledge (see, e.g., McCloskey 1985). Although many of the elements I have discussed here are commonly considered in designing interpretive research, when that research is written up, they also take on the rhetorical character of signaling devices – and when those signals are missing, their absence can affect perceptions of the trustworthiness of the research evidence, findings, and truth claims.

Some aspects of interpretive research manuscripts should be clearer in light of the foregoing. Authors are not just whining when they complain

about the restrictions of journal page-length limitations and the difficulties of condensing interpretive research reports into 20 double-spaced pages. "Being there" not only takes time; it takes writerly space to convey the data details that will support an argument. Reflexive writing takes even more space (and time). Data cannot always be condensed into tables; photographs and sketches take up other sorts of space; audio excerpts from recordings cannot be published in traditional outlets (bracketing ethical concerns for the moment) – these and other matters are issues not confronted by survey researchers and regression analysts, and to the extent that such work serves as the model for journal publishing, interpretive writing is trouble, and troubled.

I hope this discussion signals some of the complexity involved in doing interpretive research systematically and in writing it up. Such research may focus on single cases, thereby qualifying terminologically as "single 'n'" or single site studies. But single 'n' cases entail many more than one observation: think of the number of interviews and chats, the number of events observed, the number of passages read in documents. Anthropology tends to treat the single site study as the norm, as Røryvik (2006) observed, in marking "multi-site" fieldwork as the exception. Yet, as with "single 'n'" case studies (in sociology, political science, and other fields), single-site anthropological work often entails multiple observational areas within its geographic, organizational or political boundaries. William Foote Whyte

(1955), for example, spent time in the pool hall, the bar, corner boys' apartments, and other places, along with the street corner. Gideon Kunda (1992) and Julian Orr (1996) observed organizational members and techs in a variety of settings; Samer Shehata (2006) studied workers on the shop floor, the manager in his office, riders on the bus going to work, male co-workers at gathering places away from work – all in the same "single site" research.

There has been much attention of late to the requirements for "evidence" in medicine, psychological counseling, and various public policy arenas. I have been rather critical of this "evidence-based" movement (because it has taken a very narrow and a largely unspoken, unreflective, and uncritical view of what constitutes "evidence") and so would hate to think that my call here for a more explicit accounting for the elements of interpretive research practices would be taken as part of this movement. Nonetheless, in my readings of manuscripts in the last few years, I have been unconvinced by many of the narratives – and I am a sympathetic, informed reader. If I am unconvinced, how much more so a reader who is less so! I think interpretive methods as a whole would profit from greater accountability on our – interpretive researchers' – part. Hence, this plea.

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