Separately and Together: Reflections on Conducting a Collaborative Team Ethnography in Dementia Care[1]

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Abstract

Ethnography has classically been conducted by a single scholar, often referred to as a 'Lone Ranger', who develops an interpretation of a cultural setting based on their immersion within it. Overshadowed by this historical icon and located within an academe that privileges individual achievement, ethnography as a group effort has received less consideration in the methodology literature. Nevertheless, team ethnography conducted by multiple researchers at one or more research sites is becoming increasingly common and requires explicit attention. With reference to the existing literature and a recent team ethnography of in-patient dementia care, this paper argues that team ethnography is not just a case of „add researchers and stir", but rather a methodology entailing specific issues and considerations. In particular, the paper explores how the two-phase design of the dementia care ethnography – which entailed researchers collecting data first as conventional lone ethnographers, then as a team with shared understandings and objectives – brings to light the opportunities and challenges of this methodology.

Introduction

With doctoral projects in the social sciences a notable exception, teamwork characterizes contemporary research practices across the disciplines and research teams seem to be growing in size and diversity with

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[1]This title is an adaptation of Siltanen et al.'s (2008, p.46) phrase 'separately together', which refers to 'working reflexively as individuals and together as a team, to forge from separate and partial perspectives a common understanding of our research subjects/objects'. The difference is that our individual and collective work had a linear element (separately then together) as well as a parallel element (separately but also together)

each new funding bid. The benefits of teamwork as an 'academic mode of production' (Mauthner and Doucet, 2008) are generally accepted as self-evident: if two heads are better than one, then four must be better than two, and so on. Clearly, there are not only intellectual imperatives implicit in this trend; as higher education budgets become tighter and competition for research funding continues to increase, two researchers on a funding bid are seen as better than one, and so on, in terms of demonstrating interdisciplinarity and potential impact. In the case of ethnographic research, the teamwork approach contrasts sharply with the enduring and iconic Lone Ranger model of data collection (Douglas, 1976), whereby a single ethnographer generates an interpretation of a particular setting through his/her immersion within it (Erickson and Stull, 1998). Nonetheless, although teams of researchers have crept quietly into the ethnography arena, team ethnography has largely escaped critical interrogation in the methodology literature (significant exceptions include Bresler, Wasser, Hertzog, and Lemons, 1996; Creese, Bhatt, Bhojani, and Martin, 2008; Erickson and Stull, 1998; Gerstl-Pepin and Gunzenhauser, 2002; Liggett, Glesne, Johnston, Hasazi, and Schattman, 1994; Mauthner and Doucet, 2008; May and Pattillo-McCoy, 2000; Sanders and Cuneo, 2010; Wasser and Bressler, 1996). This paper draws on the authors' recent experience conducting team ethnography of inpatient dementia care to investigate how the interaction between research design and teamwork processes impacts the type and quality of research findings. In particular, we argue that the benefits of team ethnography were brought to light and enhanced by the distinctive two-phase design of the project.

Team Ethnography: Definitions and Considerations

Feminist, post-modern and other critiques of knowledge, power, and identity have brought ethnography a long way from the classic anthropological model, whereby a single scholar would travel to a far-flung land and return, after a year or more, to produce a holistic literary account of the peoples encountered there. As van Maanen (2006, p. 16) argues, 'attempting to depict in writing what it is like to be somebody else – arguably, ethnography's main claim to fame – has never been a simple matter but today it appears almost Herculean, given the problematic nature of identity in the contemporary world. A certain instability, rupture, uncertainty and fluidity of meaning attends then to some of the best of contemporary ethnography'. Acknowledging that meaning is multiple, fluid, and contestable provides entrée for a team of researchers to conduct ethnography collaboratively, 'pooling their hypotheses' (Delamont 2002 in Creese et al., 2008) in order to reach a richer collective understanding. However, overshadowed as it is by the enduring

Lone Ranger and located within an academe that has traditionally privileged individual achievement, ethnography as a group effort has received little attention in the methodology literature (Lapovsky Kennedy, 1995). A notable exception has been the interest in participatory research involving collaboration between a single ethnographer and his or her research subjects, which yields a distinct set of issues and rewards[2].

Underlying an explicit focus on team ethnography is the assumption that it does not represent a straightforward replication of traditional ethnography on a larger scale (Erickson and Stull, 1998; May and Pattillo-McCoy, 2000). Certainly, taking a team approach may expand the scope of the research, generate more comprehensive data, and thus yield more robust results. There is a substantive difference, however, which distinguishes team ethnography: namely the collaborative imperative whereby multiple researchers must share their observations, confront inconsistencies between interpretations, and consider alternative evidence in an ongoing manner. Wasser and Bresler (1996, p. 7) argue that this collaborative process takes place within the 'interpretive zone', defined as 'the crucible where researchers sift, sort, and consider the meaning of fieldwork'. Each researcher brings a different set of identities, experiences, knowledge, and beliefs to the interpretive zone, and from that diversity the team must strive to derive new collective understandings through discussion and debate.

The limited literature on team ethnography brings to light several key issues which must be taken into account when considering how ethnographic teams forge their way through the interpretive zone. First is the practical issue of fieldnotes (Creese et al., 2008; Erickson and Stull, 1998; May and Pattillo-McCoy, 2000). For solo ethnographers, fieldnotes serve as 'aides-memoires, initial analyses, and archives to be visited when away from the field' (Erickson and Stull, 1998) and have been referred to as ethnographers' invisible work. For team ethnographers, fieldnotes become the very visible vehicle by which each member communicates their contextualised observations to the rest of the team. The process of transforming scribbled fieldnotes into a format that will be digestible by the entire team entails an additional level of translation, explanation, and interpretation that is unique to team ethnography. In the next step, those cleaned-up fieldnotes serve as the 'terms of engagement' for team discussions (Creese et al., 2008) or the parameters of the interpretive zone. On a more subtle level, as Creese et al.

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[2]In particular, participatory research sets out to dissolve the distinction between researcher and participants, such that participants share responsibility for developing, questioning, and changing the research. In contrast, while critical ethnography does aim to recognise and address the power relations between participants and researchers, the researcher retains primary responsibility for the direction and design of the research.

(2008) argue, fieldnotes actually become constitutive of the team as well as generating terms of engagement for the team. Through their fieldnotes, which are written with an audience in mind, each researcher tells a story about their particular place in the field and, to a greater or lesser extent, may reference the place and perspective of others on the team.[3]

The second consideration is the composition of the team, which affects the research process in two key ways. First, each researcher approaches data collection and interpretation differently, based in part on their personal and intellectual biographies, including their age, gender, ethnicity, and educational background (Creese et al., 2008; Gerstl-Pepin and Gunzenhauser, 2002; May and Pattillo-McCoy, 2000; Mitteness and Barker, 2004). Ethnographic research has long recognised that who the researcher is influences what the researcher discovers (Savage, 2000; Woods, Boyle, Jeffrey, and Troman, 2000) – but this becomes acutely obvious in team ethnography, as the juxtaposition of one researcher's fieldnotes against those of the other researchers highlights the different perspectives from which similar settings or phenomena can be viewed. Second, and in more practical terms, the collaborative process within the interpretive zone requires close teamwork over an extended period of time. Therefore, it is critical that the team is comprised of members who can work together efficiently and effectively (Barry, Britten, Barber, Bradley, and Stevenson, 1999). Whitt and Kuh (1991, p. 335) found that the composition of the research team was absolutely critical to the success of their multi-site ethnography, as well as a source of personal reward for individual team members: 'For ten months, we worked together almost constantly, a situation that occasionally tested our patience and sense of humor but also provided each team member within a tremendously powerful and satisfying learning experience as well as lifelong friends and colleagues'. Similarly, Liggett et al. (1994, p. 78) write that 'from the beginning it was clear that identifying the right blend of colleagues to carry out the research was probably the most important decision made'. Sanders and Cuneo (2010) go further in arguing that collaborative qualitative research is underpinned by a 'socio-emotional process', by which the reliability of findings depends on team dynamics and researchers' emotional relations as well as logical and conceptual decisions.

Given how closely researchers must work together despite their distinct interests, priorities and perspectives, trust is acknowledged to be another critical component of team ethnography (Sanders and Cuneo, 2010; Wasser

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[3]As an aside, Mauthner and Doucet (2008, p. 978) critique the over-reliance in team research on textual over embodied forms of knowledge, given that 'scholars are increasingly highlighting how, in practice, our understanding of the world draws on a much broader 'ecology of knowing' than suggested by dominant textual approaches'.

and Bressler, 1996). Team members must trust each other enough to share their fieldnotes, which are personally crafted and traditionally guarded with care. Individual members must also feel confident in speaking openly without fear that their ideas (and by extension their academic integrity) will be criticised, derided, or betrayed outside of the team. Sanders and Cuneo (2010, p. 337) comment on coding in particular: 'The act of coding ... is not only a cognitive assignment of text to a category, but also an emotional investment of 'who the coder is' as a professional academic researcher. A team member's challenge of a colleague's coding decision questions not only the cognitive logic of the coding assignment, but also the legitimacy and identity of the coder as a professional academic researcher'. More broadly, team members must trust each other enough to remain in a working relationship characterised by ambiguity, discomfort and dissent (Gerstl-Pepin and Gunzenhauser, 2002).

This links closely to the fourth consideration regarding team ethnography, which is the value of conflict (Agar, 1996; Creese et al., 2008; Wasser and Bressler, 1996). In the interpretive zone, 'ambiguity reigns, dialogical tension is honored, and incommensurability is seen to have special value' (Wasser and Bressler, 1996, p. 13). As already stated, when multiple researchers examine a similar setting, each will generate a different interpretation. These contradictions, rather than destabilising the research project, form the basis for further examination, discussion and, ultimately, more nuanced understandings. Embracing contradiction – rather than defaulting to the relatively safer 'politeness of academic rhetoric' (Agar, 1996 in Creese et al., 2008, p. 214) – can be exceedingly difficult, as team members must be prepared to accept alternative explanations about what they consider possessively to be 'their people' or 'their data' (Gerstl-Pepin and Gunzenhauser, 2002).

A final consideration is that, despite this important 'multi-vocality' (Siltanen, Willis, and Scobie, 2008), team members must strive to foreclose the interpretive zone by formulating consensus (Erickson and Stull, 1998) so that reports may be written, papers presented, recommendations generated, and new research directions identified. Thus, team ethnography as a methodology is underpinned by a paradoxical tension between honing in on a shared story about the data without obscuring opportunities for multiple and expansive interpretations (Gerstl-Pepin and Gunzenhauser, 2002; Lapovsky Kennedy, 1995; Liggett et al., 1994).

The literature thus suggests that team ethnography is a valuable tool for obtaining rich, broad-based descriptions of particular settings and indentifying factors and conditions across settings; however, it is not just a case of 'add

researchers and stir'. Working within the interpretive zone requires time, patience, trust and a meta-awareness of the social processes of the group as a methodological tool. The team must be explicitly positioned, because its character, tone, assumptions, and functioning will shape the research outcomes. It is important to consider what may be added through teamwork – in terms of depth and breadth of the data, contradictions and conflict, and more nuanced understandings – but also the challenges and trade-offs that might be involved. With that caveat in mind, we now introduce the inpatient dementia study, a team ethnography carried out in three dementia assessment and treatment wards in one large mental health trust in England.

Study Background

For healthcare assistants at the frontlines of care, working with dementia patients entails significant physical and emotional strain along with minimal financial compensation. In fact, this group of staff (also known as nurses' aides, direct care workers, or similar) are among the lowest-paid groups in health and social care (Noelker and Ejaz, 2005). Yet people continue to do this work, despite the low status and lack of obvious rewards. The dementia care study investigated how such workers persevere and why, with a view to providing recommendations for the recruitment, retention, and recognition of a workforce, which is adequately equipped to provide care for the growing numbers of people with dementia in years to come.

Starting in September 2008, three field researchers (KS, SB and JL) each spent three months working as part-time, supernumerary healthcare assistants in one of three inpatient dementia assessment and treatment wards in one large NHS mental health trust in central England. In advance, the principal investigator had visited each site to personally apprise staff of the research and address their questions/concerns, and information about the study was also posted on public notice boards on each ward. As field researchers, we participated in all relevant duties and recorded our experiences, observations, and personal reflections after every shift, generating through these daily fieldnotes a 'thick' descriptive account which constituted the main dataset for analysis. After conducting preliminary analyses of the data, we returned to the wards for three more months of fieldwork, first to validate initial analyses through further participant observation, then to conduct focus groups and in-depth interviews with staff[4].

Through participant observation, we were able to experience firsthand

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[4]This was not a 'multi-sited ethnography' according to Marcus's (1995) well-known definition, but what he calls a 'conventional controlled comparison', as our intention was to triangulate findings by studying three reasonably similar sites instead of one.

some of the challenges and rewards of the direct care role. Although our observations were guided by a series of broad research questions about staff motivation, stress, wellbeing, and reward, specific data-collection criteria were not defined in advance. Instead, in our fieldnotes we recorded all the issues emerging through the everyday process of putting on the same uniform and 'mucking in' with the staff team. This included: interactions between and among staff, patients, and other ward actors; the practical tasks of the job; peaks and troughs in the ward routine; moments of humor, conflict, boredom; and much more. This method allowed us to describe the healthcare assistants' role from their perspective, generating data which was grounded in their empirical world and meaningful within their frames of reference (Savage, 2000; Whitt and Kuh, 1991). Rather than relying exclusively on snapshot impressions or participants' own assessments of their actions and motivations, sustained participant observation allowed us to reach more nuanced understandings over time. For example, this approach helped us to understand how staff 'socialising' on shift, which is often cast in binary opposition to patient-centred care, contained a deliberately protective element, which served to bolster staff morale against the challenges of their role, particularly in the absence of reciprocity from patients or formal recognition (Lloyd, Schneider, Scales, Bailey, and Jones, 2011; Schneider, Scales, Bailey, and Lloyd, 2010).

Through inductive analysis of this extensive dataset, we attempted to generate grounded theory (Glaser and Strauss, 1967) about the practice of care in the settings studied. In order to address the risk of investigator bias, we were careful to draw our interpretations from within the data and to seek feedback about our emerging findings (Whitt and Kuh, 1991) from the project lead and advisory group but also, importantly, from the healthcare assistants. (As this study focused exclusively on the experiences of staff who work on dementia care wards, and given the degree of cognitive impairment among most of the patients on these wards, we did not seek information or feedback from the patients themselves.) As we gained legitimacy and trust as members of the team, staff increasingly shared their experiences with us and challenged us to share our own impressions, which provided opportunities to check and validate our emerging theory.

Although not a critical ethnography per se (see Carspecken, 1996; Hardcastle, Usher and Holmes, 2006), this study did retain a critical emphasis by encouraging active engagement from participants in developing theory; reflecting on our positioning and practices as we conducted the research and recognising the constructed nature of our interpretations (Manias and Street, 2000; Porter, 1995; Porter, and Ryan, 1996); developing our grounded theory from the assumption that the healthcare assistants' job experience is shaped

by forces of power and control within the healthcare hierarchy (De Laine, 1997; Madison, 2005); and acknowledging that 'representation has consequences' (Hall, 1985), in that our representations might have implications for how similar staff are treated in future.

Separately and Together: Team Ethnography in Two Phases

> *I am aware that my impressions now are influenced by both our collaborative analysis and by the really poor [Dementia Care Mapping] results. I think some of the things I am seeing – such as „talking over" [the patients] – were always there, but I didn't pick up on them to any great extent.*

> *Ward B, Phase II fieldnotes*

The inpatient dementia project can be understood as a 'collaborative team ethnography', to borrow a phrase from Gerstl-Pepin and Gunzenhauser (2002) which specifies collaboration throughout data collection, interpretation, analysis, and representation (in contrast to a traditional, hierarchical team approach by which such tasks are conducted sequentially by different members according to rank). Due to its distinctive two-phase design, this study provides valuable insight into the benefits, challenges, and opportunities entailed in conducting collaborative team ethnography. In the first phase of data collection, the we underwent standard staff training, then immersed ourselves in ward life for three months, working as hands-on members of the team. After approximately two months of data analysis, we returned to the same wards in Phase II to undertake further participant observation and conduct focus groups and individual in-depth interviews. In the following section, we explore the significance of this research design with reference to selected fieldnotes from each phase of data collection.

Phase I: Pre-consensus

Although this was a team ethnography, the three field researchers effectively followed the classic Lone Ranger model throughout the first phase of data collection. Taking the grounded theory approach (Glaser and Strauss, 1967), we intended to generate data from which to inductively draw theoretical propositions about the challenges and rewards of the role. Therefore, we did not set parameters for data collection in advance; nor did we systematically share our observations throughout Phase I, in order to avoid prematurely imposing an interpretive frame on each other's observations and impressions. Practical circumstances promoted this approach: since we were working divergent shift patterns in different hospital settings, time spent in our shared office rarely overlapped during the entire

first phase of data collection.

The complete set of Phase I fieldnotes clearly illustrates how data collection varies according to the individual researcher, although we covered the same broad topics because we were guided by the same set of research questions. Selected by the project lead and representatives from the health care trust and supported by a Project Advisory Group, the fieldwork team comprised three field researchers: KS, aged 30, a female researcher from the United States with a background in social policy and the third sector; SB (also aged 30), a male researcher who had just completed a multi-sited ethnography for his doctoral dissertation in education; and JL (aged 22), a female researcher who had just completed her Master's degree in health psychology. We brought to the team different academic perspectives, varying levels of research experience and confidence, distinct interpersonal skills and communication styles, different genders, even different accents – and in our fieldnotes we each highlighted different aspects of the setting and the healthcare assistant role. In drawing connections between our individual biographies and our approaches to data collection and analysis, we recognise that this process is an inherently selective and speculative process. Thus, we proceed with caution as we explore the following examples from a set of fieldnotes that each researcher wrote after an afternoon shift about halfway through Phase I.

Perhaps due to her disciplinary background in psychology, JL was acutely aware of interpersonal relationships between and among staff and patients on the ward. In a reflective memo about her distinctive contribution to the research team, she writes: '*I really enjoyed sitting with staff and having everyday chit-chat with them; it made me feel relaxed and included in the team… It's possible that this is why I was able to recall more conversational detail*'. As an example, she recorded in her fieldnotes:

> *As I was leaving the locker room, I bumped into H/CO, who was on her way in to collect her bag and head home after the early shift. H/LA was not far behind her and when they bumped into one another H/LA asked H/CO why she hadn"t any make-up on today. H/CO told her she couldn"t be bothered this morning as she was so tired. H/LA told her she looked tired. H/CO said she had been on nights over the weekend, with a day off on Sunday and then back in today (Monday) on an early. She said she couldn"t believe how little time they"d given her to recover from nights, but*

[5]To protect their anonymity, participants were assigned standardised code names. The prefix 'H/' refers to healthcare assistant and 'Q/' to qualified nurse.

that she had a couple of days off now to rest[5].

By noting this interaction in some detail, JL captured important information about relationships between staff as well as data on shift patterns, challenges of the job, and coping strategies.

In contrast, SB struggled to recall specific conversations. However, his fieldnotes reveal significant attention to detail about technical and procedural aspects of patient care and extensive personal reflection about caring for patients with dementia. He took particular interest in medication as it related to both patient care and the staff experience, for example:

> *Q/LJ also mentioned [patient name], who she said has just been a different person since he was prescribed the diazepam. Q/LJ said that he will even say things like 'good morning' and 'how are you' when you wake him up in the morning. Apparently a similar therapy is being sought for [another patient], who is on trazodone [a psychoactive compound] at the moment, which I think is an anti-depressant rather than a sedative, and who can be an extreme challenge to get up in the morning, because he resists and fights all the way. ... Almost all the patients are on typical anti-psychotics, which are usually strongly sedative, which could be seen as a form of 'prior restraint'. ... I don't know if other staff take the same interest in these issues as I do, but there is definitely indistinction there, in the already-muddy waters of 'restraint' etc. The daily performances of medication definitely impact other staff. Medication is unpopular among staff and patients – it is an unpopular job for the nurses, it is an unpopular task for whoever may be asked to give one or other of the more difficult patients their medication.*

This entry reveals SB's particular interest in how staff negotiated the provision of person-centred care within an institutional setting, with attention to the stress and strain that this negotiation may engender.

Given her background in applied research and social policy, KS tended to note more frequently how staff engaged with the organisational policies and procedures which shape their role. For example, in the following fieldnote, she highlighted two staff members' reactions to a new patient care initiative:

> *H/HN demonstrates little affection when referring to or addressing patients; however, she seems to have pride/*

confidence in her job knowledge and skills, as evidenced by her vitriolic response to a new initiative that she perceives to be inappropriate (as described below).

KS went on to write:

After spending some time chatting with various patients in the day room – which was still very quiet and settled – I sat near H/HN to catch some of her conversation with FS/ER [the food service manager]. ... H/HN was talking quite heatedly about a new initiative, which is to provide patients the opportunity to choose their menu the day before. The topic came up, I gather, when [patient name] began loudly (and repetitively) announcing her evening's food choices to the room at large after looking at the menu posted on the board in the dining room – something which happens every day, as soon as the menu has been posted. 'You're having a sandwich and chips, [patient name]' corrected H/HN, 'don't you remember? That's what you ordered.' [Patient name] didn't seem to comprehend that; she paused for a moment and then picked up her original mantra. H/HN and FS/ER then discussed the inappropriateness of this initiative, since (a) patients forget what they chose and (b) 'there are always choices on that meal cart' (FS/ER) and 'there's always something on that meal cart I would choose to eat' (H/HN). 'Don't break what's not broken, is what I say', H/HN went on.

This note provides insight into how staff engaged with the initiatives that construct their options and experience in fulfilling their role.

While considering the effects of the researcher's individual differences on our approach to data collection and analysis, we are not dismissing the influence of social, organisational, and/or circumstantial factors. For example, given that SB conducted participant observation on a specialist 'challenging behaviour' unit, it seems likely that he was exposed to a more prominent discourse around the use of sedative medication, another reason why he may have recorded more notes about medication than the other researchers. As a different illustration, SB reflects on how his status as the only male member of staff on his ward offered both opportunities and constraints: although the playful manner in which he was able to interact with many of the staff helped build rapport, it also seemed that staff sometimes used this gendered relation, deliberately or not, to maintain distance and avoid potentially difficult self-disclosure. That is, it was not just that SB's gender influenced what he

'chose' to see and record in his fieldnotes, but that the gendered relations on the ward influenced what he was 'allowed' to see – just as was certainly the case, but differently, for KS and JL.

Conducting the first phase of data collection alone required us to keep detailed and comprehensive notes which proved invaluable to our inductive analysis. It also encouraged us to keep making the familiar strange throughout the entire data collection process, rather than taking shortcuts in our observations and reflections (Wasser and Bressler, 1996) – since even though we might have become accustomed to certain aspects of 'our' research sites, we had to speak in our fieldnotes to the other researchers' lack of corresponding familiarity.

Furthermore, by keeping our observations separate throughout the first phase, we deferred our entry into the interpretive zone until we had a sizeable volume of largely independent data. No doubt we did generate limited consensus through periodic research team meetings and anecdotal comparisons which might have affected our subsequent observations in the field, as May and Pattillo-McCoy (2000) found. However, we significantly reduced the risk of 'shorthanding' the data too soon at the expense of exploring a multiplicity of perspectives and interpretations, following Wolcott's (1999, p. 86) argument that 'ethnographies that are not overly comparative are the most helpful when it comes time to draw on them comparatively'.

Interim data analysis

Phase I of data collection concluded in December 2008. The following month, the three field researchers shed their Lone Ranger guise and reconvened in the interpretive zone of team ethnography. Sequestering ourselves in a conference room, we began by coding our fieldnotes line by line using NVivo8 software, initially generating open codes (approximately 170 in total) designed to reduce the mass of text into more manageable groupings (Bowen, 2008). Throughout this coding process, we used the constant comparative method, comparing each new line of data with previous data and coding, noting negative cases as well as new concepts and themes (Glaser and Strauss, 1967).

After coding approximately one-third of the fieldnotes, we determined that we had reached theoretical saturation because we had stopped generating new codes from the data. In the interest of time, we then divided up the remaining two-thirds of the fieldnotes, exchanging roles between paper -based and electronic coding (so that each note was coded once, then checked by a second researcher as it was entered into NVivo). This allowed us to retain an ongoing dialogue even as we worked separately, enhancing

inter-coder reliability (and generating no more than five new codes).

In the next stage of the analysis, we worked collaboratively to build 'trees' of related open codes under broader axial codes (Bowen, 2008). We recorded this analytical process in memos which drew on supporting data from the fieldnotes. These memos were considerably detailed and well-developed, since by that point we had already worked through many interpretive issues in our discussions around open and axial coding. In contrast, if we had not been working collaboratively in the interpretive zone, we may have written more rudimentary or fragmentary memos along the way.

Phase II: Post-consensus

After two months of coding, building trees around axial codes, writing memos, and identifying gaps and inconsistencies in the data, we returned to our three original research sites for several more weeks of participant observation before conducting in-depth interviews and focus groups[6]. Now that we were explicitly working as a team with a preliminary consensus derived through collaborative interpretation, we approached the second phase of participant observation with a specific list of questions to ask and issues to explore. Several of these issues will be highlighted briefly below with examples from our fieldnotes and memos in order to highlight how data collection differed in Phase II.

Humour and teasing

When analysing the Phase I fieldnotes, it became clear that JL had noted many different examples of humour on the ward. In Phase II, SB and KS set out to gather similar data from their wards, if available, and all researchers agreed to note the different functions that humour may fulfil.

Reflecting on the second phase of fieldwork, KS wrote this about humour:

> *Jokes, teasing, and/or laughter weave through almost*
> *every circumstance and interaction on the ward: sometimes*
> *as overt and defining elements, at other times as a subtle*
> *undercurrent or a single conversational bracket. During the*
> *second phase of participant observation on Ward A, I made*
> *a concerted effort to identify and record specific examples*
> *in order to better understand when, where and how humour*
> *is manifest. This was a difficult task given humour's*
> *prevalence – rather like trying to isolate individual*

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[6]The initial three months working on the wards had allowed researchers to build rapport and trust with staff, which helped us obtain consent to conduct interviews and focus groups. These were completed during working shifts, and were tape-recorded, transcribed, and analysed according to the coding frame developed after Phase I.

raindrops in a deluge.

What is interesting to note, is that KS did not record many examples of humour during Phase I because of its very prevalence, rather than its scarcity. Without subsequent comparison against JL's notes, she might have overlooked the significance of humour. Instead, in Phase II, she was able to identify specific examples of its prevalence and function on the ward.

For his part, SB recorded more nuanced observations in Phase II about who participated in humour on the ward and to what extent, noting how humour between staff and patients was at times inclusive, at other times exploitive. He also noted the fact that *'humour can cover a stressful situation means that it risks effacing the possible injustice/distress from which the stress emerged.'* By conscientiously identifying moments of humour, that is, he also delved deeper into what might be masked as well as revealed in such moments.

Empathy

In Phase II, we challenged ourselves to test the emerging hypothesis that staff expressed more empathy for patients on the smaller wards than on larger, busier, wards characterised by higher patient turnover. This hypothesis was difficult to test comparatively since we returned to observe our 'own wards' with our own eyes. However, by explicitly focusing on empathy, we did each consider it from different angles than we had in Phase I. For example, JL brought more of a critical eye to expressions of empathy on her smaller ward, witnessing new examples in a new way as well as reconsidering the meaning and implications of previous examples:

> *Looking back, in Phase I I felt that there was strong empathy on the ward and that staff really did understand that the patients are going through a tormenting time. This was evident in many of their on-shift conversations. However, in Phase II, whilst I still witnessed this empathy, I saw how this understanding is not always translated into empathic care by those who claim to [feel empathy for the patients] and this allowed me to reflect on similar instances from Phase I which I had not originally recognised as significant. For example, looking back to my previous time on the ward, I am reminded of the way in which healthcare assistants empathised with [patient name] in handover and on breaks... but in her presence they rolled their eyes about her when she approached staff with all the questions, worries, and confusion that they had previously*

claimed to understand.

KS noted that there was more empathy expressed on Ward A than she had recorded earlier, while also identifying a similar disconnect between words and action:

> *I found that empathy is articulated about patients to a greater extent on Ward A than I had perhaps recorded during Phase I (especially in handover and in the interviews), but there is nonetheless a notable mismatch between this articulation and the associated patient management and care... [i.e.] the willingness to look beyond the [challenging] behaviour to its root causes doesn"t always carry over consistently into patient care on the ward itself. Perhaps this shows how much easier it is to be empathic in theory than in practice.*

These converging observations about empathy suggest there was more similarity than contrast between the study sites in the articulation and practice of empathy than we had first concluded.

Engagement and detachment

The related concepts of engagement and detachment emerged as highly significant in the analysis of the Phase I fieldnotes. Therefore, in the second phase we endeavoured to gain a more nuanced understanding about when these processes occur, whether they can happen simultaneously, and what differences there are between individual staff.

In one fieldnote, SB wrote:

> *My second phase observations and in-depth interviews affirmed our preliminary conclusion that detaching is a deliberate, conscientious process which „makes sense" in doing ward work. ... [D]etachment serves an important function for the staff as well – as we already conjectured – which is to insulate them from the stress of their work environment. This, in turn, can reduce the potential for frustration-fuelled interactions between staff and patients.*

This fieldnote suggests that further observation confirmed our initial analysis about the protective function of detachment. Similarly, JL noted detailed examples of staff simultaneously engaging and detaching, which she interpreted as 'a necessary balance which enabled them to cope with the potential strain of their environment'. The following excerpt from SB's fieldnotes illustrates how some of the defining aspects of a setting can be

overlooked by the virtue of their prevalence:

> *Wake up this morning, I'm with H/AI. She's great to work with, her cheery demeanour coupled with genuine regard for patients" wellbeing. She always observes the obvious privacy and dignity rules. She does talk over patients – in fact, I've come to realise that doing so is so generalised that it didn't even really register before ('all too familiar and poorly known'). However, there is a distinction within this. I have never seen H/AI talk about the patient that she is talking over … Also, much of the time it will not be entirely talking „over" as she will involve the patient as well.*

The tendency among staff to 'talk over patients' was not obvious to SB until he became sensitised through the other team members' observations. Once he recognised it, he was able to identify the extent to which it occurred and where it was located on the spectrum of engagement and detachment between staff and patients.

Discussion: Benefits and Lessons Learned

Conducting team ethnography makes it possible to research multiple sites simultaneously, to collect correspondingly more data, and to triangulate different researchers' viewpoints on the same phenomena. The inpatient dementia care study capitalised on these fairly straightforward benefits. However, the more subtle benefits of team ethnography come to light when comparing Phase I and Phase II of this project. First, juxtaposing different sets of fieldnotes highlights the effect of each researcher's identity and social position on the way they relate to the research setting and vice versa. In our two-phase design, we were able to explicitly interrogate our positioning after the first phase, then return to the field with a commitment to testing the limits of our individual gaze/perspectives in the second phase. As SB wrote in Phase II about the use of humour among staff: 'I noticed my own positioning within this [humour] to a much greater extent as young, male, and an outsider'.

Furthermore, conducting research in two phases presented a challenge to the paradoxical tendency within teams to silence multiplicity by 'shorthanding' the data; that is, arriving too soon at a shared narrative interpretation which may preclude observation of nuances, variations, and contradictions. Because we conscientiously refrained from sharing our findings until the conclusion of Phase I, the field researchers retained a

broader observational gaze for the equivalent of nine months of fieldwork, rather than delving too soon into critical interpretation.

Finally, conducting the data collection in stages allowed the creation of what Gerstl-Pepin and Gunzenhauser (2002, p. 151) refer to as 'multiple safe spaces in which to make the interpretive zone flourish'. We, the three field researchers, established a 'primary' interpretive zone between ourselves when we first began coding our collective dataset. However, the zone spread concentrically from there: first, to include the considerable input of our project lead and project administrator, then the input of the Project Advisory Group at bimonthly meetings, then the informal carers in their focus groups, and finally the staff themselves on shift and in the feedback sessions at which we briefly presented our findings. Paradoxically, each widening of the interpretive zone entailed both questioning/reappraisal of the data but also sedimentation of our emerging consensus, as we increasingly drew on the same descriptive language and examples over time. We return to this point below.

It could be argued that the benefits described here would have been reaped even if this ethnography had been conducted 'separately together' from the start. According to such design, team members would have engaged in conceptual discussions prior to and throughout the data collection process: exploring contradictions, negotiating perspectives, and striving for consensus each step of the way. Liggett et al. (1994, p. 79) followed this design and described how they began with an intensive five-day retreat which 'served to build mutual understandings and interpersonal relationships that set the conceptual course of the study and established the foundation of [the] teaming process' from the start. However, the benefit of the two-phase approach was that it afforded an 'a-ha moment' as soon as we withdrew from the field to analyse our notes, rather than entailing a gradual shifting of perspective. Without taking a step back from the field in order to confront our large collection of data, the team members might not have noticed with such abrupt clarity what we hadn't noticed before – and would not then have had such a clear opportunity to address contradictions and fill gaps when we returned to fieldwork in Phase II.

Lessons Learned

As described above, the field researchers' objective in the first phase was to collect data from our respective study sites with minimal restrictions. As a result, although we ended up with a rich and extensive dataset, not all of the data addressed the main research questions or provided sufficient detail for comparison between wards. The potential mismatch between fieldnotes was ameliorated, however, by guidance provided during Phase I by academic members of the Project Advisory Group who had expertise in ethnographic

fieldwork. Following their guidance, we adopted a three-column fieldnote template (observation, reflection, and interpretation) and occasionally focused our observations on agreed themes, such as the morning routine, mealtimes, medication administration, and ward spaces. Thus we strove to find a balance between systematising our approach – in order to generate a sufficient amount of comparable data – without imposing analytical frames which would guide our observations too strictly. Our dataset may have been improved by negotiating this balance earlier in the project, however.

Conversely, there was the risk of establishing consensus too early. Conducting participant observation as healthcare assistants in dementia care settings was unfamiliar, unnerving, and exciting. For each of the field researchers, there were only two other people who would understand what it felt like to be a 'spy on the wall' (as JL was labelled by one participant) in this setting: struggling to learn the job, become accepted as a member of the team, and manage the physical and emotional toll of the work while retaining a critical research perspective. Therefore, without explicitly defining the parameters of our discussions, we nonetheless allowed ourselves to reflect on our experiences with each other when we met. These discussions provided essential emotional support and validation, counteracting the 'traditional loneliness' of the ethnographer (Woods et al., 2000). However, it is likely that these informal discussions influenced the way we saw things as we progressed through Phase I, engendering an (albeit limited and implicit) consensus about the job experience.

More striking was the way that we began shorthanding the data after the interim data analysis period, based on our emerging consensus. In writing memos and papers and widening the interpretive zone, we began to increasingly adopt the same interpretations and reference the same exemplars – which may have blinkered us to alternative data and interpretations when we conducted the second wave of data collection and analysis. Gerstl-Pepin and Gunzenhauser (2002) encountered this challenge in their study, finding that collecting data from multiple sites in order to enrich the analyses actually risked suppressing individual voices. With less critical reflexivity, Creese et al. (2008, p. 212) describe the process by which their team converged on one particular interpretation of one research subject: 'There is never only one account to tell. However, as a team we came to privilege and share particular accounts of Deepa. This was achieved, in part, through the writing of the fieldnotes'. KS encountered the limitations of consensus quite poignantly in an interview with H/BC, a gentle and industrious worker, when H/BC admitted to feeling alienated from the staff team: *'"I just [stay] with the patients," she told me, "there is not much you can do on your own as you know, but I just do what I can"'*. Having focused on the

(positive) importance of the team as a source of motivation and reward, a theme which emerged strongly through our analyses, KS had almost missed this important example of the negative effects of normative teamwork, quietly articulated by someone that she had assumed to be an exemplary member.

Finally, we noted above that analysis was undertaken in multiple safe spaces, including among the researchers but also with project advisors, carers, and staff. As well as simultaneously opening and foreclosing data interpretation this approach introduced another risk. Whereas the Lone Ranger might have chronicled their interpretive process along the way in order to establish a trustworthy 'audit trail', much of our collaborative discussions went undocumented. That is, although meetings were minuted and the essence of discussions captured in memos and subsequent analyses, the intervening layers of detail were lost. In particular, it would have been useful to record in some detail the discussions which underpinned the development of our open and axial coding frame, rather than allowing the codes themselves to be the only artefact (Sanders and Cuneo, 2010). In retrospect, we agree with Jones et al. (2000, quoted in Creese et al., 2008, p. 200) that it would have been useful to transcribe these meetings because 'they gave us a fieldwork space for reflection and for scrutiny of the knowledge-building we were engaging in. They also provided a way of acknowledging our own reactions to the fieldwork experience'.

Conclusion

This paper contributes to the small but growing literature on collaborative team ethnography by specifically highlighting the interactive effect of research design and team dynamics on the quality and type of data collected. With Phase I data collection primarily following the Lone Ranger tradition and Phase II data collection characterised by conceptual collaboration and consensus, this project combined solo and team approaches to ethnography.

Clearly, the process provided ample lessons for the future. Developing from the start a clear template for fieldnotes without imposing over-strict conceptual parameters is one lesson learned. Another lesson is to acknowledge the risk of establishing common interpretations and shorthanding the data prematurely, which can constrict data collection and analysis. More broadly, it has become clear that the benefits of team ethnography are best gleaned by reflecting critically on the team from the beginning – rather than in retrospect, when writing up the results. As Siltanen et al. (2008) argue, working 'separately together' requires reflexivity within the team, as well as individual reflexivity and reflexivity between researcher and participants. The team approach is underpinned by significant and

problematic assumptions: for example, the assumption that multiple partial understandings can be combined into a greater whole (Gerstl-Pepin and Gunzenhauser, 2002); that disembodied, textual fieldnotes can be collectively understood and analysed (Mauthner and Doucet, 2008); and that team members will work effectively together despite individual egos and ambitions. Had we confronted these assumptions, formalised a process for team reflexivity, and captured team communication from the start, our teamwork might have 'worked' even better

Nonetheless, the collected fieldnotes from the project, together with memos, reflections, and interview transcripts, are dense, diverse, and rich with insight. Covering three similar but distinct research sites allowed us to discover moderate ground between the abstract universality of the healthcare assistant experience and the infinite particularity of each individual ward. Triangulating our different understandings of similar contexts brought to light and sensitised us to the varying influences of gender, age, disciplinary focus, personality, social positions, and so on, as well as identifying gaps and inconsistencies in the data set. In addition, the three field researchers were able to derive support from the team for the emotional as well as professional demands of the participant observer role. In short, without foreclosing individual observation too soon, we are confident that the 'interpretive us' achieved more than any of us might have individually.

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