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An Open Exploratory Spirit?
The Cardiff School of Ethnography 1974-2017

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Preface and Acknowledgements

This intellectual history of ethnography at Cardiff 1974 – 2017 has been compiled thanks to the labour, practical and intellectual, of many people. Much of the research mentioned was funded by the UK's ESRC. Originally the idea of chronicling the intellectual development and achievements of the 'Cardiff School' of Ethnography came from Lyn Lofland, but she is not in any way responsible for what we have done with it. Robert Dingwall organised a meeting of the Couch-Stone (SSSI) Symposium at Nottingham University in 1996, and encouraged us to draw up and present a history and an intellectual manifesto for ethnography at Cardiff. This was done by Paul Atkinson, Amanda Coffey and Sara Delamont, with a comment by Robert G. Burgess. That presentation was critiqued by, among others Anne Murcott and Gary Alan Fine. In 2015 a conference to celebrate the 40 year career of Paul Atkinson was held in Cardiff and a range of papers was given which also inform this volume. A special issue of the journal *Qualitative Research* Vol 17, No.2, 2017 edited by Beck Dimond and Jamie Lewis serves as a *Festschrift* for Paul Atkinson, and therefore also explores the Cardiff School of Ethnography's standpoint and achievements. Mrs R.B. Jones and Ms. Clancy Pegg have word-processed this volume. Chapters 2 and 7 are new, written in 2017, the others are lightly updated versions of their 2001 originals.

Introduction

Sara Delamont

The title of this volume is adapted from Faris's (1967) history of the Chicago School of Sociology covering the period from 1920-1932. In his final chapter he stresses, repeatedly, the open-minded attitude to theory that characterised Chicago sociology in that period. We have appropriated his enthusiasm for such an open minded approach to social science. There is one caveat about Chicago, and one about our own *hubris*. The caveat about Chicago sociology is that the official accounts and most well-known histories of its first, golden age', such as that by Faris have been exposed by Mary Jo Deegan (1988, 2001, 2014) as 'malestream', 'purist' narratives. They largely ignore the practical and intellectual contributions of women, and take an uncritical stance on how the more applied and politically engaged disciplines and research areas were systematically defined as *not* sociological and re-categorised as home economics, or social work, or removed from the University all together (trade union activity and settlement work). A theorised account of those processes of 'purification' can be found in Delamont (1991, 1992b, 2003: 81-95). Ethnography at Cardiff has not been codified in such ways, but we do aspire to the spirit that Faris (1967:130) invokes. He argues that Chicago sociology in its golden age was based on 'vigorous penetration into new fields', coupled with a 'deliberate and even determined avoidance of a constricting school of thought'. Faris was convinced that this led to 'a body of developing sociology incomparably broader in content' than other schools which displayed an 'openness to influences from other traditions'.

In taking a title from Faris we are not claiming that sociology, or ethnographic research at Cardiff, can be compared to that at Chicago. Such a comparison would be an act of *hubris* and would rightly invite derision. However there has been a strong tradition of ethnographic research here since 1974

that has, as the volume shows, an identifiable intellectual coherence. In Cardiff the ethnographic work has been grounded in an explicit, and reflexive, blend of American empiricism in its methods and European theoretical perspectives such as structuralism into an identifiable 'tradition'.

A Brief History

There was a sociology department at Cardiff University from 1963, but the emphasis on excellence and innovation in ethnography began in 1974. There was one experienced ethnographer already in post (Anne Murcott) and Paul Atkinson joined her. When Sara Delamont joined them in 1976 a critical mass of staff existed. All three had first degrees in social anthropology, the discipline in the UK which has always prioritised ethnography as its central method (See MacDonald, 2001). Indeed in the 1960s Social Anthropology was the only social science in which participant observation was the standard, taken for granted, data collection method. In UK sociology only community studies (Frankenberg 1966) used ethnography, as Murcott had done in the re-study of the Oxfordshire town of Banbury (Stacey *et al.* 1975).

In Cardiff Murcott, Atkinson and Delamont recruited doctoral students, taught qualitative research methods at undergraduate and postgraduate levels, and developed an agenda of research projects in the sociology of health and illness, of education, of occupations, professions and enculturation into occupations, and on deviance. These were, and are, core topics researched by the leading scholars of the Second Chicago School (Fine 1995) such as Olesen, Geer, Becker, Hughes and Strauss. Newer empirical areas, not central to the Second Chicago School programme of work, but intellectually parallel to it, were studies on gay and lesbian identities and on leisure activities. Alongside those empirical areas work began on the advancement of methods, especially through the publication of Hammersley and Atkinson, (1983, 1995, 2007 and forthcoming) an advanced, agenda-setting textbook.

The central features of the 'manifesto' of the Cardiff School of Ethnography are a rejection of 'the paradigm mentality' (Delamont and Atkinson, 1995), coupled with the exploration of new theories, and innovations in data collection, analysis and representation.

The key themes of the Cardiff School are spelt out at greater length in a series of publications: in the introduction to Atkinson, Coffey, Delamont, Lofland and Lofland (2001); the editorial introduction to the first issue of *Qualitative Research* (Atkinson, Coffey and Atkinson, 2001); and in much greater depth and breadth in Atkinson, Coffey and Delamont (2003) *Key Themes in Qualitative Research* and Atkinson, Delamont and Housley (2008) *Contours of Culture*.

There are six chapters following this introduction: Delamont explains the manifesto and outlines the key themes, Atkinson discusses Cardiff as an historical, geographical and metaphorical frontier, Coffey explores contestation and representation, Burgess provides a view from an informed outsider, Smith explores recent methodological developments in Cardiff ethnography, and Hammersley reflects on Atkinson's standpoint.

Sociological Priorities and Achievements of the Cardiff School of Ethnography 1974-2017

Sara Delamont

Introduction

This chapter outlines the major sociological priorities and achievements of the Cardiff School of Ethnography from 1974-2017. The overall methodological programme and its central tenets come first, then the Cardiff contribution to the major changes in the empirical agenda of British sociology, and finally there is a short section on the theoretical ideas that have been foundational to, and developed by, scholars at Cardiff.

The Methodological Developments

The agenda for methodological developments is based on a strong commitment to the following ideas.

- 1) that data should be collected, analysed and written up in a reflexive and rigorous manner, so they are of the highest possible quality however small or large the project may be.
- 2) that theorising is generally most sensibly done from the bottom up: that is starting from the data. In this precept, the Cardiff School pays homage to the spirit of Glaser and Strauss's (1967) strategies for developing grounded theory analyses, including the use of theoretical sampling and to the subsequent work of Adele Clarke (2005) and Kathy Charmaz (2006).
- 3) that discourse is an important part of all life situations and always deserves systematic attention.
- 4) that middle order concepts, such as total institutions and boundary objects are powerful ways to move between empirical case studies of superficially different areas.

5) that classic American Symbolic interactionism can usefully be mixed with other perspectives such as the French structuralist and post-structuralist ideas of Levi-Strauss, Mary Douglas, and Pierre Bourdieu.

6) that paying serious attention to the sociology of knowledge is vital in many ethnographic settings, especially those where there are occupations, professions, or subcultures with specialist or esoteric knowledge.

7) that research ought to be fun: that is all the stages of planning, gathering data, analysis and writing should be exhilarating at least some of the time.

8) that ethnographic research should be conducted with an open exploratory spirit; always infused with ideas from new theoretical movement, with new techniques of data collection and analysis, using new technologies, and experimenting with new writing conventions.

These eight precepts are basic to a further three principles about how novices should be initiated into qualitative research

9) that qualitative methods in general, and ethnography in particular, can and should be explicitly taught.

10) that all the phases of qualitative research, from design to writing up, should be taught.

11) that textbooks and 'instructional' articles such as handbook chapters should be suffused with the previous principles.

These eleven precepts have been built into undergraduate, postgraduate and postdoctoral programmes at Cardiff. Their existence for forty years, and the enthusiasm staff have shown for sharing them have meant that new recruits have been encouraged to set their own theoretical, empirical and methodological agendas, on strong foundations. These principles have been made clear in Hammersley and Atkinson (1983, 1995, 2007 and forthcoming), Delamont (1992a, 2002,

2014, 2016), and Atkinson, Coffey and Delamont (2003), Coffey and Atkinson (1996) and Atkinson, Delamont and Housley (2008). Underlying them is a commitment to face up to the familiarity problem.

The Familiarity Problem

As well as the recurrent attention to the eleven precepts, there has been a firm adherence to the argument first posed by Geer (1964) and Becker (1971) that the job of the sociologist in her own culture is to make the familiar strange. This is the opposite of the job of the anthropologist, who has to strive to make the strange familiar. One crucial part of the sociological 'fight' against familiarity when researching their own society is to develop good foreshadowed problems (the ethnographic equivalent of the positivist hypotheses), an idea explored at some length in Delamont, Atkinson and Pugsley (2010).

In 1964 Blanche Geer set out the importance of using the early period of fieldwork to ensure that the researcher's preconceptions and familiarities do not predetermine, and over determine, the project. In that paper she described her preliminary work among liberal arts freshmen at the University of Kansas, and how she forced herself to develop good foreshadowed problems for the full ethnography which was eventually published as Becker, Geer and Hughes (1968). She was particularly concerned that researchers needed to ensure they did not come out of the field without data because they could not see beyond, or behind, what 'everybody knows' (p384). Her specific example was novice fieldworkers studying hospitals in America. Howard Becker (1971) extended the argument to research in schools. He argued that for both higher education and school settings, it was 'like pulling teeth' (p10) to get novice researchers to 'see' anything interesting, or even to 'find' anything to write down because American schools were:

so familiar it becomes impossible to single out events that occur in the classroom as things that have occurred, even when they happen right in front of you. (p10)

Becker went on

it takes a tremendous effort of will and imagination to stop seeing only the things that are conventionally 'there' to be seen (p10)

I first read those diagnoses as a doctoral student and a young post doc in the sociology of education, while living with a young researcher who did ethnographic fieldwork in medical settings. Little published since has impressed itself on me as so fundamentally important. The diagnosis (the familiarity problem), the task (to fight familiarity, or make the familiar strange) became recurrent motifs in my teaching, my ethnographies and my critical frame for reviewing papers, books and research proposals. Cardiff ethnographers have taken these ideas from key figures in the Second Chicago School (see Fine, 1995) and used them to inform their research in educational and medical settings, and many others.

It is easy to take up a position of moral superiority when teaching and reviewing the work of others such as criticising them for failing to articulate serious strategies to fight familiarity. It can be harder, much harder, to be that tough on oneself, especially when choosing fieldsites and topics for papers. Essentially Geer and Becker argued that ethnographers needed to work hard to make the familiar strange, but did not offer any specific strategies to do so. In Cardiff we have developed seven well-rehearsed strategies for fighting familiarity:

- a) revisit ethnographic studies from past eras
- b) study the phenomena in other cultures
- c) study the phenomena in unusual settings in our own culture
- d) take the standpoint of non-standard actors in the setting
- e) take the standpoint of a researcher who is 'other'
- f) being self-consciously non-sexist, or anti-racist

g) take an ethnomethodological standpoint

The first three strategies rely on reading in a purposive way: so if the research is on hospitals in the UK, reading about formal healthcare and the sick role in other cultures, about unorthodox 'cures' such as spirit possession, or searching out accounts of hospitals carried out from the laundries or the mortuary attendants or the administrators chasing bad debts. Such reading needs to be done when developing the foreshadowed problems, during the fieldwork, and at the analysis and writing stages. Strategies four to seven need to be actively considered when designing the project and also pursued during the fieldwork and the analysis, the writing up. The familiarity problem was revisited by Mannay (2010). She outlines the strategies proposed and adopted by ethnographers to challenge familiarity since 1971 and makes a clear case for the use of visual research methods as a powerful way forward. So that would give a researcher eight strategies to make the familiar strange.

Since 1974 qualitative methods have spread widely in the social sciences beyond anthropology and the symbolic interactionist sociology. Cardiff has been mainly associated with its eleven precepts rather than following ephemeral fashions and seeking endless novelty for its own sake. Here I have outlined some of key controversies to which Cardiff scholars have made contributions. Perhaps the biggest methodological controversies in which Cardiff ethnographers have been active are those around autoethnography and about the collection and use of narrative data in general and especially illness narratives.

In 2006 the *Journal of Contemporary Ethnography* published a special issue on *Analytic Autoethnography?* edited by Leon Anderson. Those papers set out a range of positions in the ongoing debate about autoethnography. Atkinson (2006) and Delamont (2009 and 2013) are subsequent contributors. Generally Cardiff ethnographers have been resistant to the autoethnographic turn, primarily because of its lack of rigour and its focus on the social scientist rather than some idioculture, social setting or social processes. Insofar as there is a continuum of ethnographic positions on autoethnography, the Cardiff approach has been relatively conservative:

preferring more conventional 'peopled' ethnography (Fine, 2003) conducted in social settings rather than introspective research on the sociologist herself.

Challenging the primacy of the interview in qualitative research has also been a consistent theme of Cardiff ethnographers. In particular Cardiff sociologists have regularly challenged the popular, if the regrettable, tendency to gather interview data, fail to analyse them rigorously, and to present them as if they were 'authentic', 'true' and morally superior to all other data. Atkinson (1997) and Atkinson and Silverman (1997) set out the Cardiff position, as do Atkinson and Delamont (2006b). Silverman (2017) is a current re-statement of this position. Interview data, of whatever kind, do not provide a window through which to see events unfold. Nor do they give 'truthful' or 'authentic' viewpoints that can, or should, stand alone, unanalysed. Interview data are rhetorical and performative, and have to be understood in that way. The task of the social scientist is to analyse those data and use them to build sociological accounts of social phenomena as they are understood by the informants and by the researcher. Of course interview data are valuable, but only when analysed sociologically.

The same is true of documents, which are often valuable both in their own right and as one data source in a project. Prior (2012) is a recent discussion of the proper sociological approach to documentary sources. The most important document-based Cardiff research project (Fincham *et al.* 2013) is a study of the legal paperwork on young men who had committed suicide, called a sociological autopsy.

The growth of visual, multi-modal and multi-sensory qualitative methods has been a central part of the Cardiff School of Ethnography's work. Mannay (2015) and Dicks (2014) have been the main innovators here. These newer approaches have included a focus on material evidence (such as Hurdley's (2013) work on domestic possessions), on embodiment (Delamont, Stephens and Campos, 2017) and studies of time and space (which are explored in Section 3).

The importance of technological changes for the collection, analysis and dissemination of results is explored in Chapters 5 and 7. The use of software packages to analyse text, and the development of qualitative methods in virtual worlds and from social media, have been built into a wider agenda at Cardiff. The eleven precepts are as relevant to an ethnography in an on-line world, or a focus group constructed on-line as they are to a 'classic' project following a mountain rescue team into the Welsh hills as night falls.

2) Changes in the Empirical Agenda

There are six empirical areas of British sociology to which research by Cardiff ethnographers has made contributions which have resulted in the boundaries and shape of the discipline being reconfigured. These are the focus of this section of the chapter, which does not present all the empirical, qualitative, work done but only the boundary shifting contributions. The six are

- 1) Expanding the scope of, and dissolving the boundaries between, the sociology of health, illness and medicine and the sociology of science and technology.
- 2) Changing the scope and focus of qualitative sociology of education.
- 3) Theorising and re-invigorating the qualitative sociology of professions, professionalization and professional socialisation.
- 4) Reconfiguring British sociology's understanding of the domestic and the everyday.
- 5) Developing a sociological approach to children and childhood
- 6) Creating a sociological literature focusing on bodies and performativity.

Each of these areas is associated with one or more of the methodological issues already outlined above.

Health and Science are the two areas where Cardiff ethnographers have been most consistently using qualitative research over forty years to change the boundaries between these two previously

separate subdisciplines of UK sociology. In 1974 the sociology of medicine, as it was then called, was defined in ways that, in practice, meant there was no research on the scientific and laboratory based aspects of medicine. In the next decade the subdiscipline renamed itself the sociology of health and illness with a new specialist journal (*Sociology of Health and Illness*) founded in 1978. That journal has been edited in Cardiff three times in its history, and the current shape of the research area has been routinely formed and developed here (Bloor, 2001). From Atkinson's (1981) ethnography of medical education through his project on haematology (1995) to Thomas's (2017) monograph on pre-natal screening, there have been Cardiff researchers active and creative. Central to the development of sociological work on medicine, health and illness has been a focus on combining ethnographic observation with interview data, and documents, while paying attention to knowledge. The classic symbolic interactionist research on this area such as Becker *et al.* (1961) focused so carefully on the observation of social processes that the content of the medical curriculum was *de facto* neglected. Cardiff work rebalanced the field to demonstrate it was vital to give sociological attention to knowledge alongside attention to interaction.

In 1974 the sociology of science and technology was predominantly Mertonian functionalist in its theory, quantitative in its methods and overwhelmingly focused on elite men doing physics. In the years since there has been an explosion of interactionist and constructionist work, and the focus was widened both to encompass biological and medical sciences and to investigate women scientists, non elite workers and students in scientific contexts. The sociological focus has widened to study ordinary science (such as PhD supervision and routine work) as well as 'breakthroughs', such as the discovery of the gene for myotonic muscular dystrophy (Bachelor et al, 1998) or graphene or the Higgs Boson. Cardiff's contribution has been mainly in the border country between medicine and science, especially in the fast developing field of the new genetics. An ESRC funded unit, CESAGEN, produced studies of the interface between science, medicine and families (e.g. Featherstone et al. 2005). There has also been research on the everyday and breakthrough labour of geneticists, on

stem cell banking, bioinformatics, mitochondrial DNA, and cultured (in-vitro) meat. Laboratory ethnography, in a wide sense, (Stephens and Lewis, 2017) has been conducted in a variety of settings. Ethnographic research has also been conducted on medical ethics committees (Hedgecoe, 2014) and on conferences which are a key part of medical and scientific work (Dimond et al. 2015).

The sociology of education in 1974 was, in the UK, predominantly quantitative, mainly focused on schools, and largely about boys and men. It focused on which institutions boys and men entered and left, without attention to the knowledge taught in them, to the interactions which went on in them, or the education of girls and women. Disabilities, sexualities and sexual orientations were entirely absent from the subdiscipline. The Cardiff ethnographic work has been central to moving all these boundaries. Atkinson consistently argued that medical education was just as legitimately the object of the sociologist of education's gaze as schooling. He also did an exegesis of the work of Britain's most famous sociologist of education Basil Bernstein (Atkinson, 1985) which explored uses of Bernstein's work far beyond schooling.

The group produced many studies of occupational and professional socialisation (of journalists, midwives, psychiatrists, accountants, doctoral students) and of non-school settings where education takes place (eg. adult education, vocational training, martial arts classes). Delamont and Atkinson (1995) used the theme of fighting familiarity to argue for a re-focusing of the ethnography of education, an argument re-iterated in Delamont, Atkinson and Pugsley (2010) and Delamont (2014). Expanding the focus of the sociology of education to include girls and women has been as important as widening the definition of 'education'. Renold (2005) is the most well known of the scholars working at that task. She is the main scholar in Cardiff concerned to put sexualities and sexual orientations onto the educational research agenda. Coffey and Delamont (2000) is an overview of that work. Redrawing the boundaries of sociology of education to include those with disabilities such as the blind, learning disabled, deaf or mentally ill has been important, most recently in the work of Ceri Morris (2016). In the UK the early 1970s saw a call for sociology of education to pay

research attention to the knowledge, and power over that knowledge, regarded as appropriate for inclusion in the official curricula (and assessment systems) of educational institutions (Young 1971).

The Cardiff ethnographers of education, broadly defined, accepted those ideas and have consistently focused not only on the interactions in educational settings, but also on what knowledge is valued, and indeed not valued and excluded from, schools, universities and professional education.

Professions have been of sociological interest in America from the Mertonian perspective, and to symbolic interactionists (Atkinson 1983). Cardiff research on professions, professionalization and professional socialisation followed the interactionist research strategy of ethnographic immersion, but added a focus on scrutinising the knowledge base and knowledge claims of those occupations. Work on teachers, accountants, journalists, scientists, doctors, and recently on opera singers.

The everyday and the domestic were central to Hurdley's (2013) innovative study of domestic interiors and memory. Everyday experiences have been the focus of ethnographic work by Smith (2011), Hall 2003, (2017) and Moles (2008). These studies of the material, the spatial and the temporal are more fully developed in Smith (qv). Two underlying sociological concerns here are the work of Barbara Adam on time (Adam, 1990) and John Urry (2007). When John Urry published *Mobilities* he set out a research agenda for sociological theory and methods that was engineered to do research and theorize the data in a globalised and highly mobile world.

The other reason for an empirical focus on the everyday and the domestic, a realignment of British sociology, has been driven by feminist sociologists, and addressed issues central to the lives of women that had traditionally been seen by the opinion formers in the discipline as entirely unimportant, or had not been 'seen' at all. Restructuring sociology to increase its focus onto the previously 'all too familiar' aspects of society such as housework and money management has been a major achievement of feminist sociology. There are nine empirical areas which are, in 2017, clearly 'sociological', that were not researched by mainstream scholars in 1974.

- 1) Emotional work
- 2) Caring
- 3) Housework
- 4) Food, drink and cookery
- 5) Childbirth
- 6) Money
- 7) Leisure
- 8) Time and who controls it
- 9) Domestic violence

Delamont (2003: 42-52) expands on how these areas came to be of sociological concern. In Cardiff, the area of food, drink and cooking was pioneered sociologically by Murcott (1983), and there were early ethnographic studies of childbirth and of caring and carers.

Children and Childhood had been left by sociologists to psychologists for most of the twentieth century, in contrast to deviant adolescent men, who were the focus of the sociology of deviance and who had received a good deal of sociological attention since Thrasher's (1927) *The Gang*. In the past thirty years the boundaries of sociology have moved to encompass a sociology of children and childhood.

Bodies and performativity is the final area which has seen a major, even seismic, shift in the boundaries of what is deemed appropriate for sociological enquiry. Cardiff ethnographers have studied an opera company (Atkinson, 2006a) changing embodiment in two martial arts (Delamont, Stephens and Campos, 2017). Scott's (2007) pioneering work on the sociology of shyness, which was a Cardiff doctoral thesis, is the most important work of this type.

3) Theoretical Advances

The theoretical advances that can be attributed to the Cardiff School of Ethnography discussed here are once again about boundary shifting in UK sociology. There are three main areas where Cardiff sociologists have worked to move boundaries: that between symbolic interaction and (a)

ethnomethodology (b) CA and (c) DA, that between symbolic interaction and the post-structuralist work of Bernstein and Bourdieu and that between classic symbolic interactions and feminist sociology. Atkinson and Housley (2003) set out the history and potential of interactionism in British sociology, and some of those ideas are developed in Atkinson, Delamont and Housley (2008). These books are the most coherent statements of the theoretical position of the boundaries between symbolic interactionism and ethnomethodology, CA and DA. The work of Smith and Atkinson (2016) on Cicourel is a recent development.

The post-structuralist sociology of Bernstein and Bourdieu has been deployed in the studies of occupational socialisation, professions and education throughout the 1974-2017 period (Delamont, 1989). Most recently it has been utilised in the work on martial arts (Delamont, Stephens and Campos, 2017).

Feminist sociology grew up in the USA and the UK after 1968, and has been an influence on, and contributed to, the on-going work of Cardiff ethnographers. Delamont (1989, 2003) has been the most consistent feminist critic of symbolic interactionist theory, or rather the theoretical assumptions of symbolic interactionist research at Cardiff. Delamont (1989) for example provides a new model to explain the exclusion of women from the core sets of science and the professions.

Overview and Conclusions

In the 1990s these developments were published in a series of books, The Cardiff Papers in Qualitative Research. These have been recently re-issued as e-books by Routledge. After 2000, much of the work has been published in journals, especially *Qualitative Research* founded in Cardiff by Atkinsons, Coffey and Delamont, in the Sage four volume sets of key papers in research, and in monographs, textbooks and edited collections such as Smith and Delamont (2018). The *Festschrift* for Paul Atkinson, a special issue of *Qualitative Research* published in 2017 (Vol 17 No 2) edited by Dimond and Lewis, showcases the current thinking of the Cardiff 'School'.

Cardiff as Frontier

Paul Atkinson

My title, of course, is meant to be allusive and ironic. There have been many 'frontiers' in the history of Sociology – most notably the identification of the zone of transition and processes of acculturation and disorganisation in Chicago in the early decades of this century; and the sociological framing of 'night as frontier' in Melbin's (1987) memorable formulation. Strictly speaking, Cardiff has never featured in quite that guise. It could have done. As a city, Cardiff could well have presented the sociological community with social processes to be studied analogous to the social and intellectual conditions of Chicago, over much the same period. Like Chicago, Cardiff was a place of little importance prior to the emergence of the region in the industrial revolution: and its significance rested on its proximity to major centres of production and its strategic location in terms of transportation. Cardiff was primarily a docks town, with one of the busiest commercial port areas in the world in the nineteenth and early twentieth centuries. Its population expanded considerably over the period. Physically – and without the benefit of the great fire of Chicago – Cardiff is a product of the late Victorian and Edwardian eras. While never a vast melting pot quite like the great metropolitan centres of the United States, Cardiff's population was diverse. Like most dockland cities, it had significant populations of immigrants. Cardiff had, for instance, visible and sizeable communities of Chinese, Somali, Greek and Italian settlers. Indeed, Cardiff has a reputation (reasonably well deserved) for having had a settled, ethnically diverse population for generations, and good relations between them. This reputation has been gained partly by virtue of a collective amnesia concerning racially based disorder at various key points in the twentieth century.

Like many dockland areas in North America, the UK and elsewhere, the Cardiff docks suffered a protracted period of decline and decay from the 1960s onwards. When coal and iron exports

vanished, it did not become a container port, or a departure point for cruise ships. In recent years – again, like many cities with similar histories – Cardiff has experienced a regeneration of the waterfront that had been its commercial docks. Baltimore provides a repeated point of reference. In a classic transition from an industrial modernity to a late-modern or postmodern condition – from the sphere of production and distribution to the sphere of leisure and consumption, the docks are now characterised by museums and displays (*e.g.* Techniquest, a Roald Dahl centre and a Doctor Who ‘Experience’, a TV studio and restaurants. There is a new Opera House and the *Senedd* (the home of the regional parliament). I shall return to the postmodern moment a little later.

Like Chicago, Cardiff had an early University Settlement. It can reasonably be argued that Social Work has been a feature of the University’s work virtually since its foundation in 1883. Unlike Chicago’s Hull House, however, the settlement house did not become part of a tradition of systematic social inquiry. Ironically, however, Cardiff did feature in the early twentieth century American Sociological canon. The theme of ethnicity linked the two. St Clair Drake, one of the doyens of American sociology of race, used Cardiff as a research site. It is, in passing, notable that nobody actually pays any attention to that particular episode in sociological history. It is intriguing from my point of view to speculate that had that research taken place against today’s academic culture, the opportunity would have been seized to establish formal partnerships and co-funding between the Universities of Chicago and Cardiff. The theme of race in Cardiff was later to form the topic of subsequent sociological-cum-anthropological research in the UK. Most notably, Little’s (1947) studies of race relations took Cardiff and its docklands as a key research site.

To some extent, then, Cardiff was more of a research site than a research centre for many years. While it could – as I have suggested – have developed an indigenous research tradition on social

change, ethnicity, community and like, that never happened. Despite the notoriety of areas of Cardiff known proverbially for their local colour (such as the so-called Tiger Bay docks area), there never grew up a distinctively Cardiff urban sociology. By contrast, insofar as Wales produced an indigenous tradition of its own, it was its post-war series of rural community studies, predominantly in Welsh-speaking villages, inspired by Alwyn Rees (1950) and his circle in Aberystwyth.

Cardiff was and is, in a small way, placed at various social and intellectual frontiers and transitions. Turning its back on preoccupations with the *Gemeinschaft* of Welsh rural and community studies that were characteristic of other sites, notably Swansea (see Harris, 1990 and Savage, 2010) Cardiff and its milieu prompted sustained attention to the modern division of labour, with a focus on intellectual and professional elites, their reproduction and everyday work. Neither distinctively Welsh nor English in social composition or culture, Cardiff turned outward, to American and European sociological inquiry. Its development and its intellectual flavour has also reflected the transition from a 'classic' ethnography towards a more problematic, 'postmodern' perhaps, set of perspectives on the contexts and conduct of ethnographic research. If the 'first' Chicago School passed by Cardiff or Cardiff ignored it – despite their obvious parallels and occasional points of contact – the Second Chicago School had much greater direct impact. The development of ethnographic research, and the integration of interactionist perspectives, which emerged at Cardiff in the 1970s, reflected a much more direct and conscious development of American interactionism and the inspiration of authors such as Everett Hughes and Anselm Strauss. The intellectual affinities were, perhaps, less with Chicago itself as with the California versions of Chicago, in San Francisco, Davis and elsewhere. (see Fine, 1995).

Rather than a pure copying of American interactionist sociology, Cardiff's own 'little tradition' of qualitative research was grounded in, and developed out of, a mix of intellectual traditions. Indeed, it is arguable that insofar as the Cardiff School (if there is one) has any interest beyond the purely parochial, then it lies in the fact that Cardiff is both frontier and microcosm. It is a frontier between various intellectual strands – American, British and European – and a microcosm of how ethnographic methods, interactionist sociology and other intellectual current have intersected. Cardiff too is microcosmic of how those intersections have themselves changed, how boundaries have shifted, and how the overall intellectual project has changed. These inspirations outlined in Chapter 2 have continued to provide a bedrock for the subsequent ethnographic work. They were never adopted in a pure form, however. From the outset, there was a sense of dissatisfaction. Many of the research sites we chose were concerned with the production and reproduction of specialist knowledge: medicine, psychiatry, pharmacy, genetic science, social work, accounting, journalism. The interactionist tradition was found to be incomplete for those purposes. The recurrent preoccupation with institutional processes left little space for the principled exploration of knowledge itself. The eclectic approach to ethnographic work was thus imbued with various approaches to expert and everyday knowledge – structuralist, phenomenological, ethnomethodological.

What is noticeable, then, is that – quite typically for the British context – Cardiff's was an 'impure' development of the interactionist approach. It was also a highly pragmatic one, with relatively little concern for the history ideas, such as successive re-evaluations of interactionism's intellectual roots. It is, I think, characteristic of a good deal of British intellectual life that schools of thought which elsewhere exist in tightly grounded forms and in mutual isolation can flourish in hybrid versions and in close proximity within the same institutional setting. The eclecticism of much of Cardiff's work, and its mingling of different intellectual traditions, has been a strength, and is itself a microcosm of much British social science.

Indeed, one can follow the terminology suggested by Basil Bernstein (e.g. 1971: see Atkinson 1985). Socially, the British higher education system has been characterised by strongly classified segments. That is, the academy has strong symbolic boundaries, insulating it from the external social environment. The British university has traditionally been socially exclusionary, basing its social fabric on the ideal of sponsored social mobility. It has fostered strong pressures towards academic specialisation and subject loyalty. On the other hand, within those strong boundaries, competing paradigms may be allowed to co-exist. Furthermore, in some contexts, the boundaries between would-be paradigms are allowed to weaken, and various hybridisations and compromises allowed to emerge. This is hardly attributable to a national characteristic, although several decades ago Perry Anderson (1969) suggested it as a trait in British intellectual life. This terminology is inexact: it is part and parcel of our academic heritage that we are suspicious of the very idea of the intellectual. Anderson suggested that for a variety of historical factors – not least the influence of refugees from totalitarian European regimes of both the right and left – British academics were typically suspicious of grand theoretical schemes and their totalising influence. A sceptical attitude and firm commitment to empirical investigation seemed the order of the day.

It is a characteristic consequence of such a general academic style that exotic, foreign ideas can be accommodated through a process of 'domestication'. This is true, for instance, of much European social thought. French structuralist thought was, in British circles, rendered accessible and usable through empirically grounded work. In anthropology, for instance, people like Edmund Leach (1961) and Mary Douglas (1966) used the ideas of Claude Levi-Strauss without ever being completely seduced by his grand vision and his most general theoretical propositions. The same is true in literary criticism, where authors such as David Lodge (1981) took key ideas from the Parisian structuralists and adapted them into more pragmatic and empirical approaches to literary criticism.

Not all of British intellectual life has been characterised by such perspectives, but it is, I think, a recognisable trait across many disciplines. While we tend to be socially exclusive, and operate with strong disciplinary boundaries, those disciplines themselves have often been allowed to be fairly eclectic. This may reflect a number of institutional factors. On the other hand, notwithstanding the particular status of Oxford and Cambridge, the UK scene is much more multi-centred than say, France. While it may seem that British academics have all been to a small number of schools and universities, in comparison with Paris we are probably much more diverse. We do not have generations of intellectuals in the same tradition as the Normaliens. In comparison with the United States, on the other hand, we are so much smaller. Academic departments and research groups can develop, it is true, but they can rarely afford to do so in perfect isolation from other influences: many of us have to be generalists rather than narrow purists. It is, I think, for such reasons that we have had remarkably few purist groups. The general intellectual style has been more eclectic on the whole. We have not had, for example, people working within an exclusively interactionist tradition. Which is why, incidentally, early attempts to establish UK branches of SSSI attracted so little support. Likewise, the approach to ethnographic research has been pragmatic and eclectic too. It is for the same reasons, one suspects, that the Cardiff experience was not atypical. The approach to studying education, medicine, organisations and professions was always coloured by a liberal admixture of structuralist and poststructuralist perspectives on the construction and reproduction of knowledge. Likewise, the boundaries within and between interpretative sociologies in general were seen as permeable. Ethnomethodology was welcomed with some scepticism, and deployed as appropriate in the context of broader ethnographic work. These interests were added to the anthropological perspectives that informed the early years, drawing on Levi-Strauss and Barthes, rather than the subsequent fashion for Foucauldian discourse analysis. In a similar vein, the influence of European sociology was a significant element. The phenomenology of Merleau-Ponty and Schutz were early influences.

In terms of the development of ethnographic work, the result was also eclectic. As I have said, the immediate inspiration came from anthropology rather than interactionist sociology. The latter was, if anything, 'filled in' retrospectively. It is noticeable that for the original participants in the ethnography group, the continuation of the Edinburgh work again at Cardiff represented no conscious rebellion against other sociologies. None of the protagonists had formal qualifications in sociology, and there was nothing to rebel against in that sense. Indeed, it was a continuation of taken-for-granted ideas rather than a conscious formulation of any particular programme.

It is instructive to compare the methodological map of forty years or so ago with the present scene. Anthropologists at that time (the 1970s) took for granted the essential nature of ethnography. That was so in two related senses. First its epistemological status was assured: whatever theoretical disputes might erupt in anthropology – as they did, often quite floridly – the bedrock of ethnography was treated as a secure foundation. Second, the majority of anthropologists took it for granted by never teaching ethnographic methods systematically at all. It was an oral tradition (if anything). (Mills, 2008, Delamont, Atkinson and Parry 2000).

In the absence of formal training, and with a sparse methodological literature, coupled with a lack of a culture of explicit discussion of a variety of possible and novel methods, the generation of researchers that included Delamont, Atkinson and Murcott had to fend for themselves. They were largely self-taught. The Second Chicago School (Fine 1995) provided research exemplars, and some elements of guidance, but there was no direct contact, and the culture was partially reproduced at a distance, at one or two removes.

For the local culture, the publication of the first edition of Hammersley and Atkinson's *Ethnography; Principles in Practice* in 1983, marked a turning point. While one cannot claim that it was the major

determinant, it also coincided with something of a change in the academic culture: interestingly it coincided almost exactly with Burgess's (1984) *In the Field*. And since that period, Cardiff has again been something of a microcosm of much wider – even global – changes. From being an avowedly minority commitment in sociology, ethnographic research methods (and qualitative methods more generally) have become widespread, fashionable, and institutionalised. The methods and methodology literature on qualitative methods, and on ethnography, are now so extensive that no scholar could claim to have assimilated all the books and papers. The days of the purely oral tradition have gone, for good or ill; the autodidact may still exist, but is a throwback.

Cardiff has, of course, been subject to the same market forces as any other centre in the UK beyond. We have all moved from an implicit model of academic socialisation to an explicit one. Forty years and more ago, the key attributes of the ethnographer were primarily personal, and based on experience and haphazard mentoring. (Delamont, Atkinson and Parry, 2000). Now we expect explicit training, and are more likely to think in terms of skills rather than personal qualities. The transition reflects and contributes to a major shift within the academy. The insertion of terms such as 'training in skills' is more than stylistic. To take Basil Bernstein's (1971) useful ideas once more, there has been a major cultural shift towards 'visible pedagogy' in the universities, and an increasingly tight 'framing' of knowledge acquisition. Competencies are increasingly subject to definition, ever more finely sifted, taught and assessed. The units of knowledge acquisition, such as credits and specific competencies are equally finely graded, and subject to rational-calculative accounting mechanisms.

Cardiff's engagement with ethnographic methods has reflected and promoted such tendencies. After Surrey, Cardiff was early in the market with a Masters course in Research Methods, in which ethnographic work featured prominently. Through that course, and the methodological work that

came out of Cardiff throughout the 1980s and 1990s, it has been a major site of knowledge reproduction in ethnography. In that respect, Cardiff has helped to promote a version of ethnography that has converged with versions promoted elsewhere, to become a kind of orthodoxy. There is a strong, emergent paradigm of ethnographic research, widely transmitted and reproduced through publications and training courses. It is often a second-hand version of methodology, produced and reproduced through secondary sites of re-contextualisation. The classification and framing of such knowledge is often very strong, as semi-scholars as van Gennep (1967) called them seek to classify and codify research methods, paradigms and so on – usually far beyond the bounds of common sense. Cardiff ethnographers must plead guilty to helping to promote this emergent orthodoxy. Through our training and supervision, through our methodological and empirical publications, we have helped to sustain the success of a version of ethnographical research that has become diffused through a wide range of sites and disciplines. We have not done so single-handed. On the contrary, there have been increasingly large numbers of academics involved in the process.

There are countervailing tendencies. There are various heterodoxies to which we have contributed as well. We have – not always intentionally – contributed towards the intellectual crisis of ethnography and ethnographic representation. We have continued the tradition of eclecticism by drawing on literary and cultural theory in analysing ethnographic texts and the textual reconstruction of social realities. (Atkinson 1982, 1990, 1992, 2014, 2017; Coffey and Atkinson 1996; Atkinson, Coffey and Delamont 2003; Atkinson, Delamont and Housley 2008). We have tried to explore some of the centrifugal forces and have led us and others away from a simple endorsement of the emergent orthodoxy of ethnography. Indeed we could be accused of double standards. We are not alone in this either. We contribute to and benefit from the standardisation and codification of ethnographic methods. We do so while simultaneously appearing to render them problematic and undermine their foundations.

This reflects perhaps, what Thomas Kuhn (1962) once called the 'essential tension' between the stable, conventional pursuit of research, and the instability of innovation and experiment. It is also worth adding that we ourselves do not see quite such a radical disjuncture as do others (Delamont, Coffey and Atkinson, 2000; Atkinson and Delamont, 2008). We do not, for instance, see the crucial examination of ethnographic representation as a radical departure from earlier, more classic approaches. We see them, rather, as logical extensions from the preoccupations of anthropology and symbolic interactionism with forms of language and representation. It is a logical development of the principle of *reflexivity* rather than a completely novel version of it.

Cardiff is no different from any other centre. We have simultaneously reflected and projected changes in the wider academic community. Insofar as we have made a contribution, we can be pleased with the ways in which our ideas have been disseminated and our publications distributed. Equally, we must share in some of the potential blame than may accrue. There has been, I feel, an undue emphasis on *method* among younger scholars and recent converts. One might almost suggest that method has become fetishized. Method dominates. Method is, of course, global, and publishers and pedagogues want skills that can be translated freely on an international scale. Method has become transformed into commodity knowledge. Even while commentators like Lincoln and Denzin (1994, 2000) write of the seventh moment in the development of qualitative work, there are many teachers and practitioners celebrating the boundaries of crudely conceptualised paradigms, and patrolling the borders of orthodox practice. In erecting their perimeter fences, they even subordinate sociological or anthropological theory to methodology.

Insofar we have, in however a modest a way, contributed to that development, then we feel ambivalent. Ethnographic research is treated with a seriousness it was not always accorded twenty years ago. But popularity and respectability come at a price. The frontiers we now survey are complex. The terrain of ethnography has become fractured, and Cardiff continues to stand at the

interaction between some of them: the contrasts between traditional empirical work on the one hand, and postmodern, feminist perspectives on the other; the intersection between methodological orthodoxy and the carnivalesque proliferation of methods and representations; the relationships between ethnography and literary and cultural studies. These are not issues for our simple resolution, but for creative exploration. We attempt to do so, increasingly constrained by the exigencies of resourcing and institutional pressures for short-term gain.

Contestation and Representation

Amanda Coffey

Introduction

This chapter reviews some of the recent developments concerning the analysis of qualitative data and the ethnographic representation of social realities. The postmodern turn in ethnography, and in the social sciences more generally, has inspired many commentators to identify and to explore a varied range of ways to report and represent the social or the cultural. In recent years there has emerged a process of destabilisation: taken-for-granted methods of representing the outcomes of social research are being challenged and refashioned. An outcome of this is a celebration of diversity across various representational modes. We do not believe that it is necessary to endorse the exaggerated rhetoric and the extreme formulations of postmodern inquiry in order to take seriously the issues we have been grappling with and discuss briefly here. Indeed, we believe that although the postmodernist turn has often provided the inspiration, earlier – more classical – versions of sociological or anthropological understanding furnish justifications for the exploration of ethnographic representation. We contrast this centrifugal movement with a current centripetal tendency amongst qualitative researchers to converge on a particular type of data management and analysis. This model combines methodological perspectives associated with grounded theory and the techniques of computing. We note that the use of micro computing strategies for qualitative data analysis is becoming widespread and this includes almost a globalising process within the research community. Towards the end of the chapter we identify a strategy which synthesises the themes we have outlined.

Debates and Representation

Contemporary debates over the ethnographic representation of cultural phenomena have concentrated on the textual construction of reality (Atkinson 1990, 1996). Emerging most starkly within the discipline of anthropology, (see Spencer, 1999, 2001 for a summary of these debates up to 2001). The publications of Clifford and Marcus (1986) followed by Behar and Gordon (1995) were central to the anthropological emergence. Such debates have now spread to sociology and the ethnographic endeavour more generally. At the centre of such debates is the critical appraisal of ethnographic writing and the social production of the ethnographic text. Traditionally, the professional and academic status passage has been completed and confirmed by the construction of a major text. The anthropological monograph, therefore, was both the culmination of the ethnography and the legitimating mark of the anthropologist. The relationships between fieldwork, text production and the discipline of anthropology, have then developed over time. The anthropologist was identified with his or her people, who in turn were identified with and in the ethnography. The ethnographic monograph thus became the embodiment of the discipline itself and the identify of its practitioners. Within the classical period of British and American anthropology the ethnographic monograph enshrined a series of standardised representations of societies and (by implication) of their authors (Boon 1982). We recognise that there are, of course, other modes of ethnographic representation, including film. They are as conventional and artful as any written text (cf Crawford and Turton 1992, Loizos 1993).

Given the importance of the ethnography as textual product it is little wonder that radical assaults on its status should strike at the roots of the discipline. Thus in recent years, anthropology – once so stable – has experienced a ‘crisis of representation’. The textual foundations have been shaken and, along with them, the intellectual faith that has informed their production and reception. The status of ethnographic texts has also come under scrutiny from within sociology (Atkinson 1990, 1992, Hammersley 1992). In many ways this has proved a less critical issue for sociology than for

anthropology, not least because ethnographic methods and monographs are much less central to sociology as a whole. Important though qualitative research is in many fields of empirical sociology, it does not underpin the entire academic enterprise as it does for anthropology. The critiques of ethnography in sociology have sometimes followed directions similar to those in anthropology, (see Hastrup 1992, Richardson 1990, 1994). Several of the positions which have contributed to such critiques have been associated with the general thrust of postmodernism. Postmodernism in general has certainly contributed to reappraisals of cultural representation, in the human sciences and beyond. It should also be acknowledged that recent developments are not dependent on postmodernism per se. Many of the current tendencies can be understood as developments of anthropology and sociological perspectives, rather than radical departures from them.

It is possible to identify a number of contributions to the contestation of ethnographic representation, and a number of trends which have emerged out of such claims. Contributions have come from a range of intellectual directions. The weakening of cultural (and indeed) disciplinary boundaries has been spurred by a movement which we might usefully call the 'rediscovery of rhetoric'. Rhetoric is no longer consigned to the margins of legitimate scholarship. It has more recently been recognised as central to scholarly work and production. The classical theory and practice of rhetoric was concerned with argumentation and persuasion. The separation of rhetoric and science at the Enlightenment implied a radical distinction between two contrasted sets of commitments. On the other side are ranged rhetoric, persuasion, opinion and ornamentation. The aspirations of modern scholarship were firmly rooted in such dualities. The separation of rhetoric from logic in the creation of modern disciplinary knowledge parallels a number of other, equally fundamental, separations and dichotomies. It established the possibility of an observer armed with a neutral language of observation (because it was untouched by rhetoric) and thus allowed for the elementary distinction between that observer and the observed. The rediscovery of rhetoric creates the possibility of removing such distinctions: of removing the distance between the subjects and

objects of inquiry, and questioning the taken-for-granted canons of science and reason (Coffey 1999). It reminds us that scientific accounts and texts have rhetorical qualities. It challenges cherished distinctions between scientific fact and textual production, or between the reality of the natural-scientific fact and textual production, or between the reality of the natural-scientific world and narrative accounts of the social world. (For other accounts of representation of the natural and social, see: Lutz and Collins 1993; Bazerman 1988; Myers 1990; Latour and Woolgar 1979).

The work of authors such as Edward Said (1978) has placed this weakening of cultural boundaries more overtly within an ideological perspective. Said's sustained commentary of the *orientalism* of western observation has served to strengthen the case that traditional ethnographic texts have a privileging effect. That is, the cultures which have been represented have been reduced to the subjugated and muted objects of a dominating discourse. In enumerating and classifying the exotic characteristics of the oriental, then, the privileged observer has established a position of authority, which is inscribed in the texts of exploration, description and classification (Marcus 1992). A virtually identical set of issues can be described for the encounters of the old world with the new in the conquest and appropriation of the Americas. From the earliest accounts of the Spanish conquests through to the accounts of nineteenth-century explorers and ethnologists, the continent has been populated by others and appropriated through the accompanying representations. The texts of exploration and exploitation repeatedly inscribe the metropolitan perspective and the alterity (otherness) of the new world (Pratt 1992: Todorov 1984).

Feminist theory and praxis has also questioned the thus far privileged position of observer-author. Here the argument has not been about the over-or-under representation of men and women as ethnographic authors, but rather about the relationships between feminism, gender and ethnography at more fundamental levels. Clough (1992), for instance, articulates a feminist view, drawing on psychoanalytic perspectives. She argues that from a feminist standpoint one can see the standard realist accounts of ethnography as incorporating unconscious fantasies and desire

concerning race, gender or class. Realism, she argues, suppresses those unconscious processes under the guise of actual discourse Wolf (1992) also addresses the feminist perspective on ethnography and representation. She suggests that reflexive, self-critical attitudes are particularly characteristic of feminist thought. Feminism in general encourages an examination of power and powerlessness, the mutual obligations of researcher and researched. She implies that feminist scholars were exploring these issues independently of their becoming fashionable topics among male anthropologists. As Wolf also suggests, the heightened sensibilities of feminist scholars have led directly towards problems of representation. In a similar vein Mascia-Lees *et al.* (1989) draw attention to a concern among feminist anthropologists for modes of understanding (including writing) that do not reduce women to the position of voiceless *objects*, but treat them as subjects in their own right, entitled to their own voices. This echoes the very foundations of the feminist research process – the concern with voice and authority, accounts and experience (Smith 1987, Olesen 1994).

The proper representation of social reality has therefore become contested (for a more detailed discussion see Atkinson and Coffey 1995). A major consequence of this has been critical attention on the production (and reading) of ethnographic texts. The historical and stylistic continuities with so-called realist fiction have been well documented (Krieger 1983, 1984, Atkinson 1992, Cappetti 1993). Literary realism has been identified as the dominant mode of representation, implying an impersonal, all-but invisible-narrator (Van Maanen 1988). As a style, as a collection of literary devices, realist writing is a massively familiar one for the construction of factual authoritative accounts. There is, therefore, the danger of taking it for granted, and hence of treating it as a natural way of representing the social. Despite this tendency towards a realist approach, it remains by no means clear that literary realism is the only – or even the best – way to produce accounts of varied social worlds. Indeed, as Atkinson (1982) noted, there is something of a paradox in the use of what one might call a ‘straightforward’ realism for ethnographic purposes. There is a tension

between the conventions of realism and the assumption of most ethnographic work. For most ethnographers – whether sociology or anthropology be their primary discipline – recognise the complexity of social life and its collective representations. Equally, they recognise the complexity of social life and its collective representations. Equally, they recognise the fundamentally constitutive nature of language. That is, language-use creates and constructs social reality. Interpretive anthropologists, for instance, are committed to the ideals of ‘thick’ description’, while symbolic interactionists equally endorse an interpretative sociology that places language at the heart of an essentially constructivist view of reality and representation. And yet conventional realism is founded on a very different treatment of language. Such realism has historically encouraged little or no explicitly concern for the language of representation itself. Realism treats language as a taken-for-granted resource. The realism of conventional writing may therefore result in ‘thin’ description. Such arguments – that narratives and descriptions from a single, implicit point of view may not do justice to the complexity of cultural forms – have given rise to various alternative approaches. These epitomise the diversity of more recent ethnographic work and reflect the interpretative turn in ethnographic writing and representation.

Various commentators have called for texts that are more open, messy and fragmented in order to do at least two things: firstly to challenge and highlight the very conventionality of such ethnographic writing and secondly to allow for more creative and complex modes of representation (cf Mulkay 1985). Further, therefore, while the conventionality of all modes of representation is recognised, there is more than a hint in such arguments that complex texts may be more faithful to the complexities and contours of social life. We have discussed some of these alternative forms of representation elsewhere (Atkinson and Coffey 1995, Coffey and Atkinson 1996). The sorts of alternative representational modes we have in mind include: a dialogic approach, (Dwyer 1977, 1979, Allan 1994, Holquist 1990); ethno-drama or ethno-theatre (Paget 1990, Ellis and Bochner 1992); and poetry (Richardson 1992). These approaches are in turn closely related to the promotion

of biographical and autobiographical work in anthropology and sociology: in particular, on the 'writing' of lives and selves (Stanley 1992: Hastrup 1992).

The general affinities between experimental ethnographic writing and postmodernism are clear. Postmodernism, in recognising and celebrating the diversity of types and representations, encourages a variety of genres. It also encourages the blurring and mixing of genres. It questions the monovocal expression of authenticity in favour of polyvocal texts and the celebration of diversity. There is, therefore, much in the postmodernist movement to commend various radical re-evaluations of ethnographic writing. It is equally arguable that the possibilities for textual experimentation are contained within the modernist movement in literature. Modern literature provides us with a multiplicity of textual formats and devices for the construction of written representations. Modernist fiction found many ways, for instance, of representing the mingling of external events and inner dialogue; of reconstructing the minutiae of extraordinarily detailed description; of linking factual reportage with the fantastic. By adopting some of these 'new' conventions and by experimenting in similar ways ethnographic texts can also be viewed as undergoing modernist movement. Atkinson (2014) is a development of this theme written twenty years after this original text by Coffey.

Ethnographic data analysis and the computing moment.

This section represents 'the state of the art' as it was in 1996. For a more recent overview of the area see Stewart, forthcoming.

It is possible to identify a different trend in the treatment of ethnographic data . This is a trend toward a sort of homogeneity in (particularly) the analysis of data. We believe that an emerging orthodoxy is being adopted globally by key members of the qualitative research community. This is largely, though by no means exclusively, linked to the growth of Computer Assisted Qualitative Data Analysis Software (CAQDAS) as a subfield of expertise (Lee and Fielding, 1991; Fielding 2001).

Software packages aimed at analysing qualitative data are now widespread and it is a fast growing field. We do not intend to review that literature, or the existing software here. That has been done elsewhere and is an area to which Cardiff has contributed (Burgess 1995, Tesch 1990, Weaver and Atkinson 1994, Weitzman and Miles 1994). It is, however, important that we draw attention to the area in general, and some specific issues within it.

We note in particular, the convergence of most computer applications on a general model of data marking and retrieval. Many of the software packages may most accurately be described as computer-based applications for the storage and retrieval of data. While there are additional facilities and sophistication involved, the general notion of coding remains fundamental to such CAQDAS. The purpose of such software is, at root, twofold. First, it facilitates the attachment of codes to segments of data. Second, it allows the researcher to retrieve all instances in the data that share a code. Such code-and-retrieve approaches are exemplified in programs such as The Ethnograph, one of the most widely disseminated and used of all the applications. The underlying logic of coding and searching for coded segments differs little, if at all, from that of manual techniques. There is no great conceptual advance over the indexing of typed or even manuscript notes and transcripts, or of marking them physically with code-words, coloured inks and the like. In practice the computer can add many advantages. The speed and comprehensiveness of searches is an undoubted benefit: the computer does not search the data file until it comes up with the first example that will 'do' to illustrate an argument, nor will stop after it has found just one or a couple of apposite quotes or vignettes. The software has an additional merit that definitely marks an advance on the practical value of manual coding and searching. It can cope with multiple and overlapping codes; it can conduct multiple searches, using more than one codeword simultaneously. Software available in 2016 allows the analyst to combine codewords, in an approximation of Boolean logic, to facilitate complex searches. The co-occurrence of codings can be an important issue; finding them can be a useful tool. Since the software can handle very large numbers of codings

and separate codewords, in purely mechanical terms the computer can help with more comprehensive and more complex code-and-retrieve tasks than can be achieved by manual techniques.

Many of the software packages allow the researcher to do more than just coding, and permit the user to do things like attaching analytic or other memoranda to specific points in the text. The aim is to incorporate many of the key tasks of 'grounded theory' strategies within the software applications. A useful discussion of the convergence of (versions of) 'grounded theory' and CAQDAS has been outlined by Lonkila (1995). We would certainly not wish to deny the relevancy of 'grounded theory', nor the potential value of coding qualitative data (nor the value of CAQDAS for such work). A danger that we have indicated (Coffey, Holbrook and Atkinson 1996) is the unnecessarily close equation of grounded theory, coding and software. Grounded theorising is more than coding, and software can be used to do more than code-and-retrieve textual data. The point here is not about the full potential of CAQDAS, nor about the true nature of grounded theorising. Rather, the danger we identify lies in the glib association between the two, linked by an emphasis on data coding procedures.

In our view, the association of CAQDAS with a simplified 'grounded theory' justification can be misleading to students and researchers to whom it is introduced. CAQDAS offers a variety of useful ways of organising data in order to search them, but coding data for use with computer programs is not *analysis*. It is important to avoid the misapprehension that coding and computing lend a scientific gloss to qualitative research. The growing 'respectability' of qualitative methods, together with an adherence to canons of rigour associated primarily with other research traditions, can lead to the imposition of spurious standards (Fielding 2001). The categorisation of textual data and the use of computer software to search for them appear to render the general approach akin to standardised survey or experimental design procedures. In our view, qualitative research is not enhanced by poor imitations of other research styles and traditions. Analytic procedures which

appear rooted in standardised, often mechanistic procedures are no substitute for genuinely 'grounded' engagement with the data throughout the whole of the research process. It is worth noting that the 'usefulness' of such computer programmes imply that you have collected and inputted all of your data, and this suggests that data collection and data analysis are discrete and linear.

There are various evaluations of the methodological and practical value of various software applications. The general approach has spawned its own area of expertise. Here we wish to note that many of the analytic strategies implied by code-and-retrieve procedures are tied to the specific inputting requirements of computer software strategies. As a consequence, there is an increasing danger of seeing coding data segments as an analytic strategy in its own right, and of seeing such an approach as the analytic strategy of choice. That should not be the case. Coding segments of text, with or without the addition of analytic memoranda to selected segments, is by no means the only way of managing and manipulating data. It is not even the only way of thinking about the use of computers for qualitative data analysis. The qualitative research community should not endorse the computer-based code-and-retrieve strategy as the automatic approach to management and analysis. The computer-based handling of textual data is a useful extension of the capacities of word-processing and textual data storage. The indexing or coding of text in that context is a useful heuristic to approach the data themselves.

Two Trends Combined.

We have juxtaposed two alternative tendencies in the analysis and representation of ethnographic research. On the one hand we have identified a tendency towards pluralism and polyvocality. Such an approach emphasises the variety of analytic strategies that are available, and equally emphasises the variety of representational modes that are open to the ethnographer. This position (or collection of positions) include those associated – broadly speaking – with a postmodern sensibility. On the other hand, we have a distinctly modern tendency towards a common approach, approximating

perhaps to an industry-wide 'gold-standard', based on an elementary set of assumptions and procedures for the organisation and management of qualitative data. While we have recognised the heuristic value of such procedures, we have been cautious as to their general effects and consequences if widely but uncritically adopted.

At Cardiff we have contributed to the thinking behind and literature about the debates and developments we have identified in this paper. To conclude we turn to a further development that might usefully be thought of as a possible future direction for the analysis and representation of ethnography. We believe that there is a use for computing strategies in contemporary and future ethnographic work, and we believe that unlike much of the code-and retrieve computing, this approach represents a genuine and generic advance over manual methods of data management. We refer to the exploitation of hypertext and hypermedia techniques.

The normal conventions of text printed or otherwise, determine a linear, sequential mode of presentation and processing. The reader, to paraphrase Lewis Carroll, should start at the beginning, carry on until the end is reached, and then stop. Of course, even with printed words on the page, life is not exactly as rigid as that: one may start at the middle, one may skip sections; one may cheat and read the ending first. But the text itself is fixed in its linear form. Likewise, working with files of text on computers may equally involve a linear structure. Again, one may scroll up and down, or jump from the beginning to the end, one may (as with a novel) move to a bookmark. But the processes are often essentially the same. The idea underlying hypertext is essentially different and relies on a view that text is non-sequential.

Hypertext may not be an especially new idea, but it may prove to be one whose time has come. In essence, the underlying ideas are fairly simple. They are predicated on the view that the reader's relationship with a given text (such as a literary work or a work of reference) need not necessarily be restricted to the linear reading of that text in a predetermined sequence. Its approach is non-linear, more akin to browsing and following up cross-references. Hypertext software allows a reader to

follow, and indeed to create, diverse pathways through a collection of textual materials. Hypertext applications thus support a much more interactive relationship between the text and its readers. Readers can, in a sense, become authors of their own reading; they are not simply the passive recipients of a determinate textual form (Dicks, Mason, Coffey and Atkinson 2005).

This approach has exciting possibilities for qualitative researchers. Many people working with qualitative data – whether they use field notes, interviews, oral history or documentary sources – feel frustrated by the necessity of imposing a single linear order on those materials. It is, after all, part of the rationale of ethnographic and similar approaches that the anthropologist, sociologist, historian, psychologist or whoever, recognises the complexity of social inter-relatedness. We recognise the over determination of culture, in that there are multiple, densely coded influences among and between different domains and institutions. It is, therefore, part of the attraction of hypertext solutions that a sense of dense interconnectedness is preserved – enhanced, even – while linearity is discarded.

The use of hypertext (and indeed hypermedia) is not an easy option. There will always be practical limitations, of course, and there are also limits placed by human cognitive capacities. There is, for instance, the possibility that is widely recognised, of becoming lost in ‘hyperspace’ if the whole thing becomes too complicated, and the user cannot get back to where he or she started, or cannot navigate to where she or he actually wants to be. There is a practical and cognitive cost to be borne in return for abandoning the linear printed text. The demands on authors and readers can be considerable. We do not achieve greater complexity without some further investment of concentration and imagination.

The implementation of hypertext applications for the analysis, authoring (and reading) of ethnographic data would not absolve the ethnographer from writing his or her theorised account, and the tasks of writing sociological or anthropological commentary must also be accomplished. The hypertext may facilitate a more flexible and more densely linked set of relationships between ‘data’

and 'theory', but it does not remove them. The final construction of a hypertext ethnography may thus prove a costly and time-consuming effort. However, the opportunities afforded by such approaches are potentially wide, especially when we enter the world not just of hypertext, but of hypermedia. It is possible to incorporate not just textual data, but information in other media as well. The ethnographer may look forward to a time when a reader can choose to hear extracts from interviews or other spoken data, or find video images when an expansion button is clicked on, or have a wide array of graphic images, the 'ethnography' itself might be published in hard copy, as a conventional book, but there might be another 'ethnography', consisting of an array of information stored in different media, accessed via a computer and a CD drive, through which the professional social scientist and the student or lay reader could navigate pathways and pick up information appropriate to their respective interests and levels of sophistication. The systems and software all exist now, and qualitative researchers are starting to exploit them. Predictions in this area are often doomed to failure, and one would be foolish to try to second guess how and to what extent they will be taken up. There is no need to assume that all future ethnographers will become 'cyberpunks', and any more than one need predict that fate for all literary critics (even though the same opportunities exist in the humanities, and indeed are rather more advanced there than in sociology or anthropology). It is, however, worth noting that 1995 saw the publication of what was claimed to be the first anthropological monograph to be accompanied by an interactive compact disk (CD-I) (Kersenboom 1995).

It is not necessary to endorse all the criticisms of postmodernists, feminists and postcolonialist critics in order to recognise the value of research and representations that allow for a plurality of analyses and interpretations. Likewise, it is not necessary to subscribe to the most extreme versions of textualism in ethnography to recognise that there is room for representations that are even more open and more complex than are conventional ethnographic texts. We do not fall into the trap of thinking that hypertext is the embodiment of postmodernism, nor that it solves all the problems

posed by critics of conventional ethnographic epistemology. We do, however, believe that the tasks of cultural exploration and representation will be invigorated by the systematic exploitation of such approaches. Indeed, we believe that in the near future, when virtual reality systems, global information links and the like will be commonplace, the traditional ethnographer, reliant on written texts for the primary means of representation and grounded in realist prose, could well seem like a dreadful anachronism.

Working in Border Country

R. C. Burgess

When I was first invited to act as a discussant in this symposium I thought about a title for my contribution which I thought could be called Working in Border Country or Working in Bandit Country. In the end I have chosen the former, although the latter might apply in the light of developments that have occurred in relation to ethnographic studies in Britain over the last twenty years, in which members of the 'Cardiff School' have made an important contribution. Indeed, ethnographers across the UK might feel that they have worked in 'Bandit Country' given that ethnography has often been 'sniped' at by members from a range of disciplines. Certainly, twenty years ago ethnography was trying to establish itself in disciplines such as sociology. Surveys of teaching indicated that it was rarely taught, apart from three or four lectures in general courses on research methods (Wakeford, 1979; Burgess, 1979). Whole courses devoted to the subject were very much a late-1970s development and then only in a few isolated cases. The widespread use of ethnographic methods was very much something that occurred in the mid-1980s. Furthermore, the proponents of ethnography may feel that they have worked in bandit country as their projects and proposals have been 'ambushed'. Many have not been seen as central to the discipline or sufficiently mainstream to warrant support, or to warrant funding. Indeed, it is relatively rare to find research foundations willing to fund an ethnographic study. Instead, multi-site studies have had to be devised to acquire funds. Indeed, the Cardiff team, together with a team from Warwick, have recently been engaged in studies of postgraduate students in higher education (Burgess, 1994). At first we were asked to do the equivalent of *Boys in White* (Becker et al, 1961), but this time in respect of postgraduate social scientists. On reflection, Atkinson and Delamont, and I costed the whole proposal only to discover that it would probably have amounted to some ten times what we initially

received to do our qualitatively based inquiry which predominantly used in-depth and unstructured interviews.

Some commentators might regard my description of working in bandit country with some cynicism. However, they may feel that the idea of working in border country is more appropriate. Indeed, many ethnographers and ethnographic studies have been confined until relatively recently to the borders of their discipline, partly on the basis of funding that is available, and partly on the basis that the invisible college of researchers engaged in ethnographic inquiry lacked seniority until just under ten years ago. With many British ethnographers becoming senior members of the profession, it has been much harder to confine the activities to the borders of the disciplines, as strong networks have been able to flourish within and beyond the UK. However, there is still a symbolism associated with work in border country. Cardiff does exist near the border of England and Wales, and this, in itself, needs to be examined.

The Substantive Border.

The geographical position of Cardiff has given rise to considerable potential for ethnographic inquiry. In particular Sara Delamont has often made the point that when studies are conducted in Britain it is often England that is the point of study, rather than the different countries that comprise the United Kingdom. Accordingly, she and her colleagues have often contributed studies based in Wales. In these circumstances, the Cardiff School has focused not so much on ethnography as a method, but ethnography as the study of culture. In this respect, the Cardiff ethnographers have provided a detailed portrait of the way in which schooling, training, and so on, operates within Wales and is informed by, and in turn informs, a Welsh culture. Most recently they have been involved in a study sponsored by the Equal Opportunities Commission, that focuses on gender and schooling in the United Kingdom, where Cardiff has contributed on the Welsh experience.

The Cardiff team have also contributed ethnographies in a range of other areas, including Health and Higher Education. Here socialisation and occupational socialisation has played a key role, yet the studies that have been completed have also focused on a border or boundary. For example sociological studies of education have for far too long concentrated almost exclusively on schools rather than other educational circumstances, and within the frame of schooling on those experiences that take place in state schools as opposed to independent schools (Burgess, 1986). In a refreshing way, the members of the Cardiff school have conducted studies of education and schooling that go beyond the traditional secondary school. Furthermore, the Cardiff team have also examined independent schools, and educational circumstances beyond schools through studies of higher education. In this respect they bring new insights from the borders of subject areas and share these insights using concepts that have been traditionally associated with ethnographic enquiry. However, it is not just the substantive comments that have been contributed by the Cardiff team.

The Methodological Border.

As the papers have demonstrated, the Cardiff ethnographers have made substantial contributions to ethnographic study and the development of ethnographic methods. This has been done not only through teaching the subject at undergraduate and graduate level, but also through research supervision of a whole range of doctoral theses that have focused on the systematic training of young researchers. Within this tradition, the Cardiff School have resisted the temptation to argue the merits and de-merits of qualitative as opposed to quantitative research, but rather have made strong statements about the way in which systematic ethnographic enquiry can be conducted. The major members of the team have produced their own texts and readers which have had a major impact on the discipline of sociology (Hammersley and Atkinson, 1983), and also provided a major contribution to sub-fields of sociology, especially medicine and education (Delamont 1992a). As Atkinson and Delamont's work indicates, they have paralleled much of what has gone on in the discipline and in methodological writing. In particular, they have contributed to the systematic study

of social situations using a range of all collection procedures that are rooted within the tradition of symbolic interactionism, and which include observation, participant observation, unstructured interviews, narrative analysis, the use of documentary evidence, oral histories, and so on. However, they have also written on such topics as data analysis, writing and dissemination, and also the use of technology. Indeed, Amanda Coffey's paper indicates the way in which the work of founding members of the Cardiff School has been transmitted and extended by those who have worked as graduate students within the department. In particular, much work has been devoted to the use of technology and software programmes to help researchers analyse qualitative data. A distinctive feature of the Cardiff School has not been to discuss technology in a simplistic or technicist way, but rather to indicate how it can be used to aid the researcher in making more systematic and detailed use of their data, allowing researchers to develop a much stronger set of cases than can be developed from those data that have been collected and subsequently analysed. A further methodological strand in the work of the Cardiff team has been work on writing and dissemination. This has included Paul Atkinson's systematic inquiry presented in the *Ethnographic Imagination* (Atkinson, 1990), as well as Sara Delamont's practical assistance for postgraduate students who are developing research theses and dissertations for the first time. All these areas have been important for the development of sustained ethnographic enquiry. But they are also more important than that. The general requirement of the Cardiff team that sociologists should be reflexive about their activities has had an impact on the methodological work that has been conducted in the discipline. In that sense, the Cardiff ethnographers have shifted reflexively out from the borders of ethnography into the main stream of sociology. Once again, we see the discovery of yesterday is the common sense of today. Many would regard the demand to be so reflexive to be fundamental to the discipline, yet one only has to look at the books and papers produced some ten years ago to appreciate the novelty involved in this approach.

Future Directions.

The papers that have been presented in this symposium chart some major achievements that have occurred in British ethnography over the last twenty years that are well reflected in the contribution that has been made by the Cardiff School. Many of the changes that have occurred have had an impact upon the shape of sociological teaching and research in British higher education. New courses have been developed, new studies have been designed and more detailed portraits have been provided of people in Britain by ethnographers who have focused on social circumstances in their own society rather than in other societies. Furthermore, the work of ethnographers in Britain, and in Cardiff in particular, has resulted in individuals working not just on the deviant or the exotic, but also on a range of everyday circumstances, in hospitals, in schools, in further education colleges, in higher education, in accountancy firms, and so on.

Accordingly, we have learnt much about the conduct of ethnographic enquiry and the process of doing research where the researcher himself or herself, has been central to the tradition. However, the current exigencies of higher education, together with the limitations placed on funding has meant that classic ethnographic studies can no longer be funded and conducted within British society with any ease. The idea of the lone researcher conducting a single study over a long time period is very much a dwindling vision. Accordingly, British ethnographers need to think about ways forward. To what extent is it that ethnographers need to think about:

1. The use of mini case studies which provide insights into fundamental processes across a range of sites. This might lead to the development of longitudinal case study work, or indeed, to multi-site case study work.
2. Researchers need to consider ways in which data analysis might be more systematic in this area so that we understand not only the use of technology and software programmes but also the way in which concepts and theories can be used in order to reach a greater understanding of the situations we observe.

3. We need to consider the time-scales over which ethnographic studies can be conducted. What is the ideal period for an ethnographic study? To what extent can grounded theory play a part in de-limiting the field of study and also promoting data that are reliable and valid?
4. How can different methodological approaches be brought to bear so that we no longer talk about ethnography as an approach that competes with the survey, but rather complements the survey in order to provide new insights into areas of social life.

Conclusion.

The Cardiff School has provided insights into the development of the ethnographic tradition in Britain in general and in sociology in particular. It is hard to imagine that some twenty years ago, when Paul, Sara, Martyn Hammersley and I were postgraduate students, there were very few ethnographic studies of British social life, with the result that reading lists relied heavily on an anthropological literature. We have attempted to change all that partly by conducting ethnographic studies, and partly through contributions that we have made to establishing a methodological literature. The consequence is that we have pushed forward the borders of our discipline and pushed forward the boundaries of our subject. But this is something that is constantly being done by scholars working in a range of traditions, and the Cardiff School is no exception. Here a new generation of researchers are beginning to push forward into new substantive areas and new methodological terrain which will, in turn, inform the discipline of sociology and promote the advanced study of detailed aspects of British society. Long may they continue with this important work.

Doing it Tidy: the open exploratory spirit and methodological engagement in recent Cardiff ethnographies

Robin James Smith

In this section I describe and explore a number of core principles of the ethnographic work that has practiced and developed the 'open exploratory spirit' described elsewhere in this text, and espoused in the teaching of fieldwork methods at Cardiff. There are, I suggest, various strands of work that have been influenced by the earlier seminal ethnographies of Atkinson and Delamont, and their methodological writings and teaching that, although certainly distinct, bear out a family resemblance in terms of their commitment to thoroughly sociological analysis and critical engagement with methodological development and innovation in various substantive projects. Here, I discuss the various methodological contributions made by Cardiff ethnographies and the lessons that such studies have for others looking to do ethnography 'tidy'.

'Doing it tidy' is a local south Wales phrase recently adopted by Paul Atkinson to describe something of both the Cardiff approach to ethnography and, more generally, the ways that fieldwork and ethnographic practice might and should be done. It was also the title of the talk that I gave at Paul's *Festschrift* conference in 2015; a talk I develop herein. 'Tidy' has a number of meanings beyond the more generally recognised sense of things being 'in their place'. 'Tidy', for locals, is a positive term, meaning 'good' or 'pleasing' or agreeing in the affirmative: "Are you coming for dinner?", "Tidy!". The metaphor is worth pursuing further in both senses. In line with the general sense, Cardiff ethnographies might well be characterised by a certain 'tidiness' in their analytic and theoretically informed attention to social orders and organisation. Although there is a recognition of and engagement with 'mess' and 'complexity' and 'uncertainty' (see below), an abiding concern within the

'Cardiff School' has been with the ways in which people themselves make sense of the complexity of the social world. Which is to say, a concern with how people find and manage mess in their everyday lives. This is not to say, of course, that Cardiff ethnographies bear out a disdain for methodological complexity or resort to a naïve empiricism – far from it. A 'tidy' approach to ethnography is thus "predicated on the recognition that local social organisation and the conduct of everyday life are complex, in that they enacted through multiple modes of social action and representation" (Atkinson, Delamont, and Housley, 2008: 31-2). In this sense, a number of projects have examined aspects of social complexity found in 'discourse and social interaction', 'narrative', 'materials', 'places and spaces', and 'visual and sensory cultures' not as matters to be 'celebrated' as conceptual or methodological novelties but as, first and foremost, *phenomenological social orders*. Various Cardiff ethnographies, have been characterised by both a contribution to the leading edge of methodological development whilst, at the same time, providing a critical engagement with the methods employed in the studies themselves. In this sense, then, the Cardiff School might be said to have contributed to various 'turns' (the visual, the spatial, the mobile) whilst, at the same time, remaining resolutely resistant to spurious claims to novelty and intellectual 'wilful ignorance' (Atkinson, 2015). The methodological work of the Cardiff School thus represents a sustained programme of methodological inquiry.

A second sense of 'tidy' might be taken to relate to the 'open exploratory spirit' itself. 'Doing it tidy' – in place of the American, 'doing it right' – implies a sense of openness to a wide range of approaches, theoretical influences, field sites and means of doing fieldwork. Not a restricted code of practice, but a recognition of the breadth of ways in which fieldwork and ethnographic writing can get done whilst retaining a focus on rigour, the politics of representation through writing and other means, and perhaps above all, an *analytic sensibility*.

In focussing this paper, I recognise a theme running through a number of ethnographic and qualitative studies conducted in the past decade or so. Namely, a sustained critical engagement with various methodological innovations within the social sciences and the affordances of various emergent technologies for the doing of fieldwork. These projects have been characterised by an empirical engagement with innovation in which the contribution of any particular 'new' way of working has been thoroughly 'field tested' in relation to the production, capture, analysis, and representation of qualitative materials. Again, the visual, the mobile, the spatial, and so on, have consistently been explored and handled as social orders, accomplished, and handled in situations by people, but also, as imbricated in the production of the contours of various social settings from regenerated waterfronts and city centres, to post-industrial communities and their rural surrounds, to science education centres and university buildings. This stands in contra-distinction to the wider pursuit of methodological innovation and the uptake of 'qualitative research' across the social sciences and beyond. As noted by Housley and Smith (2010) "...the growth of qualitative methods across the social sciences represents a space through which innovation and post-disciplinary collaboration is promised; however, it also serves to obscure disciplinary logics and thereby facilitate analytical accounts for phenomenon for which there are no questions". We might also note that innovation is also often justified and accounted for in pursuit of 'everyday life' in such a way that ignores or, equally as often, aims to negate previous established concepts and studies that have done much to shed light on what it is that people do to make social organization possible and which, ironically, both obscures and 'explains' everyday practices.

An implicit influence, recently foregrounded (Smith and Atkinson, 2016), is an enduring concern with method and measurement practices (Cicourel, 1964). In some instances this has led to the direct empirical study of social researcher's practices (Housley and Smith, 2015), in other cases conceptual critiques (Housley and Smith, 2010; Smith, 2014). In the majority of cases, however, Cardiff

ethnographies have served as critical test beds for methodological development and inquiry. The projects display a deep interest in method not in terms of a narrow development of technique but, rather, with the ways in which methodological and measurement practices necessarily involve the making of practical decisions by the researcher in the doing of their craft. In many ways, Cardiff ethnographies have sought not only to practice the ethnographic maxim of making “the strange familiar, and the familiar strange” but also to adopt the principle in scrutinizing their own research practices in a direct manner. In what follows I describe a number of these projects. This is not an exhaustive list, and I of course apologise to colleagues, past and present who have contributed in various ways but whose names or work does not appear here. These projects are, however, indicative of the core principles of ‘doing it tidy’.

Hypermedia, multimodality and digital ethnography

A key strand of methodological innovation and critique at Cardiff concerned experiments with the affordances of digital multi-media in terms of the documentation, analysis, and representation of ethnographic and qualitative data. Early projects were concerned with the methodological examination of the use of CAQDAS (Weaver and Atkinson, 1994) and the affordances of hypermedia for analysis and the then nascent ‘digital ethnography’ (Dicks et al, 2005). Cardiff was one of the first centres in the United Kingdom to investigate the possibilities of digital research for analysing field notes and other materials in ways that went beyond the standard ‘code and retrieve’ model. This foundational work was followed up in the Economic and Social Research Council (ESRC) and National Centre of Research Methodology (NCRM) funded *QUALITI* (Qualitative Methods in the Social Sciences: Innovation, Integration, and Impact) suite of methodological projects (see, for example, Hurdley and Dicks, 2011). The ‘Watching, Listening, Reading and Clicking: Representing Data Through Different Media’ project examined the ways in which audiences understood and constructed, epistemologically and ontologically, various multimedia representations of the same project. Such work provided early

developments in the representation of ethnographic projects as digital artefacts whilst at the same time enabling an empirical inquiry in to the relative epistemic status of multi-modal representations of research findings. Of course, in keeping with the ethos of the School it wasn't so much the novelty of going 'beyond the text' that was interesting here but an analytic concern with how the science centre itself employed a range of multi-modal and multi-sensory media to communicate aspects of physics, chemistry, and biology to an audience of families and children and how this was 'understood' by visitors (see, for example, Coffey et al, 2006). This, then, underpinned the second layer of the research in which the findings of the project were produced as a traditional academic essay, a video, and as a website through which the audience could choose how to read it and in what order. Despite these initial experiments, the *publication* of academic research still lags behind the technologies and multi-modal engagements that researchers have with the field. (Although see the 'Innovative Ethnography' series edited by Phillip Vannini (2012) for an attempt to bridge this gap.)

Most recently, various Cardiff ethnographers have been involved in analysing developments in the disruptive impact of new technologies and aligned emergent forms of 'digital social science'; again, demonstrating both the continued and necessary contribution of ethnographic and qualitative studies and analysis (Smith, 2014). Roser Beneito-Montagut (2015) has, for example, conducted ethnographic fieldwork investigating older people's use and understandings of social media, whilst other work has investigated the affordance of 'big data' in dialogue with existing forms of research (see Edwards et al, 2013). Gareth Thomas has written of the impact of digital technologies upon the experience of pregnancy and neo-natal diagnoses (Thomas and Lupton, 2016; Thomas 2017), Jamie Lewis and Andy Bartlett have studied the emerging field of 'bioinformatics' (Lewis et al., 2016) and Neil Stephens (2013), on *in vitro* meat laboratories. So, whilst many are questioning the validity and worth of ethnography in 'digital society', it is clear that there is much work to do and many opportunities for ethnographers.

Visual, participatory, and collaborative methods

Another strand of ethnographic work at Cardiff has involved the development of collaborative, participatory, and co-produced research methodologies, often built around visual materials. In ways similar to those outlined above, this work is also characterised by substantive concerns and critical methodological development. Through the early work of QUALITI (see, Hillman et al., 2008; and a series of films produced by Bambo Soyinka) and, later, that of the Wales Institute for Social Economic Research Data and Method (WISERD), ethnographic projects located in Cardiff and Heads of the Valleys communities provided sites for the development and critical exploration of a range of methods and encounters in and through which to engage with young people's lives and experiences of their locale. Experimenting with various elicitation exercises and visual resources the projects sought to explore the relationship between locality and the experience of 'growing up' and the spatial and mobility practices of young people. A series of localities studies conducted by WISERD researchers investigated the 'worlds' of young people living in communities of the Heads of Valleys. Here, researchers worked with and alongside young people, often completely handing over processes of data capture and the devices to them, in order to get closer not only to their subjective experiences but to better understand their local knowledges and skills (that were often not recognised in formal educational settings). In a separate programme of research, Dawn Mannay has pioneered, and critiqued, the use of a range of visual practices including sketching and drawing (Mannay 2010) to develop a way of researching with young people that is 'less extractive' (Singha and Back, 2013). Going beyond standard talk or observational based approaches, the active and participatory research encounters provided ground from which to also explore and critically consider shifting power relations within the research encounter in a way that goes beyond well-trodden discussion of positions held by researcher and researched to consider representation, communication and responsibilities to 'non-consenting others' (Mannay 2015). More recently, this strand of work in

Cardiff has been developed by Emma Renold and colleagues in a number of projects that have explicitly sought out collaborative encounters with young people that, again, go transgress traditional forms of research interaction and representation (see, for example, Libby et al, 2017). Coproducing artefacts and multimedia representations with young people about their own lives has proved tremendously effective both in terms of the research itself but, perhaps more significantly, in terms of communicating with policy makers and stakeholders. Such work has demonstrated the potential and power of ethnographic and qualitative research to make meaningful impact in a domain routinely assumed to be dominated by statistics and graphs.

Place, mobility practices and (the) walking (interview)

A recurrent concern across a number of Cardiff ethnographies, and one certainly present in the various projects mentioned thus far, has been with 'place' and 'mobility' and, more specifically, how place might be said to be constituted in and through multiple social orders. A central concern here has been the analysis of the ways in which 'place' and 'mobility' intersect with the everyday lives of particular groups and individuals. Various QUALITI and WISERD projects made use of walking interviews to investigate the intersection of mobility, place, and experience in explorations of 'third spaces' (Moles, 2008). Tom Hall, Brett Lashua, and Amanda Coffey (2008) experimented with how the roving interview or 'soundwalk' disrupted both the production and assumptions of the 'conventional interview' as a kind of pristine data extraction situation. The soundwalk, instead, allowed for the unexpected and the contextual – a street sweeper driving past whilst conducting an interview on a busy street, for example – to enter in to and, more importantly, be analysed as salient within the research encounter. In this, and other work, the walking interview was used to explore the worlds of key informants, not simply as a novel method nor uncritical celebration of walking per se, but as a means to better understand the organisation of experience for groups whose lives were intimately bound up with public space (see Hall et al., 2009; Ross et al., 2009). More recently, Kate Moles' work (Moles and Saunders, 2015) has

explored the production of 'place' through soundwalks. Here, participant-produced audio tours produce an alternative understanding and mobile biography of place, acting as both methodological experiment and disruption of marginalizing and stigmatizing narratives of peripheral urban areas.

Street-level mobilities and walking practices were also the focus of Tom Hall and Robin Smith's ethnography of urban outreach workers (Hall, 2016; Hall and Smith, 2015; Smith, 2011; Smith and Hall, 2016); not, in this instance, as a research practice but as a practice already employed by their informants. In this sense, walking was taken as an 'already in the world' ethno-method through which outreach workers not only searched for their rough sleeping clients but also a pedestrian encounter with the city through which their knowledge of place is accomplished. Walking the city with outreach workers over the course of six years not only enabled an intimate knowledge and competency in the practice, but also a street-level ethnographic view of the 'politics of public space'. Methodologically, the project experimented with Global Positioning System technology as a means of both capturing where the team moved but also as an experiment in capturing their 'local knowledge' enacted in movement. Hall and Smith (2014) were, however, dissatisfied with and highly critical of the ways in which GPS traces, plotted on a base map, misrepresented and, in fact, inverted the mutually constitutive relation between knowing and going.

In a less obvious sense, perhaps, the ethnographic study of Capoeira – a diasporic Brazilian martial art with roots in the Portuguese colonies of West Africa – by Neil Stephens and Sara Delamont (2014), is also concerned with mobility, space and place. In their 'two-handed' ethnographic practice, Delamont and Stephens investigated the ways an embodied practice such as Capoeira could be studied by a separate participant and observer. Capoeira is thus mobile in two senses – the direct embodied sense

of the whirling spins and kicks, and as a cultural practice born of mobility in the 16th Century and then in the globalized world and the various ways in which it is glocalised.

The significance of these projects has, then, been both a contribution to the mobilities paradigm and the continued demonstration of how careful attention paid to local practices can also shed light on some of the 'big questions' of contemporary social science in a world on the move (Sheller and Urry, 2006; Büscher et al., 2016).

Skilled practices and material manipulations

Cardiff ethnographers have also focused on the ways in which materials are worked with by people in everyday and professional settings and how materials are themselves active in the production of experience, place, memory and home. Taking the latter first, this has long been a central concern in the work of Rachel Hurdley. Paying attention to institutional arrangements and uses of spaces such as corridors (2010a) and domestic arrangements centred on the mantelpiece (2013), these studies have both shed light upon the often unnoticed and unremarked relationship of materials to people's everyday lives. They have also – in keeping with the previous themes of work – considered matters of data capture, representation, research ethics (Hurdley, 2010b). Hurdley's work characterises a kind of 'everyday multimodality'; a combination of archive documents from Mass Observation, interviews with people about their own mantelpieces, sketches, photography and video. I say 'everyday' because, again, the approach in this work reflects an attempt to get closer to the ways in which everyday life is itself complex and comprised of multiple orders, yet sensible and storyable to those that live it.

Finally, there is the most recent work of Paul Atkinson that, drawing upon his earlier studies of practice and pedagogy in the medical context, provides what he describes as a 'micro-ethnography' of

precise and skilled practices (2015). In a series of studies, Atkinson has turned his ethnographic eye to an attention to the actual detail and lived competencies employed by people when doing, for example, shaping metal, blowing glass (2013b), or life drawing. The contribution of these studies is found in the provision of detailed descriptions of practice that demonstrate and describe the ways in which people themselves are skilled practitioners and innovators; developing, critically testing, and teaching and transmitting methods for the production of a vase or screen print, say, in *just this way*. Indeed, and as might be expected, this attention to detail and analytic sociological sensibility is the essence of 'doing it tidy'. It might seem odd to cite being analytic as a quality of the work of the Cardiff School, yet, we note, far too often analysis is lost to reportage and subjective assessment. A closeness to lived practice, a clarity of description, and an analytic rigour, often gained through 'just enough' or an 'aliquot' of fieldwork (Atkinson 2016) is preferred over appeals to subjective 'experience'.

Conclusion

I have described some of the key work that has been conducted at Cardiff in the past decade or so. I have focused on work that both demonstrates 'doing it tidy' and has critically engaged in methodological development and innovation. There are further works – notably PhD studies – that have been completed and are ongoing in the School that fit in to one or other or both categories. 'Doing it tidy' is a sensibility toward ethnography, developing from the principles of the Cardiff School discussed elsewhere, that acknowledges a wide programme of ethnographic work that shares something like the following core principles:

- An analytic treatment of social orders in cultural and social context
- Sustained methodological inquiry and critique, coupled with a healthy scepticism of innovation and novelty
- Critical engagements with technology, multi-media, and the digital (both as research methodology and as found in the world)

- An attention to the specificity of practice
- An engagement with areas of everyday interest alongside and as where to find 'big' societal issues
- A faith in faithful description
- A strong influence of symbolic interactionism, Goffman, and ethnomethodology
- 'Theoretically informed' ethnography drawing on multiple influences (including French post-structuralism and ANT)

Developed over the course of the past forty years in and through the work of a large number of researchers, PhD students, collaborators and colleagues, the Cardiff School of Ethnography, guided by Sara Delamont and Paul Atkinson and those who now follow, looks set fair to continuing making substantive and methodological contributions in to the 21st Century.

Reflections on Paul Atkinson's *For Ethnography*

Martyn Hammersley

In this chapter Hammersley uses the opportunity afforded by the publication of *For Ethnography* (Atkinson 2014), where he encourages 'micro-ethnography' (p121), but insists that this must be located within the broader context of institutional and societal organisation (p87), to reflect on the achievement of the Cardiff School. This Chapter examines a number of themes from that book.

The status and character of ethnography

A good place to start is with the title of his book. As this makes plain, *For Ethnography* is a manifesto. Atkinson is particularly keen to emphasise the distinctiveness of this approach to social research, and this derives in part from his critical attitude towards much qualitative work today, specifically where interviews or focus groups are relied on as the main or exclusive method, and where the primary aim is to document people's personal experiences or perspectives. He describes the orientation behind this trend as Romantic, or sentimental, since it fails to recognise that expressions of personal experience and viewpoint are always constituted in and through interactional and discursive forms, rather than being reflections of 'authentic' individual selves or transparent representations of the world. He criticises much autoethnography on the same grounds.

Now, I certainly agree with him that there is a great deal of apparently unthinking reliance on interviews and focus groups, and frequently a misplaced preoccupation with 'giving voice' to people, as well as a tendency to neglect the situated and constructed character of all accounts. However, I want to question the way he privileges ethnography over other approaches to social research, and his distinctive interpretation of the character of ethnography as concerned with describing social forms, 'the intrinsic, indigenous modes of social organisation' (p74).

Atkinson treats ethnography as superior to other approaches, in effect as the gold standard. He writes 'Ethnographic fieldwork is not just a way to conduct social research. It is a very special way. It is, if not the way, a distinctive way of knowing and being as a social scientist [...]' (p3). And, a little later on the same page, he describes it as the 'most rewarding and faithful way of understanding the social world'. These are bold and contentious claims, but he offers little direct argument in support of them, perhaps because for him they are an article of faith. He does not discuss the relationship of ethnography to the broad range of competing methodological stances that now exist in social science. Yet, given that we live at a time when, in powerful quarters, there is strong emphasis on the value of 'big data', randomised controlled trials, and mixed methods, it is surely necessary in any 'manifesto' to address the question of the distinctive value of ethnographic accounts in comparison with the products of these other sorts of social research. In fact, I believe that doing this is urgently required.

Turning to what Atkinson says in his book about ethnography itself, he does not discuss the diverse range of interpretations that have come to be given to the term. Elsewhere he has declared himself a 'reluctant methodologist' (Atkinson 2012), and in part this is a reaction against the way ethnography has come to be 'overlaid with all sorts of epistemological and theoretical "positions" and disputes' (p5). However, while I sympathise – there is indeed a great deal of spurious philosophising in the field of qualitative research – in my view he underplays the fundamental differences in orientation now to be found in the field.

Furthermore, while he claims that 'an old-fashioned approach to ethnographic fieldwork lies at the heart of' what he writes (p4), there are respects in which his stance is at odds with a considerable amount of past work coming under this heading. For example, while he largely rejects the use of interview data as a source of information about people's lives and perspectives, much work categorised as ethnography has used this as a key complement to participant observation, and sometimes even as central. Similarly, his formalist interpretation of the goal of inquiry is different

from the more substantive foci common in a great deal of ethnographic work, where the aim for instance is to describe and explain some problem in the lives of a group or type of people, the strategies they use to deal with it, the consequences of these, etc. He also rejects the idea that ethnography is concerned with discovering what people actually do as against what they say they do, or what is really going on as against what is portrayed in official or other accounts (pp99-101). Yet much work that would often be included under the heading of ethnography has been committed to such concerns. While debating what sorts of work are 'truly' ethnographic (p121) is fruitless, it is important to recognise that what is being presented here is a distinctive version, indeed in many ways it is an explication of the approach behind Atkinson's own ethnographic work, and that of some of his colleagues at Cardiff. It is of great value for that reason, but there are questions that can reasonably be asked about it.

One way of describing the distinctive kind of ethnography Atkinson proposes would be to say that it is ethnography in the service of a 'sociology of everyday life'. He frequently refers to 'everyday life' or 'mundane reality' as the focus of inquiry, and this no doubt reflects the influence of Simmel, Goffman, and ethnomethodologists. For example, Garfinkel (1967:1) declared that ethnomethodology involves 'paying to the most commonplace activities of daily life the attention usually accorded extraordinary events, to learn about them as phenomena in their own right'.

There are at least three rather different arguments that could be put forward in support of this focus. One is that everyday life is a worthwhile but neglected topic. In these terms its study complements other kinds of sociological and anthropological investigation. Alternatively, it may be argued that we cannot understand the issues and problems with which other sorts of social science are concerned without taking account of the mundane social processes that these inevitably involve. In other words, it can serve as a foundation for sociological investigation. The third position is that studying everyday social processes is the only rigorous or worthwhile form of social science.

Atkinson seems to take the second of these positions, along with Simmel (1950, 1977, 1980) and Goffman (1975), whereas I tend to adopt the first.

His position here is closely related to what I will refer to, for the sake of convenience as a commitment to constructionism and formalism.

Constructionism

Atkinson's commitment to constructionism is, I suggest, evident in his attitude towards interview data. Agreeing with Silverman (1993), he writes that:

we cannot approach interview data simply from the point of view of 'truth' or 'distortion', and we cannot use such data with a view to remedying the incompleteness of observations. By the same token, we cannot rely on our observations in order to correct presumed inaccuracies in interview accounts. On the contrary, interviews generate data that have intrinsic properties of their own. In essence, we need to treat interviews as generating accounts and performances that have their own properties, and ought to be analysed in accordance with such characteristics. (p95-6).

Silverman's position is that interview data can tell us little more than what goes on in interviews, and, for the most part, it seems that Atkinson agrees with this. He writes that 'a recognition of the performative action of interview talk removes the temptation to deal with such data as if they gave us access to personal or private "experiences"' (pp99-100). He suggests that, instead of being concerned with whether informants' accounts provide reliable information, we should focus on credibility as a property of those accounts themselves, addressing the question: 'How does the informant construct a plausible account?' (p101). And as illustration he cites his own treatment of reports of alien abduction.

Atkinson's constructionism seems to be derived both from French structuralism and from ethnomethodology, though I think the latter is probably the most important influence here. But,

taken more broadly, we can think of 'constructionism' (or sometimes 'constructivism' – the phrasing he uses) as starting from a rejection of naive realism: the idea that the accounts people produce (including those of researchers) are generated by the world impressing its character upon their senses, this 'impression' then being 'expressed' in a manner that simply corresponds to or captures that world. This, I suspect, is what Atkinson is dismissing when he insists that 'language-in-use is never a neutral medium of representation' (p93). He is insisting that processes of perception and cognition necessarily shape what we experience, and what we take to be true, that these are dependent upon the role of language, and that language must be seen not as some fixed, internal, generative structure but rather as a diverse collection of social practices. Thus, what is taken to be real or true is necessarily a socio-cultural product, and will vary accordingly: in short, reality is socially constructed. From this it may be further concluded that, since we do not have unmediated contact with reality, the validity of any judgment about what is real, or true, is relative to the socio-cultural framework within which it is formulated, or is conditional upon the discursive resources employed.

While much of this argument is sound, in my view the relativist or sceptical conclusion drawn from it is fallacious: it relies implicitly upon the definition of knowledge put forward by the empiricist epistemology that it rejects. Atkinson seems to toy with this fallacy in his insistence that ethnographers should not evaluate the truth of the claims people make in interviews, or use informants' accounts to check observational reports, but focus instead on the discourse resources they are employing to render their accounts plausible (p101). More broadly, he appears to suggest that we can avoid the problem of treating data as representing features of a world that exists beyond it by focusing on the forms through which it is presented to us. Thus, we can use interview data to study the forms displayed in and through it, and similarly when we are using observational data our focus should be on how the performances observed are constructed, and the methods or principles of organisation they involve.

In my view, this drastic remedy is unnecessary because the problem has been falsely diagnosed. While I agree that our goal should be to document and explain facts – rather than to evaluate people’s actions or the institutions in which they participate – this does not rule out assessing the likely validity of people’s accounts as sources of information in pursuit of factual knowledge. In other words, if we reject the false sceptical conclusion drawn from constructionism, while we can certainly decide to investigate social forms, this type of investigation is not privileged over other more substantive foci, either as a foundation or in any other sense; and we can use interview accounts as a means of gaining information about the world alongside and in combination with observation.

Formalism

Atkinson cites Simmel and Goffman in support of his commitment to a formalist approach, and criticises the failure to adopt this in much qualitative research today. He writes: ‘We have lost sight of the multiple ways in which social conduct is patterned through routine and ritualized methods of conduct. We forget that cultural domains display codes of organization and of signification’ (p13).

This may be true, but there is a problem that neither Simmel nor Goffman are very clear about what is meant by ‘form’, and much the same is true of Atkinson’s use of the term.

It seems to be used in four different senses. The first is central to literary or textual analysis, where for example ‘narrative’ is treated as a language form that has specifiable constituents which must occur in some particular order (as identified, for example, by Labov 2013: p102). What we have here is something like the constitutive rules that define particular types of game, though of course the rules can change over time, or develop differently in different contexts; and one game can morph into another. A second, slightly different kind of ‘form’ consists of patterns of social action or interaction that have the character of rituals or routines. One example is the sort of ‘interaction ritual’ studied by Goffman in the context of what he calls focused gatherings or encounters. An example from Atkinson’s (2013a) own research is the opera ‘master-class’ as a distinct type of pedagogical interaction. In these examples, much of the emphasis is on regulative rules, which are

seen as guiding behaviour partly because of a commitment to preserving 'face'. A third type of form, and the opera master-class could also be seen as an example of this, would be certain sets of occupational skills, such as those deployed by the artists engaged in glass-blowing that Atkinson (2013b) has also studied.

Atkinson also identifies a more general type of social form: 'modalities', 'orders', or 'codes' that represent whole fields of social phenomena. Following Goffman, he refers to the 'interaction order', distinguishing this from the 'political' and 'economic' orders (p73). But he also adds: the 'semiotic order', the 'spatio-temporal order', the 'material order', and the 'aesthetic order'. One possible parallel here is with the different systems that operate within the human body, such as the nervous system, the digestive system, and so on. However, whereas these are relatively well-defined, despite the fact that they are interrelated, this is not true in the case of Atkinson's typology of orders, or for that matter Goffman's. It is not always clear exactly what each 'order' refers to and how this is separable from what the others denote, even in analytic terms. For example, there are questions about how the 'interaction order' can be separated from those practices that make up the 'semiotic order'. And how are we to distinguish between the semiotic and material orders when the latter is described as involved in 'cultural codes' and as providing 'semiotic resources' (p21)? Furthermore, do not all of these necessarily operate within spatio-temporal frameworks – indeed Atkinson emphasises that perceptions of time and space are constituted interactionally and/or semiotically. And it is not clear whether he sees these orders as overarching forms that contain, or frame, other more specific ones, or whether they serve as a series of laminations (perhaps in line with the metaphor of 'contours' used in an earlier book with Cardiff colleagues: Atkinson et al., 2008).

Aside from this, though, it is clear that what Atkinson means here by 'order', 'code', or 'modality' is rather different from the more specific textual and interactional forms mentioned earlier. In part, at least, these orders comprise what we might call constitutive frameworks of interpretation. Atkinson states that 'experience itself is constructed by and through socially shared, culturally prescribed

forms' (p13). Simmel has provided the most developed account of this kind of form. He starts from Kant's notion of the categories of understanding that structure our experience of the physical world, notably space, time and causation. Kant famously argued that these are not present in the 'things-in-themselves', only in 'the-world-as-it-appears'. For Simmel, though, rather than being intrinsic to a transcendental subjectivity through which we apprehend the world, they operate immanently within that world, generated by the multifarious processes he refers to as Life. Furthermore, he extends this approach beyond the frameworks of scientific understanding of the physical world, of morality, and of aesthetics, the three fields on which Kant focused, to include history and sociology as distinctive interpretive frameworks. Goffman pursues a similar approach, albeit examining more mundane frameworks in *Frame Analysis*.

There is, however, a significant difference between what Atkinson has in mind here and the arguments of Simmel, and perhaps also Goffman, in that he stresses the need for ethnographers to attend to all of the modalities he lists in examining any particular social situation. In *Contours of Culture*, he and his co-authors write that we should not:

seek to render social life in terms of just one analytic strategy or one cultural form. The forms of analysis should reflect the forms of social life: their diversity should reflect the diversity of cultural forms; their significance should be in accordance with the significance of their social and cultural functions. (Atkinson et al., 2008:34)

Yet, from Simmel's point of view, if not from Goffman's, forms as interpretive frameworks involve discrepant ways of ordering reality and are therefore effectively incommensurable. This would seem to imply that in any particular study the focus would need to be on one or other of them: they cannot all be given the same weight. Thus, while the materials that make up other 'orders' may come within focus, they would only be taken into account selectively and would be constituted in the terms of the order that is the main concern. Given this, there is an area of uncertainty about the character of this third type of social form in Atkinson's discussion. There are also questions about

how knowledge of this and other kinds of social form is to be generated, and it is to this issue that I will now turn.

Generating theory through abduction

While Atkinson rejects the metaphor of theory-building (pp61-2), he argues that 'we can and should move between different versions of local reality, in order to move our analysis to the generic level, developing concepts – even models – that capture recurrent features of social life across a range of social situations or cultural domains' (p14), and he claims that ethnography has generated cumulative knowledge of this kind in some fields (pp36-7). So he insists that, while ethnography requires 'the methodical exploration, analysis, and re-construction of a given social world' (p6), it also goes beyond this to produce 'generic concepts and formal analyses that transcend the local and the particular' (p14). In this sense he claims that 'ethnography is generalisable' (p34), this residing in our 'capacity to generate reconstructions of social processes and social actors, in such a way as to remain faithful to the complexities of the particular setting, while drawing out the generic links and comparisons' (p66).

Here Atkinson's position is perhaps close to that of Geertz (1973), in that he does not see any conflict between the production of idiographic description and of theoretical or explanatory understanding. Indeed, the latter is to be achieved through the former. But how? Do these not involve incompatible orientations (Gomm et al 2000; Hammersley 2008:ch3)? After all, identifying recurrent, generic features in particular cases surely requires abstraction from some of the detail of those cases, that which is deemed irrelevant to the particular knowledge being pursued. Surely this involves ignoring some of the complexities?

Atkinson's proposed solution to this problem is abductive inference, a concept which he draws from the pragmatist tradition, and specifically from the work of Charles Peirce (Fann 1970). He writes that

much of what passes for qualitative data analysis 'is unhelpfully flat, reducing the complexity of everyday life to a set of themes that remain otherwise under-developed', and suggests that:

an implicit emphasis on inductive logic, rather than the abductive logic in the original pragmatist tradition, can easily have a deadening effect on the conceptual complexity of the analysis. An adequate and sensitive understanding of a given cultural system or social setting may emerge from the ethnographer's thorough knowledge of it, but it will not emerge from peering at 'the data'. So we really must free notions of analysis from a close dependency on 'data'. (p61)

Later, he comments that: 'we are in danger of losing sight of the role of imagination in the ethnographic enterprise. In contrast, we need collectively to encourage the sort of extrapolation and speculation that a thoroughly abductive logic implies', suggesting that 'our analysis therefore resides in the skill with which we interrogate simultaneously the local and the generic' (pp66-7).

Atkinson's conception of what abduction involves is similar in some respects to that of Peirce, for whom it involves the development of a potential explanation for some puzzling phenomenon not just through examining the phenomenon itself but also by drawing on experience of other similar and different cases, as well as deploying both existing scientific knowledge and, as just noted, imagination. However, in more general terms Peirce's model of scientific inquiry is very different from what Atkinson seems to have in mind. Peirce treats abduction as aimed at developing a causal theory to explain some type of phenomenon, whereas Atkinson interprets it as addressing the question: 'what is this a case of?' (p65 and elsewhere) – a rather broader question, not specifically causal or explanatory in character. And this raises a more general issue about the character of the ethnographic product. I have already noted a tension between the declared goals of describing particular social worlds and documenting 'generic processes'. But, aside from this, along with Geertz, Paul seems to see the latter task as a form of description rather than of causal explanation. This contrasts with Peirce's concern with developing scientific theories that capture general causal relations.

For this reason, Peirce saw abduction as one among several modes of reasoning (including deduction and induction) that all play a crucial role in scientific inquiry, he did not treat it as standing alone. For him, abduction led to theoretical ideas from which implications must be deduced, and these hypotheses were then to be tested in order to assess the likely validity of the theory, a process he sometimes referred to as retroduction. As with most other qualitative researchers who have appealed to the concept of abduction (for example, Tavory and Timmermans 2014), Atkinson neglects these complementary forms of reasoning, and thereby apparently the need for testing the accounts generated through abduction. Perhaps he regards it as simultaneously generating and testing the ethnographic account produced, but as we have seen he emphasises the role of imagination in abduction and the need to gain distance from the data, thereby minimising any empirical constraint on the accounts produced. For me this sails too close to the model of imaginative literature and art. While these are, of course, of great value in themselves, they are at odds in important respects with the task of social science.

Conclusion

In discussing Atkinson's book, I have concentrated on problems and disagreements, since that is more interesting than listing all the respects in which we agree. Paul may not regard the issues I have raised as needing resolution for sound ethnographic work to be done, but in my view they are quite fundamental, and must to be tackled if social inquiry, including ethnography, is to flourish, or perhaps even to survive, in what is an extremely challenging present and likely future. While I certainly agree that there is a great deal of misleading and often inept methodological philosophising about qualitative research, there are nevertheless fundamental issues that require attention. They may not be resolvable in any straightforward sense, and total agreement about them is unlikely, but they must be faced. By providing a distinctive account of the task and requirements of ethnography, Paul's book is an excellent stimulus to reflection about them.

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