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Abstract

Anyone involved in ethnographic research knows that in practice participant listening is an important technique employed by ethnographers, particularly among those of us who live in an 'interview society'; yet its importance is barely acknowledged in the ethnographic literature. It is curious that ethnographers seem not to have reflected much on a gap between what we say we do and our real life practice. Based partly on my own research into schools and schooling, alongside the work of various other practitioners, I argue the need to better acknowledge the importance of *engaged listening* for ethnography, and the ways in which personal style (visual learners versus aural learners) impacts ethnographic data production. I also examine the use of interviews in social research, exploring ways in which we might construe 'the interview' conducted with an ethnographic imaginary as an 'experience-near' event in Western settings: they offer truly ethnographic moments.

Keywords

ethnographic method, anthropology, participant observation, engaged listening, ethnography of the senses, interview society

What makes a reported sight more objective than a reported sound, smell or taste? Our bias for one and against the other is a matter of cultural choice rather than universal validity. (Fabian, 1983: 107–108)

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Anthropology ≠ ethnography ≠ participant observation

In the 2007 Radcliffe-Brown lecture, noted anthropologist Tim Ingold (2008) pointed out that anthropology and ethnography are not one and the same thing. What should be a statement of the obvious, he asserts, has been overtaken by a clear tendency in the recent past for anthropological writers to equate the two, 'exchanging anthropology for ethnography more or less on a whim' (p. 69). As one trained in anthropology in the past 20 years, I know what Ingold means. I also think that the equation elongates, with ethnography and participant observation often whimsically interchanged by many an ethnographer; indeed, Hammersley and Atkinson (1983) identify ethnography and participant observation as cognate terms. Such easy slippage triggered a degree of frustration for one of the elder statesman of sociology, the University of Chicago trained sociologist Herbert Gans (1999). Reflecting on 50 years as a social researcher, Gans laments the ways in which 'ethnography' has come to subsume, and even attack, just about everything in qualitative research, including his own beloved methodological approach – participant observation. Gans claims that when he trained as a participant observer in the 1940s none of his cohort in Chicago used the term ethnography. Anthropologists, according to this particular Chicago school's way of thinking, were doing descriptive studies and were called, somewhat pejoratively, 'ethnologists' (p. 546). While the attack made by Gans on the postmodernist turn towards ethnography in qualitative research and its links to today's academic economy (p. 544) is worth pursuing, I will not do so here. It is his refusal to be labelled ethnographer that is most useful to the issues I am raising. In affirming his right to continue referring to himself as 'a sociologist whose primary research method has been participant observation' (p. 544), he alerts us not only to the possibility of decoupling ethnography and participant observation, but also to the potential usefulness of doing so.

Ingold and Gans point us towards the relatively recent tendency for anthropologists to define themselves by their method, as participant observer-ethnographers, and for those calling themselves ethnographer in the many fields stretching beyond the anthropological borderlands to equate their research with participant observation. It is this latter equation that is mainly scrutinized here and it was a conversation I had with a fellow ethnographer at a conference that helped trigger this particular interest. Under discussion was one my favourite ethnographic works, *In Search of Respect* by Philippe Bourgois (2003). My colleague was much less effusive than I about the text, mainly because he was disappointed with its lack of descriptive power. For him it relied far too much on interview material; it contained little that he could construe as observation. This comment took me back to the day I realized, somewhat uncomfortably, that much of what I was recording as data in the document that was destined to become my PhD thesis was dialogue emerging from formal interviews and casual conversation at the government high school in which I had spent 15 months as an ethnographic researcher. Whilst the thesis and subsequent monograph (Forsey, 2007) was not devoid of observational

description, the data presented reflect more of what I heard in the field than what I saw.

While this somewhat casual observation did cause me to question whether my research was truly ethnographic, as I trust this article will show, such questioning was needless as listening is at least as significant as observation to ethnographers (Cohen and Rapport, 1995). Cohen (1994) proposes that an ethnographic self-consciousness can stimulate a sensitivity to the self-consciousness of those we study. I would like to add that such self-awareness can help us better appreciate our fellow researchers and the products of our work. For example, when reflecting upon my use of verbal data in my PhD project I began thinking about what I enjoy in novels and recognized immediately my tendency towards skipping over slabs of descriptive prose. I am much more engaged by dialogue as a reader and I doubt that my tendency to favour this literary form as both researcher and writer is coincidental. While I am not going to explore here the obviously important links between methodology and the personality traits of a social researcher, it is useful to acknowledge the distinctions some educators make in individual learning style preferences. It may well be the case that some researchers are more aural than visual and vice versa – a phenomenon that is bound to influence not only the ways in which research is pursued, but also what is pursued, and what is found.

The main aim of this article is to consider the importance of listening to the ethnographic project and to open up the possibility of placing *engaged listening* on a similar footing to participant observation in our conceptualization of ethnographic practices. I do not seek to create a new dogma, or a fresh set of false equations, rather the aim is to ask my fellow ethnographers to look again at what we say we do and consider this up against what we actually do. It is a truly ethnographic enterprise. There are two reasons for doing so; first, because it is intellectually interesting to scrutinize ethnographic practice and consider some of the possible gaps in our awareness and knowledge; second, because of the unnecessary discomfort I and a number of others have witnessed among our colleagues, especially postgraduate researchers, who sometimes feel a deep sense of inadequacy because they are not doing a classical (I would call it mythical) participant observer study. If we trace the two-part equation (anthropology = ethnography = participant observation) backwards we can maybe imagine the dilemmas faced by some who can feel their disciplinary identity to be slipping away from them – a response that is particularly pronounced among those anthropologists conducting research ‘at home’ (Hockey, 2002: 209–10).

A telling example comes from another notable anthropologist – Sherry Ortner. In introducing her book *New Jersey Dreaming*, which reports on research conducted among her former classmates at Weequahic High School, Ortner (2003) describes herself as a dyed-in-the-wool participant observer uncomfortable with producing a text based on ‘talking head’ interviews. She points to severe limitations in interview-based studies, especially those conducted amongst people who do not relate to each other in some way, claiming there is a loss of richness and depth when

compared with full-scale participant observation. Much later in the text Ortner muses on what to do by means of follow-up research and writes of giving up on the idea of continuing with a study of the children of her classmates whose stories fill this text: the list of names was too large; it was hard enough keeping track of the class of 58 let alone their children; but finally, and perhaps most revealingly, she suggests:

. . . they constitute even less of a community than the Class of '58 and I really don't think that I want to do another interview-based, talking heads project. I'm trying to think of something more ethnographic, more place-based, if not actually in a single site, that would also allow me to get back to a more cultural perspective, compared to the heavy sociological bent of this book. (p. 261)

Ortner clearly does not believe the work she did amongst her former classmates to be ethnographic or anthropological even, given that she depicts her 'talking head' research, somewhat pejoratively, as sociological. The two-part equation, participant observation = ethnography = anthropology, and some of its implications are very evident here.

Ortner's candid reflections help exemplify the sort of discomfort Hockey (2002) attributes to those anthropologists who find themselves using interviews extensively in their research. Including herself in this group, and writing as one engaged in the 'Anthropology of Britain', Hockey asserts that many researchers involved in 'anthropology at home' feel their disciplinary identity to be slipping away from them as a consequence of doing interview-led studies rather than 'proper' ethnography. As one engaged in the anthropology of Australia, and more specifically in research conducted in my home state of Western Australia, I join with Hockey in questioning the sort of methodological and epistemological biases and limitations that are captured in Ortner's reflections and the discouraging implication that anthropological studies of industrialized societies necessarily require that participant-observation/ethnography yield to pragmatism.

Hockey (2002) argues a case for interview-based studies to be considered ethnographic, asserting that research interviews are culturally appropriate ways of participating in British society. Before grappling with this apparently radical idea, I first want to take a step back and consider the spaces and connections between ethnographic rhetoric and practice. The main issue explored in the next section is the methodological and epistemological implications of the tendency to equate ethnography with participant observation – that ethnography can only be realized by, or with, participant observation (Delamont, 2004; O'Reilly, 2009; Silverstone et al., 1991). More to the point, I want to suggest that listening is at least as significant as observation to ethnographers. Ethnography is arguably more aural than ocular, the ethnographer more participant listener than observer.

Seeing voices

There is a good deal of consensus that Western culture, and scientific culture in particular, is dominated by a bias for the visual (Bull and Back, 2003; Fabian, 1983; Howes, 1991; Pole, 2004; Rice, 2003; Tyler, 1984). Tyler (1984) attributes this 'reality' to the hegemony of things over words, through which seeing becomes knowing and the visual arts come to form a common sense 'hard framework' of all thought. In other words, the dominance of the visual in Western thought has relegated the experiences of touch, taste, smell and listening to a secondary status, which are more than usually filtered through a visualist framework (Bull and Back, 2003). This is not the place to revive or rehearse the particular arguments for and against the dominance of the visual, although Bull and Back's reminder that Foucault's much celebrated critique of Bentham's panopticon and the surveillance society ignored the listening devices built into panopticon walls offers a telling insight into what can be missed in the drive to emphasize the visual. Instead of conceptualizing hearing and associated sonic practices, as a historical residue somehow separate from the visual, it is important to recognize how hearing operates in tandem with the 'panopticon, perspectivism, commodity aesthetics, and all other key visual practices of the modern era we now know so much about' (Erlmann, 2004: 5). According to Bull and Back (2003: 5), 'The history of surveillance is as much a sound history as a vision history.' This assertion can be applied usefully to ethnography's history.

Fabian (1983), the cultural anthropologist whose thoughts on the dominance of what he terms visualism are captured to some degree in the epigraph opening this article, critiques a disciplinary proclivity to elevate vision as the 'noblest sense'. In arguing that in anthropology 'the ability to visualize a culture or society almost becomes synonymous for understanding it' (p. 107), he points to some possible explanations for the ethnographer's tendency to ignore the fact that much of what is actually recorded as data is sonic rather than visual. No less a figure than Malinowski described ethnographic research as 'a long conversation', one in which 'not only words are exchanged but from time to time also things, animals, people, gestures and blows, but where nonetheless language plays a most prominent part' (in Bloch, 1977: 278). There is something more than a little curious about this portrait given Malinowski's credentials as the instigator of participant observation as *the* distinctive method of anthropology (Gupta and Ferguson, 1997). Clearly the visual metaphors of the overall anthropological/ethnographic project can be conceptualized as part of a large umbrella that also captures what is heard under its canopy; casual conversation and formal interviews can be construed as part of what is 'observed' in the field. However, while participant observation provides us with a convenient shorthand phrase, there is some useful conceptual ground to be made from listening more attentively to the field and allowing the ear a fairer hearing than it appears to get.

I advocate a democracy of the senses. It is not good enough to 'denounce vision and replace it with a new sensibility based on the ear' (Erlmann, 2004: 5), and I

certainly do not want to fall into the epistemological trap of reproducing the sort of unhelpful visual/auditory divide so cogently critiqued by Rice (2005). Our senses are indeed very difficult to separate out (Bull and Back, 2003), but for this exercise it is necessary to do so if only in terms of how they are conceptualized and the impact this has on the rhetorical devices employed to explain and justify social research.

The linguistic anthropologist Moerman (1988: 8) argues that ethnographers 'collect the droppings of talk', that our primary data are the things said as part of 'socially organized scenes'. The British anthropologists Cohen and Rapport (1995) concur, arguing that above all else, what ethnographers do is listen. Their focus is on the anthropology of consciousness and they assert that it is mainly through listening to people that we access human consciousness. Nesbitt (2002), a religious studies scholar, is one of the very few researchers I have located who has self-consciously explored ethnography as a process of participant listening. While she does not make any explicit links between research production and personality, she implies it in her discussion of religious commitment and decisions taken by individual researchers to engage in particular types of ethnographic study. Nesbitt identifies herself as 'a Quaker ethnographer' and she argues that her religious convictions predispose her to reflexivity, an 'alertness to challenge of terms and definitions' and to listening attentively to the social scenes in which she is engaged. Listening, she argues, not only 'patterns Quaker spirituality' (Loring, 1997: 2), it is also a prerequisite of ethnographic research (p. 141).

Cohen and Rapport (1995: 11) cite Malinowski and his commitment to gaining a 'clear idea of the metaphysical nature of existence' to support their contention that the uncovering of human consciousness has been an unacknowledged goal of anthropology through much of its history. Reiterating their point about listening being the primary activity of ethnographers, they hint that this is often lost in the rhetoric surrounding 'the cliché of participant observation' (p. 12). If the uncovering of human consciousness has been a major, but somewhat hidden, goal of anthropologists/ethnographers for the past century or more then it is probably no coincidence that the aural proclivities of the ethnographer have been similarly obscured. The rhetorical commitment to moving beyond what people say they do to seeing what they actually do provides such a compelling rationale for ethnography it is no wonder that a certain hierarchy of the senses has emerged placing participant observation at the apex of the methodological pyramid.

I have not *yet* conducted a systematic study of the research literature to test out my hypothesis that ethnography is at least as much about conversation as it is about observation. However, even if my quantum estimate is somewhat off the mark, my own reading of ethnography and hearing of countless research articles assures me that a significant enough portion of ethnographic writing is based more upon what was heard in the field than what is seen there. And often what is reported as the 'seen' are in fact observations of people conversing, singing, listening, speechmaking – noise-making.

I reiterate the point that vision, visual experience and visual expressions of experience are rightfully part of anthropological/ethnographic thought and discourse (Fabian, 1983). What I am asserting is the need to knock participant observation 'off its perch'. However, if it is true that participant observation has come to occupy the methodological high ground in the ethnographic terrain under somewhat false pretences, then we should consider the possibility of opening up spaces for other qualitative research techniques, particularly interview-based studies, to move from the peripheries of this territory towards more central positions. Hockey's proclamation about the research interview as a legitimate form of participation in British culture (2002: 210) can be generalized to a discussion of 'anthropology/ethnography at home', a much misused term (Cohen and Rapport, 1995: 10) but one that can be mobilized productively enough in a discussion of the ways in which ethnographic research is impacted by its setting. The next section brings Hockey's argument into dialogue with a number of other researchers grappling with research projects that do not readily lend themselves to the traditional participatory methodologies of the broad church that should be, and is, ethnography.

Experiencing 'the near' West

The ghost of Malinowski continues to haunt anthropologists and ethnographers in sometimes unknown or unacknowledged ways. As Stocking (1983) points out, the somewhat distorted 'euhemerist myth making' of Malinowski has produced an archetype of fieldwork that idealizes anthropological/ethnographic practice as a lone activity in which the fieldworker lives for at least a year among the natives (Gupta and Ferguson, 1997). Whilst archetypes do not have to be 'true' to weave their magic, they do tend to stand as an ideal to be emulated as far as possible, 'a compelling glimpse of things as they should be, at their purest and most essential' (Gupta and Ferguson, 1997: 11). As Gupta and Ferguson conclude, the most significant factor impacting on the acceptability of research as truly anthropological is the extent to which it depended upon extensive time spent in 'the field' (p. 1).

Fieldwork lies at the mythic heart of anthropological/ethnographic research; it is what makes one a 'real anthropologist' (Gupta and Ferguson, 1997: 1; Salzman, 1986), and 'the guts of the ethnographic approach is found in direct, prolonged, on-the-spot observation' (Spindler and Spindler, 1992: 63). But what if we do not live among our research participants, or spend significant amounts of time with them in situ? What if we cannot? Can we still call what we are doing ethnography? And if not, does this lessen the importance of such studies?

As long as participant observation is positioned at the apex of the methodological hierarchy in anthropological and/or ethnographic studies then we run the risk of impoverishing not only our research processes but also our disciplinary identities (Hockey, 2002). The risks are arguably greater for anthropologists doing research 'at home' where the research may well be based less on traditional fieldwork and

more on various types of interview. Thinking about my own workplace, a department of anthropology that has always had a commitment to what British social anthropologists used to refer to as 'comparative sociology', the postgraduate students most likely to express anxiety about the methodological legitimacy of their work are those conducting research amongst settler Australians. These concerns arise because the researcher cannot 'live with the natives' in the same way as they might should they move to a Pacific Island or Indonesian village to do their work. The field is *apparently* messier for the 'at home' students, less bounded and less conducive to living at close proximity to one's research subjects. Whilst worries about the spatial boundedness of the field are more peculiarly anthropological concerns (Passaro, 1997), as Hockey (2002) points out, many a postgraduate student in cognate disciplines is reluctant to name their rich qualitative research as ethnographic lest their methods be judged inadequate by an external examiner when measured against the apparent power of the term.

A study that challenges the myth of the one true ethnography is Silverstone et al.'s (1991) exploration of 'the fine grain of the relations between domestic culture' (p. 206) in the UK and information and communication technologies. Silverstone and his colleagues found participant observation curiously inadequate to their needs. They began the research by asking the families to record in diaries their various engagements with communication technologies and followed this up with a week of participant observation in the homes of these families. While they readily acknowledge that the observations helped them escape the limitations of self-reporting, they argue quite forcibly that the research provided little more than a superficial gloss on the culture of the families in their study:

It quickly became clear that 'hanging in' would certainly provide a more or less coherent account of family life (though powerfully mediated through the person of the ethnographer) but it would not provide, within the relatively short period we had to undertake it, either a systematic analysis of technology and family interaction, or a strong basis for any point-by-point comparison between families, and it would not enable us satisfactorily to contextualise families historically and geographically – within time and space relations. (pp. 210–11)

To some extent I share the objections that can be raised by 'dyed-in-the-wool' participant observers who might counsel a certain patience in such a study. Spending more time with fewer families, allowing the depth of the encounter to reveal more about the participant's encounters with the technological instruments in their homes, what they mean to the individual persons and how they facilitate or hinder good communication could provide something of a remedy for the problems raised by Silverstone et al. But this is to return to the condescending myth of the pure and true ethnography and, as Hockey (2002) explains, many of the settings in which Western researchers conduct 'anthropology at home' are very different from the Malinowskian archetype. For one thing, the social spaces captured in many a classic ethnography reflected a cultural and physical climate that was conducive to

extended conversations in outdoor settings, 'open-air anthropology' as Malinowski called it (Stocking, 1983: 111). What this emphasizes are the differences in the socio-spatial arrangements in the settings of so many classic participant studies and Western models of differentiated private and public space (Hockey, 2002). Spending extended periods of time with families in the industrial West is simply not practicable or even desirable in many instances (Yee and Andrews, 2006) and part of the impetus for identifying the difficulties associated with participant observer studies in 'the West' arises from the realities of social interactions that are often 'spatially dislocated, time-bounded and characterized by intimacy at a distance' (Hockey, 2002: 211; see also Passaro, 1997).

As Hockey points out, whilst anthropologists can now be less apologetic about researching locally produced 'exotica', participant observation still occupies the methodological high ground. That it does this despite the fact that much of what passes as anthropology/ethnography is based upon what we hear rather than what we see is fascinating to contemplate. Hockey (2002: 209) makes the point that, for anthropologists at least, interview-led research 'risks being seen as a second choice, imposed by force of circumstances'. This tendency was evident in my own conceptualization of an interview-based research project into school choice that I described in the initial funding proposal as a pilot study, one that I hoped would open up the space for the 'real thing' – a participant observer study of the school choice phenomenon. Forty-five interviews later I was able to recognize the initial project is not 'the poor relation or handmaiden of a participant observation study' (Hockey, 2002: 210) but a substantial study in its own right, and I trust the discussion of this article has made it clear that this is not an easy step for one trained as an anthropologist/ethnographer to take. The research was conducted with what I have come to call an ethnographic imaginary (Forsey, 2008a), but was it ethnographic? Is there a need to call this, or any interview-led study, ethnography? These are questions that carry through into the final two sections.

Interviews and the ethnographic imaginary

In their recently published handbook of how to do ethnographic research, Crang and Cook (2007: 35) define ethnography as 'participant observation *plus* any other appropriate methods'. Included in the 'other' category are interviews, focus groups, video or photographic work, statistics, modelling, archive work, and so on. Defining and describing ethnography as a method is standard practice (see Walford, 2009), and as is clear from Crang and Crook's description, the conventional equation of ethnography = participant observation is alive and well. But what happens if we return to defining ethnography by its purpose rather than as a method? Shifting the focus in this way can allow us to gently remove participant observation from its lofty perch. In the democratic spirit to which I imagine many an ethnographer being attracted, we can permit a more equal commitment to all of

our senses and allow *engaged listening* to sit alongside participant observation as an equally valid way of gaining ethnographic knowledge. The same could be said of touch and smell and taste in particular conditions. Such a move would allow those of us identifying as ethnographer and/or anthropologist to perhaps be less dogmatic about what we do and how we do it. The benefits would flow to the 'discipline' or approach that is ethnography as well as to those researchers who struggle for disciplinary legitimacy because of the questions they are interested in and the ways in which they have to pursue them.

One purpose of ethnography is to provide 'a detailed, in-depth description of everyday life and practice' (Hoey, n.d.). If we add to this the interpretive and analytical imperatives that drive ethnographic practice we arrive at a definition of ethnography as a formation of study aimed at understanding and explaining the cultural context of lived experience. The data usually emerge from some sort of collective rather than from an individual; however, insofar as a person's life is engaged always in 'the contact zone', where 'cultures meet and horizons fuse' (Hastrup, 1997), it is possible to construe some life history work as ethnographic. The product is cultural interpretation, and the means for obtaining such understandings and interpretations can be many and varied, but it is difficult to imagine arriving at adequate understandings through survey work or by research that does not seek some form of deep contact that engages with the cultural and structural content of a person's life. The aim of the ethnographer is to listen deeply to and/or to observe as closely as possible the beliefs, the values, the material conditions and structural forces that underwrite the socially patterned behaviours of all human beings and the meanings people attach to these conditions and forces. When we conduct research with an ethnographic imaginary these are some of the aspects of human existence that we aim to uncover.

The spatial shifts in anthropological projects have already been alluded to, but the shifts in the object and purpose of the study also need elaboration. My own move from a field-based study of the effects of neoliberal change on a school community (Forsey, 2007) towards the process of choice evident amongst parents, students and teachers in my home town parallels these shifts in a small way (Forsey, 2008b, 2010). At a time when the study of 'a culture' and the production of a portrait of a people is less desirable, and perhaps less attainable, than it once was, there has been a shift in focus towards themes and processes as 'objects of study' that are not always amenable to observation. I can think of no useful way of observing school choice in practice; it takes place in an instant but is usually the result of some process of discernment. The best way to capture what people do is to meet with them, to interview them and invite them to tell me what they did, why they did it and how this impacted upon them. But I and my research associate (Marnie Giles) did not simply ask research participants about school choice. We conducted the interviews with an *ethnographic imaginary*, aimed at revealing the cultural context of individual lives as outlined above.

To conduct interviews with an ethnographic imaginary is to ask questions beyond the immediate concerns of the research question. They probe biography, seeking to locate the cultural influences on a person's life, looking later to link this to the pursued question, or, in the inductive spirit of ethnography, to even change the question. When Giles and I set out to find out what we could about the ways in which parents, students and teachers choose their schools, we did so seeking to find out as much as we could about the experiences of schooling for adults and child alike. We wanted to know where people grew up, how they were parented and socialized and how this influenced their experiences and choices. We asked about the work the adults did, the work their parents did, and their various aspirations. We wanted to know about the individual's experiences of formal education, their philosophies of life and education, how they viewed contemporary Australia in terms of equity and choice, and so on. In other words, listening beyond the immediate experience of locating a school as parent, student or teacher, we wanted to know about a person's social milieu, their cultural influences, in order that we might be able to make links with previous and current decision-making about schooling. Participant observation would not have allowed us to get to this sort of ethnographic information.

Beyond the ethnographic imagination there are ways in which interviewing mirrors life in many corners of the globe. Those of us living in the so-called Western world live in an 'interview society' (Silverman, 1993, cited in Holstein and Gubrium, 1995: 1). Not only are we bombarded with various forms of 'the interview' as part of our news and entertainment, employment is contingent upon our successful negotiation of interview processes, we are interviewed for bank loans or welfare payments and our conversations can sometimes take the form of an interview in our spatially dislocated, time challenged lives (Briggs, 1986; Forsey, 2008a). In this world 'of consultants and confessional chat shows', of indefinite employment, of personal coaching and online chat-rooms, interviews begin to resemble forms of participant observation (Lofland, 1971). The perceived inadequacies of the interview as a research instrument, with its extraction of fragments of time from individual lives, the restriction or even forbidding of 'embodied access to other life-worlds' and its reliance upon verbal accounts of life instead of direct experience, in fact reflect 'the ordinary features of everyday social interactions which Westerners currently live with and negotiate' (Lofland, 1971: 220). Indeed, according to Lofland (1971: 220), the apparently 'experience-far' method of interviewing can be interpreted as 'experience-near' in Western settings. Hockey (2002) makes a similar point in asking whether the distinction between real life and research interview hold up to critical scrutiny. As she suggests, the disembodied experience of the research interview can resemble a world in which relationships are often conducted in the bounded time slots of phone or email contact, or in and around cultural activities that transcend local and global spaces.

Interviews, regardless of setting, can enable us to locate the biography of the individual, and groups of persons, in the broader cultural domains in which they

live. Consequently, we should be able to link their personal story to the broader context and issues we are seeking to describe and analyse in the formal reports of our research data. In the case of research conducted 'at home' (an almost wholly anthropological concern), these contexts and issues are matters the researcher may well be already caught up in as part of her/his daily life. The interview in these circumstances, especially when conducted with an ethnographic imaginary, can be construed as a form of participant observation, or more accurately of engaged listening.

Ethnography as engaged listening

This article was never intended as a self-help manual for 'wannabe ethnographers'. If things are too hot in ethnography's kitchen I presume people know what to do. Rather, this article is offered as an invitation for those calling themselves ethnographers to reconsider what they do and how they think about and represent ethnography, both within and outside of the academy. Looking beyond what we say we do to what we actually do as a collective, I think it is fair to say that ethnographers report more of what they hear in the field than what they observe, that we listen to people at least as much as we watch them and that it is therefore useful to allow engaged listening to sit on an equal footing with participant observation when discussing what it is that ethnographers do. Doing so affords a more liberal attitude towards what is and what is not ethnography, but it does not allow anything and everything to be captured under the ethnographic canopy. Defining ethnography according to its purpose rather than its method encourages participation in, and engagement with, the lives of our fellow human beings. Being with people as they conduct their everyday duties and pastimes remains the preferred mode of ethnographic practice, but it does not have to be synonymous with it. Indeed, as I have suggested, sometimes fleeting engagement offers a more accurate reflection of lived experience than does any form of 'deep hanging out'. Life in postmodern spaces in a globalized world is often chaotic, uncontrolled and unmanageable (Passaro, 1997), and our methods need to respond to this reality.

Focusing on the purpose of ethnography can also allow us to judge the outcomes of ethnographic research more by the quality of the representation of the lived reality than with how much time one spent in living this with the persons captured in ethnographic text. Of course, this is how any work should be judged, regardless of the conditions under which it was produced, but too often work in progress is assessed according to how it is being done rather than by the strength of its findings and the skill of the analysis. Gupta and Ferguson (1997) capture this dynamic well in pointing out that, despite ethnography starting to be recognized as flexible and opportunistic, and for complicating our understanding of the various places, people and predicaments of our time via greater attentiveness to different forms of knowledge, we continue to be captive to a mythology which privileges field-based knowledge as the core of ethnographic research.

Gupta and Ferguson's solution is to decentre 'the field' as the privileged site of anthropological/ethnographic knowledge, acknowledging it instead as but one element in a multi-stranded methodology for the construction of 'situated knowledges' (Haraway, 1988). While to some extent 'one name is as good as any other as long as the work gets done' (Gans, 1999: 541), the ethnographic methodological hierarchy has evolved to the point where participant observation has been elevated to an equivalent status with ethnography. This demands that conceptual/semantic work be done. We need to push participant observation back into the ethnographic toolbox, despite seeming at times to be the toolbox into which observation, formal and informal interviews, conversation analysis, textual analysis and a host of other activities are placed. It is helpful to acknowledge, its enormous usefulness notwithstanding, that participant observation is but one tool among many. Unless we do so then, as Hockey (2002) points out, we run the risk of maintaining an unnecessarily narrow conception of what is worthy to be called ethnographic. Ethnography will be restricted to the study of 'islands and isolates' – to villages and institutions. We also risk losing the contributions of those whose professional duties and funding sources militate against extended participant observation. But most significantly, we place unnecessary boundaries between the ethnographic 'us' and those 'others' whose work is judged as inadequate despite it being richly ethnographic not only in its imaginary, but also in its practice.

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