Literature Review: Collaborative Ethnography

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In this brief literature review I aim to draw out some aspects of collaborative ethnography that seem pertinent to our project, and suggest some resources for further enquiry. In order to show what is specific to collaborative ethnography as an approach it is necessary to first substantively discuss ethnography – its parent methodology.

Ethnography

Ethnography is an important qualitative methodology, especially within the social sciences, in particular anthropology. According to John Brewer, ethnography:

> is the study of people in naturally occurring settings or 'fields' by means of methods which capture their social meanings and ordinary activities, involving the researcher participating directly in the setting, if not also the activities, in order to collect data in a systematic manner but without meaning being imposed on them externally (Brewer, 2000, p. 10).

As a methodology, ethnography is characterised by the following process: initially the researcher will spend extensive time in the 'field', often living with the people they are studying. This first phase (sometimes referred to as 'participant observation') involves a combination of approaches to data gathering: participating in activities to a greater or lesser extent, observing activities, formally and informally interviewing group members (who are often termed 'informants' in the literature), and examining documents and literature produced by the group. Researchers produce 'field notes' on this process as they go in a variety of forms, for example text, drawing, sound recordings, film, or photography. This initial period of immersion and empirical work is followed by the researcher returning to their home (institution/"desk') to organise and write up their field notes into some kind of narrative form. This is a constructive work of 'cultural description' (Wolcott, 1973): re-describing what the researcher has observed, traditionally into the form of a single-authored monograph. A sharp distinction between field and desk, or fieldwork and writing is present from the anthropologist Bronislaw Malinowski's pioneering work onwards (for example, Malinowski, 1922). Malinowski saw a distance between field and desk as necessary for the production of scholarly work, a process which he saw as inherently 'anti-social' (Mosse, 2006, p. 937). I will return to the implications this history has for collaborative ethnography later in this paper.

Historical Developments

Ethnography developed in the early 20th century, primarily in Britain and North America. First associated with anthropologists such Malinowski, and Franz Boas, ethnography was initially a method of gathering detailed information and producing knowledge about 'exotic lands' much desired by the European (and especially British) administrators of empire.

Ethnography formalised as an approach after the period of conquest and empire building, when the successful governance and assimilation of colonised people as new subjects of empire were priorities. For an important, critical historical account of how the production of knowledge of/about the colonised 'other' contributed to social control see Edward Said's *Orientalism* (Said, 1979).[ii] The second early phase of development was within the Chicago school of sociology led by Robert Park. Their studies focused on experiences of North American urban life, especially the lives of dispossessed and marginalised people: those seen as 'deviants' and 'outsiders'. As we can see, a preoccupation with difference is inbuilt in both these early approaches to ethnography, with asymmetrical power relationship between the researcher and researched, often expressed also in differences of language, race, class and gender. As many have commented, within the social sciences more generally there is a tendency for researchers to 'study down' producing more work on the working class, ethnic minorities, and poor or marginalised people than the powerful, and the middle or upper classes. As Laura Nader pointed out in an influential article in 1972, this is partly because more powerful members of society are better at protecting themselves from being researched and represented (Nader, 1972). An interesting historical instance of collaborative research with non-academics is the British Mass Observation movement.[iii] Initiated in 1937 by the poet Charles Madge, filmmaker Humphrey Jennings and anthropologist Tom Harrisson, the project invited volunteers to research and report back on their daily lives using photography and diaries, and answering surveys. Once the material was collated, the researchers analysed it, proposing patterns and connections between accounts. The creators of the project were more interested in how people's accounts of their daily life reflected contemporary feelings, and about their own interpretation of their actions and their self-selecting processes, rather than what the reports could be used to evidence about the way people spent their time. As such, this could arguably be seen as a proto-collaborative ethnography.

As we saw, Brewer suggests a key component of ethnography is the study of 'naturally occurring settings.' As such, ethnographers do not *create* a group to study, but identify a pre-existing group, and then immerse themselves in their culture/ way of life. Traditionally the ethnographer aims not to interfere, influence or change their host culture through their involvement. However, the role that the ethnographer may play in re/creating, re/shaping or maintaining that group through the study of these people *as* a group is something ethnographers are increasingly reflexive about. For example, James Clifford influentially referred to the production of field notes as an act of 'inscription' in which 'the flow of action and discourse has been interrupted, turned to writing' (Clifford 1990 quoted in Atkinson, 1992, p. 17). Simply the fact that the ethnographer is noting and developing questions based on these observations to bring back to the group, shows that interference is intrinsic to the endeavour of ethnography. There are also important questions to raise here about what constitutes a culture or society, for example what are the limits of the culture, especially within contemporary globalisation? Which cultural practices are truly shared by all members? Is the culture imagined to be open or closed, by whom? The historical tendency within ethnography has been to treat cultures and group as if bounded, employing an 'island' metaphor as a way of making analysis more manageable (Kuklick 1996 in Wardle and Gay y Blasco, 2011, p. 118). Another consideration which has long preoccupied ethnographers is when to end their fieldwork. In other words when has the ethnographer experienced enough of the host culture been experienced to enable meaningful analysis? For instance, cultures look very different dependent on the time of year, in periods of celebration, or according to

whom you spend your time with. As an illustration of this, important feminist interventions into ethnography often showed how different cultures appear if you spend time with female informants rather than (the traditionally consulted) male group members (Abu-Lughod, 1988; Abu-Lughod, 1990).

Representing Complexity

The influence of George Herbert Mead and Herbert Blumer's 'symbolic interactionism' on the early development of ethnography shows in the strong emphasis placed on the meaning making of group-members rather than that of the researcher. In order to understand how the host culture is lived and experienced in terms of beliefs, rituals and practices, ethnographers often focus on the ordinary activities of 'daily life' (also variously referred to as 'micro-social,' 'face-to-face,' or 'mundane interaction' in the literature). As Brewer notes, this emphasis on lived understandings has the effect of predisposing ethnographers to study topics that people can and will tell you their beliefs about (Brewer, 2000, p. 35). The extent to which the researcher can truly shake off their own assumptions and the shaping hands of their parent culture, and be led by the meaning making of research participants is a long running point of tension within the literature.

Ethnography has always been aesthetically entangled with fiction, developed out of older writing forms such as the travelogue, and drawing on literary devices to compellingly conjure up cultures (see Atkinson, 1992). As Ruth Behar notes, early 20th century women anthropologists such as Zora Neale Hurston and Margaret Mead didn't separate creative writing from critical writing, and experimented with different forms. At the time, this closeness to fiction was seen to endanger the young discipline's claims to the status of a science, and their experimentation was marginalised. This work was rediscovered by a new wave of feminist anthropologists in the 1980s and 1990s (Behar, 1995), a key figure in this later period of feminist textual innovation is Laurel Richardson (Richardson, 1990, 1993, 1998) See (Crockett Thomas, forthcoming).

Traditionally ethnographic writing aimed to capture as much detail about the field studied as possible, Clifford Geertz influentially termed this style of writing 'thick description' (Geertz, 1975). Importantly, thick description serves as a performance of the researcher's authority – of their being sufficiently inside the culture (sometimes called an 'emic' perspective (Fetterman, 1989, p. 12)) to provide a full and compelling account of its (singular reality). However almost since its formation, some have seen the conflation of ethnographic representation with the complex human activity studied as a fiction, and have sought to foreground this in their accounts, for example in Gregory Bateson's *Naven* (Bateson, 1936; Wardle and Gay y Blasco, 2011, pp. 118–9). Since the 1980s, critiques of research which rests its truth claims unreflexively either on numerical proof (positivism) or people's accounts (humanism) have become more commonplace, with many scholars suspicious of claims to authenticity, final truth or a reality that can be accessed in an un-mediated form. An important ethnographic text in this regard is Margery Wolf's *Thrice Told Tale* (Wolf, 1992). This is a sophisticated demonstration of the power of different writing forms to produce differing effects – not least the effect of scientific veracity. In her text Wolf presents three different versions of the same events, as fieldnotes, as a classic anthropological analysis, and as a fiction. Here, the reader is able to see for herself how the ethnographer emphasises

different parts of her research to produce a different version of the real. This is not to imply that ethnographers committed to attempts to represent contradiction and complexity treat all things as equally true or meaningful. Ethnographers such as those coming from science and technology studies (STS) (Latour and Woolgar, 1979; Strathern, 1992; Latour, 1996; John Law and Mol, 2002; Mol, 2003), or those who draw on the work of Mikhail Bakhtin and attempt to reproduce a 'polyphony' (Bakhtin, 1984, 1982) of voices and perspectives in their work, still attempt to establish 'partial truths' (Clifford, 1986) and make interpretive judgements about a reality that is nevertheless conceptualised as emergent.

The Reflexive Turn

In the 1980s, critical interventions made by feminist, postructuralist, and postcolonial scholars encouraged researchers to think more carefully about the politics of their practice, and check their assumptions about difference, representation and knowledge (see Haraway, 1988). Within anthropology this played out through intense debates around the capacity of anthropologists to represent other people's cultures (see Clifford and Marcus, 1986). Qualitative approaches to the social sciences in particular have become increasingly explicit about the role of the researcher in producing and shaping research narratives. This intellectual shift is now commonly referred to as the 'reflexive turn' in knowledge production. For some this imperative to reflect on their role as producers of knowledge is experienced as a crisis in scholarly legitimacy, but for many it is helpful: recognising that knowledge is always partial and shaped by context actually improves the quality of the analysis, discouraging researchers from making generalised claims that might be unsupported or contradicted by their data.

Reflecting on the history of ethnography, Amanda Coffey posits the existence of a 'hidden ethnography': an aural culture of professional anecdote and storytelling about the material which was excluded from accounts because it was feared to be too controversial to publish (Coffey, 1999, p. 90).[iv] Sometimes this was for the sake of protecting participants especially those with practices that are seen as deviant (see for example, Crapanzano, 1986, 1992). Or because the production of scholarly authority has traditionally depended upon presenting the impression of 'objective,' 'scientific' distance between the researcher and the research/ed. Feminist researchers (for example Hochschild, 1983, 1989) led the way in putting into practice the mantra that 'the personal is political' and rejecting the performance of distance and authority in favour of intimacy. Here, they acknowledge the complex emotions and non-verbal affectivity of the relations that underpin ethnographic research and knowledge.[v] As Kirsten Hastrup writes, the question of what constitutes evidence: 'is enfolded within the relational nature of anthropological knowledge that – epistemologically – precludes the use of evidence as an independent measure of validity' (Hastrup, 2004, p. 461). For Hastrup, unable to prove through an appeal to evidence, ethnography:

> aims at a kind of *explanation* beyond the truth of the events themselves. Anthropological knowledge, then, is not simply knowledge about particular events, practices and ideas, but about the processes by which these come to appear meaningful, perhaps inevitable or mandatory, possibly contestable or even mad (Hastrup, 2004, p. 468).

Similarly, Marylin Strathern writing about the influence of chaos theory, and non-Euclidean geometry on contemporary anthropology writes ‘fractal graphics could describe the patterning of maps or genealogies, but they would be maps without centers and genealogies without generations. It is the repetition, the not-quite replication, to which the viewer is compelled to attend’ (Strathern, 1991, p. xx). This invitation to de-center our thought, and attend to the patterning of non-identical repetitions and new mutations, perhaps provides a mooring in approaches which aim to work with complexity. I hope such approaches might help us in our attempt to make sense of the multiple meanings and interpretations that collaborative practice will produce.

Collaborative Ethnography

Although a ‘still-emergent practice’ (Lassiter, 2005, p. 150), collaborative ethnography is growing in popularity within the social sciences. Luke Eric Lassiter, one of its most prominent exponents (Lassiter, 1998; Lassiter et al., 2002; Lassiter, 2004) argues for its recognition as a distinct method that differs from earlier models based on reciprocation. He quotes at length from his mentor Glenn Hinson:

> Reciprocation entails an act of return, a giving back for something received. In the ethnographic process, this sets up a model of exchange where one thing granted (e.g., an interview) yields an appropriate reciprocal response (e.g., help planting a garden). What this does not imply is constant ongoing discussion, where the project that yields that interview in the first place is co-conceived by both participating parties (Glenn Hinson quoted in Lassiter, 2005, p. 17).

According to Lassiter *collaborative* ethnography is:

> an approach to ethnography that deliberately and explicitly emphasizes collaboration at every point in the ethnographic process, without veiling it—from project conceptualization, to fieldwork, and, especially, through the writing process. Collaborative ethnography invites commentary from our consultants and seeks to make that commentary overtly part of the ethnographic text as it develops. In turn, this negotiation is reintegrated back into the fieldwork process itself (Lassiter, 2005, p. 16).

Lassiter’s emphasis on deliberate and explicit collaboration foregrounds the fact that all ethnography is collaborative whether or not this is acknowledged by ethnographers. In short, it cannot happen unless the people whose experiences the researcher is interested in understanding are willing to talk to the researcher about them: ideas are produced through dialogue. This is reflected in his use of the word ‘consultant’ rather than the ethnographic standard: ‘informant’. Most ethnographers return some version of the text they’re producing to their research participants to check that they feel adequately and accurately represented, but few attempt to co-author texts. Indeed, even within collaborative ethnography while all texts are ‘arguably co-authored, not all can actually be co-written’ (Glenn Hinson 1999 paraphrased in Lassiter, 2005, p. 144).

Endeavours to think with or produce work in equal partnership with those whose lives the ethnographer is studying is by no means new. According to Lassiter, in the USA collaborative ethnography 'has a rich but marginal heritage' (Lassiter, 2005, p. 18). In his account he foregrounds the important contributions made to a more democratic ethnography by research participants turned author-critics such as the Native American Vine Deloria's *Custer Died for Your Sins* (Deloria, 1969). Comparably in South America, a commitment to social engagement has evolved a distinctively collaborative approach to ethnography (Ramos 1990 in Rappaport, 2010, p. 3). An important influence on South American anthropology is the participatory action research associated in particular with Orlando Fals-Borda (see, for example Fals-Borda and Anisur Rahman, 1991).

Lassiter asks 'when does anthropology serve the… relationships created and maintained by anthropological practice' (Lassiter, 2005, pp. 18–19)? His desire for a more collaborative form of working partly came out of concerns about how his research could be more relevant for the people he consulted. A related concern was the impact of his participants seeing themselves represented in the ethnographer's text. Although experiencing representations of oneself can be a positive or unproblematic experience for participants, David Mosse argues that once it is read by research participants, 'ethnographic writing begins to have significant social effects of its own. The detachment of writing is now socially experienced by others… It may be anti-social' (Mosse, 2006, p. 937). Mosse reflects back on Malinowski's insistence on separating field and desk, and what this means for the experience of participating in ethnography. He continues 'when the desk collapses into field, something important has changed in the structure of ethnographic practice. We are starkly confronted with the essentially relational nature of anthropological knowledge' (Mosse, 2006, p. 937).

The Reflexive Turn and Collaborative Ethnography

There is an inferred transparency and ease to increased researcher reflexivity, however reflecting on or articulating (perhaps troubling) feelings about research and fellow research participants can be difficult for the researcher as well as the researched. Further, expressing these to research participants whether on paper or in conversation can be painful, awkward or risk destroying those relationships (Brettell, 1993; Hollway and Jefferson, 2000). This risk is heightened in participatory research where you need to be able to work together for the research to happen. Here, there is a danger that in order to preserve the collaboration members are unable to be honest with each other, or fall into a position of mutual reinforcement, or into a hierarchy of knowledges.

Mosse (2006) has written a very interesting account of his fraught experiences of working as an ethnographer as part of a complex and ambitious international development project which championed participatory methods. In his resulting ethnography Mosse argued that 'the project did not "work" because it was well designed' (Mosse, 2006, p. 941). What he means by this is that participation was built into the project at every level, but the size of the group and conflicting agendas of multiple agencies involved made collective practice and analysis impossible. From his perspective, what actually made a success of the project was actors taking 'control over the interpretation of events' (Mosse, 2006, p. 940). Unsurprisingly, his analysis was seen as a betrayal of his and his fellow actors' commitment to participatory

methods, and further, evidence that the ethnographer himself as exercising too much control over the interpretation of events. Here 'ethnographic writing threatened the project as an "epistemic community" – a set of relationships around shared meanings – drawing anger from those dominant figures whose prestige was most closely tied to authorised representations' (Mosse, 2006, p. 945). Mosse's experience reminds us not to underestimate the way in which the language of participation can be utilised by powerful actors (including the researcher) in ways that undermine the potential for real collaboration (which includes conflict).

Theorising Collaborative Ethnography

Unlike the natural sciences, the social sciences often focus on topics about which non-researchers have lay knowledge or a 'common sense' understanding. The difference between a common sense understanding and an understanding reached through a research process is not always obvious especially when using qualitative or creative methods. This is especially important to consider when undertaking collaborative ethnography because of the way ethnography foregrounds the meaning-making of research consultants. Within ethnography participants are treated as 'experts in their own lives.' Additionally, all participants bring knowledge to the collaboration that comes not from direct experience but from exposure to previous representations of the topic. The question of how much authority to give people's accounts in a group with different people (including academics from different disciplines), who therefore have different understandings, needs to be negotiated carefully by the group. For example, how should a scholar who wants to honour 'partial truths' treat essentialist claims made by a fellow research participants?

Lassiter notes that like all methodological approaches collaborative ethnography has limitations, and he highlights the difficulty of equitable collaboration when the discipline has emerged from western anthropology and has implicit 'ethnocentrisms about the construction of equity, democracy, and social justice' (Lassiter, 2005, p. xii). This returns us to the challenge to the researcher to de-naturalise their own positionality. Elaine Lawless developed a collaborative approach to writing that she terms 'reciprocal ethnography' (Lawless, 1992, 1993), and her work is an important contribution to collaborative ethnography. Lawless's approach is based on a desire to deny the 'hierarchical constructs that place the scholar at some apex of knowledge and understanding and her "subjects" in some inferior, less knowledgeable position' (Lawless, 1993, p. 5). As such, her research participants are involved in the co-theorisation and direction of the project. The differing positions taken by herself and her research participants are woven together throughout the text.

A similar approach was taken by Les Back and Shamser Sinha in their collaborative study of the experiences of young migrants in London:

> This study encourages its participants to become observers of their own lives and produce their sense of the social world that they inhabit through making representations, be it through photography, creative writing, collage and scrapbooks. The analytic procedure developed in the collaborative work is not about assuming that they capture a straightforward authentic reality. Rather, we treat these representations as a means to produce or assemble the social world through the work itself. These poems, pictures and images offers a basis to enter into dialogue with the participants over what is contained within them, including their blindnesses or assumptions as well as insights and understandings. The work, then, offers us the possibility

> to shuttle between different horizons of understanding in which the participants also become authors (Back et al., 2012, p. 143).

I have quoted at length from their description of this project, because of the way in which they skilfully negotiate some of the issues I have just raised around collaboration, authority and authenticity. Their approach was cognisant of the danger of creating a 'pseudo-participatory form of research… where the interviewer remains in control of the topics, order and structure of the research dialogue' (Gibson and Brown 2009, paraphrased in Sinha and Back, 2014, p. 5). Research participants chose the kinds of methods they employed and commented on the development of the research design and outputs throughout the project. In a journal article arising from the project they attribute co-authorship to one of the research participants to reflect her contribution to the work (Back et al., 2012, p. 152 footnotes 2 and 4). Elsewhere they describe their approach an attempt to develop more 'sociable methods' (Sinha and Back, 2014), emphasising the importance of dialogue and exchange. They felt this was particularly important considering the history that the group had of interrogation and extractive interviews by the courts and immigration service (Sinha and Back, 2014, p. 2).

Michael Fischer and George Marcus suggest that within anthropology, collaborations are still 'less well articulated or recognized' than more individualistic forms of ethnographic writing (Marcus and Fischer, 1999, p. xvii; Lassiter, 2005, p. 18). Joanne Rappaport's (2010) article is particularly helpful in addressing this deficit as it focusses substantively, not on the content of what has been produced through a particular collaborative ethnography, but on 'collaboration as a space for the coproduction of theory' (Rappaport, 2010, p. 2). Rappaport seeks to understand 'how researchers come to learn through collaboration' (Rappaport, 2010, p. 2). She contends that this is what makes collaborative ethnography different from similar participatory methodologies where 'subjects participate as research participants but have little control over the research' (Rappaport, 2010, p. 2). Instead, collaborative ethnography shifts 'control of the research process out of the hands of the anthropologist and into the collective sphere of the anthropologist working on an equal basis with community researchers' (Rappaport, 2010, p. 6). Rappaport's research was conducted in collaboration with a southern Colombian indigenous organization. In this case the research team was comprised of indigenous researchers, Colombian anthropologists, and North American scholars. Here, co-theorisation was made possible through a commitment to long-term dialogue, trust, and crucially – interlocutors who could take the lead in co-theorising (Rappaport, 2010, p. 23). Co-theorisation was aided by the indigenous participants in Rappaport's project beginning by reviewing and critiquing previous anthropological accounts of their culture, including accounts of themselves as a people. In her account of this Rappaport (2010, p. 7) draws on Rey Chow's description of this process as a critical awareness: a form of 'being-looked-at-ness' which is a feature of the kinds of autoethnography undertaken by those 'formerly ethnographized' (Chow, 1995, p. 180).

Within Rappaport's project, group members had different priorities, and the indigenous members participated primarily because the research would be helpful for the intellectual and political agendas of their indigenous organisations, rather than for academic ends (Rappaport, 2010, p. 10). Much of her essay explores the tension between these different positions, and the moments when dichotomies of inside/outside are mobilised thus eliding differences within the indigenous cultural groups into a more homogeneous

indigenous (insiders)/ non-indigenous (outsiders). Rappaport recognises that research participants sometimes adopt a temporary 'strategic essentialism' (Spivak, 1990) in order for their experience to be recognised and taken seriously. As such, she advocates the adoption of what Paul Gilroy (1993) terms an '"anti-anti-essentialist" perspective, which, through complicity—and not just intellectual but also political—melded the urgency and utopian standpoint of [indigenous participants] Perdomo and Piñacué with the thick description of "good ethnography"' (Rappaport, 2010, p. 23).

From this brief review we might surmise that collaborative ethnography is a challenging enterprise, but that the risks also pose opportunities for much innovation. As a final example of this, Lassiter recounts an episode in the creation of Hinson's *Fire in My Bones* (Hinson, 1999), in which the ethnographer was made to promise a key consultant in the study that he would include a direct invitation for the reader to convert to Christianity in the final monograph (Lassiter, 2005, pp. 147–149). Here Hinson faced a dilemma knowing how the inclusion of such a direct message would risk compromising the text's appearance of scholarship in the eyes of its academic readers. However, he felt that not to do so would render the collaboration a charade. As an inventive solution Hinson presented the book as a chronological account of a religious service. In his evocative description of the service, he managed to create a container for the conversion message that satisfies both audiences and collaborators.

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[i] Distant Voices (2017-2020) aims to explore and practice re/integration after punishment through creative collaborations (primarily songwriting) and action-research. It is a partnership between the universities of Glasgow, Edinburgh and the West of Scotland, and the Glasgow-based arts charity Vox Liminis. The project is funded by the Economic and Social Research Council (ref: ES/P002536/1). The project website is: https://www.voxliminis.co.uk/projects/distant-voices/. Phil Crockett Thomas is the project's research associate.

[ii] Said draws on Michel Foucault's philosophy to understand the interrelationship of knowledge and power: 'power-knowledge' (Foucault, 1990), and his related concept of 'governmentality' (Foucault, 1991).

[iii] For more information about both the initial and the current project see http://www.massobs.org.uk/.

[iv] Shane Blackman's (2007) article is an interesting account of this 'hidden ethnography'. Here he discloses his ethnographic experiences of sharing drugs with, getting drunk with, and having sexual, and emotional relationships with his research participants. Such actions could be seen to be an unethical abuse of power, and Blackman does not lionise his behaviour, however he argues that this intimacy was an integral part of establishing bonds of trust that enabled the research to happen.

[v] For helpful critical accounts of the deployment of reflexivity as a resource rather than a paralysing trap I recommend (Behar, 1996; Skeggs, 2002).