A Century of Covert Ethnography in Britain, c.1880 – c.1980

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Abstract

The purpose of this thesis is to explore the history of covert ethnography in Britain between the 1880s and 1980. During this century, a range of academic and non-academic social researchers have used the method of covert ethnography. The starting point for this thesis is the observation that there is no adequate and sustained explanation of covert ethnography as a historical phenomenon. It is argued that the fragmented nature of the existing historiography precludes a full understanding of this important historical phenomenon. It is the intention of this thesis to bridge the gaps in the historiography, as it stands, and to promote an inclusive historical account of covert ethnography in Britain across time.

Through an analysis of covert ethnographic projects undertaken in Britain between the 1880s and 1980, with particular attention being paid to the structure and language used by covert ethnographers, this thesis will locate the use of this research method in its historical context. This thesis will chart the changes and continuities over time in the use of covert ethnography and demonstrate how key forces, such as the establishment of new models of ethnographic research and the development of ethical concern regarding covertness, shaped the use of covert ethnography significantly. This thesis will contribute a more comprehensive account of covert ethnography to the existing historiography.

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Acknowledgements

Having been a graduate student for over four years, I have mastered the art of procrastination and, even when it came to writing my 'Acknowledgements', I found time for a quick 'Google' of 'PhD Acknowledgements'. I was not surprised to discover that there is an academic discourse on the construction of PhD acknowledgements. I was, however, surprised to learn that they can be a hotbed of 'fawning' and 'vanity', and that they 'cannot be seen as merely a naïve listing of gratitude'.[1] I beg to differ, and would like to give my wholehearted thanks to the following:

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[1] Ken Hyland and Polly Tse, '"I Would Like to Thank My Supervisor": Acknowledgements in Graduate Dissertations', *International Journal of Applied Linguistics*, Volume 14, Number 2, (2004), p.261.

Author's Declaration

I declare that, except where explicit reference is made to the contribution of others, that this thesis is the result of my own work and has not been submitted for any other degree at the University of Glasgow or any other institution.

Signature:

Printed name: Gillian Nelson

Introduction

The Scope of this Thesis

Covert ethnography, the secret participation in, and observation of, a social group for research purposes, has a very long and complex history in Britain. It is reported, for example, that James V, king of Scots, would disguise himself as the 'gudeman of Ballengiech' in order to wander unhindered amongst his sixteenth-century subjects.[1] More recent and well-known examples of covert ethnography include the Victorian journalist James Greenwood's account of a night spent disguised as a tramp in the casual ward of a workhouse, and George Orwell's semi-fictionalised *Down and Out in Paris and London* based on undercover research into poverty.[2] Besides familiar examples such as these, however, many other, often more obscure, examples of covert ethnography have been published by a broad spectrum of individuals, from journalists to academics, on a variety of subjects, from common lodging houses to evangelical church groups. In the course of this thesis, a number of less well-known examples of covert ethnography will be discussed, including the journalist Hugh Massingham's account of working-class community in the 1930s, and the sociologist Roger Homan's study of Pentecostal believers in the 1970s.[3]

There are many reasons for undertaking an historical analysis of covert ethnographic research. As a method of research, covert ethnography has enjoyed a complex relationship with other methods of social research, often being used to represent the antithesis to survey research, or academic research more broadly. Covert ethnography has also been, since the 1880s, successively marginalised from and assimilated into the canon of professional social scientific methodologies. However, non-academic researchers have persisted in using the covert ethnographic method. Furthermore, the historiography of covert ethnography is fragmented, with historical accounts of different aspects of the research method occasionally overlapping, but infrequently engaging with each other. These aspects of the history of covert ethnography will be explored in more detail below.

In this historical account of covert ethnography, an attempt will be made to engage with the existing historiography and to reconcile the fragmented accounts in order to

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[1] Mark Freeman, ''Journeys into Poverty Kingdom': Complete Participation and the British Vagrant, 1866-1914', *History Workshop Journal*, Number 52, (2001), p.99; Andrea Thomas, 'James V, (1512-1542)', *Oxford Dictionary of National Biography*, Oxford University Press, (2004), http://wwwoxforddnb.com/view/article/14591 , [accessed 26th March 2010].
[2] James Greenwood, 'A Night in a Workhouse', in Peter Keating (ed.), *Into Unknown England, 1866-1913: Selections from the Social Explorers*, (Glasgow: Fontana, 1976), pp.33-54; George Orwell, *Down and Out in Paris and London*, (London: Victor Gollancz, 1933).
[3] Hugh Massingham, *I Took Off My Tie*, (London: William Heinemann Ltd., 1936); Roger Homan, 'Interpersonal Communication in Pentecostal Meetings', *Sociological Review*, Volume 26, Issue 3, (1978), pp.499-518.

produce a more complete analysis of this research method. All practitioners of covert ethnography, from the 1880s until 1980, will be considered as participants in the same broad enterprise. Particular attention will be paid to situating examples of covert ethnography in their historical context and to charting essential continuities and changes over time in the use of this methodology.

As suggested above, the practice of undercover social research has a long history, but this thesis will focus on covert ethnography published in Britain between the 1880s and the 1970s. The 1880s have been chosen as a starting point because it was during this decade that social crises, such as unemployment and homelessness, and fears of working-class revolt prompted a rush of social research into aspects of poverty, including covert research.[4] During the 1880s, Charles Booth commenced his survey-based research into poverty, which is often seen as the beginning of modern empirical sociology.[5] It was also in the 1880s that 'new journalism' emerged, a tradition of campaigning journalism spurred on by W.T. Stead's scandalous undercover exploration of child prostitution.[6] The development of empirical sociology and the emergence of 'new journalism' in the 1880s, which will be discussed in chapter 1, constitute important contextual developments that shaped the practice of covert ethnography significantly.[7] The decision to focus exclusively on covert examples of ethnography means that the ethical implications of such research can be addressed fully in later chapters. Before the analysis of covert ethnography in Britain between the 1880s and 1980 can begin, there are some key terms which must be defined and discussed. Subsequently, an account of the existing historiography of covert social research will be given before the aspects of covert ethnography which this history will focus on are outlined.

A Discussion of Key Definitions

Participant Observation

Throughout this thesis, the terms 'participant observation' and 'ethnography' will be used interchangeably, but it is important to recognise the differences as well as the links between these two key concepts. Participant observation, on a basic level, can be defined

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[4] Mark Freeman and Gillian Nelson (eds.), *Vicarious Vagrants: Incognito Social Explorers and the Homeless in England, 1860 – 1910*, (Lambertville, New Jersey: The True Bill Press, 2008), p.20; Gareth Stedman Jones, *Outcast London: A Study in the Relationship between Classes in Victorian Society*, (Oxford: Clarendon Press, 1971), chapter 16.
[5] Charles Booth, *Life and Labour of the People in London*, 10 Volumes, (London: Macmillan, 1892-1897).
[6] W.T. Stead, *The Maiden Tribute of Modern Babylon: The Report of the Secret Commission*, Anthony Simpson (ed.), (Lambertville, New Jersey: The True Bill Press, 2007). First published in the *Pall Mall Gazette* in 1885.
[7] See below, p.32.

as a research technique in which the researcher observes a social group of which he or she is also a member. Such observation can be conducted overtly, with the knowledge of the social group, or covertly, without the knowledge of the researched group. However, a survey of the relevant literature suggests that the definition of 'participant observation' is not straightforward and that it is, to some extent, a contested concept.[8] 'Participant observation' has variously been defined as a 'research technique', a 'situation', an 'epistemology' and a 'procedure for generating understanding of the ways of life of others'.[9] It has even been used to refer to a group of specific research methods which a participant observer may use in the course of their fieldwork, such as direct observation, formal or informal interviewing and interpretation.[10]

Perhaps the most fundamental aspect of the contestation over the definition of 'participant observation' coalesces around its problematic methodological status. Some commentators, including Gary Easthope, have dismissed participant observation as a research method unworthy of social scientific status.[11] In his account of the history of social research methods published in the 1970s, Easthope explained to his readers that participant observation was 'more akin to art than science' and that the 'distinction between accounts written by participant observers and accounts written by good journalists is indefinite and blurred'.[12] He went on to emphasise the 'essentially artistic' nature of participant observation and its 'inherently non-scientific' status due to the impossibility of scrutinising the method and replicating the 'experiment', a key method of verification in the context of the laboratory and the physical sciences.[13]

Danny Jorgensen, an experienced participant observer, has recognised the tendency of those who think that the social sciences should conform to the methodological standards of the physical sciences to dismiss participant observation as non-scientific. In contrast, Jorgensen suggested that 'ardent advocates' of participant observation tend to think of it as a 'special methodology' which is fundamentally different from positivistic methods of

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[8] Danny Jorgensen notes the existence of contrasting conceptions of the term. Danny L. Jorgensen, *Participant Observation: A Methodology for Human Studies*, (London: Sage, 1989), p.7.

[9] Nicholas Abercrombie, Stephen Hill and Bryan Turner, *The Penguin Dictionary of Sociology*, 4th Edition, (Harmondsworth: Penguin, 2000), p.256; G. Duncan Mitchell (ed.), *A New Dictionary of Sociology*, (London: Routledge & Kegan Paul, 1979), p.138; Thomas A. Schwandt, *Dictionary of Qualitative Inquiry*, (London: Sage, 2001), p.186; Thomas A. Schwandt, *Qualitative Inquiry: A Dictionary of Terms*, (London: Sage, 1997), p.110.

[10] Schwandt, *Qualitative*, p.111.

[11] Gary Easthope, *A History of Social Research Methods*, (London: Longman, 1974), pp.87-92. See also François Dubet's discussion of 'participant observation' where he raises doubts over the methodological status of this method and the validity of the information gathered or generated through the use of participant observation. François Dubet, 'Participant Observation' in William Outhwaite and Tom Bottomore (eds.), *The Blackwell Dictionary of Twentieth-Century Social Thought*, (Oxford: Blackwell, 1993), pp.447-448.

[12] Easthope, *A History*, p.87.

[13] Ibid., pp.88 & 92.

research.[14] To some extent, Jorgensen celebrates the artistic status of participant observation within the research community, suggesting that

> practitioners of participant observation have resisted formulating definitive procedures and techniques. Its practice has been regarded as artful and inappropriate for any kind of linear, mechanical presentation…For many of its zealous practitioners, participant observation is an art form and almost literally a way of life appropriately constituted as an oral tradition.[15]

Jorgensen appeared to embrace participant observation's marginal social scientific status. However, other social scientists who have recognised that 'there is no systematic and practically tested methodology for participant observation comparable to those of other sociological research methods' have regarded this as a problem in need of remedy.[16] Jürgen Friedrichs and Harmut Lüdtke, for example, have outlined a 'methodological foundation' for participant observation which met 'strict empirical criteria'.[17] They presented a highly structured map for the use of participant observation which promoted a standardized methodology with uniform observation obtaining results more amenable to quantification.[18] It would seem that Friedrichs and Lüdtke were attempting to impose a structure upon the practice of participant observation which would guarantee its status as a social *scientific* method at the expense of any artistic associations, which commentators such as Jorgensen were quite willing to perpetuate.

The fact that 'participant observation' appears in the majority of social science dictionaries and encyclopaedias suggests that, despite the reservations of some commentators, participant observation is generally regarded as a method of research of some social scientific significance.[19] Beyond this, however, there remain considerable differences between the ways in which social scientists characterise participant observation in terms of its disciplinary affiliations, its ideal type and the legitimate use of the method. For example, in *The Penguin Dictionary of Sociology*, participant observation is defined as a research technique in which 'the sociologist' observes a social collectivity of which he or she is also a member. According to this dictionary definition, it is only in its 'looser usage' that the term participant observation can be used to describe 'the form of observation in which the observer is known to be an outside investigator by those being studied' and that

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[14] Jorgensen, *Participant observation*, p.7.
[15] Ibid., p.8.
[16] Jürgen Friedrichs and Harmut Lüdtke, *Participant Observation: Theory and Practice*, (Hants: Saxon House, 1975), p.ix; Mitchell (ed.), *A New Dictionary*, p.138; Schwandt, *Dictionary*, p.186; Schwandt, *Qualitative*, p.110.
[17] Freidrichs and Lüdtke, *Participant observation*, p.ix.
[18] Ibid., p.ix-3.
[19] For example, see: Abercrombie, Hill and Turner, *The Penguin*, p.256; Mitchell (ed.), *A New Dictionary*, p.138; Schwandt, *Dictionary*, p.186; Schwandt, *Qualitative*, p.110.

both sociologists *and* anthropologists regularly make use of.[20] Similarly, the 1979 edition

of *A New Dictionary of the Social Sciences* defined participant observation as a

> situation where a social research worker becomes as much as possible a member of the group which he is studying and participates fully in the life of the group. The ideal form, where the observer is wholly a part of the group and not known by the group members to have any other role, is only attained by using the technique of the spy. In many cases, therefore, the term is used very loosely to describe research carried out by observers, such as anthropologists, who have gone to live for a time in communities and shared their day-to-day lives even though their dual roles are recognized.[21]

Thus, between them these two publications clearly establish participant observation as a

principally sociological research method which should, ideally, be practised covertly.

In contrast, in his discussion of participant observation in the edited collection

Methods, Ethics and Models, Jeremy Boissevain assumed that the participant observer was

not a sociologist but an anthropologist, working in a 'foreign culture'.[22] He suggested that

participant observation 'basically involves living for an extended period alongside the

people being studied…To get to know the people he is studying the ethnographer must as

far as possible share their experiences. It is obviously essential for him to learn the local

language'.[23] Again, contrasting with the constructions of participant observation discussed

above, Boissevain suggested that the ethnographer should avoid 'cover stories' wherever

possible in the field and, as far as possible, the participant observer should 'stick as close to

the truth as he can'.[24] Here then, participant observation is characterised as an

anthropological method ideally employed overtly in the field.

Like Boissevain, in their brief discussion of participant observation in their 1971

publication, *Survey Methods in Social Investigation*, Claus Moser and Graham Kalton

associated participant observation with the world of social anthropology.[25] In their account,

Moser and Kalton effectively constructed the ideal participant observer.[26] They

emphasised the importance of the participant observer's 'skill and personality' to the

success of their research endeavour and clearly located this required skill (if not

personality) within the community of academic social scientists. For Moser and Kalton,

the 'training', 'skill', 'awareness' of risks, 'understanding' and 'special abilities' of

sociologists or anthropologists made them the ideal participant observers.[27]

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[20] Abercrombie, Hill and Turner, *The Penguin*, p.256.
[21] Mitchell (ed.), *A New Dictionary*, p.138.
[22] Jeremy Boissevain, 'Ethnographic Fieldwork', in Jessica Kuper (ed.), *Methods, Ethics and Models*, (London: Routledge, 1987), pp.22-25.
[23] Boissevain, 'Ethnographic', p.22.
[24] Mitchell (ed.), *A New Dictionary*, p.138; Boissevain, 'Ethnographic', pp.23-24.
[25] Claus Moser and Graham Kalton, *Survey Methods in Social Investigation*, 2nd edition, (Aldershot: Dartmouth Publishing Co., 1971), pp.249-254.
[26] Moser and Kalton, *Survey Methods*, pp.253-254.
[27] Ibid., pp.251-252.

Thus, the accounts of both Boissevain and Kalton and Moser add weight to the notion that participant observation is a research method most readily associated with the social scientific discipline of anthropology. Moser and Kalton explicitly stated that the ideal participant observer should be a skilled academic social scientist. However, this is a notion which is not supported unanimously within the social research community. James Spradley, for example, asserted that 'anyone' could conduct their own ethnographic project and that a 'background in the social sciences' was an unnecessary prerequisite to becoming a participant observer.[28]

It is not possible to fully assimilate all of these contrasting definitions of participant observation into one synthesized definition, but some salient points have emerged around which the notion of participant observation can be located. Participant observation is a research method which involves researchers situating themselves within the communities or situations they wish to study. The field in which the participant observer works can be abroad or at home. Except when circumstances dictate otherwise, participant observers can often choose the extent to which they reveal their identity and research purposes to the communities under study, locating themselves upon a continuum between overt and covert. Again, the extent to which the participant observer participates or just observes can be a matter of choice on the researcher's part. Although participant observation has been incorporated into the social sciences to an extent, doubts continue to be raised about its academic and scientific status. Participant observers are generally assumed to be social scientists, but that does not mean that only social scientists have used the method of participant observation.

Ethnography

In his dictionary of terms related to qualitative research, Thomas Schwandt has noted the tendency for the term 'participant observation' to be used as a synonym for 'ethnography'.[29] However, it should be noted that 'ethnography' can be used to describe a phenomenon which encompasses the research methodology of participant observation. Ethnography, according to Boissevain, includes fieldwork conducted using the method of participant observation, *and* the subsequent analysing and writing up of the data collected

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[28] James Spradley, *Participant Observation*, (London: Holt, Rinehart & Winston, 1980), p.v.
[29] Thomas A. Schwandt notes the tendency for commentators to use 'ethnography' as a synonym for 'participant observation'. Schwandt, *Qualitative*, p.44. In his book, James Spradley uses the terms 'participant observation' and 'ethnography' interchangeably. Spradley, *Participant*, p.v, for example.

during the fieldwork.[30] Similarly, Schwandt defines ethnography as uniting 'process and product, fieldwork and written text'. He explains that '[f]ieldwork, undertaken as participant observation, is the process by which the ethnographer comes to know a culture; the ethnographic text is how culture is portrayed'.[31] Essentially, ethnography is a process which entails using participant observation, overt or covert, as a research method and then producing a text based on this episode. There are examples of participant observers who have used the term 'ethnography' to describe their publications. For example, in the 1970s Jason Ditton used the method of covert participant observation to study crime in an industrial bakery and he used the term 'ethnography' in his title: *Part-Time Crime: An Ethnography of Fiddling and Pilferage*.[32]

Social Exploration

According to Mark Freeman, the phrase 'social exploration' has been used to describe the use of participant observation in mid-nineteenth to early-twentieth-century Britain.[33] The term 'social explorer' has been used to describe Victorian and Edwardian social researchers who used research methodologies approximating participant observation. Again, like participant observation, social exploration could be practised overtly or covertly. However, there are two points to note about 'social exploration' which distinguish it from being simply the historical equivalent of participant observation. First, whereas the participant observer can use the chosen research methodology to explore any social context, institution or relationship, the social explorer is particularly associated with the transgression of class barriers. Raymond Kent suggests that the practice of social exploration 'presupposes a rigid class structure in which a representative of one social class consciously sets out to explore, analyse and report upon the life of another class lower on the social scale'.[34] It should be noted that many of the covert ethnographers discussed in this thesis, not just those traditionally thought of as social explorers, have transgressed class boundaries in the course of their fieldwork.[35] However, with the practice of social exploration, it is assumed that the transgression of class boundaries is an essential

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[30] Boissevain, 'Ethnographic', p.22.

[31] Scwandt, *Qualitative*, p.44.

[32] Jason Ditton, *Part-Time Crime: An Ethnography of Fiddling and Pilferage*, (London: Macmillan, 1977).

[33] Freeman, ''Journeys'', p.99.

[34] Raymond Kent, *A History of British Empirical Sociology*, (Aldershot: Gower, 1981), p.37. Peter Keating also noted the element of class transgression implicit in the practice of social exploration. Peter Keating (ed.), *Into Unknown England, 1866-1913: Selections from the Social Explorers*, (Manchester: Manchester University Press, 1976), p.13.

[35] For example, three mid-twentieth century middle-class academic participant observers, Pearl King, Enid Mumford and Marie Jahoda, all used covert methods to explore working-class occupational groups. Pearl King, 'Task Perception and Interpersonal Relations in Industrial Training: The Development of a Training Project in the Hosiery Trade. Part I', *Human Relations*, Volume 1, Number 1, (1947), pp.121-130; Pearl King, 'Task Perception and Interpersonal Relations in Industrial Training: Part II', *Human Relations*, Volume 1, Number 3, (1948), pp.373-412; Enid Mumford, 'Social Behaviour in Small Work Groups', *Sociological Review*, Volume 7, Issue 2, (1959), pp.137-157; Marie Jahoda, 'Some Socio-Psychological Problems of Factory Life', *British Journal of Psychology*, Volume 31, (1941), pp.161-206.

component of the research project. Second, the term 'social exploration' may be more comparable to 'ethnography' than to 'participant observation' as, like 'ethnography', 'social exploration' can be used to refer to the written product of the research as well as the fieldwork. In his account of social exploration, the historian Peter Keating describes it as a 'characteristic type of nineteenth- and twentieth-century literature'.[36]

There are two terms related to social exploration which, although they overlap with the practice, need to be distinguished from it: 'social investigation' and 'slumming'. The term social investigation is, like social exploration, usually associated with mid-nineteenth to early-twentieth century social research. However, whereas social investigation can be used to describe social research undertaken using quantitative or qualitative methods or a combination of the two, social exploration is more specifically associated with qualitative social research.[37] Seth Koven has described the Victorian practice of 'slumming' as 'a form of urban social exploration' which 'bore the obloquy of sensationalism...and self-seeking gratification, not sober inquiry and self-denying service to others'. Although Koven uses 'slumming' as an umbrella term to cover a range of practices including charity work, investigative journalism and social research, it was also a term which could be used pejoratively by those who were dismissive of such endeavours.[38]

It is difficult to establish definitive descriptions of terms such as 'participant observation', 'ethnography' and 'social exploration'. However, as Kent has recognised:

> There would be many histories we could not write if clear-cut definitions were a prerequisite. Nor does history begin only when people are aware of the phenomenon under investigation or when they have a word for it…It is up to the historian to draw selected parallels between contemporary phenomena, however well or ill defined, and certain events in the past.'[39]

The overlapping yet exclusive definitions of key terms such as social exploration and participant observation are, in a way, symptomatic of the disjointed historiography of covert ethnography.

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[36] Keating (ed.), *Into Unknown England*, p.9.
[37] David Englander and Rosemary O'Day and Freeman use the term 'social investigation' to refer to Victorian and Edwardian examples of qualitative and quantitative social research. David Englander and Rosemary O'Day, *Retrieved Riches: Social Investigation in Britain, 1840-1914*, (Aldershot, Hants: Scholar Press; Brookfield: Ashgate, 1995). Mark Freeman, *Social Investigation and Rural England, 1870-1914*, (Woodbridge, Suffolk; Rochester; New York: Royal Historical Society: Boydell Press, 2003). However, Keating does include the quantitative work of Charles Booth and Seebohm Rowntree in his discussion of social explorers: Keating (ed.), *Into Unknown England*, pp.112-140 &189-200.
[38] Seth Koven, *Slumming: Sexual and Social Politics in Victorian London*, (Oxford: Princeton University Press, 2004), pp.8-9.
[39] Kent, *A History*, p.4.

The Historiography of Covert Ethnography

As it stands at the moment, the historiography of covert ethnography in Britain is fragmented and there is no coherent account of such research from the 1880s to the late twentieth century. It would appear that the historiographical literature can be broken roughly into three parts. As Lawrence Goldman has noted, the history of social research is usually presented as the history of academic empirical studies with less institutionalised examples and trends being marginalised.[40] Accounts of the history of the social scientific disciplines of anthropology and sociology constitute the most developed, if still partial, discourse on covert ethnography. The second branch of historiography which relates to covert ethnography is the work of historians on the phenomenon of social exploration. Their accounts, however, tend to focus exclusively on Victorian and Edwardian examples of covert ethnography. There is a third perspective which focuses on the content of covert ethnographies, as well as the structure, language and imagery used by covert ethnographers, rather than the historical or disciplinary significance of the research method. This literature is particularly useful for approaching and understanding covert ethnography produced across the century in question as a unified body of work. These three strands of historiography will now be considered in turn.

Covert Ethnography and the Historiography of Academic Social Science

Commentators focusing on the history of academic social science tend to suggest that the roots of covert ethnographic endeavour are to be found in the work of pioneering early-twentieth- century British anthropologists and in the interwar research output of the Chicago School of sociology.[41] By locating the birth of covert ethnography within these two independent developments, such commentators demonstrate their lack of awareness of, or disregard for, the substantial body of covert ethnography in Britain that pre-dates these traditions. Furthermore, such accounts of the origins and genesis of ethnography, encompassing covert ethnography, fail to recognise the extra-academic use of covert ethnography which has continued throughout the twentieth and, indeed, into the twenty-first century.

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[40] Lawrence Goldman, 'A Peculiarity of the English? The Social Science Association and the Absence of Sociology in Nineteenth-Century Britain', *Past and Present*, Number 114, (1987), p.167.
[41] See, for example: James Fulcher and John Scott, *Sociology*, (Oxford: Oxford University Press, 1999), p.87; Outhwaite and Bottomore (eds.), *The Blackwell*, p.447.

Other historical accounts of the academic tradition of ethnography do recognises some isolated examples of the practice which preceded the work of the social anthropologists and the Chicago School of sociology. For example, in defining the 'social survey', G. Duncan Mitchell referred to Beatrice Webb's contribution to Booth's late-Victorian study of poverty as being based on her research undertaken as a covert 'participant observer'.[42] When the Victorian and Edwardian tradition of covert ethnography is recognised in historical accounts of the academic social sciences, it is carefully distinguished from the academic tradition of covert ethnography rooted in early twentieth-century anthropology and the Chicago School. The impression is given that there is no significant connection or parity between the two traditions. For example, Jennifer Platt suggests that

> [t]he history of the *practice* of 'participant observation' needs to be distinguished from the history of the use of the *term* and of its application in the modern sense. Observers reported on their findings from personal participation before the term emerged, some well before the development of social science as an academic field.[43]

Although she recognises the existence of participant observation's 'pre-history', she nevertheless suggests that the work of British anthropologists and Chicago School sociologists provided 'the first examples in sociology of participant observation in the modern sense'.[44] Similarly, in their discussion of the development of ethnography, Robert Miller and John Brewer suggest that

> [e]thnography has a distinguished history in the social sciences. There have been 'travellers' tales' for centuries, going back even to antiquity, which count as a form of ethnographic research in that they purported to represent some aspect of social reality on the basis of close acquaintance with and observation of it. But it begins properly only at the beginning of the twentieth century with two entirely independent intellectual developments: the classical tradition of social anthropology in Britain and the Chicago School of sociology. The former referred to its practices as ethnography and the latter as participant observation.[45]

Both Platt and Miller and Brewer recognise the long tradition of ethnography in Britain which was firmly established in the Victorian era, and yet, on the rather vague pretext that it is not sufficiently 'modern' or 'proper', both fail to assimilate it fully into their accounts. Perhaps they are reluctant to draw too close a comparison between academic ethnography and earlier works of 'social exploration', as they wish to maintain the boundaries of academic social science and protect its professional status.

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[42] Mitchell (ed.), *A New Dictionary*, p.202.
[43] Jennifer Platt, 'Participant Observation', Michael S. Lewis-Beck, Alan Bryman and Tim Futing Liao (eds.), *The Sage Encyclopaedia of Social Science Research Methods*, (London: Sage, 2004), p.798. Emphasis in original.
[44] Ibid.
[45] Robert L. Miller and John D. Brewer, *The A-Z of Social Research: A Dictionary of Key Social Science Research Concepts*, (London: Sage, 2003), p.99.

Paradoxically, even ethnography carried out by academics is often characterised as having problematic social scientific status for 'failing to meet the canons of natural science'.[46] Just as early anthropologists used 'science' as a key tool in the process of the 'disciplinization' and differentiation of academic anthropology from amateur anthropology, many other social scientists have been keen to distance themselves and their discipline from any practice which is of questionable 'scientific' status, such as ethnography, particularly covert ethnography.[47] Taking the natural sciences as a model, the researcher should not become a variable in the experiment. Of course, ethnographers could rarely, if ever, achieve this level of detachment in the field. Overt ethnographers have an obtrusive presence in the field, and even covert ethnographers exert an influence on the 'natural' situation they are trying to observe.

Furthermore, the methods of data collection used by the ethnographer are often unstructured, flexible and open-ended, and this can appear unsystematic in comparison to the model of natural science practice. Moreover, the natural science model of social research promotes the quantification of data in an attempt to describe and measure social phenomena. Ethnographers rarely quantify their data and tend to describe and assess phenomena using 'extracts of natural language' and grappling with notions of quality and meaning. To the natural scientist, the terms used by the ethnographer seem 'shifty, unreliable, elusive and ethereal'.[48] According to Easthope, participant observation was 'more akin to art than to science' and, in his account of the history of sociology in Britain, A.H. Halsey was also reluctant to consider the pioneering Victorian ethnographers a part of his disciplinary history, emphasizing instead the work of historical individuals who used quantitative methods which were more in keeping with 'science'.[49] Thus, Halsey's discussion of methodology is entitled 'The Rise of Scientific Method' and he shows a preoccupation with the development of the social survey and with statistical methods.[50] He devotes considerable attention to Rowntree, A.L. Bowley and, especially, Charles Booth as pioneers of empirical sociology in Britain.[51] Meanwhile, Friedrich Engels, Henry Mayhew, Jack London, George Orwell and Andrew Mearns, all of whom could be called social explorers, are referred to by Halsey as purveyors of 'literature', which is 'of great importance to social history if marginal to the development of the survey method'.[52] The

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[46] Miller and Brewer, *The A-Z*, pp.100-101.

[47] Kamala Visweswaran, *Fictions of Feminist Ethnography,* (London: University of Minnesota Press), p.5. The academicisation of the social sciences will be discussed in more detail in Chapters 1 and 2. See below, pp.32 & 95-97.

[48] Miller and Brewer, *The A-Z* pp.100-101.

[49] Easthope, *A History*, p.87; A.H. Halsey, *A History of Sociology in Britain: Science, Literature and Society*, (Oxford: Oxford University Press, 2004), pp.29-44.

[50] Halsey, *A History*, pp.29-44.

[51] Ibid., pp.31-44.

[52] Ibid., p.37.

point is that Halsey fails to assimilate the development of ethnography, a recognised qualitative method, into his historical account of the social scientific discipline of sociology.

So far in our exploration of the historiography of academic ethnography, we have come across two factors which limit its usefulness in promoting a full understanding of the practice of ethnography across time in Britain. First, there appears to be some reluctance among some members of the social scientific community to accord scientific status even to ethnographic research carried out by academically trained researchers, resulting in its marginalisation in some accounts. Second, it has been demonstrated that much of the ethnographic endeavour which has been undertaken in Britain since the later nineteenth century has fallen outwith the remit of the academic historiography. It may be that some commentators, unable or unwilling to venture outside the world of academic social science, have failed to notice the extent to which ethnography existed prior to and outwith its academic form.

Historians and Covert Social Exploration

Historians of social research have, according to Freeman, largely focused on the history of the quantitative social survey, with the history of qualitative social research, including covert ethnographic research, being relatively neglected by historians. A substantial historiography has developed which charts the advancement of positivistic research at the expense of more qualitative and descriptive forms of social research around the turn of the twentieth century.[53] Brian Harrison also notes that historians of social research have devoted a disproportionately large amount of their attention to 'the Booths and the Rowntrees', leaving the history of qualitative empirical work in Britain relatively unexplored.[54] As such, the history of social research tends to imply that the work of the social explorers, overt and covert, has been of less significance than that of the 'social statisticians' in terms of the development of research methods.

Nevertheless, the work of the social explorers has provided a rich source of information about, and colourful illustrations of, various aspects of life in Victorian and Edwardian Britain. According to Susan Cohen and Clive Fleay, the ethnographic texts

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[53] Mark Freeman, *Social Investigation and Rural England, 1870-1914*, (Woodbridge, Suffolk; Rochester; New York: Royal Historical Society: Boydell Press, 2003), p.3.
[54] Brian Harrison, *Peaceable Kingdom: Stability and Change in Modern Britain*, (Oxford: Oxford University Press, 1982), pp.260-261.

produced by Victorian and Edwardian social explorers give historians access to rich 'authentic and revealing' descriptions of working-class beliefs, attitudes, language and behaviour.[55] However, social historians have also shown an interest in social exploration as an historical phenomenon in itself. When social historians focus on the work of nineteenth- and twentieth-century social researchers who used qualitative methods, including ethnographic techniques, they tend to concentrate on the work of a few 'overexposed' individuals. There is, for example, an extensive literature on the life and work of Henry Mayhew, a mid-Victorian overt social explorer. [56]

Some historians, notably Keating, Freeman and Koven, have, however, shown an interest in social exploration as a historical phenomenon in itself.[57] It is noticeable, however, that most are reluctant to connect the practice of social exploration meaningfully with post-1914 British social research. Freeman explains the difficulty that social historians have in forging a connection between historical ethnographic practice and the academic ethnography which began to emerge in the twentieth century. He notes that '[i]t is easy to describe the influence of Booth and Webb on the later development of social-survey methodology; by contrast, it is much more difficult to trace a line of descent between the inquiries made by the incognito social explorers and the participant-observation studies of later years'.[58] Freeman suggests that the method of participant observation has become so transformed and academicised over time that '…it is impossible to trace direct links between the journalistic pioneers of complete participation and the studies carried out in the mid to late twentieth century'.[59] Keating and Kent draw some parallels between pre-1914 social exploration and post-1914 social research in Britain. Whereas Keating indentifies some later twentieth century non-academic covert social explorers, Kent identifies similarities in the work of social explorers and the practice of community studies in the 1950s and 60s by professional sociologists.[60] However, they stop short of developing these connections into a meaningful discussion of social exploration as a substantial and legitimate forerunner to later, more modern, forms of covert ethnography.

[55] Susan Cohen and Clive Fleay, 'Fighters for the Poor', *History Today*, Volume 50, Issue 1, (2000), p.36.

[56] See, for example; Anne Humpherys, *Travels into the Poor Man's Country: The Work of Henry Mayhew*, (Athens, Ga.: University of Georgia Press, 1977); Bertrand Taithe (ed.), *The Essential Mayhew: Representing and Communicating the Poor*, (London: Rivers Oram Press, 1996), Gertrude Himmelfarb, 'Mayhew's Poor: A Problem of Identity', *Victorian Studies*, Volume 14, 1971, pp.307-320.

[57] Keating, *Into Unknown England*; Freeman, *Social*; Freeman, ''Journeys''; Koven, *Slumming*.

[58] Freeman, ''Journeys'', p.115.

[59] Ibid.

[60] Keating, *Into Unknown England*, p.31; Kent, *A History*, pp.6-7.

The Deconstruction of Covert Ethnography

Until the final decades of the twentieth century, most of the ethnography emanating from an academic context was written from the dominant perspective of ethnographic realism. From the perspective of 'ethnographic realism', we can and should carefully distinguish between observations of objective reality and interpretations of those observations. An ethnographic realist text is 'a text that claims to represent literally the ways of life, attitudes, practices, beliefs, and so on' of the research subjects. Those who advocate a postmodern approach to ethnographic work take strong exception to the doctrine of ethnographic realism, and to ethnographic realist texts.[61] Their objections stem from the fact that there are a number of assumptions built into ethnography as it has been 'done', traditionally, from a realist perspective. These include assumptions about the 'nature of interaction' between the observer and the observed, about 'what counts as legitimate, credible, and authentic knowledge', about the activity of transforming experience into a written form, and about 'what constitutes the authority of the participant observer'.[62]

By questioning these assumptions, the postmodern critique of ethnography has grown. Postmodern critiques of ethnography essentially 'question the authority of the eyewitness participant observer to represent the lives of others'.[63] This concept of ethnographic 'authority' has been of central importance in advancing the postmodern critique of ethnography, and its inclusion in the most recent of social science reference books is conspicuous.[64] As Schwandt explains,

> criticism of a text's call to authority is at once a criticism of an ideology of the immediacy of experience and the transparency of representation. For the postmodern ethnographer…all accounts are contested, partial, incomplete, written from some particular standpoint, [and] advance some particular interest...[65]

The postmodern critique of ethnography has, significantly, drawn the attention of historians to the construction of ethnographic texts. The historian James Clifford has focused his attention on deconstructing anthropological ethnographies published since around the turn of the twentieth century, when the discipline was becoming increasingly professionalised.[66] Other historians, particularly Anthony Wohl, Carol Ann Parssinen,

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[61] Schwandt, *Qualitative*, pp.42-43.
[62] Ibid., p.111.
[63] Ibid.
[64] Schwandt includes a discussion of 'authority' in his 2001 publication which was absent from his 1997 publication: Schwandt, *Dictionary,* pp.11-12.
[65] Ibid., p.12.
[66] James Clifford, 'On Ethnographic Authority', *Representations*, No.2, (1983), pp.118-146.

Lodewijk Brunt and Deborah Epstein Nord, have begun to deconstruct the work of the social explorers.[67]

Such a critique opens up the possibility that there is no one single way to 'do' ethnography. If using social scientific rhetoric and using academic status can be identified as little or no more than legitimating strategies, or textual conventions, used by some ethnographers to give their accounts authority, then the barriers which have been erected between social scientific ethnography and the work of other ethnographers, such as the Victorian and Edwardian social explorers, begin to crumble. From a postmodern perspective, all ethnographic descriptions are partial, selective and to some extent autobiographical as they are inextricably bound to particular ethnographers and the specific circumstances under which they carried out the fieldwork, and constructed the resultant text. Even without wholeheartedly embracing the postmodern critique of ethnography, it is evident that questioning ethnographers' attempts to achieve ethnographic authority provides a platform from which to explore the changing shape of ethnography over time in Britain.

In an influential article published in 1983, Clifford traced the changing use of techniques by ethnographers to produce authoritative accounts. Although Clifford focuses on the ethnographies of social anthropologists who conducted their fieldwork abroad in the twentieth century, many of his ideas raise important questions about the practice of ethnography as a whole. He identified several different ways in which ethnographers could make their texts appear authoritative, and noticed patterns over time in terms of which strategy was the most dominant. Clifford recognises that what constitutes 'good', authoritative, believable ethnography has changed over time. With specific reference to the world of social anthropology, Clifford notes that 'if fieldwork has for a time been identified with a uniquely Western discipline and a totalizing science of 'anthropology,' these associations are not necessarily permanent. Current styles of cultural description are historically limited and undergoing important metamorphoses'.[68] Ethnographic texts can be structured in a number of ways. For example, many ethnographers use a narrative structure

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[67] See: Anthony S. Wohl, 'Social Explorations Among the London Poor: Theatre or Laboratory?', *Revue Francaise de Civilisation Britannique*, Volume 6, Part 2, (1991), pp.77-97; Carol Ann Parssinen, 'Social Explorers and Social Scientists: The Dark Continent of Victorian Ethnography', in Jay Ruby (ed.), *A Crack in the Mirror: Reflexive Perspectives in Anthropology*, (Philadelphia: University of Pennsylvania Press, 1982), pp.205-219; Lodewijk Brunt, 'The Ethnography of "Babylon": The Rhetoric of Fear and the Study of London, 1850-1914', *City and Society: Journal of the Society for Urban Anthropology*, Volume 41, (1990), pp.77-87; . Deborah Epstein Nord, 'The Social Explorer as Anthropologist: Victorian Travellers Among the Poor' in William Sharpe and Leonard Wallock (eds.), *Visions of the Modern City: Essays in History, Art and Literature*, (Columbia: Columbia University Press, 1987), pp.122-134.Although the titles of both Nord and Parssinen's articles suggest that they might consider social exploration to be the precursor in a methodological and epistemological sense to modern ethnography, they largely confine themselves to strictly textual comparisons between the two.
[68] Clifford, 'On Ethnographic Authority', pp.119-120.

which purports to communicate the story of their fieldwork from beginning to end in chronological order. Other ethnographers assimilate and organise the material they gathered in the field, and present their findings in a topically structured text. How the text is structured is an essential part of the ethnographer's quest for ethnographic authority. As Clifford explains, it is the task of ethnographers to translate their experiences into texts in such a way that communicates their authority and status as the 'purveyor of truth' about the cultural experience they have studied.[69] Clifford's concept of the significance of the structure of anthropological ethnographies can be usefully applied to examples of covert ethnography in Britain between the 1880s and 1980.

The theme of journeys and travel runs through late Victorian and Edwardian ethnography. For Keating, social exploration, in its 'purest form' as it developed in the late Victorian and Edwardian era, 'tells the story of one person's journey into an alien culture'.[70] Building on Keating's analysis, Wohl has suggested that the idea of the voyage through the slum was a powerful literary device which shaped the genre of social exploration.[71] Kent has similarly noted that researchers who used an early form of participant observation typically presented their results as 'a narrative of a journey or exploration'.[72] In fact, Parssinen argues persuasively that the narrative structure of late Victorian and Edwardian social explorers' texts differentiates them from professional academic ethnography.[73]

In narrative ethnographies, the sense of travel through time and space towards understanding is clearly the organising principle of the text. As Parssinen explains, with such narrative ethnographic texts,

> both explorer and reader can find a fit representation for their respective roles and their relationship to each other: the explorer's actual movement in time and space; his corresponding development from ignorance to knowledge; and the reader's vicarious experience of the explorer's physical and educational journeys. The logic of chronology becomes the logic of causality in a voyage of discovery…[74]

According to Parssinen, ethnography can be reduced to the metaphor of the double journey; a journey through time and a journey towards understanding. Evidently, the temporal journey refers to the chronological passing of the time which the ethnographer spends in the field. The second journey involves the ethnographer moving from a position

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[69] Ibid., p.120.
[70] Keating (ed.), *Into Unknown England*, p.13.
[71] Wohl, 'Social', p.89.
[72] Kent, *A History*, p.6.
[73] Parssinen, 'Social'.
[74] Ibid., p.206.

of ignorance to a state of understanding. Although all ethnographers experience field research in this way, the way in which they construct their ethnographic texts can either articulate or suppress this sense of a double journey.[75] In adopting a narrative structure, these late Victorian and Edwardian ethnographers were using what Clifford refers to as the experiential mode of ethnographic authority. According to Clifford, this strategy achieves ethnographic authority by implying that '"You are there, because I was there."'[76] In narrative ethnographic texts, the ethnographer's experience in the field becomes the source of authority. In ethnographic accounts where the material is presented topically, 'the journey may be suppressed altogether, and the fruits of induction wrenched from time into the ethnographic present, to become static, unarguable truths'.[77]

In deconstructing ethnography, historians such as Nord, Brunt and Parssinen focus on the language, imagery and rhetorical devices used in the construction of Victorian and Edwardian ethnographic texts. Freeman has noted that the tendency to focus on the literary quality of social exploration has distorted our understanding of the significance of such texts as valid social research.[78] This is true to some extent. If we focus in on covert ethnographic texts, picking them apart in terms of language and structure, without reference to extra-textual phenomena, we risk removing such texts from their historical context, thus losing sight of the bigger historical picture. In this way, understanding covert ethnographies as a Victorian and Edwardian literary genre does, as Freeman suggests, seem to preclude the possibility of meaningfully locating the work of the covert social explorers in the history of British social research methods.

However, the two perspectives are not mutually exclusive. Using accounts which deconstruct ethnography, and incorporating their perspectives into a historically-driven analysis, can offer new insights into the history of covert ethnography in Britain. We should understand that all ethnographic texts, from the 1880s to 1980, are constructed and can therefore be usefully deconstructed. Clifford only applies his concept of strategies for the achievement of ethnographic authority to mid-twentieth-century anthropological ethnography, but it can be applied to all examples of ethnography. Parssinen only discusses the significance of the ethnographic journey in Victorian social exploration, but this concept can also be used to understand all examples of ethnography. In this thesis, the

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[75] Ibid.
[76] Clifford, 'On Ethnographic Authority', p.118.
[77] Parssinen, 'Social', p.206.
[78] Freeman, "Journeys", pp.99-101.

deconstruction of ethnographic texts will be used as a tool to understand the nature of covert ethnography and how its use has been shaped by historical context.

A History of Covert Ethnography

In an inclusive history of covert ethnography, the common use of the research method in Britain between the 1880s and 1980 must be understood to supersede the boundaries which divide the practice of covert ethnography across space and time. As it stands, the historiography of covert ethnography has become so fragmented because a range of contributors including sociologists, anthropologists and historians, have focused on one particular manifestation of ethnography to the detriment of creating a comprehensive account of ethnography in Britain. This is not to say that the boundaries which have structured our understanding of ethnography, such as academic and non-academic, are unimportant, and a critical exploration of these boundaries will constitute an integral part of this thesis. Questioning these boundaries should help to bridge the gap between social scientific and historical accounts of ethnography in Britain, and promote a more comprehensive understanding of this form of social research.

It is important to consider the history of covert ethnography in its wider context, which means situating it within the wider framework of social research in Britain since the late-nineteenth century. A key component of contextualising the use of covert ethnography over time is recognising that its practitioners consciously chose to use this method over other available research options. The choice of covert ethnography is epistemologically significant and it tells us something about the researchers and their way of understanding the social world around them. In relation to the late Victorian and Edwardian social explorers, Freeman has noted that social researchers have historically been aware of 'methodological complexities', and their choice of one research method over the other is highly significant.[79] Freeman notes that social researchers were 'often aware of the epistemological controversies that their adoption of a particular method might provoke'. The method chosen was suggestive of the researcher's convictions concerning where accurate social knowledge was to be found.[80] In relation to more recent academic manifestations of ethnography, Schwandt has made a similar point suggesting that '[p]articipant observation is also an epistemology, a way of knowing. It is a way of gaining access to the meaning of social action through empathetic identification with those one is

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[79] Freeman, *Social*, p.183.
[80] Ibid., p.186.

observing, through witnessing how the behaviours of actors acquire meaning through their connection to linguistic or cultural systems of meanings or forms of life, or both'.[81] Not only have covert ethnographers consciously chosen to use covert ethnographic methods, but they have done so by consciously rejecting other methods.

As Harrison has explained, over time methods of social research have developed not in a linear fashion but through 'syncopation and dialogue' between social researchers located at different points in the social, intellectual and academic context.[82] Eileen Yeo has explored this notion of 'contestation' in the framework of social science. She links the idea of contestation to the space occupied by different groups of social scientists in the equilibrium of intellectual and cultural power at a given moment in time. In the 1880s, for example, Yeo suggests that a relative parity of intellectual power existed between academic and non-academic social researchers, but by the 1900s the expansion of academia meant that 'understandings and representations produced in these different sites of intellectual production carried unequal weight in the culture as a whole'.[83] Yeo was focusing on the contest between academic and non-academic social scientists but the notion of contestation can be usefully applied to the history of ethnographic and non-ethnographic research, as ethnographers have occupied a shifting space in temporally specific maps of knowledge.

The key issues which will be addressed in this history of covert ethnography in Britain will coalesce around four basic aspects of the process of 'doing' ethnography. Attention will be paid to the covert ethnographers and their identity, to the covert ethnographic fieldwork, to the process of transforming this experience into a written text and, finally, to the reception and reading of covert ethnography.

Covert Ethnographers

In the historiography of ethnography in its many forms, reference is often made to the personality or character traits needed to be a good ethnographer. For example, Kent notes that the social explorers 'must be prepared to confront and endure dangers and hardships similar to those of his more exotic anthropological counterparts'.[84] Similarly, Boissevain asserted that ethnographic fieldwork in the late twentieth century could be

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[81] Schwandt, pp.185-187.
[82] Harrison, *Peaceable Kingdom*, p.261.
[83] Eileen Janes Yeo, *The Contest for Social Science: Relations and Representations of Gender and Class*, (London: Rivers Oram Press, 1996), pp.xiv-xv.
[84] Kent, *A History*, p.37.

'deeply emotional', 'traumatic', 'lonely', 'dangerous' and full of 'hardships'.[85] Other commentators suggest that the ethnographer needs great 'skill and personality' if he or she is to produce a successful ethnographic account.[86] We will pay attention to the subjectivity of ethnographers throughout this account of the history of covert ethnography. Attention should be paid to how ethnographers define and portray themselves. They could, perhaps, think of themselves as disinterested professional academics generating knowledge for the sake of knowledge, or as crusading journalists acting passionately and heroically to uncover social problems. Whichever way, the individual ethnographer has considerable scope for shaping the way in which the fieldwork is conducted and the way in which this experience in translated into a text.

Covert Fieldwork

There are many aspects of ethnographic fieldwork which can be fruitfully explored. We will pay attention to such variables as the way in which covert ethnographers gained entrée into the research field, the amount of time spent in the field, the extent to which they were covert and the related issues of the use of disguise and the extent to which they participated in the social situation under research. Although obviously shaped by the nature of the fieldwork, exploring the relationship between the observer and the observed in the field is highly revealing. Many ethnographers clearly sympathise with their research subjects, whilst others may form strong friendships with them.

Writing Covert Ethnography

Once the covert ethnographic fieldwork has been completed, ethnographers still have the task of translating their experiences into a textual account. This textual account can take many forms, including sober academic journal articles, sensationalist newspaper accounts, gritty novels and fictionalised travelogues. There is, for example, a long history of even the most academic of anthropologists using the medium of fiction to convey their ethnographic findings to a more general audience.[87] For example, a number of American anthropologists wrote fictionalised accounts of their ethnographic fieldwork with American Indians. As discussed above, Clifford recognises the importance of the transition from experience to text in the process of ethnography and he has posed the question:

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[85] Boissevain, 'Ethnographic', pp.22-23.
[86] Abercrombie, Hill and Turner, *The Penguin*, p.256.
[87] Visweswaran, *Fictions*, pp.2-3.

> If ethnography produces cultural interpretations through intense research experiences, how is unruly experience transformed into an authoritative written account? How, precisely, is a garrulous, overdetermined, cross cultural encounter shot through with power relations and personal cross purposes circumscribed as an adequate version of a more-or-less discrete 'other world,' composed by an individual author?[88]

Clifford's answer to his own question highlights the importance of the construction of the text to the practice of ethnography:

> In analyzing this complex transformation one must bear in mind the fact that ethnography is from beginning to end enmeshed in writing. This writing includes, minimally, a translation of experience into textual form. The process is complicated by the action of multiple subjectivities and political constraints beyond the control of the writer. In response to these forces ethnographic writing enacts a specific strategy of authority.[89]

The genre, structure, format, language and imagery which covert ethnographers use to translate their covert experience in the field into a text all affect what kind of people are likely to read the account, and the weight or credibility which will be attached to the findings. To say that an academic article in a refereed journal would be taken more seriously than a sensationalist newspaper account seems to be stating the obvious, but we must question why this should be the case in relation to covert ethnography.

Reading Covert Ethnography

According to recent theory, the ability of a text to make sense in a coherent way depends not so much on the author's intentions, but on the 'creative activity of a reader', and this also applies to ethnographic texts.[90] As Clifford explains,

> [t]he writing of ethnography, an unruly, multisubjective activity, is given coherence in particular acts of reading. But there is always a variety of possible readings…readings beyond the control of any single authority. One may approach a classic ethnography seeking simply to grasp the meanings that the researcher derives from represented cultural facts. But…one may also read against the grain of the text's dominant voice, seeking out other, half-hidden authorities, reinterpreting the descriptions, texts and quotations gathered together by the writer.[91]

Clifford's point brings into focus how revealing a close reading of a text can be, and this may be particularly true with reference to ethnographic texts. Indeed, there are entire publications dedicated to exploring how to read ethnographies in a critical way.[92] Of course, the close reading of covert ethnographies constitutes the bulk of the primary research required to write an account of the history of covert ethnography in Britain. However, it is also important to take account of responses to covert ethnographic texts,

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[88] Clifford, 'On Ethnographic Authority', p.120.
[89] Ibid., p.120.
[90] Ibid., p.141.
[91] Ibid., p.141.
[92] See, for example: Paul Atkinson, *Understanding Ethnographic Texts*, (London: Sage, 1992); Martyn Hammersley, *Reading Ethnographic Research: A Critical Guide*, (London: Longman, 1998).

usually in the form of reviews, but also occasionally in the form of discourse between social researchers on related issues, such as the ethics of covert sociological research after 1945.

The Structure of this Thesis

This history of covert ethnography in Britain between c.1880 and c.1980 is split into five chapters. Chapter 1 will focus on the covert ethnography produced between c.1880 and the outbreak of the First World War. Chapter 2 will cover the period of 1914 to 1945, an important transitional phase in the history of covert ethnography. Chapters 3 and 4 will analyse covert ethnographies produced by academics in the post-war period and in the 1970s respectively. Chapter 5 will focus on the non-academic covert ethnographies which were published between the end of the Second World War and the 1970s. The advantage of such a structure is that it promotes an understanding of the way in which the changing historical context has shaped the practice of covert ethnographic research over time. However, the counterbalancing disadvantage of structuring the account in this way is that it implies that the history of such covert research is excessively linear. Despite the adoption of a chronological structure, it is envisaged that both continuity and change over time in the use of covert ethnography will be addressed.

Chapter 1: Covert Ethnography from the 1880s to 1914

Introduction

In this first chapter, covert ethnography between the 1880s and the outbreak of the First World War will be explored with particular reference to three examples of such undercover research. This analysis of covert research must be situated in the wider context of late Victorian and Edwardian social investigation, including the ascendancy of survey research, the emergence of 'new journalism' and the beginnings of the professionalisation of social science. As noted in the introduction, the historiography of late Victorian and Edwardian social investigation has been dominated by accounts of the emergence and development of social survey methodology.[1] Within the historiography of the social survey, the prominence given to the work of Seebohm Rowntree, Charles Booth and Arthur Bowley has long been recognised.[2] However, as Mark Freeman has demonstrated with reference to the Edwardian period, there were a great many other practitioners of the social survey besides Rowntree, Booth and Bowley.[3]

As a research strategy, the social survey did dominate the late Victorian and Edwardian social research scene in Britain and, as an emergent technique, it is perhaps not surprising that it also dominates historical accounts of social research in this period. However, it must be emphasised that a rich tradition of other, non-survey social research also continued to develop alongside survey methodology. For example, Ellen Ross has documented the experiences of two generations of women preceding the First World War who visited working-class urban districts in Britain, in the name of philanthropy and social investigation.[4] Amongst the most well-known of this genre of women social investigators are Lady Bell, Martha Loane, Maud Pember Reeves and Helen Bosanquet, and a substantial historiography of such social research has developed, despite the relative absence of survey methodology from their armoury.[5]

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[1] See above, p.20.

[2] Brian Harrison, 'Miss Butler's Oxford Survey', in A.H. Halsey (ed.), *Traditions of Social Policy: Essays in Honour of Violet Butler*, (Oxford: Blackwell, 1976), p.27.

[3] Mark Freeman, 'The Provincial Social Survey in Edwardian Britain', *Historical Research*, Volume 75, Number 187, (2002), p.73. It should be noted that recent historians have begun examining a broader range of social investigation. See, for example: Ross McKibbin, *The Ideologies of Class: Social Relations in Britain, 1880-1950*, (Oxford: Clarendon, 1994).

[4] Ellen Ross (ed.), *Slum Travelers: Ladies and London Poverty, 1860-1920*, (London: University of California Press, 2007), p.1.

[5] Lady Florence Bell, *At the Works: A Study of a Manufacturing Town*, (London: E. Arnold, 1907); Martha Loane, *The Queen's Poor: Life as they find it in Town and Country*, (London: E. Arnold, 1905); Maud Pember Reeves, *Round About a Pound a Week*, (London: G. Bell, 1913); Helen Bosanquet, 'Marriage in East London', in Bernard Bosanquet (ed.), *Aspects of the Social Problem*, (London: Macmillan, 1895), pp.75-81. For historiography, see, for example: Susan Cohen and Clive

Alongside and overlapping with the late Victorian philanthropic movement, the emergence of 'new journalism' in the 1880s set the stage for a generation of campaigning and politically vocal journalists to engage in social research of their own. In the wake of W.T. Stead's celebrated exposés of such sensational issues as child prostitution in the *Pall Mall Gazette*, journalists became increasingly involved in tacking the prominent social issues of the period and this led some to undertake their own social research, sometimes using covert ethnographic methods.[6] Mary Higgs, although not a journalist, was a friend of Stead's and she used covert research methods extensively to study women's vagrancy.[7] Higgs' covert ethnographic research will be explored in detail in this chapter, as will the covert research of journalist Howard Goldsmid, who can quite clearly be located in this tradition of campaigning journalism, although there is no evidence to suggest he was associated directly with Stead.[8]

At the beginning of the twentieth century, British social science was slowly becoming academicised and institutionalised. In 1895, the London School of Economics (LSE) was founded with the aim of improving society through the study of poverty and inequality. Beatrice Webb, one of the founding members of the LSE, undertook covert research into the tailoring trade, and, alongside the work of Higgs and Goldsmid, her incognito research will be analysed closely in this chapter.[9] In 1903, the Sociological Society was founded by a group of historians, philosophers, biologists, journalists, politicians, clergymen, town planners, geographers and businessmen. In 1907 the Department of Sociology was established at the LSE, and the Department of Social Science and Administration followed in 1912.[10] In his account of the history of empirical sociology in Britain, Raymond Kent suggests that, regardless of these institutional developments in the field, there was no significant improvement in the output or quality of social research produced prior to the First World War.[11] Nevertheless, these developments

Fleay, 'Fighters for the Poor', *History Today,* Volume 50, Issue 1, (2000), pp.36-37; F.K. Prochaska, *Women and Philanthropy in Nineteenth-Century England*, (Oxford: Clarendon Press, 1980); Martha Vincinus, *Independent Women: Work and Community for Single Women: 1850-1920*, (London: Virago, 1985).

[6] For example, J.R. Widdup, the editor of *The Socialist and North-East Lancashire Labour News*, spent a night disguised as a tramp in the casual ward of Burnley workhouse and wrote about this experience in his newspaper. Mark Freeman and Gillian Nelson (eds.), *Vicarious Vagrants: Incognito Social Explorers and the Homeless in England, 1860 – 1910*, (Lambertville, New Jersey: The True Bill Press, 2008), p.31. His account was later published as a pamphlet: J.R. Widdup, *The Casual Ward System: Its Horrors and Atrocities. Being an Account of a Night in the Burnley Casual Ward, Disguised as a Tramp*, (Manchester: The Labour Press Society, 1894). Reproduced in Freeman and Nelson (eds.), *Vicarious*, pp.189-205. Stead's account of child prostitution has been reprinted: Anthony Simpson (ed.), *The Maiden Tribute of Modern Babylon: The Report of the Secret Commission*, (Lambertville, New Jersey: The True Bill Press, 2007).

[7] Rosemary Chadwick, 'Higgs, Mary Ann (1854–1937)', *Oxford Dictionary of National Biography*, Oxford University Press, (2004), http://www.oxforddnb.com/view/article/38523 , [accessed 25th March 2010].

[8] Howard Goldsmid, *Dottings of a Dosser, Being Revelations of the Inner Life of Low London Lodging-Houses*, (London: T. Fisher Unwin, 1886).

[9] LSE History, http://www2.lse.ac.uk/aboutLSE/lseHistory.aspx , [accessed 20th March 2010].

[10] Raymond Kent, *A History of British Empirical Sociology*, (Aldershot: Gower, 1981), pp.89 & 97-98.

[11] Kent, *A History*, p.99.

form an important backdrop to the analysis of late Victorian and Edwardian covert ethnography.

As indicated above, this chapter will focus on the work of three covert ethnographers: Goldsmid, Webb and Higgs. However, it is important to remember that there are many examples of late Victorian and Edwardian covert social exploration, some of which have been relatively fully explored by historians, such as James Greenwood's account of 'A Night in a Workhouse' and Jack London's *The People of the Abyss*.[12] Olive Malvery, social commentator, novelist, journalist and friend of Stead's, produced the most commercially popular work of Edwardian covert social exploration. However, although the historian Judith Walkowitz has written about Malvery's photojournalism, her use of covert research methods remains relatively unexplored.[13] There are, however, other covert social researchers from this era, such as George Zachary Edwards and Christopher Holdenby, whose lives and work remain largely untouched by historical analysis.[14]

Goldsmid's late Victorian covert exploration of common lodging houses has been selected for detailed study because of its relatively 'untouched' status as a historical source. Webb's covert ethnographic research is of particular significance given her subsequent career as a prominent social reformer and social scientist. Higgs has been selected because she immersed herself time and again in the world of vagrancy, leaving behind a rich collection of ethnographic work. Each of these researchers and their work will be described in more detail below before an analysis of the structure and language used in their accounts is undertaken. Subsequently, discussion will focus on three issues: the contested definition of covert ethnography as literature or social science, the significance of reformism in covert research and the relationship between covert research and the wider research community.

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[12] James Greenwood, 'A Night in a Workhouse' in Peter Keating (ed.), *Into Unknown England, 1866-1913: Selections from the Social Explorers*, (Glasgow: Fontana, 1976), pp.33-54. For historical analysis of Greenwood's work see, for example: Seth Koven, *Slumming: Sexual and Social Politics in Victorian London*, (Oxford: Princeton University Press, 2004), pp.25-87; B.I. Diamond, J.O. Baylen and J.P. Baylen, 'James Greenwood's London: A Precursor of Charles Booth', *Victorian Periodicals Review*, Volume 17, (1984), pp.34-43. Jack London, *The People of the Abyss*, (London: Journeyman Press, 1977). For historical analysis of London's work see, for example: Joan Hedrick, *Solitary Comrade: Jack London and his Work*, (Chapel Hill: University of North Carolina Press, 1982); Alex Kershaw, *Jack London: A Life*, (London: Flamingo, 1998).

[13] Cohen and Fleay, 'Fighters', p.37; Mark Freeman, '"Journeys" into Poverty Kingdom: Complete Participation and the British Vagrant, 1866 - 1914', *History Workshop Journal*, Volume 52, (2001), pp.105-106; Olive Malvery, *The Soul Market: with which is Included "The Heart of Things"*, London: Hutchinson, 1906).; Judith Walkowitz, 'The Indian Woman, the Flower Seller and the Jew: Photojournalism in Edwardian London', *Victorian Studies,* Volume 42, (1998), pp.3-46.

[14] George Zachary Edwards, *A Vicar as Vagrant*, (London: P.S. King & Son, 1910). Reprinted in Freeman and Nelson (eds.), *Vicarious*, pp.279-314; [Ronald George Hatton] Christopher Holdenby, *Folk of the Furrow*, (London: Smith, Elder & Co., 1913).

Late Victorian and Edwardian Covert Ethnography

Howard Goldsmid and London Lodging Houses

In 1886, Howard Goldsmid wrote *Dottings of a Dosser, Being Revelations of the Inner Life of Low London Lodging-Houses* based largely on his covert sojourns in a number of common lodging houses in the metropolis.[15] As a result of his work 'in connection with one of those grand East-end institutions which undertake the rescue of destitute gutter-children', Goldsmid came into contact with boys of all ages who frequented the common lodging-houses of London and invariably spoke about them in terms of horror and disgust.[16] Goldsmid's curiosity about lodging-houses grew out of his philanthropic work and he determined

> to see the 'kip-ouses' from within as well as from without; to learn from experience as well as from rumour the sort of accommodation with which our poorest brethren are compelled to be content, and to know from personal investigation who the 'dossers' are, and what is their lot in life.[17]

Goldsmid was keenly aware of the effect that social exploration, in this case, overt social exploration, could have on the reading public. In his opening chapter, he referred to the revelations of Andrew Mearns' 'The Bitter Cry of Outcast London' and George Sims' 'Horrible London' and 'How the Poor Live', which had appeared in the press comparatively recently. Goldsmid recognised that the work of Mearns and Sims had temporarily seized the public's imagination and pulled at their heartstrings. Their work encouraged a brief flurry of further inquiry as 'slumming' became popular, and, 'with the appointment of a Royal Commission', 'the public conscience was salved'. However, interest quickly subsided and sympathy dissipated. Goldsmid wondered if the public would remain unmoved until they were confronted with scenes of even greater horror.[18] Despite his reservations about the superficiality and temporary nature of the public's response to the work of Mearns and Sims, Goldsmid dedicated his book 'to the public which feels, the public which reflects'. He hoped that the public would react to the plight of the dossers in the same way they had reacted to Mearns' and Sims' accounts of slum housing, and push for legislative change.[19]

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[15] In the same year, Goldsmid published a covert account of lodging houses on Bath's Avon Street in the *Bath Daily Chronicle*. '"Girls of the Town" Give Avon Street a Bad Name', http://www.thisisbath.co.uk/news/Girls-Town-Avon-Street-bad/article-469483-detail/article.html , [accessed 24th March 2010].
[16] Goldsmid, *Dottings*, pp.8-9.
[17] Ibid.
[18] Ibid., p.7.
[19] Ibid., p.3.

Little is known about Goldsmid and the only source of biographical information about him appears to be an autobiography of his daughter, Gwen Cash, and, given her focus on her career rather than her early life, even this source gives us little information. Goldsmid was born into a Jewish family in Birmingham where his father was a diamond merchant and president of the local synagogue. Despite his father's hope that he would become a rabbi, Goldsmid became a journalist and distressed his family further when he fell in love with a Christian and eloped.[20] Goldsmid was leader of the socialist-leaning Young Liberals and, backed by the chocolate manufacturer and philanthropist George Cadbury, he established the weekly *Arrow* in Redditch to improve the working conditions of local needle-makers.[21] Goldsmid appears to have died at some point between 1891 (the year Cash was born) and 1917, when she emigrated with her mother to British Columbia; she described her father as 'long-dead' at this time.[22] Returning to London in the mid-twentieth century, Cash was disappointed to be unable to find her father's book, as his publisher's office had been 'bombed and all the documents blown to smithereens' during the war.[23] Cash wrote that '[a]s a record of Daddy's experience among England's down-and-outs…those "dottings" would have been valuable to historians as well as endearing to me'.[24]

Goldsmid's account of common lodging houses in late Victorian London is a rich source of information for historians, and *Dottings* is a particularly engaging and darkly humorous example of covert ethnography. Goldsmid spent many uncomfortable nights in common lodging houses across the capital city, including the 'Beehive' on Brick Lane, which Goldsmid found to be full of bugs, but not one bee, and the 'Little Wonder' on Flower and Dean Street, which was particularly notable for the coarse language and moral laxity of the women residents he encountered.[25] It is striking that echoes of Goldsmid's account can be found in other covert ethnographic accounts of homeless accommodation published decades later. In the 'Beehive', Goldsmid described the narrow beds, tightly packed together, with dirty sheets. Due to a combination of the bugs, the foul stench of alcoholic breath and the stumbling, brawling and snoring of his companions, he did not sleep well and, feeling nauseous, crept out early.[26] Writing in 1973, Robin Page described

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[20] Gwen Cash, *Off the Record: The Personal Reminiscences of Canada's First Woman Journalist*, (British Columbia: Stagecoach, 1977), p.98.
[21] Cash, *Off the Record*, p.163.
[22] Ibid., p.12.
[23] Ibid., pp.102 & 163.
[24] Ibid., p.102.
[25] Goldsmid, *Dottings*, pp.12-25 & 78-89.
[26] Ibid., pp.24-25.

a remarkably similar experience in a Salvation Army hostel near Liverpool Street station in London. In the crowded dormitory, the beds were only two feet apart from each other and Page watched as one man found a giant louse in the seam of his pillow. Lying in an 'uncomfortable supine posture' in an attempt to avoid 'hordes of TB germs invading from one side and a column of marching lice advancing from the other', Page got little sleep as a result of his fellow residents 'passing wind', shouting and fighting, and he left hurriedly at 6a.m.[27] In the last chapter, these similarities between covert accounts of homelessness across time will be addressed in more detail.[28]

Beatrice Webb and Sweated Labour in the Tailoring Trade[29]

Harshly dismissed by her mother as being the only one of her ten children to be of below-average intelligence, Webb is now remembered as one of 'the most prominent and productive pioneers of social science in Britain'.[30] Webb engaged with the issue of poverty and the working classes in a variety of ways, through philanthropy and research. As a 'visitor' for the Charity Organisation Society, Webb investigated claims for financial assistance made by residents of Soho and worked as a rent collector at the Katharine Buildings. Investigating the conditions amongst dock workers and in the garment industry, Webb contributed to her cousin, Charles Booth's, poverty survey, *Life and Labour of the People of London.*[31] As an expert on poverty and industrial working conditions, Webb gave evidence to the House of Lords Committee on the Sweating System in 1888 and 1889 and, as a 'skilled administrator and forceful exponent of her causes', Webb was responsible for the influential Minority Report of the Royal Commission on the Poor Laws in the first decade of the twentieth century.[32] A great deal has been written about Webb and her work but relatively little attention has been paid to her covert ethnography.[33]

Based on the research she conducted for Booth's survey, Webb had two articles published in the *Nineteenth Century* periodical in 1888. The first article, 'East London Labour', was a sober and lengthy inquiry largely based on quantitative data about the structure of the tailoring trade in London and the role of London's Jewish people in this

[27] Robin Page, *Down among the Dossers*, (London: Davis-Poynter, 1973), pp.64, 67 & 68-69.
[28] See below, pp.240-243.
[29] Beatrice Potter did not marry Sidney Webb until 1892, four years after she had her ethnographic research published, but for the purposes of consistency, she will be referred to as Beatrice Webb or Webb throughout this thesis.
[30] John Davis, 'Webb, (Martha) Beatrice (1858–1943)', *Oxford Dictionary of National Biography*, Oxford University Press, (2008), http://www.oxforddnb.com/view/article/36799 , [accessed 25th March 2010].
[31] Charles Booth, *Life and Labour of the People in London*, 10 volumes, (London: Macmillan, 1892-1897).
[32] Ross, *Slum*, pp.262-264.
[33] See, for example: Deborah Epstein Nord, *The Apprenticeship of Beatrice Webb*, (Amherst: University of Massachusetts Press, 1985); Carole Seymour-Jones, *Beatrice Webb: Woman of Conflict*, (New York: HarperCollins, 1993).

trade.[34] The second article was based on an episode of covert research Webb conducted into the tailoring trade. Whilst lodging in the East End of London, Webb worked incognito as a trouser-hand for a Jewish-owned firm in the East End of London.[35] This second article, 'Pages from a Work-Girl's Diary', was reproduced in 1898 under the title 'The Diary of an Investigator' in Beatrice and Sidney Webb's *Problems in Modern Industry*.[36] However, despite being reproduced by the Webbs, there is considerable evidence to suggest that she regarded her covert ethnography with regret and this will be discussed in more detail later in this chapter. In 'Pages', Webb described three days spent as an incognito trouser hand. On the first day, she searched for and secured employment in a sweat shop. The second day was spent in employment, and the text ends when Webb finished work at the end of the third day.[37] In 'Pages', Webb described how she struggled with her incognito and with the tailoring work too, being demoted to the less demanding tasks usually reserved for outdoor hands.[38]

In fact, the text of 'Pages' conceals the amount of experience that Webb actually obtained of the sweating system in the manufacture of cheap clothing. Whilst Webb referred to 'Pages' in her autobiography as 'little more than a transcript of … [her personal]…diary, with the facts just enough disguised to avoid recognition and possible actions for libel; and experiences sufficiently expurgated to be "suited to a female pen"!' she also referred to it as a 'dramatized version of but a few of the facts'.[39] This second assessment would appear to be closer to the truth. Between the end of 1887 and the beginning of 1888, Webb undertook extensive research into the sweating system using a number of methods. She interviewed employers and employees, visited home workers, accompanied collectors who gathered payments for sewing machines door-to-door, and trained as a trouser-hand, in preparation for her personal investigation.[40] Furthermore, according to her autobiography, Webb undertook far more than the three days' covert research as a trouser-hand that her 'Pages' article suggested. In *My Apprenticeship*, Webb recalled that she undertook her covert fieldwork in the spring of 1888 by

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[34] Beatrice Potter, 'East London Labour', *Nineteenth Century*, Volume 24, Number 137, (1888), pp.161-183.
[35] Beatrice Potter, 'Pages from a Work-Girl's Diary' *Nineteenth Century*, Volume 24, Number 139, (1888), pp.301-314.
[36] Beatrice Webb, 'The Diary of an Investigator', in Beatrice Webb and Sidney Webb, *Problems of Modern Industry*, (London: 1898), pp.1-19. 'Pages' has also been reproduced in Ellen Ross' collection of Victorian and Edwardian women's research into London poverty: Beatrice (Potter) Webb, 'Pages from a Work-Girl's Diary', Ellen Ross (ed.), *Slum Travelers: Ladies and London Poverty, 1860-1920*, (London: University of California Press, 2007), pp.266-279.
[37] For the first day see: Potter, 'Pages', pp. 301-303, the second day: pp.303-310 and the third day: pp.310-314.
[38] Potter, 'Pages', pp.302, 305, 307, 309 & 310.
[39] Beatrice Webb, *My Apprenticeship*, (Harmondsworth: Penguin, 1971), pp.323-324 & 325.
[40] Webb, *My Apprenticeship*, pp.315 & 322.

> getting employment as a 'plain trouser-hand' in several workshops, being soon dismissed from the first, but voluntarily leaving the last and (from the standpoint of the worker) lowest of the lot, in order 'to better myself', when I had secured all the information I required.[41]

Rather than spending one day looking for work and two days as a trouser-hand for one employer as we are told in 'Pages', Webb had extensive knowledge and first-hand experience, both covert and overt, of sweated industry.

Mary Higgs and Women's Vagrancy

Born in 1854, the eldest daughter of a Congregationalist minister, Higgs was the first woman to take the natural science tripos at Cambridge. In 1879, Higgs married a Congregationalist minister and started a family before settling in Oldham, north of Manchester, in the early 1890s. Receiving an OBE in 1937 for her services to Oldham, Higgs dedicated time and effort to improving her local community, establishing the Beautiful Oldham Society in 1902, as well as writing columns for local newspapers.[42] In addition to her church-related work and religious writing, Higgs maintained a friendship with Stead, the pioneering 'new journalist' of the 1880s, who encouraged her interest in science, specifically the emerging field of psychology.[43] She also wrote and lectured on prominent social questions of the day, even arranging annual summer schools for the study of these problems attended by settlement workers and other philanthropists. An advocate of reform in a number of areas, most prominently vagrancy, Higgs also engaged in philanthropic enterprise. She initiated employment schemes for destitute women, pioneered mother and infant welfare centres and provided emergency accommodation and lodging-houses for the women of Oldham.[44]

Between 1903 and 1905, Higgs published no fewer than four ethnographic texts, all relating to women's vagrancy. In 1904, in association with the Women Guardians and Local Government Association of Manchester, and prefaced by the Reverend Canon Hicks, Higgs anonymously published *Five Days and Five Nights as a Tramp among Tramps*, which went through no fewer than ten editions.[45] Also in 1904, under the pseudonym 'Viatrix', her article 'The Tramp Ward' was published in the *Contemporary Review*.[46]   In 1905, she undertook a further two covert ethnographic projects publishing 'A Night in a Salvation Army Shelter' and 'Three Nights in Women's Lodging-Houses' on

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[41] Webb, *My Apprenticeship*, pp.323-324.
[42] Chadwick, 'Mary Higgs', pp.1 & 3.
[43] Ibid., p.1.
[44] Ibid., p.2.
[45] [Mary Higgs] Social Investigation by a Lady, *Five Days and Five Nights as a Tramp among Tramps*, (Manchester: Women's Guardians and Local Government Association, 1904).
[46] [Mary Higgs] Viatrix, 'The Tramp Ward', *Contemporary Review*, Volume 85, (1904), pp.649-667.

the basis of these experiences.[47] In 1906, Higgs published *Glimpses into the Abyss* in which she reprinted the four titles mentioned above, as well as a prize-winning essay that she had written on vagrancy, and other chapters based on yet more ethnographic sojourns in Manchester, a northern town and London.[48] As Deborah Epstein Nord has suggested, Higgs seems to have been one of the few women, along with Malvery, who used covert social exploration on a sustained basis, and the sheer volume of her ethnographic output warrants her prime position in any discussion of Victorian and Edwardian covert social exploration.[49]

Higgs' *Glimpses into the Abyss* has been referred to, on more than one occasion, as a clear precursor of George Orwell's similar but much better known account of casual work and vagrancy, *Down and Out in Paris and London*, published in the 1930s.[50] Despite the fact that Higgs was held in high regard during the Edwardian era and considered something of an authority on vagrancy as a result of her ethnographic and philanthropic experiences in this field, her work and achievements are largely forgotten today.[51] Higgs' relative invisibility in history is compounded by the lack of recognition of her ethnographic work. For example, writing in the early 1980s, when he was a sociology professor at Washington University, Murray Wax wrote a short article about how he taught research methods to advanced undergraduate students of anthropology and sociology.[52] The core texts he used included the work of Hortense Powdermaker and William Foote Whyte, who between them had conducted extensive ethnographic fieldwork, both covert and overt, in their native America and in foreign lands.[53] Whilst Wax provided the students with practical guidance on ethnographic work, such as how to take fieldnotes and organise them, he also developed as series of lectures about 'the pioneers of fieldwork'.[54]

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[47] Mary Higgs, *A Night in a Salvation Army Shelter*, (Oldham: 1905); Mary Higgs, *Three Nights in Women's Lodging Houses*, (Oldham: 1905).

[48] Mary Higgs, *Glimpses into the Abyss*, (London: P.S. King and Son, 1906). In *Glimpses*, 'The Tramp Ward' is called 'A Northern Tramp Ward', pp.136-174. The essay is the first chapter of *Glimpses*, 'Vagrancy: An Essay Awarded Girton Gamble Prize, 1906', pp.1-86. The other ethnographic episodes are presented in chapters 6 and 7, pp.232-268. The second section of chapter 7 about a London casual ward was reprinted from the *Daily News* in April 1905, p.259.

[49] Deborah Epstein Nord, *Walking the Victorian Streets: Women, Representation and the City*, (London: Cornell University Press, 1995), p.14.

[50] Ibid., Chadwick, 'Mary Higgs', p.2.

[51] Cohen and Fleay, 'Fighters', p.37; As well as producing practical advice on the provision of lodging-houses, she also gave evidence to relevant inquiries including the Departmental Committee on Vagrancy in 1906 and the Departmental Committee on the Relief of the Casual Poor in 1930. Chadwick, 'Mary Higgs', p.2.

[52] Murray L. Wax, 'Apprenticing a Career', *Anthropology and Education Quarterly*, Volume 14, Number 3, (1983), pp.184-187.

[53] Wax, 'Apprenticing', p.185. Hortense Powdermaker, *After Freedom: A Cultural Study in the Deep South*, (New York: Viking Press, 1939); William Foote Whyte, *Street Corner Society: The Social Structure of an Italian Slum*, (Chicago: University of Chicago Press, 1943).

[54] Wax, 'Apprenticing', p.185.

Surprisingly, Wax presented Webb amongst such ethnographic pioneers, but not Higgs.[55] This may be simply because Webb wrote 'Pages from a Work-Girl's Diary' in 1888, fifteen years before Higgs commenced her first stint of ethnographic fieldwork, thus making Webb more 'pioneering' in terms of chronology. However, it could also be significant that Webb was associated throughout her research career with Booth, regarded as a pioneer of empirical sociology in Britain in his own right, and went on to have a subsequent high profile career (along with her husband, Sidney) in the social sciences. Perhaps Webb's use of covert ethnography seemed so significant to Wax because of her subsequent illustrious career in the social sciences, casting retrospective legitimacy on her earlier and isolated use of covert ethnography. The point is that Higgs' ethnographic output was more substantial, complex and widely read than that of Webb, and many other Victorian and Edwardian covert social explorers, and it deserves to be recognised as such.

The Structure of Covert Ethnography from the 1880s to 1914

In terms of structure, Webb's 'Pages from a Work-Girl's Diary' most closely conforms to the narrative form usually associated with late-Victorian and Edwardian ethnography.[56] Appearing as a short article in a periodical, Webb constructed 'Pages' as a narrative account of three days spent undercover as a trouser-hand. It is only in passing that we learn that the narrator was 'an investigator' and that she was disguised as a trouser-hand. Her text constitutes a flowing narrative, not even being broken into sections according to days, as you would expect in a diary. No preface or introductory paragraph was included to set out the premises of the project and neither are any conclusions offered, thus the text presents an uninterrupted narrative. Webb made no claims to special knowledge in 'Pages' and there is little sense of her developing experiential authority of issues relating to sweating in the manufacture of cheap clothes.

This may be because Webb never intended 'Pages' to be a particularly effective and convincing piece of social research. Rather, having recently had her first article published and feeling like a 'recognized social investigator' Webb had decided to produce something more literary on the sweating system.[57] In her own diary, Webb wrote 'I have it in mind to make it more of a picture than the article on Dock Labour, to dramatize it. I cannot get this picture without living among the actual workers. This I think I can do'.[58]

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[55] Ibid., p.187.
[56] See above, pp.22-26.
[57] Webb, *My Apprenticeship*, pp.310 & 313-314.
[58] Ibid., p.313.

As suggested earlier, Webb reorganised, condensed and 'dramatised' her actual experiences as a covert trouser-hand, perhaps in order to produce a concise and satisfying story.

The use of a narrative structure would arguably go hand in hand with the establishment of experiential authority: the longer the ethnographer spends in the field, the more experience they gain and thus they have more knowledge and their texts appear more authoritative. Webb appears to have severed this link between experience and narrative. For the sake of producing a convincing narrative, she in fact suppressed the extent of her covert experience. The fact that Webb rearranged, and may even have fictionalised, aspects of her narrative does not necessarily detract from the value of her account. Topically arranged ethnographies involve extensive reorganisation of material observed in the field. What is important about the translation of Webb's field experience into text is that she essentially underrepresented the amount of time she spent the field and the experience she had. Whilst 'Pages' reflects the tendency for late Victorian ethnographers to use a narrative structure, it suggests that the attainment of experiential authority was not always a priority. However, Webb's ethnography may be an exception to the general rule as the work of both Goldsmid and Higgs shows a clear tendency towards using experience as an indicator of authority.

In 1886, just two years before Webb published 'Pages', Goldsmid published *Dottings of a Dosser,* in which he determined 'to learn from experience' and 'from personal investigation' about the dosser's way of life, clearly seeing experience as the route to knowledge. He implied that there was a clear link between setting forth his experiences and truthfulness:

> In the following pages I have endeavoured …to set forth my experiences in the common lodging-houses, and the conclusions I deduce from them. The sketches there depicted may be ill-drawn, but they are not exaggerated, and I have stated nothing which has not come under my own observation. In every case I have given chapter and verse for what I have written.[59]

In turn, truthfulness was associated with a narrative account of experience when Goldsmid expressed the idea that if he 'faithfully narrated without extenuation or exaggeration' the things he had seen and heard in the common lodging houses, letting the people he encountered 'speak for themselves', then his research may achieve its purpose and direct public attention to the state of metropolitan lodging houses.[60] The notion of ethnography as a double journey, developed by Carol Ann Parssinen and discussed in the introduction, is

[59] Goldsmid, *Dottings*, pp.8-9.
[60] Ibid., pp.26-27.

clearly embedded in *Dottings*.[61] Goldsmid understood his fieldwork as a journey or as 'a pilgrimage' and, furthermore, a journey which the reader could share vicariously through reading the text.[62] At the end of the first introductory chapter, Goldsmid wrote:

> Are you prepared, reader, to meet such company? If so, come with me round some of the places I have visited. You will have the advantage that, while my tour was made in the flesh, yours may be completed in the spirit. And much is to be learned from such an expedition, even if made only in the imagination, by those who have but very dimly realized the fact that there are dens of misery unutterable, and of vice indescribable, in some quarters of this wealth-teeming, yet poverty-producing, metropolis.[63]

The sense of journey was palpable in Goldsmid's text as he entreated his readers to 'come with [him]' as he completed a 'tour' or 'expedition' around the doss houses of London.

In contrast to Webb, Goldsmid saw the use of a narrative structure to convey experience as a way of establishing the truthfulness and authority of his account, not simply a way of generating a good story. Given that Goldsmid's *Dottings* is a sizeable publication when compared side by side with Webb's short ethnographic article, it is perhaps not surprising that it has a much more complex structure. Unlike Webb, Goldsmid paid considerable attention to establishing the purpose of his project and even situating himself in the longer tradition of social exploration to some extent.[64] These aspects of Goldsmid's work will be discussed in more depth elsewhere. The point to take note of here is the contrast between *Dottings* and 'Pages' in terms of their structure. Comprising thirteen chapters and a brief dedication, *Dottings* included six chapters which were narrative accounts, each relating Goldsmid's experience of spending the night in a different common lodging house.[65] Thus, *Dottings* does draw on the use of experience, placing fieldwork and not analysis at the foreground of the text, as a means of achieving experiential authority, the form of ethnographic authority most readily associated with early ethnographies.[66]

However, the fact that Goldsmid presented no fewer than six discrete narrative accounts suggests that Goldsmid's ethnographic text has more than just experiential authority. One of the criticisms most readily made of social explorers and their research is that it was typically based on brief and isolated experiences. For example, J.R. Widdup spent just one night in the casual ward of one workhouse and based his ethnographic

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[61] Carol Ann Parssinen, 'Social Explorers and Social Scientists: The Dark Continent of Victorian Ethnography', in Jay Ruby (ed.), *A Crack in the Mirror: Reflexive Perspectives in Anthropology*, (Philadelphia: University of Pennsylvania Press, 1982), p.206; See above, pp.24-25.
[62] Goldsmid, *Dottings*, pp.90 & 99.
[63] Ibid., p.11.
[64] See 'Dedication' and chapters 1, 11, 12 & 13: Ibid., pp.3, 7-11, 108-119, 120-131 & 132-137.
[65] See chapters 2,3,5,6,8 & 9: Ibid., pp.12-25, 26-37, 52-62, 63-71, 78-89 & 90-98.
[66] See above, pp.22-26.

account on this one-off experience.[67] The results of such research could be challenged on the basis that the institution investigated may have been atypical and therefore the findings would not be applicable to other, similar institutions. Goldsmid seemed to recognise this problem and his repetition of narrative accounts goes some way to counteract this criticism. Far from coming to conclusions based on isolated experiences, Goldsmid made reference to the same touchstones in each institution in each of his six narratives. He usually commented on the cleanliness of the kitchen and the appearance of his fellow lodgers, as well as what they ate, said and did. He would then describe the bedroom, noting how many beds there were, and how many beds the regulations allowed, as well as the cleanliness of the beds.[68] This repetition allowed Goldsmid and his readers to compare and contrast the different institutions and, through cumulative experience, to make generalisations about the state of common lodging houses. Goldsmid claimed to have visited both the best and worst of London's doss houses because '[i]t would be grossly unfair, and it would rather prejudice my case than otherwise, were I, in describing the deplorable condition of the common lodging-houses, to show only one side of the medal'.[69]

In his tenth chapter, after six chapters had been devoted to separate narrative accounts of nights spent in different common lodging houses, Goldsmid summarised his experience of visiting yet more London lodging houses in the course of his investigation. Goldsmid implied that his investigations came to a logical conclusion because his continued visits to common lodging houses were no longer adding to his knowledge of the topic. When Goldsmid wrote that he 'might extend these descriptions *ad infinitum*…but it would only be to prolong a nauseating record of filth, stench, and abomination', he was demonstrating the authority and veracity of his results through repetition and replication of his findings in lodging house after lodging house which he had visited.[70] In a sense, Goldsmid's structural strategy of presenting numerous narratives of experience reflects the legitimating assumption normally associated with more quantitative methodologies, such as the survey method: that authority and reliability were to be found in numbers, the replication of results and the capacity to generalise findings.

Besides his six narrative chapters, Goldsmid included three chapters in *Dottings* which did not conform to the narrative pattern usually associated with late Victorian social

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[67] Widdup, 'The Casual'. Widdup's work was similar in this sense to the much better known account written by James Greenwood in the 1860s: Greenwood, 'A Night', pp.33-54.
[68] Goldsmid, *Dottings*, Chapters 2, 3, 5, 6, 8 and 9.
[69] Ibid., p.63.
[70] Ibid., p.106.

explorers. In addition to the chapter mentioned above in which Goldsmid summarised his experiences of doss houses not featured in the six narratives, he included chapters entitled 'Kip-'ouse kids' and 'No doss money', neither of which were based directly on any specific episode of ethnographic research, and were consequently not written in a narrative way. 'Kip-'ouse kids' was constructed by Goldsmid based on his cumulative covert experience of lodging houses where he observed children, and through his philanthropic work with boys and male youths in the east end of the city.[71] Those with 'no doss money' had to sleep out on the streets of London and, though he dedicated a chapter to the plight of these rough sleepers, Goldsmid did not spend a night on the streets himself. Instead, he presented anecdotes related to him by women, sometimes with children to support, who had taken to sleeping out.[72] The significance of these chapters lies in demonstrating that while late Victorian ethnographic texts were indeed dominated by a narrative structure, they were not confined to such a structure. Social researchers such as Goldsmid were capable of using strategies of textual authority other than the experiential authority conferred by the presentation of a convincing narrative. Goldsmid strove to convince his readers, not just by telling a story about this experience, but by demonstrating the extent and exhaustiveness of his experience as well as drawing on his knowledge gained from working on a regular basis with members of a social group who frequented the institutions under investigation.

Higgs' account presents us with a similarly structurally complex example of Edwardian ethnography, where the use of narrative and experiential authority are far from straightforward. Given the volume of ethnographic work produced by Higgs, it would be impossible to give more than a cursory account of the structure of each individual pamphlet and that of her main monograph, *Glimpses into the Abyss*. Here, we will therefore focus on the structure of *Glimpses* as an ethnographic text where Higgs reprinted her previous ethnographic accounts in full.[73] As with Goldsmid's *Dottings*, the sense of a double journey through time and towards understanding can be found in *Glimpses*. Higgs usually had a female companion who accompanied her on her covert visits to lodging houses and workhouse casual wards, and, in the preface to one of her texts, the Reverend

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[71] Ibid., pp.39-51.
[72] Ibid., pp.72-75.
[73] As has been mentioned already, *Five Days,* 'The Tramp Ward', *A Night* and *Three Nights* were all published individually before they were included in Higgs' 1906 monograph *Glimpses*. In *Glimpses*, the previously published texts are included in full although they have been subject to reorganisation. For example, the preface to *Five Days* is included as an appendix in *Glimpses* as is the preface to *Three Nights*. See Higgs, *Five Days*, p.3; Higgs, *Glimpses*, pp.317-318; Higgs, *Three Nights*, pp.2-4; Higgs, *Glimpses*, pp.319-323.

Canon Edward Lee Hicks referred to Higgs and her friend as 'the two pilgrims'.[74] Higgs also referred to their covert ethnographic fieldwork as their 'pilgrimage' and 'our research expedition'.[75] Of course, it is not surprising that Higgs' text was imbued with a sense of travel, given that she and her friend often stayed in their covert role for days at a time. At one point, Higgs and her companion spent five consecutive days tramping between lodging houses and causal wards.[76]

However, this sense of journey is somewhat complicated by two factors. First, the idea of a physical journey across space is challenged by the competing notion of downward journey. Higgs' ethnographic research was described as a 'descent into the Inferno' and the title itself, *Glimpses into the Abyss* has connotations of a downward journey.[77] Higgs wrote about her research in terms of a 'descent from respectability' and as having 'dropped out of civilisation'.[78] Second, Higgs' use of travel metaphor is peppered with irony rather than being used for strictly narrative purposes. It appears that Higgs usually arranged to stay with friends in the local area following her covert ethnographic episodes, creating a cover story by telling them that she and her companion had been on a 'walking tour' before joining them.[79] Nearing the end of one ethnographic stint, Higgs mused that only a little modification of their disguise was necessary to allow them 'to pass for shabby tourists on a "walking expedition." Our luggage had been sent on, and supplies of money awaited us. Therefore the only problem was that of changing from "tramps" to "tourists"'.[80]

By drawing an ironic parallel between vagrants tramping from one shelter to another and the middle-class notion of walking tours for pleasure, Higgs may have been making an oblique reference to the fashionable practice of slumming, explored extensively by Seth Koven, effectively distancing her research from such aimless voyeurism.[81] Just as Jack London feigned disbelief that Thomas Cook, a travel company with extensive knowledge of comparatively distant lands, could not guide him around the east end of London, Higgs' comparison of vagrants tramping and the middle-class practice of walking tours, or even of slumming, was intended to highlight the incongruity of the two

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[74] Higgs, *Glimpses*, p.317. Higgs' openness about the presence of a companion on her covert outings contrasts with James Greenwood's failure to mention his companion, Bittlestone, who was not mentioned at all in 'A Night in a Workhouse'. Freeman and Nelson, *Vicarious*, p.27; Koven, *Slumming*, pp.33-36.
[75] Higgs, *Glimpses*, p.232.
[76] Higgs and her companion claimed to have walked for six hours through heavy rain to reach one workhouse tramp ward: Ibid., pp.119-120.
[77] Ibid., p.317.
[78] Ibid., p.138.
[79] Ibid., p.88.
[80] Ibid., p.134. Higgs also referred to their 'walking tour'. See: Higgs, *Glimpses*, pp.88 & 135.
[81] Koven, *Slumming*. See above, p.16.

practices.[82] Beyond this sense of physical journey, however, Higgs also demonstrated a consciousness of the other type of journey that Parssinen locates at the root of social exploration, the journey from ignorance to understanding.[83] Listing her ethnographic experiments, Higgs noted that she had undertaken these explorations, 'each time with increasing knowledge'.[84]

Higgs' *Glimpses*, like Goldsmid's *Dottings*, was constructed in such a way as to include six ethnographic narratives. Each of Higgs' six narrative chapters relates to one ethnographic episode, which sometimes covered a period of several days, and entailed visiting different types of accommodation available for women vagrants in London and the north of England. Higgs' narrative accounts constituted the bulk and the conceptual heart of her text. In fact, in her preface to *Glimpses*, Higgs described how her use of other research methods and sources of information had not illuminated 'the root problems of poverty', and explained that it was only by recourse to covert exploration that she was able to understand vagrancy and its concomitant moral degradation.[85] In the preface to one of her ethnographic texts, which was originally published as a pamphlet before being incorporated into *Glimpses*, the link was established between narrative and truth when Higgs wrote that 'the following narrative may be relied upon as true in every detail. The facts were burned in upon the minds of the two pilgrims and were put on paper at once'.[86] In his review of her work, William Beveridge commented that Higgs' accounts 'in their bare recital of impressions…carry the stamp of truth, and are indeed completely in accord with other evidence'.[87] By relegating the prefaces which appeared when the ethnographies were published individually to appendices, Higgs allowed her ethnographic narratives to follow on from each other, uninterrupted, in the order that she and her companion undertook the fieldwork.[88] Much like Goldsmid's *Dottings*, Higgs' ethnographic episodes have authority through cumulative experience and repetition. In fact, in the opening paragraph of her third ethnographic narrative, Higgs noted that one reason for undertaking further ethnographic research was that it had been 'suggested in several quarters that our experiences might have been exceptional' after just one episode of covert work.[89] Higgs cross-referenced her text extensively, using footnotes to refer the reader between discrete

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[82] London, *The People,* p.11.
[83] Parssinen, 'Social', p.206; See above, pp.24-25.
[84] Higgs, *Glimpses*, pp.vii & 135.
[85] Ibid., pp.v-vii.
[86] Ibid., p.317.
[87] Beveridge, 'Review', p.583.
[88] Higgs, *Glimpses*, pp.87-268.
[89] Ibid., p.136.

ethnographic episodes and her analysis of the problem of vagrancy to correlate and substantiate her findings.[90]

Like Goldsmid, Higgs made use of narrative to demonstrate her experiential authority and, like Goldsmid, she also used alternative strategies of textual authority. Higgs' narrative ethnographic chapters were effectively sandwiched between other non-narrative texts. For example, the preface to *Glimpses* and the first chapter, 'Vagrancy', preceded the run of narrative ethnographic chapters.[91] Similarly, the closing sixty or so pages of *Glimpses* cannot be described as narrative ethnography. Rather, they included discussions of the relative merits of proposed solutions to vagrancy such as labour colonies, as well as summaries of the findings of a government committee on vagrancy.[92] Although these aspects of *Glimpses* will be discussed in more detail elsewhere, it is important to make reference to them here in relation to the overall structure of the text. These sections of the text established Higgs as an individual who could speak authoritatively on women and vagrancy for a numbers of reasons, not just because of her experience of covert social exploration amongst them. It is established that Higgs was involved in philanthropic efforts to support vagrant women and was acknowledged as an expert on their plight, appearing before the Departmental Committee on Vagrancy and making speeches on the matter.[93] Furthermore, her prize-winning essay and other analytical passages of *Glimpses* clearly demonstrated that Higgs' understanding of women's vagrancy was firmly rooted in knowledge of the statistical, legislative and socio-historical context of the issues, not just her covert experience.[94]

There are two more aspects of Higgs' *Glimpses* which challenge the notion that late Victorian and Edwardian ethnography can be associated by and large with a narrative structure leading to the development of experiential authority. First, as part of her prize-winning essay, Higgs included a series of letters she had received from a male vagrant after he had heard her speak publicly on issues relating to vagrancy.[95] Secondly, in the eighth chapter, Higgs wrote about what she referred to as 'a symposium in a common lodging-house', reporting on a meeting she and her companion had with the residents of one common lodging house. As well as singing hymns to the vagrants to keep their spirits up,

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[90] See, for example, Ibid*.,* pp.235 & 249.
[91] Ibid., pp.v-xi & 1-86.
[92] Ibid., pp.269-238.
[93] Ibid., pp.xi & 2-4.
[94] Ibid., pp.1-86 & 269-328.
[95] Ibid., pp.33-46.

Higgs also sought and recorded their opinion on the state of accommodation for itinerant people such as themselves.[96]

These sections of Higgs' text are significant as they go some way toward breaking down what Clifford refers to as the 'monophonic authority' typical of traditional ethnography by introducing a collaborative element to the production of knowledge, by allowing the informant a degree of authorship.[97] Usually associated with post-1950s professional ethnography, Clifford notes that giving the informant 'an autonomous textual space, transcribed at sufficient length' signifies the use of an alternative textual strategy whereby monographic authority is destabilised in favour of heteroglossia.[98] This is effectively what Higgs was doing when she included the letters written to her by a vagrant and, to a lesser extent, when she reported on her meeting with some vagrants in a common lodging-house. Of course, the extent to which an ethnographic text can be multi-authored is questionable. The ethnographer maintains an executive, editorial position responsible for the 'virtuoso orchestration' of their text and there is the possibility, for example, that Higgs edited the letters before she included them, or that she only reported aspects of the meeting which concurred with her own opinions.[99] Nevertheless, Higgs' decision to include these elements in her text at all demonstrates the complex ways in which she sought ethnographic authority, going beyond the experiential mode and narrative structure associated with late Victorian and Edwardian ethnography.

This discussion of the structure of Webb, Goldsmid and Higgs' texts has shown that whilst experiential authority and the associated narrative structure are indeed key elements of late Victorian and Edwardian ethnographies, it is too simplistic to see this as a defining feature of such research. Ethnographers such as Goldsmid and Higgs brought much more to bear on their texts than just their covert experience and they often used complex textual strategies which built on more than just experiential authority, using techniques associated with professional anthropological ethnographers working in the second half of the twentieth century.

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[96] Ibid., pp.269-283.
[97] Clifford, 'On Ethnographic', p.139.
[98] Ibid., p.140.
[99] Ibid., pp.139-140.

Language and Imagery in Covert Ethnography from the 1880s to 1914

As discussed in the introduction, there is a trend in the historiography of social exploration to focus on the literary aspects of social exploration, especially the language and imagery used by these late Victorian and Edwardian social researchers.[100] In fact, in his influential introduction to the social explorers, Peter Keating suggests that the tradition of social exploration was 'best defined in terms of the language which the writers themselves used to describe their activities'.[101] Other scholars have echoed Keating's point that language, the rhetoric and imagery they used, was the defining feature of social explorers' work.[102] Lodewijk Brunt, for example, finds the language and stylistic devices employed by social explorers to characterise their objects of study one of the most striking features of the genre, whilst for Anthony Wohl the language of travel and exploration became almost a cliché of social exploration.[103]

Like Wohl, other commentators have noted the use of language relating to travel and exploration in the work of social explorers.[104] The use of such a vocabulary reinforces the notion of ethnography as a double journey, which we have already discussed in relation to the structure of ethnographic texts. In fact, we found that both Higgs and Goldsmid drew upon this journey analogy, and that this manifested itself in the vocabulary they used. It has been noted that the language and imagery used to describe research subjects in works of social exploration deploys this rhetoric of journey and exploration. For example, the rural poor, according to Freeman, were constructed by social investigators as 'remote and alien' by using language which represented them as 'foreign and savage'.[105] Brunt detected no fewer than four closely related characteristics of language used by social explorers to describe their research subjects, which in combination represented the poor as the antithesis of bourgeois society. First, they associated the poor, their research subjects, with animals and non-human phenomena such as 'pigs' and 'vermin'. Second, the poor were frequently described as supposedly primitive peoples such as 'savages' and 'Papuas'. The third characteristic of the rhetoric of the social explorers, according to Brunt, is the

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[100] See above, pp.25-26.

[101] Keating (ed.), *Into Unknown England*, p.9.

[102] The centrality of such language and imagery to the work of the social explorers may have been overplayed somewhat. Potter Webb's ethnographic account is almost entirely free from the rhetoric which Keating and others use to define the genre. Potter Webb made only one reference to her search for work as a 'weary pilgrimage' which could be construed as a reference to her research as a journey: Potter, 'Pages', p.303.

[103] Lodewijk Brunt, 'The Ethnography of "Babylon": The Rhetoric of Fear and the Study of London, 1850-1914', *City and Society: Journal of the Society for Urban Anthropology*, Volume 41, (1990), p.77; Anthony S. Wohl, 'Social Explorations Among the London Poor: Theatre or Laboratory?', *Revue Francaise de Civilisation Britannique*, Volume 6, Part 2, (1991), p.89.

[104] Such as, for example: Mark Freeman, 'Folklore Collection and Social Investigation in Late-Nineteenth and Early-Twentieth Century England', *Folklore*, volume 116, (2005), p.55.

[105] Freeman, 'Folklore', p.55.

association between the poor and an absence of firm roots in society and a lifestyle characterised by mobility. The fourth and final association that social explorers made was between the poor and 'physical abnormality and moral inferiority'. Brunt suggests that the use of these four discreet vocabularies can be seen as the deployment of a 'rhetoric of fear' by the social explorers.[106]

Many commentators find the use of such language and imagery to be emblematic of social exploration as a genre which encompasses the covert studies we are interested in. The general consensus would appear to be that the purpose of using such vocabularies and imagery was to create literary impact by shocking the audience with the 'otherness' of the research subjects. Kent, for example, suggests that the primary intention of using the imagery of foreign exploration was 'to draw attention to the inequalities in society and to force upon the reader an awareness of his social blindness'.[107] Similarly, Wohl says that the dramatisation of slum-life through language 'was necessary to convey its social significance and to arouse the middle-class reader's interest'.[108] We therefore gain the impression that the use of such vocabulary and imagery by social explorers was nothing more than a superficial attempt to play up the 'otherness' of the research subjects.[109] There were, of course, instances where covert ethnographers (and other social explorers) clearly used such language simply for literary effect. Goldsmid, for example, in a couple of isolated incidences, used an animalistic vocabulary to describe lodgers and lodging houses. He referred to a young couple in a doss house as 'poor creatures' and described a particularly filthy kitchen as 'a magnified rat hole – so dark, so stenchful, so unwholesome, does it appear'.[110] Here, Goldsmid appears to have used this imagery to elicit sympathy for the lodgers, and to highlight the abominable state of lodging houses.

Brunt suggests that by describing the poor using such rhetoric, the social explorers were making 'a strong effort to place the research subject in a dimension different from the one of the researcher.'[111] In one way, this is exactly what many social explorers were trying to do; they were using language to emphasise the otherness and alienness of the poor research subjects compared to the typically middle-class social explorer and audience.

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[106] Brunt, 'The Ethnography', pp.81-82.
[107] Kent, *A History of British Empirical Sociology*, p.38.
[108] Wohl, 'Social', p.77.
[109] Deborah Epstein Nord has recognised the deeper significance of the way some social explorers used certain vocabularies to describe their research subjects. Her thesis will be explored with reference to the relationship between ethnography and anthropology: Deborah Epstein Nord, 'The Social Explorer as Anthropologist: Victorian Travellers Among the Poor' in William Sharpe and Leonard Wallock (eds.), *Visions of the Modern City: Essays in History, Art and Literature*, (Columbia: Columbia University Press, 1987), pp.122-134.
[110] Goldsmid, *Dottings*, pp.17-18 & 85-86.
[111] Brunt, 'The Ethnography', p.81.

However, what Brunt implies is that there was something deliberately deprecating about the language and imagery employed by social explorers in relation to the poor before 1914. Furthermore, he suggests that the use of such rhetoric was specific to the genre of social exploration prior to 1914, thus precluding comparisons between late Victorian and Edwardian covert ethnographers and their modern day counterparts:

> Nowadays we form part of cultures in which an undistinguished use of a rhetoric of fear, especially by researchers who want to be taken seriously, will generally be considered as a lack of self-control. Some generations ago, perhaps, nothing much was thought of anthropologists referring to their informants as 'savages' or 'primitives.' Such practice is unthinkable today.[112]

Brunt's thinking on this issue seems a little skewed. It is unthinkable that modern-day ethnographers would use terms such as 'savages' because their meaning and significance has changed over time. In the late Victorian and Edwardian era, theories of race and evolution were bound up together and the notion that some 'races' were at different developmental stages was widely believed. In hindsight, of course, it is unsettling that such notions were used so widely, but that does not mean that they were used by the social explorers in a derogatory way. Far from demonstrating a 'lack of self-control', the use of such language by some social explorers may be evidence of a more sophisticated understanding of their research subjects. Emphasising otherness and alienness, using alien and foreign and even hellish rhetoric can be understood as a tacit acknowledgement by the social explorer of research environments as distinct cultural and social entities. To some extent the poor and sub-sections of the poor, such as trouser-hands or vagrants, did, like foreigners both 'civilised' and 'savage', inhabit their own worlds with their own internal cultural and social rules.[113]

We can illustrate this possibility with reference to Higgs' covert ethnography. Brunt identified the association between the poor and an absence of firm roots in society and a lifestyle characterised by mobility as one of the four key characteristics of social explorer's rhetoric. Given that Higgs was researching vagrancy, it is not surprising that she should make reference to the peripatetic tendencies of her research subjects, but it does fit nicely with Brunt's rhetoric of mobility. Higgs wrote about single women she observed who 'drift[ed]', unable to recover a stable position.[114] She referred to an inmate of one workhouse casual ward as a 'fellow-traveller' and described her research subjects as

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[112] Brunt, 'The Ethnography', pp.83-84.
[113] Freeman acknowledges this explanation with specific reference to the rural poor: 'The persistence of an exploratory tradition in the literature of rural social inquiry…illustrates the perceived inaccessibility and even alienness of rural communities to the social investigator.' Mark Freeman, *Social Investigation and Rural England, 1870-1914*, (Woodbridge, Suffolk; Rochester; New York: Royal Historical Society: Boydell Press, 2003), p.184.
[114] Higgs, *Glimpses*, p.106.

'migrating women'.[115] The important point to note is that Higgs was not using this rhetoric of mobility as either a simplistic literary device or as a way of denigrating itinerant women. Her use of this descriptive language was linked to her convictions about vagrancy as a way of life. In *Glimpses*, Higgs discussed at length how the 'industrial revolution' had wrought changes on the labour market which meant that work was 'a fluctuating quantity, and men and women had to travel'.[116] She also understood vagrancy as a historical phenomenon and one that was linked to theories of 'racial' progression.[117] Therefore, it can be suggested that Higgs' use of such rhetoric was not simply for literary effect or an attempt to denigrate her research subjects. Her use of such a vocabulary was, in fact, in keeping with her convictions regarding the social, economic and biological roots of peripatetic vagrancy.

Literature or Social Science?

So far, we have focused predominantly on the literary aspects of late Victorian and Edwardian ethnographies but, as Freeman notes, the association between social explorers and literature, especially fiction, has only exacerbated the exclusion of such work from most historical accounts of social inquiry.[118] The extent to which ethnographies from this period should be considered as literature or social science is something which many commentators have remarked upon. Wohl focuses on the social exploration of London and, although he comes to the conclusion that it was indeed a distinctive type of literature, he is aware of the tensions inherent in this genre between the perception of London as a theatre or as a laboratory. Wohl's main theme, which we can usefully apply to the whole of Britain rather than just the capital city, is that the literature of social exploration

> was full of ambiguities because it vacillated between viewing London, on the one hand, as a laboratory and, on the other, as a theatre, between science and art, between reason and emotion, between individualizing or careful categorizing and stereotyping, between objectivity and subjectivity.[119]

Meanwhile, Brunt makes it clear that late Victorian and Edwardian social explorers were arguably proto-social scientists, 'ethnographers *avant la lettre*', whose work had an anthropological quality, but he is nevertheless far more concerned with their use of 'bombastic language'.[120] So, whilst Wohl sees ethnography vacillating between the two poles of literature and social science, Brunt sees it as being both simultaneously. Jackson offers yet another perspective suggesting that, from the 1880s onwards, 'the novel

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[115] Ibid., pp.110 & 133.
[116] Higgs, *Glimpses*, pp.106 & 284-302.
[117] Ibid., pp.1-11. This theory will be explored in more detail below. See below, pp.66-67.
[118] Freeman, *Social*, p.4.
[119] Wohl, 'Social', p.81.
[120] Brunt, 'The Ethnography', pp.77-78.

gradually lost some of its territory to emergent sociology'. Whereas beforehand the whole social landscape had belonged to the novelist, Jackson suggests that social scientists such as Webb, Booth and Rowntree claimed more and more territory from the novelists, 'prospecting it in quite different ways' from the 1880s onwards.[121] It is difficult to know where covert social explorers, other than Webb, fit into this model. They could be thought of as a subset of social scientists 'prospecting' the social landscape in their own covert ethnographic way. However, it is equally possible that there is no room in Jackson's model for late Victorian and Edwardian ethnography, stranded in a no-man's-land between novels and social scientific surveys. In this section, we will explore both the literary and social scientific aspects of late Victorian and Edwardian covert social exploration. This will allow us to situate ethnography in relation to literature and emergent social science around the turn of the twentieth century.

Late Victorian and Edwardian Covert Ethnography as Literature

There are aspects of late Victorian and Edwardian ethnography which suggest a greater similarity to imaginative literature than to social research as a body of work. At least one of our late Victorian ethnographers, Webb, had, in fact, contemplated a literary career.[122] We have already discussed how these covert social explorers used the narrative form to construct their texts in detail. Such a narrative form can make these ethnographic accounts seem like novels or, in the case of Goldsmid and Higgs' work, a series of short stories detailing the researcher's adventures in the field. We have also explored the colourful and descriptive language used by pre-1914 ethnographers to convey their observations and, again, such a flair for writing aligns these authors with novelists rather than social scientists. However, there are other aspects of the covert social explorers' work which make definitive categorisation as either literature or social research problematic.

Covert social explorers did, for example, frequently 'quote' the direct speech of themselves and those they observed in their texts; sometimes attempting to record the working-class dialect that they heard.[123] Given that they were working covertly and would have been unable to take notes on what was said in the field, the extent to which this

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[121] Brian Jackson, 'Introduction', in Beatrice Webb, *My Apprenticeship,* (Harmondsworth: Penguin, 1971), p.12.
[122] According to Jackson, Webb often felt 'tempted' or 'haunted' by the prospect as if it were 'some dark angel beckoning her away from more useful work' and Harrison points out that when she was training to be a social investigator: Jackson, 'Introduction', p.12; Brian Harrison, *Peaceable Kingdom: Stability and Change in Modern Britain*, (Oxford: Clarendon, 1982), p.305.
[123] See, for example: Goldsmid, *Dottings*, pp.13-14; Potter, 'Pages', p.304. She also quoted one worker as using the term 'Round the Corner' which she translated for the reader as 'The East End term for the lady you take to the theatre or the music-hall.' Potter, 'Pages', p.311.

quoted dialogue required imaginative reconstruction adds to the literary quality of their work. Some late Victorian and Edwardian ethnographers also included humorous episodes in their accounts, a concept almost entirely alien to other forms of sober and dry social research.[124] Goldsmid, in particular, used several cultural reference points in *Dottings* which again added a literary touch to his work. He referred, for example, to Charles Dickens' *Sketches by Boz* and, in ironic credulity, to the Victorian celebrity chef, Alexis Soyer, who had published, amongst other works, books entitled *Soyer's Charitable Cookery, or, the Poor Man's Regenerator* and *A Shilling Cookery for the People*.[125]

Perhaps the main reason why late Victorian and Edwardian ethnography has been categorised as literature by so many commentators is the prominence of the ethnographer in the text. All ethnographies are, to an extent, about the ethnographer and there does appear to be an element of self-exploration, as well as social exploration, inherent in such work. As Parssinen has pointed out, by undertaking covert research, social explorers 'were obliged to grapple with the paradox that lies at the heart of participant observation' in that 'they had to stand both inside and outside their experience and then make what they had discovered coherent for others'.[126] Of course, all social research is about the researcher to some extent, but this sense is heightened with ethnographic research, where all the research findings are essentially filtered through their experience in the field. The personal nature of ethnographic research is further heightened when the results are presented in a narrative format, where the ethnographer is obliged to appear as the central character, explaining what he or she did, saw and felt in the field. Nord has noted how Higgs' text differed from that of other women social investigators, such as Bosanquet and Pember Reeves, who used non-ethnographic research methods. In Higgs' work, it was Higgs who appeared as the central figure in her narrative.[127]

Higgs, Goldsmid and Webb all wrote their ethnographic accounts in the first person and regularly reflected on their own emotional and physical condition in the field. This is not surprising, given the importance that some ethnographers attached to the notion of experience. As already discussed, experiencing the phenomenon under investigation was thought to be one way of conveying the truth. This sentiment was most prominent in Higgs' text where, following harsh treatment in a workhouse, she thanked God 'for every

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[124] Higgs, *Glimpses*, pp.185-186 & 238-239; Goldsmid, *Dottings*, pp.69-70.
[125] Goldsmid, *Dottings*, pp.13 & 94-95; Elizabeth Ray, 'Soyer, Alexis Benoît (1810–1858)', *Oxford Dictionary of National Biography*, Oxford University Press, (2004), http://www.oxforddnb.com/view/article/26076 , [accessed 25th March 2010].
[126] Carol Ann Parssinen, 'Social explorers and social scientists: the dark continent of Victorian ethnography', in Jay Ruby (ed.), *A crack in the mirror: reflexive perspectives in anthropology*, (Philadelphia: University of Pennsylvania Press, 1982), p.208.
[127] Nord, *Walking*, p.230. Reeves, *Round*; Bosanquet, 'Marriage'.

bit of suffering that I may bring home the truth'.[128] She also challenged her readers to doubt her experience as the basis of knowledge, asking 'Do I exaggerate? It must be *felt* to be realised'.[129] The reading audience could vicariously experience the situations being researched because the ethnographers consistently reflected on their own responses. On emerging into the street after his first night in a common lodging house, Goldsmid was at pains to stress the extent of his nausea. He described the morning breeze upon him as 'more delicious than words can express' and said that he 'would not mortgage the prospect of a change of garments and a bath for all the fabled wealth of El Dorado'.[130] Throughout her account, Webb continually shared her own emotive and physical response to playing the role of a sweated labourer. She wrote about feeling weary after searching for work and about the pain in her fingers and back after one day of finishing trousers.[131] When the mistress of the sweatshop threatened Webb with dismissal because of her shoddy work, she felt an 'ominous lump' in her throat and tears filled her eyes.[132] Higgs also communicated her own reaction to the role she was playing. She wrote about an incident which occurred when she and her companion visited a workhouse casual ward in a northern town which had a large 'sleeping-out' problem.[133] As they queued to be admitted to the tramp ward, Higgs noted how the official was 'threatening and violent in language' towards each vagrant.[134] Higgs recalled what happened when she reached the front of the queue:

> I gave particulars which were true, and in answer to the question, 'Have you been here before?' could truthfully say 'No.' But this was not enough. 'And what are you doing here?' 'I am going on to my husband.' 'You've no business to be here imposing on the rates. Do you know I could give you three months for it? I've a good mind to send you off and make you tramp to him to-night.' I was so dumbfounded, my friend says, I replied, 'I wish you would!' Then he proceeded to insinuate I was a woman of bad character; my eyes fell and my face flushed, and I suppose gave colour to his statement. Reply or justification was worse than useless. I grew so confused I could not state correctly the number of my children, but said I had 'one or two.' Evidently a bad character, leaving children up and down the country. 'See you don't come here again. I shall know your face, and it will be worse for you if you do.'[135]

By expressing their own physical and emotional state in the field, Higgs, Goldsmid and Webb allowed their readership to imagine themselves going through the same experience.

Furthermore, articulating their own experience in the field was one way in which the late Victorian and Edwardian ethnographers could develop an empathetic understanding of how their research subjects felt. Higgs was particularly aware of how her

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[128] Higgs, *Glimpses*, p.169.
[129] Higgs, *Glimpses*, p.171. Emphasis in original.
[130] Goldsmid, *Dottings*, p.25.
[131] Potter, 'Pages', pp. 301 & 309.
[132] Ibid., p.310.
[133] Higgs, *Glimpses*, pp.136-174.
[134] Ibid., pp.140-142.
[135] Ibid., p.142.

own experience could shed light on how genuinely destitute women felt when they sought shelter in the places she had visited. When Higgs was approaching the Salvation Army shelter to find a bed for the night, she worried that if they questioned her about 'her soul' she would be forced to reveal her true identity as a researcher. In her state of anxiety about being judged by staff at the shelter, Higgs mused that she was 'probably sharing the feelings of [her] poor sisters (your feelings undergo a curious assimilation to those of the class you represent)'.[136] Higgs clearly felt that she had become integrated with the vagrants that she studied and was empathetically able to articulate their feelings. Indeed, she felt qualified to write from the perspective of one vagrant amongst many:

> We are 'on the road,' and nothing in front can be so bad as what lies behind. We are tramps and 'mouchers'; we can beg, for we need pity…One thing we could not do – we could not at this moment work for an honest living. It is physically impossible…Strength to work has gone. One might perhaps tramp, for the air is reviving, and people are kind to a wayfarer. Do you wonder at our *national tramp manufactories?*[137]

Higgs was, however, keenly aware of the difference between herself and those she observed. She was experiencing workhouse casual wards, lodging houses and shelters as a middle-class woman in disguise, not as a genuinely distressed and destitute woman. After spending the night in a London tramp ward, Higgs and her companion became quite ill and required medical attention and a period of recovery. Higgs reflected on how such harsh treatment would affect a true vagrant who would not be in such full health as herself and suggested that he or she may have died as a result.[138] She made a similar assessment in relation to another particularly tough ethnographic stint, writing:

> I felt a mere wreck. Only two days ago I was in full health and vigour. It was no absolute cruelty, only the cruel system…Before me lay home and friends, a loving welcome, good food, sympathy, and rest. What about my poor sisters? 'I have nobody, nobody in the wide world; I wish I had,' said the poor soul next to me…[139]

Late Victorian and Edwardian ethnographers usually wrote in the first person and, as we have seen, referred frequently to their own physical and emotional state in the field. While this may have been part of the reason why commentators have categorised their work as literature, it must be remembered that communicating their experience in the field to the reader and using it as a way to understand their research subjects was part of the remit of their research.

The ethnographers' covertness added another dimension to the element of self-exploration in their research. Their use of disguise clearly allowed them access to social

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[136] Ibid., p.179.
[137] Ibid., p.172. Emphasis in original.
[138] Ibid., p.268.
[139] Ibid., p.168.

spaces and roles on terms which would not have been the same had they been overt researchers. For example, it is unlikely that the discourteous workhouse official referred to above would have insinuated that Higgs was a prostitute had he known she was a highly educated middle-class minister's wife visiting the casual ward for research purposes. Nevertheless, Wohl dismisses covertness as simply another 'dramatic device' in the social explorer's armoury, 'one that planted in the mind of the reader an extra element of terror to the enterprise of penetrating the slums'.[140] Higgs, Goldsmid and Webb all made a feature out of their covertness, but the extent to which this added 'terror' to their accounts is questionable. Only Webb could be construed as having used her disguise as a 'dramatic device' in the way that Wohl suggests, describing herself as being so 'painfully conscious' of her disguise that consisted of buttonless boots, a short and bedraggled skirt, an ill-fitting coat and a bonnet upon her unkempt hair, that she was too uncomfortable to buy food anywhere.[141] Webb used her covertness to generate tension in her account, playing on the notion that her incognito was precarious and that she may be found out at any moment. Both Goldsmid and Higgs used their covertness in very different ways.

Goldsmid's covertness afforded him a sense of freedom and the opportunity to participate anonymously in the life of London's dossers. His disguise was in itself quite dramatic. Before he embarked on his first visit to a common lodging house, Goldsmid described himself:

> I have surveyed myself most carefully, and my impression is that 'my own mother wouldn't know me'. My face – well, perhaps the less said about it the better, for it is absolutely repulsive by reason of the dirt that covers it. My shirt matches my face, and my coat accords with the appearance of both. Waistcoat and shirt-collar have alike been discarded, and a particularly unclean neckcloth of the familiar costermonger type has taken the place of both. My boots are broken and patched. My hat is a frowsy looking specimen of the flexible 'deerstalker' type. I don't exactly know what I look like.[142]

Once disguised in this way, Goldsmid felt able to behave in a reckless manner, participating rather too freely in the dosser's way of life. For example, Goldsmid was walking through the Ratcliff Highway area of London when a drunken man called him a 'sanguinary Fenian' and pushed him. Goldsmid

> returned the push with all the interest I could, and somehow or other – it is not for me to explain how – my assailant found himself in the gutter, whence he immediately sprang, averring that I was a 'jolly old cock,' and offering to 'stand a pot.' Declining the proffered hospitality, I continued my peregrinations… [143]

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[140] Wohl, 'Social', p.92.
[141] Potter, 'Pages', p.302.
[142] Goldsmid, *Dottings*, p.12.
[143] Ibid., p.91.

Shortly after this incident, Goldsmid was again involved in another rather unnecessary scrape with some more local residents of the Ratcliff Highway area. As Goldsmid was walking past a group of loitering men, someone threw a 'particularly frowsy and evil-smelling cap' in his face. Rather than simply leaving the cap and walking away, Goldsmid caught it and pretended to throw it back to the men before walking off with the cap in his hand, 'chuckling' to himself. They gave chase and Goldsmid flung the cap in the face of the leader and ran off.[144] For Goldsmid, covertness led to an escape from middle-class sensibility and adventure. Higgs, on the other hand, used her covertness in a much more prosaic yet effective way.

Unlike Webb and Goldsmid, Higgs played down the dramatic nature of her transformation from middle-class philanthropist to downtrodden vagrant. She remarked on a number of occasions how easy it was to change from lady to tramp and back again. During her fieldwork, Higgs observed one 'coarse and fat' lodger put on a tattered bodice but covering it with 'a most respectable cloak' and bonnet, emerging almost a lady. Higgs reflected:

> Evidently the 'clothes philosophy' is well understood in Slumdom, for whatever purposes it is used. Indeed, it has given me somewhat of a shock to realise that many of these, even if dwellers in actual filth and disease, would not be distinguishable in any way from ordinary individuals.[145]

In fact, Higgs said that it was 'so surprisingly easy to become a tramp that it is strange it has not occurred to Guardians personally to test conditions by sampling each other's workhouses'.[146] Higgs was trying to make a point by demonstrating the ease with which she and her companion could disguise themselves as tramps and, with a few modifications to their outfits, transform themselves back into respectable women.[147] She maintained that all that lay between destitution and even women of 'the higher ranks' was a matter of clothes. Once a woman looked 'down-trodden', she would struggle to find legitimate work, and then a spiral downwards into vagrancy and prostitution was begun.[148] Nevertheless, Higgs was aware of the social distance traversed by donning even her minimalist tramp disguise. Rather than using her covertness for dramatic purposes, Higgs used it for ironic effect. Dressed as tramps, Higgs and her companion wandered streets and parks in search of shelter, often passing by the residences of Higgs' well-to-do friends. She emphasised how slight a disguise could temporarily cut her off from her own world and social circle.

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[144] Goldsmid, *Dottings*, p.92.
[145] Higgs, *Three Nights*, p.20.
[146] Higgs, *Glimpses*, p.138
[147] Ibid., p.134.
[148] Ibid., pp.190-191. Higgs frequently commented on the lack of facilities for personal hygiene and laundering clothes in accommodation for vagrants. See, for example, Higgs, *Glimpses*, pp.243, 249-250 & 253-254.

We can see, therefore, that whilst the covertness of social explorers may add to the literary qualities of their work, it cannot be dismissed as simply a 'dramatic device'.

Covert ethnographic research involves researchers surreptitiously embedding themselves in an alien social setting, often undertaking unpleasant experiences in the name of research and, at least around the turn of the twentieth century, in the name of social progress. To some extent, therefore, there is a sense that undertaking ethnographic research is an act of heroic self-sacrifice. When Greenwood spent just one night in a workhouse casual ward, it was suggested that he should receive the Victoria Cross for his gallant undercover journalism.[149] There was an element of heroism in the work of Higgs, Webb and Goldsmid, but it was most clearly developed in Goldsmid's *Dottings*.[150] Goldsmid's resolve to continue his covert research was severely shaken by his first night in the 'Beehive' lodging house, and he communicated his personal stoicism and higher purpose to the reader when he mused on the horrors of the 'Beehive', which

> were enough to turn me from my purpose. 'It is, after all,' I argued with myself 'no business of mine. I can do no good. If the public is aware of the existence of these haunts of destitution and degradation, my interference will benefit no one. If, on the other hand, they are ignorant of the state of things, does not that very ignorance demonstrate their apathy?'…But it appeared to me that it would have been the veriest cowardice to have abandoned altogether a task which I firmly believe to have been none the less necessary because self-imposed; and I was not without hope that the things to be seen and heard…might…sounding clarion-tongued, direct popular attention to great and crying evils.[151]

Higgs, on the other hand, was not so concerned with constructing herself as an ethnographic heroine. She was accompanied by her unnamed yet 'brave little friend and companion' on most of her ethnographic sojourns.[152] Twice, however, Higgs was without her usual companion and, being unwilling to venture out alone, had to be shadowed once by a policeman in plain clothes and on the other occasion by her son disguised as a 'working man'.[153] Higgs did not stay overnight in vagrants' accommodation on either of these occasions, presumably because it would have necessitated staying alone, without the support of a fellow incognito woman.

As we have mentioned previously, Webb, far from constructing herself as a heroic researcher, appeared to be uncomfortable in her dual role as investigator and work-girl. Higgs was confident in her incognito status and, after a while, she and her companion had

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[149] Freeman, "'Journeys'", p.104.
[150] After Higgs had decided that covert research was necessary, some months passed before she felt able to arrange to face the ordeal. Higgs, *Glimpses*, p.vii. Similarly, Webb noted in her diary that creating her 'picture' of the sweating system would only be possible by 'living among the actual workers. This I think I can do.' Webb, *My Apprenticeship*, p.313.
[151] Goldsmid, *Dottings*, pp.26-27.
[152] Higgs, *Glimpses*, p.162.
[153] Ibid., pp.250-251, 254, 255 & 258.

gathered enough experiences of tramping that they could reciprocate in conversation without lying, and even their 'educated' speech did not mark them out especially from other vagrants.[154] In contrast, Webb almost seemed to be engaged in a paradoxical project of assimilating herself into an alien social group whilst simultaneously highlighting and maintaining her distance from her working-class research subjects. Webb noted that her fingers were 'unhardened' and that she was a 'true hater of needlework' who doubted she could actually finish trousers.[155] She reported that the mistress said she was more suited to marrying a respectable man than 'making her own living' in a sweatshop.[156] Webb was also careful to note that although she was herself disturbed by her unkempt appearance, the mistress referred to her as 'tidy-looking' and having a face and voice which set her apart from the other workers.[157] Her lack of confidence in her incognito could be interpreted as demonstrating Webb's desire to not be too easily identifiable with the sweatshop workers she observed.

Higgs' subjectivity was also apparent in her ethnographic account and, in particular, her religiosity frequently shaped her text. During a particularly harsh ethnographic visit to a workhouse casual ward, Higgs highlighted the failure of the workhouse officer to display Christian virtues towards the inmates.[158] She also questioned the idea that a country professing to be enlightened and Christian could consider the workhouse a suitable refuge for the destitute.[159] Higgs also offered spiritual comfort to fellow vagrants, such as one ill and weary old woman in a casual ward, and by singing hymns 'that might leave some ray of comfort in sorrowful hearts'.[160] The other aspect of Higgs' subjectivity which informed her ethnographic account was her gender. Of course, given that Higgs was researching women's vagrancy, it is not surprising that she should reflect extensively on gender and women's gender-specific experiences of destitution. What was more striking, however, was Higgs' understanding of how her gender, coupled with her working-class disguise, transformed the way that men reacted to her. The ways in which men looked at and spoke to Higgs and her female companion manifestly changed when they were disguised as tramps, and Higgs wrote that she 'had never before realised that a lady's dress, or even that of a respectable working woman, was a *protection*.'[161]

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[154] Ibid., pp.161-162.
[155] Potter, 'Pages', pp.301, 303 & 305.
[156] Ibid., p.313.
[157] Ibid., pp.302 & 313.
[158] Higgs, *Glimpses*, p.154.
[159] Ibid., p.155.
[160] Ibid., pp.158 & 165. In Chapter 5, we will discuss the covert ethnography of Jeremy Sandford, which was also shaped significantly by his religious philosophy. See below, pp.214-217. Jeremy Sandford, *Down and Out in Britain*, (London: New English Library, 1972). First published in 1971.
[161] Nord, *Walking*, p.232. Higgs, *Glimpses*, p.94. Emphasis in original.

She told her readers that the 'bold, free look of a man at a destitute woman must be felt to be realised'.[162]

Whilst Goldsmid's dosser disguise brought him anonymity, invisibility and a sense of freedom in the slums, Higgs' disguise invited rather than inhibited the attention of the men she encountered. As Nord rightly points out, Higgs' 'class disguise exposes her as a woman and increases her personal and sexual vulnerability'.[163] However, Nord seems to suggest that Higgs' gender limited her effectiveness as a social researcher. She argued that 'femaleness itself constitutes an object of curiosity' and subverts the ability of female social investigators, including covert ethnographers such as Higgs, 'to act as either the all-seeing eye or the investigator of public life. To see without being seen, or to be seen without becoming spectacle, is rendered impossible. No reserve or incognito protects the female rambler.'[164] Nord suggests that Higgs is able to transcend the status of the spectacle only by staying in sex-segregated shelters, thus escaping the male gaze.[165] However, it is arguably possible to be spectacle and spectator at once. Higgs' experience of being a spectacle, of being subjected to the gaze which fell on all female tramps, was a part of her research. Assimilation into the role occupied by other women vagrants, and not invisibility, was Higgs' aim. The object of covert ethnography is not necessarily to disappear but to blend in. Gender *is* a limitation for the incognito ethnographer, as being male or female necessarily restricts the roles one can play. However, femaleness is not a particular limitation, and Nord appears to be missing the point when she emphasises how Higgs' gender affected her experience in the field. This was not a flaw in her research; it was one of the strengths. Through her incognito experience, Higgs learned that destitute women experienced the male gaze differently from better dressed and more secure women.

Late Victorian and Edwardian Covert Ethnography as Social Science

In his article about the use of covert social exploration to investigate vagrancy in Britain between 1866 and 1914, Freeman is insistent that the social explorers were not 'creative writers' and that the tendency of academic commentators to focus on the literary quality of the genre has distorted their significance as valid social research.[166] Of course, analysing the construction of these texts, the vocabularies used and the subjectivities of the researchers is important in understanding the phenomenon of covert ethnography.

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[162] Nord, *Walking*, p.15.
[163] Ibid., p.230.
[164] Ibid., p.240.
[165] Ibid., pp.235-236.
[166] Freeman, '"Journeys"', pp.99-101.

However, we should not lose sight of the status of these texts as examples of late Victorian and Edwardian social research. As Freeman says, 'there is also a case for situating them in the context of contemporaneous developments in social-survey methodology, and hence of sociological understandings of participant observation'.[167] In fact, many commentators have acknowledged links between Victorian and Edwardian social exploration and the nascent social sciences in Britain, but they have tended to do so in a superficial manner.

The tendency to associate social exploration with social science and the social scientific methodology of ethnography can be seen clearly in the titles of articles written about the social explorers. We have, for example, Brunt's 'The Ethnography of "Babylon"', Nord's 'The Social Explorer as Anthropologist' and Parssinen's 'Social Explorers and Social Scientists: The Dark Continent of Victorian Ethnography'. However, such commentators tend to suggest that the connection between the work of the social explorers and social scientists was, on the whole, shallow and somehow inconsequential. Wohl, for example, argues that the genre of social exploration took on 'the coloration of a social science' because of the middle-class desire to expose and know more about the lives of the working classes.[168]

Brunt recognises the possibility of a correlation between the research method used by social explorers and what was to become 'the principle of anthropological fieldwork' in the future. However, he also dismisses the social explorers' methods as 'rather simple' and distances their work from other, more scientific, examples of Victorian and Edwardian social research. Henry Mayhew, a prominent mid-Victorian overt social researcher, journalist and natural scientist by training, and particularly Booth and Rowntree, are singled out by Brunt as proto-social scientists, due to their consideration of methodological problems. The social explorers were excluded from this category of proto-social scientists because they 'did not try to convince the readers by stressing that their data were representative or reliable, but by an evocative and dramatized style of presentation'.[169] Brunt implies that such accounts derived their authority and impact not from their reliability and soundness as pieces of social research, but rather through style, drama and hyperbole. For Brunt, their lack of explicit methodological development and the literary quality of their texts mean that late Victorian and Edwardian ethnographers such as Goldsmid and Higgs cannot be categorised as social scientists.

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[167] Ibid., p.101.
[168] Wohl, 'Social', p.82.
[169] Brunt, 'The Ethnography', p.79.

The problem with Brunt's evaluation is that, around the turn of the twentieth century when Goldsmid, Higgs, Webb and other covert ethnographers were conducting their research, the social sciences in Britain were not yet clearly established. It is therefore difficult to say with any certainty what criteria constituted social scientific research at this time. Wohl points out that the dualism between laboratory and theatre in social exploration 'was but part of nascent social science'. From its beginnings sociological writing has been characterised by a 'primary evocation' between positive language relating to the empirical and objective on the one hand and dialectical language, more subjective and descriptive, on the other.[170]

In 1906, the year Higgs published *Glimpses*, the *Times* reported on a meeting of the Sociological Society at the University of London, at which Professor Wenley from the University of Michigan gave a speech. He suggested that, while sociology was firmly established in American universities, it continued to be 'excluded' from British university curricula. Although he admitted there was no agreement as to what precisely sociology should be, he was of the opinion that, whatever it was, 'it must be a science conducted by scientifically trained and competent experts, and not merely a pottering round so-called problems of local or even national origin by well-meaning enthusiasts…Otherwise it might become, as in some cases, a mere appendage of "commercial education," or, more respectably, of experimental psychology, or more fashionably, of slumming'.[171] So, around 1900, the descriptive and subjective nature of the covert social explorers' work did not necessarily exclude them from nascent social science in Britain, but their lack of training and association with the disreputable practice of 'slumming' did estrange them.

Nevertheless, some late Victorian and Edwardian covert social explorers did consider themselves a part of the wider social scientific research community in Britain. Wohl suggests that even though most of the social explorers were journalists by profession, they regarded themselves as 'empiricists in the true English Baconian and Newtonian scientific tradition' and prided themselves on their factuality and objectivity.[172] Higgs certainly considered her own ethnographic work to be in keeping with the principles of scientific research. Particularly in the introductory sections of *Glimpses*, Higgs used a scientific rhetoric to describe her research into vagrancy. She referred to the accommodation she had provided for sheltering destitute women as a 'social microscope'

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[170] Wohl, 'Social', p.86.
[171] *Times*, 'University intelligence', Thursday 25th January, (1906), p.7.
[172] Wohl, 'Social', p.82.

and referred to her study of 'the microbes of social disorder'.[173] For Higgs, 'exploration was the method of science' and she likened observing social class distinctions to the way 'observation led her to classify objects observed in physical studies'.[174] For Higgs, ethnographic research was an 'experiment'.[175] Some particularly substantial links can be traced between the work of late Victorian and Edwardian social explorers and the emerging social scientific discipline of anthropology.

Anthropology and Late Victorian and Edwardian Covert Ethnography

By the 1880s, the use of anthropological analogy was a well established and, even in the mid-nineteenth century, it was fairly common in the work of Charles Dickens and Friedrich Engels.[176] According to Nord, from the 1880s onwards, a number of social explorers made extensive use of an anthropological analogy to support their work as they 'rediscovered' poverty in Britain. It was also around this time that there was a heightened interest in Empire and Britain's imperial status. Missionaries, explorers, anthropologists and travellers went to the Empire to learn, conquer and convert.[177] The sense in which social explorers were caught up in this imperialistic drive to conquer unknown peoples, and apply it to sections of their own native land, was revealed by Webb in her diary in 1888. In rather uncomfortably imperialistic tones, she wrote:

> And I enjoy the life of the people at the East End; the reality of their efforts and aims; the simplicity of their sorrows and joys; I feel I can realize it and see the tragic and the comic side. To some extent I can grasp the forces which are swaying to and fro, raising and depressing this vast herd of human beings. My painstaking study of detail will help towards the knowledge of the whole, towards which I am constantly striving; I shall leave steps cut in the rock, and from its summit man will eventually map out the conquered land of social life.[178]

Missionaries, explorers and anthropologists returned to Britain with tales of their exotic and tortuous journeys with wild tribes. These stories captured the imagination of the British audience and, according to Nord, the genre of social exploration depended on 'this heightened interest in the Empire for much of its language, its form, its legitimacy, and its moral authority'.[179] A number of commentators have suggested that a relationship existed between late Victorian and Edwardian social exploration and the social science of anthropology. However, as with the association between social exploration and social

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[173] Higgs, *Glimpses*, pp.v & vi.
[174] Ibid., pp.vii & vi.
[175] Ibid., p.vii.
[176] Nord, 'The Social', pp.127-128.
[177] Ibid., p.123.
[178] Webb, *My Apprenticeship*, p.323. Higgs often used the image of civilised and uncivilised to demarcate her society and that of the vagrants and their accommodation. Having spent a night in a common lodging house, she spoke of cleaning herself 'as prelude to a civilised existence.' Higgs, *Glimpses*, p.249.
[179] Nord, 'The Social', p.124.

science as a whole, there is confusion over the nature of the relationship between the social explorers and anthropology. Whilst some commentators emphasise the way social explorers borrowed language and imagery from anthropological writing, others suggest that much more substantial and meaningful connections can be made between these two branches of social research.

Nord suggests that the use of an anthropological analogy, in which the inhabitants of slums in Victorian Britain were compared to the uncivilised people of foreign lands, constituted a 'virtual literary convention' of social exploration from the mid-nineteenth century onwards.[180] Wohl also sees the relationship between social exploration and anthropology as being primarily literary rather than methodological or theoretical. He suggests that 'from its inception the genre of social exploration was influenced by both anthropological and imperial travel literature'. Social explorers used an anthropological rhetoric to construct the poor as a race apart, as animalistic and less evolved. As far as Wohl is concerned, the new genre of social exploration was one which stressed the differences rather than the similarities between the classes, cementing class divisions in racial terms, thus making them even more sensational.[181]

Mayhew's use of an anthropological framework in his study of the working classes in mid-Victorian London has received most academic attention. Wohl suggests that the connection between the jungle and the urban savage had been made before Mayhew undertook his social research, but it was Mayhew who popularized the image of the urban savage, applying anthropological terms to sections of the British population.[182] For Wohl, although Mayhew's use of anthropological terminology gave his work the appearance of scientific research, it was in fact simply a way of making his account more dramatic and sensational.[183] Brunt is similarly sceptical that there was any truly social scientific weight behind Mayhew's use of an anthropological rhetoric and theory. He sees Mayhew's description of the London poor as 'nomads' as no more than a vague effort to rationalize the fear and alleviate the guilt bourgeois society felt in relation to the poor. As Brunt notes, at the beginning of the twentieth century, Higgs used a similar line of reasoning to Mayhew, also describing the poor as a nomadic people left over from an earlier stage of evolution. Although Mayhew's (and Higgs') racialised understanding of the poor, with their 'protruding jaws', seems distasteful from the perspective of the twenty-first-century

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[180] Nord, 'The Social', p.122.
[181] Wohl, 'Social', pp.88 & 91.
[182] Ibid. Nord also argues that Mayhew was 'the real master' of the anthropological analogy in the 1850s. Nord, 'The Social', pp.127-128.
[183] Wohl, 'Social', pp.88-89.

historian, we must not disregard Mayhew's (and Higgs') use of anthropological theory too hastily.[184]

Nord suggests that the use of an anthropological rhetoric comparing sections of the Victorian British working classes to the uncivilised peoples of foreign lands changed meaning and significance over time as it was employed by different social explorers.[185] London and Sims both used the anthropological analogy in their texts but, for Nord, they used it with little anthropological intent: it was little more than a clever and ironic rhetorical strategy for them.[186] Mayhew's use of the analogy differed qualitatively from that of his contemporaries and later social explorers. According to Nord, he used it in a more purposeful and sustained way, not merely as a rhetorical strategy, but from the standpoint of an anthropologist.[187] In using the anthropological analogy, what Mayhew was doing was to treat the street-folk of London as an ethnologist observing foreign tribes, acknowledging their cultural difference.[188] As Brunt suggests, Higgs put forward a similar theory to Mayhew's to help explain vagrancy in Britain. Just as Nord suggests that anthropological value can be found in Mayhew's work, social scientific value can similarly be found in Higgs' *Glimpses*.

Higgs proposed a theory of racial psychology and devolution which she used to explain vagrancy 'on a scientific basis'. In the opening pages of *Glimpses,* Higgs proposed what she described as a 'very simple theory' which was elucidated in more detail in one of her other publications. She explained that the psychology of the individual retraced the path of the psychology of the 'race' and that some social groups become 'permanently stranded on lower levels of evolution' from the 'foremost classes or races'. These psychological characteristics were not innate but manufactured by undesirable social conditions which caused 'widespread devolution'. This devolution can only be reversed through 'wise social legislation' to change the environment of the stranded social groups.[189] Higgs went on to explain elsewhere in her account how the specific circumstances of the 'industrial revolution' had shaped the specific contours of late Victorian and Edwardian vagrancy.[190] Once this theory was in place and our understanding of society was no longer 'purely empirical', Higgs suggested that the sciences of sociology

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[184] Brunt, 'The Ethnography', pp.82-83.
[185] Nord, 'The Social' p.122.
[186] Ibid., pp.124-125.
[187] Ibid, pp.127-128.
[188] Ibid., p.130.
[189] Higgs, *Glimpses*, pp.ix-xi.
[190] Ibid., pp.284-302.

and psychology would be reduced to 'scientific order'.[191] In this sense, Higgs' work had a theoretical basis which she assumed was a key marker of being scientific.

Higgs' use of theory drawing on evolutionary, psychological and socio-economic factors to explain vagrancy in Edwardian Britain marks her ethnographic work out as being particularly social scientific rather than purely literary. Neither Webb nor Goldsmid developed such a complex explanatory framework in their ethnographies. Nevertheless, there is one aspect of the work of all three which again emphasises the social scientific nature of late Victorian and Edwardian social exploration. Keating observes that

> time and again the explorers emphasize that poverty dictates how people behave and that it is meaningless to try to bring to bear easy middle-class morality…The acknowledgement of patterns of behaviour in many respects quite different from their own does bring the role of the social explorers nearer to that of the anthropologist: again and again they suspend moral judgement and their own deeply-held beliefs in the more crucial cause of human understanding and compassion.[192]

There is evidence of this social constructionist view of poverty in the work of Webb, Goldsmid and Higgs. Webb listened to some of the younger sweatshop workers discussing their boyfriends and social life, and concluded that

> You cannot accuse them of immorality for they have no consciousness of sin…They live in the Garden of Eden of uncivilised life; as yet they have not tasted the forbidden fruit of the Tree of Knowledge of Good and Evil, and the heaven and hell of an awakened conscience are alike undreamt of.[193]

Goldsmid adopted a similar social constructionist perspective on the condition of the vagrants he observed, although he expressed it in much less flowery language. He posed the question:

> Are the lodging-houses unwholesome and filthy because the tenants are so; or are the condition and habits of the dosser to be ascribed to the squalor and insanitary condition of their lodgings – it were a mockery to say to their homes? I incline to the latter opinion…You cannot expect people whom custom and the law alike compel to herd and breed like swine to live like human beings.[194]

Higgs reflected that, although the prevailing idea in her 'class of society' was that tramps were 'so incorrigible, and so determined to lead a nomad existence' that the only deterrent was to severely punish vagrancy, her ethnographic experience had taught her that there were social pressures leading to vagrancy, 'an actual forcing of lives into nomad existence'.[195]

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[191] Ibid., p.xi.
[192] Keating (ed.), *Into Unknown*, pp.18-19.
[193] Potter, 'Pages', p.311.
[194] Goldsmid, *Dottings*, pp.120-121.
[195] Higgs, *Glimpses*, pp.155-156.

Other commentators have noted the tendency of social explorers to recognise and try to understand the people they observed as culturally different. Brunt says that the awareness the social explorers developed of the trials and tribulations of the poor through their shared experience led them to understand aspects of behaviour which would seem to the outsider as somehow distasteful or unfathomable. As Brunt says, social explorers often strove to explain the internal logic of aspects of impoverished life to their readership, such as prostitution.[196] In her perceptive article addressing the form of Victorian ethnographic texts, Parssinen notes that despite their range of occupations, social explorers shared a '…determination to make public an alien culture set, paradoxically, right in their midst'. In doing so, they 'charted an essential methodology in seeking first-hand knowledge of the poor'.[197]

We can see, therefore, that there are both literary and social scientific aspects of late-Victorian and Edwardian ethnography and it would be misguided to define such work categorically as either literature or social science. Such social researchers were working at a time when the social sciences were becoming defined in Britain, and the key markers of social-scientific research were becoming established. Perhaps it is more useful to think of the covert social explorers occupying a continuum between the endpoints of literature and social science, without being categorically one or the other.

Philanthropy and Reformism in Covert Ethnography from the 1880s to 1914

The vast majority of late-Victorian and Edwardian social researchers, including ethnographers, had a reformist or philanthropic agenda which informed their work to some extent. As Kent has argued, in this era 'the distinction between publicly campaigning for reform and systematically collecting information relevant to it was not strongly argued for'.[198] Reformism, philanthropy and social research were overlapping and, to some extent, mutually supportive activities between the 1880s and the First World War. Most social researchers studied aspects of poverty in an attempt to understand social problems and generate solutions, whilst philanthropists and reformers could use social research to give their proposals for reform a solid empirical foundation. It can be argued that a sense of class-based culpability motivated many of the middle- and upper-class responses to poverty in the late Victorian and Edwardian era. In the 1880s, Webb detected what she referred to as 'a class-consciousness of sin' amongst sections of the middle and upper

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[196] Brunt, 'The Ethnography', p.80.
[197] Parssinen, 'Social', pp.205-206.
[198] Harrison, *Peaceable*, p.304.

classes. This sense of guilt often motivated such individuals to dedicate their means and strength to the reorganisation of society on a more egalitarian basis.[199] This same notion of culpability which engendered reformist and philanthropic responses towards the poor can also be applied to the practice of social research at this time. The work of social explorers, according to Keating, was 'as much inwards as outwards' where middle-class researchers displayed his or her 'own guilty conscience'.[200]

Goldsmid clearly understood the relationship between research, reform and a sense of class-based responsibility for the continued existence of poverty amidst plenty. His philanthropic work and subsequent research betrayed his own 'class-consciousness of sin' but Goldsmid also demonstrated how researchers could capitalise on the guilty feelings of their audiences to stimulate further research and reform. As noted above, in *Dottings*, he reflected that the press coverage of Mearns and Sims' work had thrown light upon appalling scenes of poverty, stirring people's hearts and moving them to action and Goldsmid hoped to engender the same reaction with his account of common lodging houses. After a Royal Commission had been established and legislative change made in the aftermath of the press uproar caused by Mearns' and Sims' work, Goldsmid wrote that 'the public conscious was salved' and their interest subsided.[201] He perceived a link, therefore, between research, reform and the notion of middle-class responsibility and guilt on the part of the researcher and the reader.

Kent suggests that it was, in fact, their shared interest in social reform which united late Victorian and Edwardian empirical sociologists, not their commitment to an academic discipline. Although Kent argues that this reformism should not necessarily invalidate the contribution these researchers made to sociology, the overlapping of research, reform and philanthropy in their work has called their social scientific status into question.[202] It has been suggested that because researchers, such as Webb, 'always only had one eye on research – and the other on the Government of the day', a tradition of 'university sociology' did not flourish. Instead, we have had 'a freelancing, extramural, line of inquiry and proposal'.[203] Thus, the reformist element of pre-1914 social research is characterised as a barrier to the development of academic sociology. Furthermore, when the social sciences did progressively become academicised and professionalised, the link between

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[199] Webb, *My Apprenticeship*, pp.194-195.
[200] Keating (ed.), *Into unknown England*, p.29.
[201] Goldsmid, *Dottings*, p.7.
[202] Kent, *A History*, pp.8-9.
[203] Webb, *My Apprenticeship*, p.22.

research and reform was undermined.[204] We can see, therefore, that the presence of a reformist or philanthropic agenda is one of the ways that amateur social research has been distinguished from the professional social research which was beginning to emerge in the early nineteenth century. Here, we will explore the role of reformism and philanthropy in late Victorian and Edwardian covert ethnographic research. First, we need to look at the practical relationship between philanthropy and ethnographic fieldwork. Second, we will discuss reformism in ethnographic texts.

Philanthropic work provided the opportunity for social research. It was his philanthropic work 'with one of those grand East-end institutions which undertake the rescue of destitute gutter-children' which first allowed Goldsmid to come into contact with those who frequented common lodging houses, the institutions he would go on to research.[205] For Webb and Higgs, philanthropic work *was* social research. Before she wrote 'Pages', Webb had spent two years living and working as a rent collector in the Katharine Buildings in London, a working-class tenement block built by philanthropists and she remarked that 'this occupation was certainly well fitted to form part of my apprenticeship as a social investigator'.[206] In the preface to *Glimpses*, Higgs made it clear that she made little distinction between her philanthropic work and social research. Higgs implied that her 'rescue work' with destitute women was part and parcel of her 'social research'. Higgs provided accommodation and 'remedial treatment' for women vagrants and described the cottage where the women stayed as a 'social microscope' where every case was 'personally investigated'. As her rescue work expanded into new areas, so did her opportunities for social observation.[207] Thus, in a sense, philanthropic work was a form of observational social research.

In turn, ethnographic fieldwork provided opportunities to be philanthropic. In Goldsmid's account of his visit to one particular lodging house, he recalled chatting to a man with a two-year-old child in the kitchen. It transpired that his wife and youngest child had gone to beg money from relatives to pay their night's lodging but had been unsuccessful. Goldsmid implied that he gave them enough money to cover their night's shelter.[208] Higgs' ethnographic fieldwork also provided opportunities for philanthropic endeavour which she seized on a number of occasions. During one ethnographic episode, Higgs and her companion tried to help various young and distressed women whom they

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[204] Freeman, ''Journeys'', pp.103-104.
[205] Goldsmid, *Dottings*, pp.8-9.
[206] Webb, *My Apprenticeship*, pp.31 & 266-267.
[207] Higgs, *Glimpses*, pp.v-vi.
[208] Goldsmid, *Dottings*, p.33.

believed could be rescued from the vicious circle of vagrancy and prostitution, telling them where they might receive shelter and support, and contacting their parents.[209] Of course, there was the danger that such acts of charity and salvation would jeopardise the researcher's covert status. Higgs explained their inability to help an unfortunate girl directly 'because of our *incognito*'.[210] It was difficult to 'struggle for [a] girl's salvation…without exciting suspicion' when Higgs and her companion were disguised as lodgers themselves.[211] So, we can see not only that philanthropy constituted a form of social research for some late Victorian and Edwardian social researchers, but also that their covert ethnographic fieldwork presented opportunities for researchers to act on their philanthropic impulses. The mutually reinforcing relationship between philanthropy, reformism and social research is strengthened further when we consider the extent to which some covert ethnographers used their texts as vehicles for the promotion of their proposed solutions to social problems.

Keating has noted that in the Victorian and Edwardian period both overt and covert social explorers tended to advocate state-led social change rather than individualist and paternalist solutions.[212] Higgs certainly advocated reform at state level to tackle the problem of accommodation for destitute women frequently and forcefully throughout *Glimpses*. Higgs' ethnographic study of the vagrants is inextricably bound up with her concern over the state of accommodation for vagrants, a critique of current legislation and suggestions about how it might be improved. Sometimes Higgs included her calls for reform in the midst of the narrative sections of her ethnographic text. For example, during her account of *Three Nights in Women's Lodging Houses*, Higgs mused on the plight of women vagrants whilst she sat outside a park waiting for her companion after they had spent the night in a lodging house. She called for 'national provision' of shelter for women as well as 'national recognition of the right of the individual to employment subsistence'.[213] In the penultimate paragraph, Higgs reiterated her reformist agenda when she called for the provision of 'suitable and sufficient women's lodging houses'.[214] Many of the non-ethnographic elements of *Glimpses* were also concerned primarily with what legislative changes should be made with reference to homelessness amongst women. In her prize-winning essay which was based on her ethnographic findings, Higgs discussed how

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[209] Higgs, *Three Nights*, pp.10, 14, 18 & 19.
[210] Ibid., p.10. Emphasis in original.
[211] Ibid., p.13.
[212] Keating (ed.), *Into Unknown England*, p.19.
[213] Higgs, *Three Nights*, pp.12-14.
[214] Ibid., p.23.

other European countries had framed their vagrancy legislation.[215] She also looked at what legislation she thought was necessary in Britain, referring to the status quo as 'a radically wrong vagrancy system' and advocated the establishment of labour colonies.[216] Higgs had published separate pieces of work which advocated social reform before she assembled her ethnographic text *Glimpses*, including *How to Deal with the Unemployed* in 1904, and she referred to this reformist text in *Glimpses*.[217] As well as proposing her own legislative changes, Higgs frequently engaged in critiquing the findings and recommendations of government research into homelessness. Her references to the Departmental Committee on Vagrancy and the inclusion of some of its findings in the appendices confirm that Higgs was overwhelmingly motivated by the need to solve a social problem, not just to describe a social scene or understand a social group.[218]

In 1906, William Beveridge, who would later become a distinguished social reformer and economist, reviewed Higgs' *Glimpses* for the *Economic Journal*. Beveridge said relatively little about Higgs' method and devoted much more effort to discussing her proposed reforms, and expressing his own perspective on remedying homelessness. Further bolstering the link between research and reform, Beveridge commented that 'Mrs Higgs's revelations can hardly fail to hasten the reform of the present treatment of vagrancy'.[219] Beveridge made one other observation about Higgs' work which points towards a link between reformism and ethnographic social research in particular. He suggested that her research method may have prevented her from accepting the less eligibility principle to some extent. Higgs, he wrote, 'has so much entered into the vagrant's own point of view that she is a little unthinking both in her praise of such comfort as she found in the shelters, and in her criticism of the harshnesses she endured in the casual wards'.[220] Beveridge was suggesting that by using covert methods to become a vagrant temporarily, Higgs became too empathetic with vagrants. She was unable to maintain the distance between herself and her research subjects which would have allowed her to make objective and dispassionate observations about vagrancy. Freeman notes that modern commentators on social scientific methodology have recognised the danger of participant observers 'over-identifying' with the studied group and developing 'over-rapport' with them. It is thought that if the modern participant observer enters this state, they are sometimes tempted to engage in advocacy on behalf of the studied group which, if neutrality is considered to be ideal, distorts the

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[215] Higgs, *Glimpses*, pp.54-64.
[216] Ibid., pp.70-82. For discussion of labour colonies, see: Ibid., p.309-311.
[217] Higgs, *Glimpses*, p.71. Mary Higgs, *How to Deal with the Unemployed*, (London: C.S. Brown, Langham, 1904).
[218] See, for example: Higgs, *Glimpses*, pp.74 & 75, 305-308 & 312-315.
[219] W.H. Beveridge, 'Review: *Glimpses into the Abyss* by Mary Higgs', *The Economic Journal*, Volume 16, Number 64, (1906), p.583.
[220] Beveridge, 'Review', p.583.

investigation.[221] Although most late Victorian and Edwardian social researchers mixed research with reformism, the danger of 'over-identification' was something peculiar to researchers using ethnographic methods. This meant that they were particularly open to charges of impartiality in their advocacy of reform. The particularly strong link between the use of covert ethnographic methods and advocating for legislative change may have aided the marginalisation of such research. As social research became increasingly professional and regulated after 1914, supposedly scientific standards of objectivity and neutrality were promoted which ran contrary to the reformist agenda of earlier research.

Goldsmid, like, Higgs, had a strong reformist streak running throughout his ethnographic text. He did, in fact, construct *Dottings* as a call for reform rather than a work of social research. In the brief dedication which prefaced the book, Goldsmid dedicated his book 'to the public – to the public which feels, the public which reflects. Which feels for the miseries and sufferings of our poorer brethren; which reflects upon the causes that produce and intensify, and the methods which may alleviate or remove them'. He challenged the reading public to 'do its duty' and 'rescue the denizens of the common lodging-houses' which the law had permitted or even encouraged to exist. Goldsmid's wrath and reformist zeal were directed squarely at the legislature of the day. He clearly blamed 'the mischievous operations of inadequate laws, and the selfish ineptitude…of those who are charged with the administration of such laws' for the state of lodging houses, not the lodgers themselves. Their immorality was a result of this maladministration and 'intense poverty'.[222] He dedicated a whole chapter to summarising and criticising the law as it related to common lodging houses.[223] The subsequent chapter is devoted to outlining Goldsmid's practical recommendations for the improvement of such institutions.[224] Goldsmid was particularly keen in his narrative accounts to draw the reader's attention to the certificates government inspectors had placed on the bedroom walls in lodging houses which stated how many people were permitted to sleep in any one room. In one doss house bedroom, he marvelled at the incompetence of an inspector who had, according to one certificate, permitted five persons to sleep in a room where no more than four beds could possibly have been squeezed in.[225]

As we have already mentioned, Goldsmid was attempting to build on the public outcry caused by Mearns and Sims' work and garner a public reaction to his research

[221] Freeman, '"Journeys"', p.102.
[222] Goldsmid, *Dottings*, p.3.
[223] Ibid., pp.108-119.
[224] Ibid., pp.120-131.
[225] Ibid., pp.38 & 95-96.

which would push forward social reform. He recognised, however, that the public interest in poor housing conditions had not been sustained and that 'the sympathy excited, which has been more sentimental than real, went to sleep once more. Its slumbers will probably last until the curtain which shrouds the only partially depicted scenes of London wretchedness be lifted with a ruder hand, and the 'bitter cry' sound more bitter and perhaps more menacing'.[226] This suggests that if research findings were to excite the sympathies of the reading public who would in turn force the legislature to respond, then the research must be written in such a way as to shock and disgust the reader, spurring them to action. Thus, the reformist agenda of other ethnographic researchers may be connected to their use of a more dramatic and sensationalist style. Webb implied that there was a link between philanthropic tendencies and the written style of research. Referring to her first and non-ethnographic article on sweating in the tailoring trade, Webb noted in her diary that this article would be 'too matter-of-fact for the taste of the public…not sufficiently flavoured with philanthropy'.[227]

Even Webb's ethnographic text did not have anywhere near as strong an emphasis on reform or the empathetic tone found in Goldsmid and Higgs' work. In 'Pages', Webb did not explicitly suggest that reform was needed. In fact, she commented that the mistress of the workshop kept 'the regulations exactly' and had 'nothing to fear from the factory inspector'.[228] Her account did, however, end on a note of socio-economic injustice when Webb observed that one of her fellow conscientious trouser-hands had worked herself into an 'intense weariness' and yet would be paid one fifth of the amount earned by the lazy and troublesome male pressers.[229] It is difficult to explain the relative absence of reformism from Webb's ethnographic work. Writing in the 1880s, Webb had considered 'politics, philanthropy and statistical investigation' in the same sentence, and she understood how 'class-consciousness of sin' compelled the middle and upper classes to engage in philanthropy and research alike.[230] She had, however, become disillusioned with philanthropy after working with the Charity Organisation Society among the poor of Soho in the early 1880s.[231] Webb's co-operation with government inquiries and her socialist outlook suggest that she would nonetheless support social reform. Perhaps the best explanation is that when she was writing 'Pages', Webb was overwhelmingly concerned to write a cohesive and satisfying 'story' about being an undercover trouser-hand, and

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[226] Ibid., p.7.
[227] Webb, *My Apprenticeship*, p.323.
[228] Potter, 'Pages', p.309.
[229] Ibid, pp.313-314.
[230] Webb, *My Apprenticeship*, p.188.
[231] Davis, 'Beatrice Webb', p.3.

breaking off from this story to advocate reform would simply have interrupted the flow of her narrative.

Covert Ethnography and Other Methods Research from the 1880s to 1914

Between the 1880s and 1914, covert social explorers were operating as part of a crowded and diverse social research scene and we must consider their work within this wider context of late Victorian and Edwardian social research. A considerable amount of social research in Britain during this time was undertaken in an official capacity, and it was also during this era that many individuals now considered pioneers of empirical sociology were active researchers.[232] As Brunt suggests, historians interested in the social history of this period will probably be familiar with the output of various Royal Commissions and the work of individual pioneers such as Booth, Rowntree and the Webbs. However, there is much less historical awareness of the widely practised popular genre of social research. Operating at the same time as these other dominant forms of research, independent social researchers, including many covert social explorers, produced their own research which appeared in the press, magazines and as popular books and pamphlets.[233] By 1914, the social research scene in Britain was dominated by individuals such as Booth and Rowntree who used quantitative empirical methods to understand society but, as Keating suggests, this does not mean that the tradition of social exploration had died out.[234] In this section, we will explore the relationship between covert ethnography and other research methods in late Victorian and Edwardian Britain and attempt to understand how researchers navigated between methodologies.

Catherine Marsh emphasises the idea of linear change in the deployment of social survey methodology over time and, around the turn of the twentieth century, she locates a significant shift in the conceptualisation of research subjects. She suggests that 'informant' methods of inquiry, where the social researcher relied on the testimony of third parties about the research subjects, were giving way to 'respondent'-orientated work in which social researchers consulted the research subjects directly.[235] Although Marsh focuses on social surveys, we can usefully apply her ideas to other types of social research. Freeman suggests that Booth's poverty survey of London, where school attendance officers were

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[232] Kent, *A History*, p.7. Kent suggests that Booth's inquiry was unparalleled in its time and in many ways it has not been surpassed by contemporary sociologists: Kent, *A History*, p.6.
[233] Brunt, 'The Ethnography', p.78.
[234] Keating (ed.), *Into Unknown England*, p.30.
[235] Catherine Marsh, 'Informants, Respondents and Citizens', in Martin Bulmer (ed.), *Essays on the History of British Sociological Research*, (Cambridge: Cambridge University Press, 1985)', pp.214-217.

asked about poverty rather than the poor themselves, was the epitome of the 'informant' method of inquiry.[236] Marsh links this shift to the changing political and social status of the working classes, from which the groups being studied were usually drawn, and to the desire to collect a different kind of information which required face-to-face interaction between the social investigator and the population under investigation.[237] There are some problems with Marsh's thesis when applied to late Victorian and Edwardian social research in general. Freeman has pointed out that Marsh may have overestimated the element of transition from the use of informant to respondent methods at the end of the 1880s, suggesting that the choice of method 'remained contestable', with many social investigators in the early twentieth century continuing to mistrust the use of first-hand evidence.[238] It could also be added that even as early as the 1880s when Marsh's model suggests that informant methods would be in the ascendancy, covert social explorers were using a method which approximated to the 'respondent' model.[239] The point is that a linear model of change over time does not seem to work when we try to apply it to the history of covert social exploration.

Historians often imply that the development of social investigation in Victorian and Edwardian Britain has been the story of the 'progressive abandonment' of so-called journalistic methods in favour of systematic quantitative social research.[240] This is clearly not the case and, although quantitative survey-style research may have been culturally dominant around the turn of the twentieth century, covert ethnographers and other qualitative social researchers continued to operate. Eileen Yeo and Freeman have both detailed the extent to which the territory of social research was subject to contestation prior to 1914, and many covert ethnographers were aware of the epistemological implications of their research methodology.[241] Around 1900, social research was increasingly dominated by work organised on a more 'scientific' and statistical basis but this does not mean that the covert social explorers disappeared. As Freeman has suggested, the parallel tradition of participant observation continued and evolved at the same time as other more quantitative methods of research were being honed.[242] It would be much more helpful to think about the history of research methods at this time not in a linear fashion, where one replaces the

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[236] Freeman, 'Folklore', p.52.
[237] Marsh, 'Informants', pp.214-217.
[238] Freeman 'Folklore', p.53.
[239] Covert ethnography could, however, be thought of as the ultimate 'informant' method whereby the reality of a way of life is filtered through one omnipotent informant – the ethnographer.
[240] Freeman, *Social*, p.183.
[241] Eileen Janes Yeo, *The Contest for Social Science: Relations and Representations of Gender and Class*, (London: Rivers Oram Press, 1996); Freeman, *Social*.
[242] Freeman, '"Journeys"', p.99.

other, but rather by emphasising the co-existence of research methods as they developed in syncopation and contestation with each other.

For example, Goldsmid did not consider his own covert ethnographic work in isolation from that of other social researchers. Whilst distancing himself from the dishonourable occupation of slumming, he clearly saw his own covert research into the state of common lodging houses as being in the tradition of crusading journalism, consciously linking his work with that of Mearns and Sims. Furthermore, he associated the establishment of a Royal Commission on the state of housing in the 1880s and subsequent legislation with the public outcry that the work of Mearns and Sims had caused.[243] For Goldsmid, there appeared to be no doubt that official researchers were aware of and responded to other, more popular, and perhaps populist, manifestations of social research in the press and elsewhere. Even the pioneering quantitative survey researcher Booth was not averse to trying out other research methods. Booth spent three periods of several weeks living with poor families in the East End of London. Although Booth claimed that his experiences had given life to the 'dry bones' of his research, he did not consider these episodes as social research in their own right and he did not fully incorporate his ethnographic experience into his research findings.[244] This is perhaps because, as Parssinen has noted, Booth pledged not to make use of a fact without quantitative value.[245]

Although now marginalised by historians of social research, turn of the twentieth century social explorers 'enjoyed a high profile and some respect within the investigative community' and some covert ethnographers were called upon to share their knowledge with the government's own social researchers.[246] Higgs appeared before the Committee on Vagrancy as a witness after she had undertaken some of her many covert investigations of workhouse causal wards and lodging houses.[247] Following her research into clothing sweatshops, Webb appeared before the Lords Committee on the Sweating System.[248] There does, therefore, appear to have been a degree of co-operation and mutuality between covert ethnographers and other late Victorian and Edwardian social researchers, and Freeman has noted that many contemporaries recognised the parity of what social surveyors and social explorers were doing, as being part of a larger social research enterprise.[249]

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[243] Goldsmid, *Dottings*, p.7.
[244] Parssinen, 'Social', pp.209-210.
[245] Ibid., p.210.
[246] Freeman, '"Journeys"', p.99.
[247] Higgs, *Glimpses*, p.xi.
[248] Webb, *My Apprenticeship*, pp.325-329.
[249] Freeman, '"Journeys"', pp.116-117.

There was, however, also a considerable degree of contestation between social researchers over their methodological positions. Some social researchers who used either overt or covert ethnographic methods showed their open hostility towards those who used social survey methodology. Denis Crane, for example, an Edwardian covert ethnographer, denounced 'sociological students' and, similarly, Josiah Flynt and Stephen Reynolds expressed contempt for academic sociologists and their research.[250] Wohl notes too that there was a tendency amongst mid-nineteenth-century social explorers to scorn the contemporary craze for statistics, even whilst they made use of them in their own work. John Hollingshead reportedly said that '[p]ercentages, averages, and all the hocus-pocus of statistics' were 'only mists, fogs, curtains and sleeping-draughts, except to the official mind and we, the public, require something more gross – and more palpable.'[251] The relationship between covert ethnography and other methods of social research between the 1880s and 1914 was clearly a complex one involving interaction, co-operation and contestation.

Ross McKibbin has suggested that the period under discussion was one of contestation between quantitative and qualitative research and, with the publication of Booth and Rowntree's landmark poverty surveys, the hegemony of quantitative social research was consolidated. As McKibbin argues, the success of Booth and Rowntree's work established the fashion for numbers and thus 'ensured that most future enquiries into social class would be quantitative ones'.[252] Concomitantly, it is in the late Victorian and Edwardian period that we begin to see the marginalisation of covert ethnography as a methodology. McKibbin points out that the apparent triumph of the quantitative survey discredited research based on personal observation.[253] Nevertheless, covert social exploration continued to be employed as a methodology until, and beyond, 1914.[254] By taking a closer look at the very different relationships that Higgs and Webb had respectively with the covert ethnographic method, we should get closer to understanding the role that this methodology played in the world of late Victorian and Edwardian social research.

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[250] Ibid., p.115. [Walter Thomas Cranfield] Denis Crane, *A Vicarious Vagabond*, (1910); [Josiah Flynt Willard], Josiah Flynt, *Tramping with Tramps: Studies and Sketches of Vagabond Life*, (London: T. Fisher Unwin, 1900); Stephen Reynolds, *A Poor Man's House*, (London: Bodley Head, 1909).
[251] Wohl, 'Social', p.88. John Hollingshead, *Ragged London in 1861*, (1861).
[252] Ross McKibbin, 'Social Class and Social Observation in Edwardian England', *Transactions of the Royal Historical Society*, Volume 28, (1978), p.175.
[253] McKibbin, 'Social', p.175.
[254] See, for example: London, *The People*; Mary Higgs, *Three Nights* & Holdenby, *Folk*. The post-1914 era will be dealt with in subsequent chapters.

Higgs, one of the most prolific covert ethnographers, was not entirely unacquainted with other methods of research. In her preface to *Glimpses*, she outlined the progress of her research into vagrancy which had eventually led to her use of the covert ethnographic method. Higgs had, of course, used her philanthropic work with women vagrants as an opportunity for observation and experiment, but she had also undertaken research by 'investigating remedial agencies', 'interrogating social observers' and other experts, and had visited relevant institutions, gradually obtaining a mass of information.[255] However, even after this substantial amount of research 'the root problems of poverty remained dark' to Higgs. Turning at last to covert social exploration, she 'became convinced that nothing but accurate and scientific exploration of the depths' would reveal the truth and she became convinced of 'the uselessness of any other methods'.[256] For Higgs, despite her extensive experience of other research methods, social exploration was the pre-eminent and most useful research tool. Nonetheless, she made extensive and critical use of information gathered by alternative research methods. Higgs drew on statistical research on vagrancy, quoting figures from a number of sources including those gathered by interested individuals such as 'an expert workhouse official' and a Mr Fox from Somerset who had 'for a long time taken pains to observe the tide of vagrancy flowing through his union' and those collected for the Departmental Committee on Vagrancy.[257]

Whilst Higgs drew on the available statistics to make her case, she also pointed out that some statistics could be 'misleading' and that national statistics on vagrancy were 'comparatively useless'.[258] As we have already mentioned, Higgs was willing to co-operate with government-sponsored social researchers and she shared her knowledge of vagrancy with the Departmental Committee on this issue. She also drew on the findings of the Departmental Committee liberally in her footnotes and included summaries of their findings in her appendices.[259] However, she was not reticent about criticising the Committee's report and disagreeing with its findings.[260] All things considered, Higgs could see merit in both official social research and the covert observations of individuals:

> Investigations from the official point of view are interesting and instructive, and, if conducted in a scientific spirit, would eventually be of great value in solving social problems. But in the present confused state of things there is also special value in the observations of witnesses who, by descending into the abyss, explore its conditions, and form an independent judgement.[261]

[255] Higgs, *Glimpses*, pp.v-vi.
[256] Higgs, *Glimpses*, p.vii.
[257] Ibid., pp.17, 18 & 19.
[258] Ibid., pp.17, 29 & 32.
[259] For references to the Vagrancy Report in footnotes see, for example: Ibid, pp.28-29. For appendices relating to the Vagrancy Report, see: ibid., pp.305-308 & 312-315.
[260] Ibid., pp.9 & 10.
[261] Ibid., p.23.

Nevertheless, Higgs contrasted her work with that of the government by suggesting that '[l]ittle short of a revolution may be made in preconceived opinion by actual experience'.[262] In short, although she could recognise the potential utility of other research methods and was willing to use such data critically, Higgs became a champion of the covert ethnographic method. Webb, on the other hand, had a very different relationship with this research method.

Webb's career as a social researcher is especially interesting given that she appears to be the only late Victorian widely recognised as a social scientist who published an ethnographic text and it differed in research method and writing style to the rest of her output. The anomalous status of 'Pages' could be due to the fact that Webb was not particularly comfortable with the practicalities of covert social exploration. Ill-at-ease with her covertness, she described herself as feeling like an 'impostor' and felt 'rather hot' at the mention of the word 'spy' by a research subject.[263] Furthermore, she seemed to struggle to maintain her façade as a working-class trouser-hand. At first, Webb over-egged her faux working-class accent and, more than once, she and others noted her awkwardness.[264] The quality of her work as a trouser-hand was also suspect and she was demoted to the work usually given to 'outdoor hands'.[265] At one point, the Jewish mistress of the tailoring workshop seemed to question Webb's identity, remarking that she found Webb 'odd' and 'uncommon' on account of her voice and appearance.[266] Webb's apparent lack of affinity for covert social exploration may explain why she did not readily use the method again. There is, of course, the possibility that Webb played upon and exaggerated her unsuitability as a working-class trouser-hand in an attempt to protect and preserve her middle-class femininity.

Webb's failure to employ covert ethnography again during her substantial career as a social scientist and reformer may, however, have had epistemological roots. Webb's ethnographic work is of singular significance because it allows us to monitor how one social researcher navigated her way between research methodologies. By looking closely at Webb's ethnographic text and charting how she and others characterised this episode in her career, we can gain an enlightening perspective on how research methods were contested in Victorian and Edwardian Britain. The tension between ethnographic methods

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[262] Ibid., p.24.
[263] Potter, 'Pages', pp.301 & 309.
[264] Ibid., pp.302, 305 & 309.
[265] Ibid., pp.307, 309 & 310.
[266] Ibid., p.313.

(both overt and covert) and other research methods in late Victorian and Edwardian Britain was tightly bound up with the increasing hegemony of quantitative and survey-based research at a time when social science was emerging as a profession and an academic field.

In the mid-1880s, Webb recorded in her diary that whilst there were plenty of social commentators engaged in analysing the data collected by others, what she referred to as 'personal investigation' was required. She suggested that the '*Pall Mall [Gazette]* has started this but in the worst possible way, shallow and sensational'.[267] She recognised that journalists at this time were using a potentially valuable research method but in a 'shallow and sensational' way which she presumably associated with Stead, the editor of the *Pall Mall Gazette*, and his maverick 'new journalism'. Nevertheless, Webb continued 'puzzling over the methodology of social science' contemplating how 'personal observation' should relate to statistical inquiry.[268] An extract from her diary showed her thought process, in rough note form, on social exploration:

> Personal observation, and its liability to gross error unless checked by the statistical method. Bias in the selection of facts. The superior attraction of certain facts to certain temperaments…Tendency of personal observation to take its own experience of a class as a sample of the whole…Numerous inquiries check each other.[269]

Despite her reservations about the value of 'personal observation', without recourse to a statistical foundation and its sensationalist connotations, Webb decided to undertake her own episode of covert social exploration.

'Pages' was a literary success and its publication brought Webb 'temporary notoriety' although she dismissed it as a 'cheap triumph'. Webb was 'smarting' when her other non-ethnographic article on sweating industry which she described as a 'clear, detailed and comprehensive account of the facts' received a 'cold reception' in comparison to her short ethnographic 'picture'. Webb's friend, Marie Souvestre, suggested that the reaction to her two very different articles on sweating suggested that 'le public Anglais est bête!' and Webb was inclined to agree. To add insult to injury, the editor of the *Nineteenth Century*, which had published both sweating articles, paid Webb twice as much per page for her ethnographic text. Following the publication of 'Pages', Webb was obliged to appear before the House of Lords Committee on the Sweating System, where she was subjected to intense and unfriendly questions about her research and the related press

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[267] Webb, *My Apprenticeship*, p.274.
[268] Ibid., p.291.
[269] Ibid., pp.293-294.

coverage was harsh.[270] The response to 'Pages' and Webb's subsequent treatment at the hands of the Committee and the press seem to have persuaded her once and for all that such 'personal investigation' was inextricably associated with populist sensationalism, and that she should confine herself in the future to more respectable research methods. In 1890, just two years after 'Pages' was published, Webb noted in her diary that '[t]hough I am suspected of Socialism, my anti-sensationalism gives me a footing among the sternest school of *laissez-faire* economists. This position must be guarded jealously, if I am to be of some little use as a reforming agency'.[271] Webb clearly felt that if she was to maintain her status as a researcher and her power as an agent of reform, she must avoid the covert ethnographic method and its attendant sensationalism. Ten years after Webb had published 'Pages', it appeared under the title 'The Diary of an Investigator' in a book she and her husband had prepared entitled *Problems of Modern Industry*. The article appeared unchanged apart from the title and two footnotes which have been added.[272] It is worth quoting this first note in full because it suggests something about Webb's changing attitude over time to her own ethnographic research and, by extension, her thoughts on the role of ethnographic research within the practice of social research as a whole:

> *Nineteenth Century*, September 1888. Owing to the anonymity of East End life, and its ignorance of magazines and books, it was possible, by merely changing the names of streets and firms, to publish these few pages from my private diary without the risk of annoyance to the persons concerned. Such publication of the investigator's private notes is seldom practicable. But in spite of this inability to 'make copy', the student of any piece of social organisation will find it adds to the fulness [sic] and reality of his scientific work, if he supplements his collections of technical facts and his statistical tables, by detailed descriptions, for his own private use, of typical scenes and characters.[273]

By now, a decade after her covert ethnography had been published, Webb was denying that 'Pages' was a constructed text at all, and that it was merely a 'few pages' from her diary, more akin to field notes than publishable research. Furthermore, she cautioned other students against 'making copy' out of such 'notes'. Ultimately, Webb relegated ethnographic evidence to the status of supporting material for the real data of 'technical facts' and 'statistical tables'.

Webb's flirtation with the ethnographic method and subsequent disassociation of herself from it was symptomatic of the marginalisation of ethnography by those involved in quantitative empirical research. In contrast, Higgs championed the covert ethnographic method as the most scientific and reliable method of all. What we have demonstrated in

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[270] Ibid., pp.314 & 325-329.
[271] Ibid., p.328.
[272] Webb, 'The Diary', pp.1-19.
[273] Ibid., p.1.

this section is that late Victorian and Edwardian ethnographers did not undertake their research in a vacuum but they were part of a complex landscape of social research. Covert social explorers did not progressively give way to more quantitatively orientated researchers. Despite the fact that, by 1914, there was a clear quantitative hegemony, covert ethnographers continued to operate, sometimes in conjunction with, and at other times in opposition to, other social researchers throughout the late Victorian and Edwardian era.

Conclusion

Of all the periods in the history of covert ethnography covered in this thesis, the late Victorian and Edwardian period has the most developed historiography. Nevertheless, some significant points have emerged from the analysis of Goldsmid, Webb and Higgs' covert ethnography, particularly the complex relationship between the practice of covert ethnography and other types of social research which were just beginning to become professionalised. The complexity of this relationship was demonstrated most clearly in the contrasting experiences of Webb and Higgs. Webb's covert ethnographic project was an anomaly in her prominent career as a social scientist, which she appeared to dissociate herself from as she became increasingly involved in the process of the professionalisation of the social sciences. Webb's use of and subsequent estrangement from covert ethnography is symbolic of its marginalisation in social research, just as survey methodology began its ascendancy. Higgs, however, was a champion of covert ethnography and she saw no contradiction in her incognito vagrancy and her engagement with other research methods and data. Her marginalisation in the historiography of social researchers, however, suggests that the importance of the method, and its connection to the continued practice of covert ethnography, have not been fully realised. In the next chapter, the professionalisation of the social sciences, with which Webb's estrangement from covert ethnography was associated, gathers momentum and the social explorer's model of covert ethnography becomes marginalised as an academic model of the method emerges.

Chapter 2: Covert Ethnography from 1914 to 1945

Introduction

The historiography of ethnography, including covert ethnography, between 1914 and 1945 is patchy, with most historians of social exploration ending their analyses in 1914 or earlier. In his article, Lodewijk Brunt imposes fairly concrete time boundaries on the phenomenon of social exploration suggesting that it was a widely practised and popular genre 'from the middle of the 19th century to World War I'.[1] The latest British ethnography that Brunt makes reference to in his article is Mary Higgs's *Glimpses into the Abyss,* published in 1906.[2] In her article about Victorian ethnography, Carol Ann Parssinen locates the production of ethnographic literature quite specifically in England between 1850 and 1910.[3] In his influential discussion of social exploration in Britain, Peter Keating took 1913 as his endpoint but, unlike other historians in this field, he explained the reasoning behind stopping just before the First World War.[4] His justification for concentrating on the late Victorian and Edwardian period of social exploration was that it was during this time that an important turning point occurred in the practice of social exploration.[5]

According to Keating, it was during the first decade and a half or so of the twentieth century that the older tradition of social exploration blended into the newer techniques of sociological analysis. Concomitantly, the social explorer was displaced by the increasingly professional sociologist. This change in rationale and personnel brought about a shift, according to Keating, in terms of the tone of research. By around 1913, the 'desired pose ceases to be one of passionate involvement and arduous physical exertion, and is replaced by calm, studied, "scientific" objectivity'. Keating captures the sense in which the emphasis in social research was on cataloguing, estimating and quantifying by the outbreak of the First World War.

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[1] Lodewijk Brunt, 'The Ethnography of "Babylon": The Rhetoric of Fear and the Study of London, 1850-1914', *City and Society: Journal of the Society for Urban Anthropology*, Volume 41, (1990), p.78.
[2] Brunt, 'The Ethnography', pp.86-87.
[3] Carol Ann Parssinen, 'Social Explorers and Social Scientists: The Dark Continent of Victorian Ethnography', in Jay Ruby (ed.), *A Crack in the Mirror: Reflexive Perspectives in Anthropology*, (Philadelphia: University of Pennsylvania Press, 1982), p.205.
[4] Peter Keating (ed.), *Into Unknown England, 1866-1913: Selections from the Social Explorers*, (Glasgow: William Collins Sons and Co., 1976), pp.9-31.
[5] Keating (ed.), *Into Unknown England*, pp.9-10.

However, Keating also points out that the advent of 'more sophisticated sociological methods did not make the traditional social explorer obsolete. Instead they acted as a help and inspiration, providing him with trustworthy information and an up-to-date terminology'.[6] Although the increasing dominance of the sociologist as social researcher in the very early 1900s seemed likely to push the social explorer to the point of extinction, Keating points out that the tradition of social exploration continued. According to Keating, this is because quantitative social research 'leaves many problems unsolved' and the (re)discovery of shocking social problems 'gives fresh impetus to the spirit, mood and language of social exploration.'[7]

Perhaps most historians of social exploration end their accounts before the Great War because they imagine that sociology and the other social sciences came to dominate the social research scene so completely around this time that social exploration simply became absorbed by it. Of course, this was not the case, as Keating recognises, and a variety of academic and non-academic researchers continued to use ethnographic methods throughout the twentieth century. It was in the 1930s, for example, that George Orwell used ethnographic methods to explore unemployment and poverty, covertly in *Down and Out in Paris and London*, and overtly in *The Road to Wigan Pier*.[8] A tradition of non-academic social research persisted throughout the period, but in the shadow of, under the influence of and, in some cases, in opposition to, the growing dominance of academic social research.

Despite Keating's description of the post-1914 era being dominated by the ascendancy of professional sociology, in his account of the history of academic empirical social research in Britain, Raymond Kent characterised the period after 1914 as being generally quiet and bereft of any great leaps forward in terms of methodology. He suggests that interwar empirical sociology offered only 'elaborations and extensions' to established sampling procedures and replications of pre-1914 studies.[9] Even social researchers working during this period recognised a sense of inertness in the use of research methodology. Writing in 1942, social psychologist Marie Jahoda, one of the covert ethnographers featured in this chapter, noted that although the outbreak of the Second World War had encouraged the production of an abundance of social research, the quality

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[6] Ibid., p.27
[7] Ibid., pp.30-31.
[8] George Orwell, *Down and Out in Paris and London*, (London: Victor Gollancz, 1933);George Orwell, *The Road to Wigan Pier*, (Harmondsworth: Penguin, 1969).
[9] Raymond Kent, *A History of British Empirical Sociology*, (Aldershot: Gower. 1981), p.103.

of this research had not advanced at all.[10] Survey work continued to dominate the social research scene in Britain from 1914 to 1945, with a definite trend in the 1930s towards the production of regional social surveys.[11] However, despite this emphasis on continuity, there were significant developments within the academic social sciences at this time.

The period 1914 to 1945 represents an important episode of transition in the history of covert ethnography and our account of this practice must be situated in the context of a number of developments which will be explored in turn. First, the establishment of Mass-Observation (M-O) in the 1930s, a research organisation which had a turbulent relationship with British academia at this time, will be discussed. M-O is important in this context because of its association with anthropological ethnographic method which, as we will discuss, had a noticeable influence on the practice of covert ethnography in Britain during this period. Furthermore, one of the covert ethnographies which will be analysed in detail in this chapter, *War Factory*, was published by M-O.[12] Second, developments in academic anthropology, including professionalisation and methodological advances, will be discussed at some length, because, as suggested above, of the importance of this discipline in shaping the use of covert ethnographic method in Britain. Third, the professionalisation, academicisation and institutionalisation of the social sciences more generally in Britain at this time will be discussed. Fourth, particular attention will be paid to the development of a methodology within the science of social psychology, 'functional penetration', which, when practised covertly, approximated covert ethnography. The development of this method can be linked to the anthropological model of ethnographic research and it was used by Jahoda, one of the researchers whose work will be featured in this chapter.[13]

In the previous chapter, and in the subsequent three chapters, the work of the covert ethnographers is discussed thematically. In this chapter, however, the work of five covert ethnographers will be analysed in turn. In the first chapter, the covert ethnographers discussed were all broadly part of the same tradition of covert social exploration, and, although we noted the emergence of an increasingly professional ethnographic model, this was not yet fully established. Similarly, in chapters 3, 4 and 5 we will focus on either academic or non-academic covert ethnography. In contrast, the covert research projects

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[10] Marie Jahoda, 'Socio-Psychological Methods of Fieldwork', *Man*, Volume 42, (1942), p.115.
[11] Kent, *A History*, pp.103 & 113. On regional social surveys, see: A.F. Wells, *The Local Social Survey in Great Britain*, (London: Allen & Unwin, 1935); D.C. Jones (ed.), *The Social Survey of Merseyside*, 3 Volumes, (Liverpool: Liverpool University Press, 1934).
[12] Mass-Observation, *War Factory*, (London: The Cresset Library, 1987).
[13] Marie Jahoda, 'Some Socio-Psychological Problems of Factory Life', *British Journal of Psychology*, Volume 31, (1941), pp.161-206.

featured in this chapter represent different points on a continuum between non-academic and academic examples of covert ethnography and it therefore makes sense to discuss them in turn. The first, Hugh Massingham's covert account of a working-class community, can be located quite firmly in the tradition of covert social exploration. The second, Celia Fremlin's account of incognito domestic service, could also be considered an example of non-academic covert social exploration, although her association with M-O complicates such a categorisation of her work. The third project, M-O's *War Factory*, for which Fremlin conducted the covert fieldwork, occupies an awkward space between the tradition of social exploration and the model of professional covert ethnography which was becoming established at the time. The third and fourth examples, Jahoda's covert study of factory work and Edward Wight Bakke's covert account of unemployment, can be firmly located within the camp of academic covert ethnography.[14]

Mass-Observation and an Anthropology of Britain

Founded in 1937 by Charles Madge and Tom Harrisson, M-O has been referred to as one of the most 'interesting and curious episodes in the inter-war years' in terms of empirical social research.[15] Although the vast majority of M-O's research output is not directly relevant to this thesis, it is important to take account of the organisation because of its relationship to anthropology (and to academia in general) and its use of method. One of the founders of M-O, Harrisson, was a 'larger-than-life ornithologist-cum-anthropologist' who undertook his own anthropological fieldwork and published his findings on the cannibals of the New Hebrides in 1937 under the title *Savage Civilisation*. When Harrisson returned to Britain, it is reported that he was struck by the notion that 'the things he had been doing in the Pacific had not been done within these shores'.[16] He suggested, using 'black' and 'white' to represent 'savage' and 'civilised' respectively, that the '*anthropological* method, freely applied to black people, can be illuminating among white. It has scarcely begun to be applied here at home…The statistical obsession among sociologists obscures the parallel'.[17]

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[14] Hugh Massingham, *I Took Off My Tie*, (London: William Heinemann Ltd., 1936); Celia Fremlin, *The Seven Chars of Chelsea*, (London: Methuen, 1940); M-O, *War Factory*; Jahoda, 'Some Socio-Psychological Problems'; Edward Wight Bakke, *The Unemployed Man: A Social Study*, (London: Nisbet, 1933).
[15] Kent, *A History*, p.117.
[16] Penny Summerfield, 'Mass-Observation: Social Research or Social Movement?', *Journal of Contemporary History*, Volume 20, Number 3, (1985), p.440.
[17] M-O, *War Factory*, p.7. Emphasis in original.

The anthropologist Bronislaw Malinowski, a key figure in the development of ethnography in the interwar era, championed the aims of Mass-Observation. He obliged Madge and Harrisson by writing an essay to open their publication, *First Year's Work* and, in this essay, Malinowski made some bold statements about the significance of M-O. For example, he suggested that M-O were right to claim as one of their main achievements to date the 'home-bringing of anthropology'.[18] Malinowski was also keen to stress his own role in the home-coming of anthropology manifest in the work of M-O. He wrote:

> For better or worse, whether acknowledged as such or not, I feel that in a way I have been responsible to a large extent for the inevitable consequences in the development of the functional method of anthropology: I mean, for its definite move towards *Anthropology Begins at Home*. From the start of my own field-work, it has been my deepest and strongest conviction that we must finish by studying ourselves through the same methods and with the same mental attitude with which we approach exotic tribes. And I have not kept my light under a bushel.[19]

He also encouraged academic social scientists to co-operate with M-O and to be supportive of the organisation's efforts to communicate the findings of social research to an audience beyond academia.[20] Co-operation was forthcoming to some extent, and according to the anthropologist Raymond Firth, by 1939, there was a significant degree of collaboration between the anthropological community and M-O:

> At the invitation of anthropologists, material from Mass-Observation, particularly from the Northtown Survey, has been presented by Mr Harrisson and others before discussion groups at the London School of Economics and the Royal Anthropological Institute, short visits have been paid by anthropologist to the Northtown Survey, and Professor Malinowski has written a long analytical essay in the volume on the *First Year's Work*.[21]

However, no extent of co-operation could entirely mask the mutual hostility which evidently existed between M-O and academia, despite Malinowski's support.[22]

As Penny Summerfield has noted, M-O's 'relationship with academia was rather like that with the press. It had friends in both camps…But in general it saw both the press and academia as rivals, and as detractors of Mass-Observation's projects.'[23] Harrisson did not pull any punches in explaining why he thought social scientists in Britain objected to M-O's qualitative style of research:

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[18] Bronislaw Malinowski, 'A Nation-Wide Intelligence Service', in Charles Madge and Tom Harrisson (eds.), *First Year's Work, 1937-38*, (London: Lindsay Drummond, 1938), p.98.
[19] Malinowski, 'A Nation-Wide intelligence Service', p.103.
[20] Ibid., p.87.
[21] Raymond Firth, 'An Anthropologist's View of Mass-Observation', *Sociological Review*, Volume 31, Issue 2, (1939), pp.169-170.
[22] Both Summerfield and Calder have noted the existence of this mutual hostility: Summerfield, 'Mass-Observation', p.444; Angus Calder, 'Mass-Observation, 1937-1949', in Martin Bulmer (ed.), *Essays on the History of British Sociological Research*, (Cambridge: Cambridge University Press, 1985), p.129.
[23] Summerfield, 'Mass-Observation', p.444.

> There is a strong tendency to regard only the numerical description of life as scientific. This largely derives from two sources. First, most of our 'social scientists' have not trained in scientific method, but in literary, philosophic, historical and other methods. This causes a feeling of inferiority, of self-questioning about personal scientificness [sic]. Statistics provide one compensation, a symbol of rectitude. Second, objective study of ourselves is so psychologically trying, so against the grain of personal prejudice, that there is an unconscious tendency to avoid the full implications of an unbiased sociology. Here again, the consolation of statistics provides an 'escape'.[24]

Social scientists, including anthropologists, 'were not slow to point out the weaknesses' of M-O's research in return, and Mark Abrams wrote off the whole principle as 'misguided'.[25] In retrospect, Angus Calder has pointed out that the methods used by M-O may not have been as pioneering as Harrisson and Madge claimed and as they persuaded other people. He suggests that they had a lack of knowledge about what social scientists before them had done.[26] Firth's forthright criticism of M-O along these lines in 1939 was testament to the difficult relationship between the social research organisation and academia.[27]

Just as contemporary commentators wrestled over the significance of this research movement in the past, M-O still occupies a rather ambivalent position in the minds of historians. Whilst some social historians, such as Summerfield, have made extensive use of M-O's archives as primary source material, others are quite scathing about M-O's contribution to the history of social research in Britain.[28] Kent, for example, dismisses M-O as 'a somewhat abortive attempt…to apply anthropological techniques to the study of British society'.[29] As a research organisation, M-O's core methods were the use of subjective accounts to document events and behaviour and the use of direct observation.[30] They rarely used the ethnographic method as we have defined it for the purposes of this thesis in a sustained manner. Indeed, given its purpose to apply anthropological field method to Britain, it is surprising that only two M-O projects resemble useful examples of ethnography in Britain between 1914 and 1945.

In 1937, one Mass-Observer, Celia Fremlin, worked covertly in a factory engaged in war production for an extended period of time and the results of this ethnographic

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[24] Mass-Observation, *War Factory*, p.7.
[25] Calder, 'Mass-Observation', p.129; Summerfield, 'Mass-Observation', p.439.
[26] Calder, 'Mass-Observation', p.124.
[27] Firth, 'An Anthropologist's View', pp.166-193. John Spencer, one of the academic covert ethnographers featured in the next chapter, provided a very unfavourable review of a 1949 M-O publication on juvenile delinquency describing it as 'disappointing', 'haphazard', 'unscientific', 'onesided', 'misleading' and 'biased'. This decidedly negative review adds weight to the notion of a strained relationship between academic social science in Britain and M-O. J.C. [John Carrington] Spencer, Book Review, *British Journal of Sociology*, Volume 1, Number 2, (1950), pp.180-182.
[28] See, for example: Penny Summerfield, 'Women, Work and Welfare: A Study of Child Care and Shopping in Britain in the Second World War', *Journal of Social History*, Volume 17, Number 2, (1983), pp.249-270; Penny Summerfield, *Women Workers in the Second World War: Production and Patriarchy in Conflict*, (London: Routledge, 1989).
[29] Kent, *A History*, p.103.
[30] Summerfield, 'Mass-Observation', pp.440-441 & 443.

project were published as *War Factory*. Around the same time, a number of Mass-Observers went to live in one working-class community known by the pseudonym 'Northtown' for the purpose of systematically observing life in an industrial town.[31] Fremlin's *War Factory*, as well as a covert research project Fremlin undertook independently of M-O, will be discussed in detail in this chapter. After 1945, M-O moved increasingly towards statistical sampling and survey methodology which was popular in market research. These statistical methods were also favoured by the post-war academic community because they were thought to be more scientific than the anthropological methods originally used by M-O.[32]

Social Anthropology, Bronislaw Malinowski and Ethnographic Method

In chapter 1, we discussed the influence which anthropology had on the practice of covert ethnography and, in the period between 1914 and 1945, an increasingly professionalised academic discipline of anthropology continued to shape the practice of covert ethnography in Britain.[33] The transition from amateur to professional anthropological fieldworker can be located in the decade between 1912 and 1922. Previously, on-the-spot observers, such as missionaries and colonial officials, had collected data in the field which trained anthropologists then analysed from the comfort of their Western academic institutions. George Stocking suggests that the 1912 edition of *Notes and Queries on Anthropology* produced by the British Association for the Advancement of Science already ostensibly reflected 'the field experience of a new breed of academician-cum-ethnographer' as it attempted to provide some basic training in fieldwork methods.[34]

However, the author of the section in *Notes and Queries* on fieldwork method, W.H.R. Rivers, expressed his hope that the guide would be 'largely used by *missionaries*', a significant group of amateur fieldworkers.[35] Thus, although by 1912 there was the implicit acknowledgement that fieldwork carried out by untrained amateurs was inferior or problematic, missionaries and other men-on-the-spot were not being actively discouraged from carrying out of such fieldwork. Rather, they were given guidelines to aid their effective collection of ethnographic data. However, by the time the influential British-

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[31] Mass-Observation, *War Factory*; Charles Madge and Tom Harrisson (eds.), *First Year's Work, 1937-38*, (London: Lindsay Drummond, 1938), pp.7 & 24.
[32] Summerfield, 'Mass-Observation', p.449.
[33] See above, pp.64-68.
[34] Stocking, 'The Ethnographer's Magic', p.91.
[35] W.H.R. Rivers, 'A General Account of Method' in Barbara Freire-Marreco and Linton Myres (eds.), *Notes and Queries on Anthropology*, 4th Edition, Edited for the British Association for the Advancement of Science, (London: Harrison and Sons, 1912), p.126. Emphasis in original.

based anthropologist Malinowski published his ethnographic account of the Trobriand Islands in 1922, he made it abundantly clear that fieldwork was the domain of the professional anthropologist. According to Malinowski, anthropological research undertaken 'by men of academic training has proved beyond doubt and cavil that scientific, methodic inquiry can give us results far more abundantly and of better quality than those of even the best amateur's work'.[36] Malinowski's emphasis on the professionalisation of fieldwork was bound up with the intention of highlighting the 'scientifically hall-marked' status of ethnographic method and constructing anthropological ethnographers as 'scientific specialists'.[37] Thus, academically trained social scientists were increasingly identifiable as the legitimate and authoritative practitioners of social research.

In some ways, this transition from amateur to professional anthropologist was bound up with the development and elucidation of an ethnographic method by anthropologists, most prominently Malinowski. This formulation of ethnographic method influenced a whole spectrum of social researchers in Britain so it is worth exploring this issue in some depth. Writing in 1939, Audrey Richards suggested that, around the turn of the twentieth century, two styles of fieldwork had dominated anthropological research. On the one hand, some anthropologists were concerned to observe a limited number of comparable and formal aspects of many different societies in order to compare them.[38] For example, Ruth Benedict studied aspects of culture across a number of 'primitive' societies and produced her findings under the heading *Patterns of Culture* in 1935.[39]

On the other hand, however, some anthropologists were more interested in studying the functioning of a particular culture in detail. These were referred to as 'intensive sociological investigations' by Richards who noted that this second type of study became increasingly dominant in the first few decades of the twentieth century.[40] Richards cited Malinowski as a proponent of this more intensive study of a single culture.[41] As a result of this shift to more intensive field methods by anthropologists, Richards noted that a distinctive type of ethnographic text emerged. Anthropologists using this intensive methods tended to produce 'monographs which give a more rounded picture of native life

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[36] Bronislaw Malinowski, *Argonauts of the Western Pacific: An Account of Native Enterprise and Adventure in the Archipelagoes of Melanesian New Guinea*, (London: Routledge and Kegan Paul, 1922), p.xv.
[37] Malinowski, *Argonauts*, p.xv.
[38] Audrey Richards, 'The Development of Field Work Methods in Social Anthropology' in Frederic Bartlett, M. Ginsberg, E.J. Lindgren and R.H. Thouless (eds.), *The Study of Society: Methods and Problems*, (London: Routledge and Kegan Paul, 1939), pp.274-276.
[39] Ruth Benedict, *Patterns of Culture*, (London: Routledge and Kegan Paul, 1935).
[40] Richards, 'The Development', pp.275-276.
[41] Ibid., pp.283-284.

than had previously been presented' by those anthropologists concerned with comparing the more formal and superficial aspects of several cultures.[42] A number of anthropologists were producing this kind of focused and detailed anthropological account around the 1930s, including Margaret Mead on Samoa and E.E. Evans-Pritchard on the Azande community in Sudan.[43] It is Malinowski, however, who is most often credited with pioneering the method of 'intensive personal fieldwork among a single people' and many commentators have cited his 1922 publication, *Argonauts of the Western Pacific,* as the first application of this intensive ethnographic method.[44]

The originality of Malinowski's work appears to have been located in both his fieldwork technique (the ethnographic method) and in the writing-up of his research (the ethnographic text). Emphasis is placed on the fact that Malinowski spent an extended period of time living amongst his research subjects, the Trobriand Islanders.[45] It is also noted that he learned some of the native language and, having sacked his interpreter, communicated directly with the islanders.[46] In his preface to *Argonauts*, Sir James G. Frazer wrote:

> Dr Malinowski lived as a native among the natives for many months together, watching them daily at work and at play, conversing with them in their own tongue, and deriving all his information from the surest sources – personal observation and statements made to him directly by the natives in their own language without the intervention of an interpreter.[47]

In his foreword to *Argonauts* Malinowski reiterated the 'intensive' and 'personal' aspects of his field methodology:

> I have lived in … [the Trobriand Islands]…for about two years…during which time I naturally acquired a thorough knowledge of the language. I did my work entirely alone, living for the greater part of the time right in the villages. I therefore had constantly the daily life of the natives before my eyes, while accidental, dramatic occurrences, deaths, quarrels, village brawls, public and ceremonial events, could not escape my notice.[48]

The intensive ethnographic method was also implicitly contrasted with the type of anthropological survey which had previously been undertaken. Richards noticed the emphasis in studies such as Malinowski's on understanding the totality of the community under study and on how it functioned as a whole, hence the term 'functional anthropology'

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[42] Ibid., p.284.
[43] Margaret Mead, *Coming of Age in Samoa*, (Harmondsworth: Penguin Books, 1929); Edward Evans-Pritchard, *Witchcraft, Oracles and Magic among the Azande*, (London: Clarendon Press, 1937).
[44] See, for example: Richards, 'The Development', p.285; Murray L. Wax, 'Tenting with Malinowski' *American Sociological Review*, Volume 37, Number 1, (1972), pp.1-2.
[45] Stocking, 'The Ethnographer's Magic', pp.97-98.
[46] Ibid., pp.98-99.
[47] Malinowski, *Argonauts*, pp.vii-viii.
[48] Ibid., pp.xvi-xvii.

which Richards associated with Malinowski.[49] An implicit anti-survey sentiment was embedded in the intensive 'functional' method used by Malinowski, and the importance of analysing communities in their entirety was emphasised in Frazer's preface:

> It is characteristic of Dr Malinowski's method that he takes full account of the complexity of human nature. He sees man, so to say, in the round and not in the flat…The man of science, like the man of letters, is too apt to view mankind only in the abstract, selecting for his consideration a single side of our complex and many-sided being.[50]

Again, Malinowski himself echoed the importance of 'dealing with the totality of all social, cultural and psychological aspects of the community'.[51] He also seemed to be criticising the survey approach and distancing his method from it when he said that there 'is all the difference between a sporadic plunging into the company of natives, and being really in contact with them'.[52]

Despite these claims to originality in method and presentation, there is some doubt concerning the truly pioneering nature of Malinowski's work. Stocking suggests that, as early as 1883, events were already in motion which would lead professional anthropologists to develop the ethnographic method now so strongly associated with the discipline.[53] Before the First World War and before Malinowski appeared on the scene, Alfred Haddon and the 'Cambridge School' of anthropologists had already begun to delineate and advocate the use of a more intensive and sympathetic field method, which contrasted with rapid survey work.[54] The fourth edition of *Notes and Queries* was finally published in 1912, behind schedule due to the number of significant advances and revisions which had to be made on the previous edition.[55] The pioneering anthropologist and psychologist Rivers, described as a shy, reticent man with a stammer and a 'complete disregard for personal gain', contributed a discussion of field method to this edition which has been referred to by Stocking as 'a programmatic systematization of the ethnographic experience'.[56] However, it is the first chapter of Malinowski's *Argonauts*, published ten years later, which is usually thought of as the ultimate prescriptive text for the intensive field method.[57]

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[49] Richards, 'The Development', pp.283-285.
[50] Malinowski, *Argonauts*, p.ix.
[51] Ibid., p.xvi.
[52] Ibid., p.7.
[53] Stocking, 'The Ethnographer's Magic', p.72.
[54] Ibid., pp.80-81.
[55] Barbara Freire-Marreco and John Linton Myers (eds.), *Notes and Queries on Anthropology*, 4th Edition, Edited for the British Association for the Advancement of Science, (London: Harrison and Sons, 1912), pp.iv-v.
[56] Stocking, 'The Ethnographer's Magic', p.90. Michael Bevan and Jeremy MacClancy, 'Rivers, William Halse Rivers (1864–1922)', *Oxford Dictionary of National Biography*, Oxford University Press, (2004), http://www.oxforddnb.com/view/article/37898 , [accessed 25th March 2010].
[57] Stocking, 'The Ethnographer's Magic', pp.111.

Between 1900 and 1914, many young anthropological ethnographers had already used the intensive method in the field, according to Stocking, yet they are generally not associated with the 'discovery' of this ethnographic method.[58] The discovery of this method seems to have been commonly attributed to Malinowski because many of the earlier practitioners of the intensive method went on to have fairly marginal careers within academic anthropology and they did not present themselves as 'self-conscious ethnographic innovators'. Malinowski, on the other hand, was an able self-publicist and, as a 'man of great ambition and no mean entrepreneurial talent, he was able to make himself the spokesman of a methodological revolution, both within anthropology, and in some ways more important, to the non-anthropological academic and intellectual community'.[59] Malinowski's 'Trobriand adventure', according to Stocking, helped to establish 'the special cognitive authority claimed by the modern ethnographic tradition' and for generations, ethnographers used his method as a model.[60]

Murray Wax credits Malinowski with revolutionising the content and practice of anthropology and notes how he decisively influenced the next generation of anthropologists. He suggests that a number of Malinowski's anthropology students went on to accomplish heroic and ingenious feats of personal fieldwork and to produce many acclaimed anthropological monographs of their own.[61] More than this, however, and crucially in terms of our interest in covert ethnography in Britain between 1914 and 1945, Malinowski's work and his discussion of method influenced many social researchers across academic disciplines, and those working outside academia, including covert ethnographers. The first chapter of *Argonauts*, according to Stocking, 'was the single most accessible statement of the "modern sociological method of fieldwork" – especially for non-anthropologists, who would be unlikely to read Rivers' chapter in *Notes and Queries*'.[62]

According to Wax, Malinowski's 'discovery' of the intensive personal fieldwork experience had an influence on sociology in Britain, 'reinforcing the development of a research technique already practised by people like...Beatrice Webb'.[63] Malinowski's influence reached beyond the reinforcement of social exploration in sociology. As

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[58] Ibid., p.84
[59] Ibid., p.111.
[60] Ibid., p.71 & 112.
[61] Wax, 'Tenting', p.3.
[62] Stocking, 'The Ethnographer's Magic', pp.110-111.
[63] Wax, 'Tenting', p.1.

discussed above, he was a vocal supporter of M-O's attempts to produce an anthropological account of Britain and one of his students, Hortense Powdermaker, wrote an ethnographic study of a community in her home country, the USA.[64] There are indications that Malinowski and his methodological revelations influenced the practice of social psychology in Britain, encouraging the use of the ethnographic method. In addition, as this chapter will show, non-academic social researchers such as Hugh Massingham appear to have been inspired by Malinowski to some extent in their ethnographic adventures in Britain. As we continue to explore covert ethnography in Britain between 1914 and 1945, the full extent of Malinowski's, and of academic anthropology's influence will become clearer.

Professionalisation, Academicisation and Institutionalisation

The professionalisation of anthropology was part of a wider process of the professionalisation, academicisation and institutionalisation of the social sciences in Britain. In chapter 1, the emergence of professional sociology on a small scale was noted and this process continued into the interwar period. Kent suggests that although institutionalisation of the social sciences began in the first decade of the twentieth century, enthusiasm for the venture was not maintained.[65] In 1903, the Sociological Society was founded and it began publication in 1907 of the *Sociological* Review, Britain's longest-established sociological journal.[66] Whilst A.H. Halsey notes that sociology 'has no agreed boundaries or birthday', he suggests that sociology as an academic subject began in 1907 when a chair of sociology was founded at the London School of Economics (LSE).[67] Kent suggests that the sociology department did not initially provide much encouragement for empirical research and, even when the LSE set up a separate Department of Social Science and Administration in 1912, there was still little emphasis on the development of empiricism.[68] Despite this evidence of a slow start for the institutionalization of empirical social science, it was during the first couple of decades of the twentieth century that universities and the state became the key players which dominated the production of social research in Britain.[69]

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[64] Hortense Powdermaker, *After Freedom: A Cultural Study in the Deep South*, (New York: Atheneum, 1969). First published in 1939.
[65] Kent, *A History*, pp.89-90.
[66] Jennifer Platt, *The British Sociological Association: A Sociological History*, (Durham: sociologypress, 2003), pp.8-9.
[67] A.H. Halsey, *A History of Sociology in Britain: Science, Literature and Society*, (Oxford: Oxford University Press, 2004), p.3.
[68] Kent, *A History*, pp.97-98.
[69] Eileen Yeo, *The Contest for Social Science: Relations and Representations of Gender and Class*, (London: Rivers Oram Press, 1996), p.279.

Eileen Yeo suggests that by around 1920, the advent of academic training for social scientists had brought about a clear demarcation within the field of social research. Increasingly, social scientists were identified as university-trained men who were conversant with statistics and survey methodology.[70] Yeo is concerned to point out that this trend towards academicisation and professionalisation within the social sciences resulted in a contraction of 'the relative space for cultural production from below' in the early twentieth century.[71] Significantly, the academicisation of social research may have displaced the production of social research by other groups around this time, not only those 'below' social scientists in terms of class or level of education. As the attainment of specific social scientific training became the hallmark of authoritative social research in the early twentieth century, the production of social research by non-social-scientific individuals may have become increasingly problematic.

However, those without social scientific training evidently continued to undertake social research in Britain using a variety of research methods. There were, of course, covert ethnographers working outside the boundaries of academia between 1914 and 1945, such as Massingham and Fremlin whose work will be explored in depth in this chapter.[72] In 1934 A. F. Wells undertook what Kent refers to as 'probably the first "survey of surveys"' and found that fifteen of the thirty-two surveys analysed had been organised by an individual who was not a social researcher by profession.[73] Furthermore, Kent suggests that many of the individuals whom we would recognise today as pioneers of empirical sociology were not committed to the academic discipline of sociology during the early twentieth century.[74] We must be careful, therefore, to not over-estimate the extent to which empirical social research had become the exclusive domain of the academic social scientist at this time, even if they did command increasing legitimacy as researchers.

Although this thesis is focused on Britain, no account of the history of covert social research would be complete without at least passing reference being made to developments in American interwar sociology. American interwar sociology was dominated by the University of Chicago which was best known for its sociology of the urban environment. The rapid expansion of Chicago created a diverse population and the Chicago sociologists, often motivated by humanitarian concerns, frequently used the method of participant

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[70] Yeo, *The Contest*, pp.221 & 227.
[71] Yeo, *The Contest*, p.279.
[72] Massingham, *I Took Off My Tie*; Fremlin, *The Seven Chars*.
[73] Kent, *A History*, pp.112-113. Wells, *The Local*.
[74] Kent, *A History*, pp.7 & 9.

observation, both overt and covert, to study sections of the population, including the homeless, musicians and the Jewish community.[75] As part of the Chicago School of sociology, William Foote Whyte used covert participant observation to study the city's Italian population. In 1943, Whyte published *Street Corner Society: The Social Structure of an Italian Slum* and it has become a classic account of covert participant observation by a sociologist.[76] These developments in American sociology during this time promoted the academic acceptability of covert ethnographic research methods.

Social Psychology and 'Functional Penetration'

One of the best documented covert ethnographic projects of the interwar years was conducted in an Austrian village under the guidance of Marie Jahoda, a celebrated social psychologist, who also conducted covert research in Britain in the early 1940s which will be discussed in detail later on in this chapter.[77] Born in Vienna in 1907, Jahoda qualified as a teacher in 1928 and in 1932 she obtained her D.Phil with a thesis based on the life-stories of over fifty elderly people. Jahoda was married to the sociologist Paul Lazarsfeld and, under the auspices of his independent research institute, Jahoda and a team of about ten research workers, all trained social researchers, based themselves in the village of Marienthal, near Vienna, for a period of six months. Marienthal had exceptionally high levels of unemployment due to the closure two years earlier of a textiles factory which had effectively employed the whole community, and the intention of Jahoda's research was to understand the social psychology behind unemployment. Her team of participant observers were covert, keeping their research agenda hidden from their research subjects, and they integrated themselves into the community by working for the winter relief organization or involving themselves in other aspects of local life.[78]

Jahoda described the research method used in Marienthal as 'functional penetration', one of the two key methods used in 'socio-psychological fieldwork' and that it was, in a sense, synonymous with the method of participant observation.[79] The point to note is that, in the 1930s, a form of participant observation, 'functional penetration', was being advocated and legitimised by social psychology, an established, academic discipline.

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[75] Nicholas Abercrombie, Stephen Hill & Bryan Turner, *The Penguin Dictionary of Sociology*, 4th Edition, (Harmondsworth: Penguin, 2000), p.45.

[76] William Foote Whyte, *Street Corner Society: The Social Structure of an Italian Slum*, (Chicago; London: University of Chicago Press, 1993).

[77] Marie Jahoda, Paul Lazarsfeld and Hans Zeisel, *Marienthal: A Sociography of an Unemployed Community*, (London: Transaction Publishers, 2002).

[78] Jahoda, 'Some Ideas on Social and Psychological Research', *Sociological Review*, Volume 30, Issue 1, (1938), pp.64-65.

[79] Marie Jahoda, 'Socio-Psychological Methods of Field-Work', *Man*, Volume 42, (1942), p.115.

The study became a social scientific classic and established Jahoda as a pioneer of empirical social research but, given the socio-political climate in 1930s Austria, the first edition of *Marienthal: A Sociography of an Unemployed Community* was published without the authors' Jewish names attached.[80]

The research led by Jahoda in Marienthal had a great influence on Oscar Oeser, a social psychologist based at the University of St. Andrews in the 1930s. He suggested that Jahoda's work was the prototype of ethnographic research of a community by a team of researchers, and he drew upon her method to explore Dundee in a similar way, except that Oeser made it very clear that his research was conducted overtly. With funding from the Pilgrim Trust, Oeser and a team of academics from a range of disciplines used the method of 'functional penetration' to research juvenile unemployment in Dundee.[81] Oeser's method of 'functional penetration' entailed a group of specialised social scientists embedding themselves in the community being studied, and 'penetrating' its institutions, which meant that the observers were to 'obtain a place and a function in the society which they are studying'.[82] Oeser emphasised that 'the observers approach the community to be studied not as reporters with notebook and camera, but as far as possible as accepted members of that community, having several definite and easily intelligible functions within it'.[83]

Oeser's use of a form of participant observation, although overt, was significant for two reasons. First, Oeser stressed the importance of conducting research which yielded useful, practically applicable results. He was insistent that participant observers had a social role to play in the community under observation. They were to make their status as inquiring scientists clear, but they were to demonstrate their practical aim of understanding in order to help. Oeser suggested rather defensively that this was not 'a Machiavellian subterfuge', and that the notion that 'pure science' operates independently of social settings and problems, was wrong. Social scientists, according to Oeser, could and should apply themselves to alleviating social problems.[84] In his emphasis on the practical applicability of research findings, Oeser prefigured one of the key tenets of post-1945 sociology. In the next chapter, the importance that leading post-war sociologists, T.S.

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[80] Jahoda, *Marienthal.*
[81] Oscar Oeser, 'Methods and Assumptions of Fieldwork in Social Psychology', *British Journal of Psychology*, Volume 27, (1937), pp.343-363.
[82] Oscar Oeser, 'The Value of Team Work and Functional Penetration as Methods in Social Investigation' in Frederic Bartlett, M. Ginsberg, E.J. Lindgren and R.H. Thouless (eds.), *The Study of Society: Methods and Problems*, (London: Routledge and Kegan Paul, 1939), p.409.
[83] Oeser, 'Methods', p.352.
[84] Ibid., p.353.

Simey and Thomas Marshall, placed on the development of empirical sociology that addressed social problems will be discussed in detail.[85] Second, Oeser's research was significant because he associated his use of 'functional penetration' with developments in the field of anthropology.

Oeser made it quite clear that the discipline of social psychology could borrow the methodologies used by other social sciences and apply them usefully to their own field.[86] Anthropology featured prominently amongst those social scientific disciplines from which social psychology could learn and the method of 'functional penetration' seems to have been built on principles of anthropological field work.[87] In a methods text book published in 1939, Oeser noted that the notion of a team of specialists working together in the field had proved useful and desirable for anthropology and that the method of 'functional penetration by a team' could be usefully applied to the analysis of complex industrial societies, such as Britain.[88] This suggests that he saw fieldwork in anthropology and social psychology as directly comparable. Indeed, Firth noted the anthropological dimension of the work being carried out in Dundee by Oeser and his team.[89]

There is further evidence that 'functional penetration' was built on an understanding of anthropological fieldwork. Oeser noted a 'trend in present-day anthropological thought' towards recognising the importance of the 'total cultural setting' in understanding any aspect of a society. In the emphasis on totality, we can see the influence of Malinowski as Oeser remarked that 'It is generally agreed that modern field anthropology has demonstrated that meaning can be assigned to cultural elements only in relation to their function…in the culture as a whole. What is true of, say, Melanesia, is also true of Scotland'.[90] Building on this idea that the work of anthropologists, and of Malinowski in particular, may have been significant in the development of 'functional penetration' as a method, we can see the origins of the 'functional' aspect of 'functional penetration' in 1930s anthropological thinking. In developing the idea of 'functional penetration', Oeser has drawn heavily on the concept of 'functional anthropology' first developed by Malinowski.[91]

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[85] See below, pp.135-136.
[86] Oeser, 'Methods', p.345.
[87] Ibid., pp.344 & 347-348.
[88] Oeser, 'The Value', pp.402 & 404.
[89] Firth, 'An Anthropologist's View', p.167.
[90] Oeser, 'Methods', p.350.
[91] See above, pp.90-95; Richards, 'The Development', pp.283-284.

Hugh Massingham and Working-Class Life in London

Published in 1936, Hugh Massingham's *I Took Off My Tie* represents a particularly intriguing and significant use of the ethnographic method. Whilst there are striking similarities between Massingham's text and the work of renowned Victorian and Edwardian social explorers, such as James Greenwood and Jack London, there are also comparisons to be drawn with the anthropological model of ethnographic fieldwork. The anthropological aspect of Massingham's work was not lost on contemporary commentators. The *Times Literary Supplement's* review of *I Took Off My Tie* appeared under the headline 'Going Native in the East End', and beneath a review of an anthropological text.[92] Massingham has been described as a shy man with a severe stutter and nervous temperament who suffered from poor health. He belonged to a family of distinguished journalists and eventually became the political correspondent for *The Observer* and, later in life, he turned his hand to fictional writing.[93]

In September 1936, however, whilst still a journalist, Massingham published an ethnographic account of the East End of London and, having been reprinted in October and again in November of the same year, *I Took Off My Tie* was popular. Despite the initial success of his publication, Massingham seems to be conspicuous by his relative absence from discussions of the history of British social research, although Keating does refer to his work as an example of the continuing tradition of social exploration.[94] Indeed, from a twenty-first century perspective, Massingham has been overshadowed by his father and brother. Massingham's father was the radical Liberal journalist Henry William Massingham and his eldest brother was Harold John Massingham, a well-known commentator on rural England.[95] Unlike his father and brother, Massingham does not appear in the *Oxford Dictionary of National Biography*.

Massingham had hoped to live amongst residents of the East End of London as a 'down-and-out and mix with them as an equal'. However, this scenario seemed impossible for Massingham to engineer, perhaps because he was unable or unwilling to give up his day-job as a journalist and immerse himself in the East End on a full-time basis, or,

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[92] 'Going Native in the East End', *Times Literary Supplement*, (September 9th, 1936), p.740.
[93] 'Obituary: Mr Hugh Massingham, journalist and author', *The Times*, (December 28th, 1971), p.8.
[94] Keating (ed.), *Into unknown England*, p.31.
[95] H. W. Nevinson, 'Massingham, Henry William (1860–1924)', *Oxford Dictionary of National Biography*, Oxford University Press, (2008), http://www.oxforddnb.com/view/article/34923 , [accessed 25th March 2010]; Malcolm Chase, 'Massingham, (Harold) John (1888–1952)', *Oxford Dictionary of National Biography*, Oxford University Press, (2008), http://www.oxforddnb.com/view/article/34922 , [accessed 25th March 2010].

because he felt unable to fake the cultural attributes necessary to pass as an East Ender.
Massingham had a contact in the East End, a pawnbroker, who found a way for him to gain
access to an East End context. A local landlord was going away for a while and the
pawnbroker had volunteered Massingham to collect the rents in his absence. Massingham
was to pose as a well-to-do man who had come down in the world and in exchange for
collecting the rents he would be given lodgings in the East End at a reduced rate.[96] Posing
as a rent collector to gain access to the East End was to prove understandably problematic:
before they had even met him, the tenants regarded him with the utmost suspicion and even
hostility. He was referred to as a spy and 'a toffy-nosed bugger' and associated with other
ominous authority figures.[97] Ultimately, this pretence had to be abandoned, and
Massingham simply posed as a middle-class man down on his luck.

Massingham's research was covert to some extent in that he had to maintain a
façade of being somewhat impoverished and reflecting this in his attire.[98] He also had to
conceal the fact that he was gainfully employed as a London-based journalist throughout
his ethnographic project. Whilst living in the East End, Massingham continued to go to
work in his City office, stopping off at his '"real"' home' to change out of his shabby East
End get-up.[99] During the course of his research, Massingham kept something like a
research diary as well as making field notes and he appeared to use these extensively in the
construction of his text. The way in which Massingham used ethnography as a research
method differed qualitatively from that of other social researchers working during this
period, and in some ways, his ethnographic endeavour had much more in common with
those undertaken in the late Victorian and Edwardian period than the work of Fremlin and
Jahoda, for example. In contrast to some of the other ethnographic accounts which will be
discussed in detail, Massingham's text was essentially a narrative account of a social
research project from conception to completion. Indeed, it is not until the third chapter of
the book that Massingham finally took up residence in the East End and began his
fieldwork.[100]

Massingham had no firm research agenda and explained that he was inspired to
take his trip to the East End by a visit he took to the Rhondda valley to study
unemployment there. He recalled that his trip to the Rhondda had made him feel

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[96] Massingham, *I Took Off My Tie*, p.24.
[97] Ibid., pp.64-67.
[98] Ibid., p.41.
[99] Ibid., p.74.
[100] Ibid., p.41.

'extraordinarily humble and far more ready to criticise my own shortcomings than to condemn the workings of society'.[101] His explanation of his book also revealed the extent to which *I Took Off My Tie* was intended to be self-exploratory rather than just an account of life in the East End:

> This book is an account of what happened to me during a prolonged visit to the East End. It is not an answer to Tolstoy's thunderous question of 'What then ought we to do?' It is a plain statement of fact set down with all the objectivity of which I am capable, with no political bias and no desire to make a case.[102]

In this sense, the tone of Massingham's work was open-ended, consciously subjective and at times like a confessional diary rather than the written report of a social research project.[103] Massingham did not make any generalisations about the nature of his research subjects based on his observations and he made very few firm advocatory statements. Indeed, the closest he came to advocating change on the basis of his research was to complain about the drabness of labour exchanges, the lack of entertainment for job-seekers, and the failure to distribute work fairly.[104]

Like many of the ethnographers of the Victorian and Edwardian period, Massingham used the language and imagery of exploration extensively in his construction of his ethnographic text. Keating noted that Massingham seemed barely aware of how many people had felt and said the same thing before him when he commented that for years he 'had been struck by the extraordinary fact that two communities were living side by side, each with its own peculiar customs, superstitions, culture, sex life and to some extent even language, and that each was ignorant of the other'.[105] When Massingham returned from the Rhondda, his sense of astonishment at the state of society was renewed. As he returned 'to London with the eyes of a traveller and no longer looking at the city with the indifferent gaze of an inhabitant, the violent contrasts between the East and the West struck me as almost a new phenomenon'.[106]

Throughout his account, Massingham returned to the theme of exploration. He spoke of 'penetrating' depressed areas, referred to Tyneside and South Wales as 'fairly well-known outposts of civilisation' and described the East End of London as 'uncharted

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[101] Ibid., p.1.
[102] Ibid.
[103] For example, Massingham admitted to his readers that 'I am ashamed to say that on more than one occasion I gave way to sudden temptation, hoisted myself on board [a bus bound for the West End], and allowed myself to be taken to Piccadilly.' Ibid., p.72.
[104] Ibid., pp.157-158.
[105] Keating (ed.), *Into Unknown England*, p.31; Massingham, *I Took Off My Tie*, p.1.
[106] Massingham, *I Took Off My Tie*, p.2.

land'. Significantly, as he tried to embed himself in the East End, he thought with bitterness that 'it would have been far easier had I decided to explore remotest Africa'.[107] Had he instead decided to explore the interior of Congo, he

> should at least have had a fairly general idea of the difficulties which that lay ahead of me. I should have known what clothes to buy, the probable attitude of the natives, and the measures to take to combat their hostility. But I had no idea what reception awaited me in the East End. I did not know what place to make for or what clothes to wear, and unfortunately there was no resident British Commissioner who could help me.[108]

It is particularly striking that at one point Massingham suggested that the East End of London was as unknown as the Trobriand Islands.[109] Given the geographical obscurity of the Trobriand Islands, it seems unlikely that Massingham would have chosen it at random to represent unknown areas of the world. It would seem more likely that Massingham had read, or was at least aware of, Malinowski's popular anthropological account of life on the Trobriand Islands and thought it a suitable parallel to his own work.

However, the extent to which Massingham intended to undertake an explicitly anthropological-style exploration of the East End is questionable given another aspect of his text. Massingham repeatedly attributed the derogatory use of anthropological-style categorisations of the East End inhabitants to Mr Johnston, a figure for whom he had very little sympathy. A middle-class man who had ended up in the East End out of financial necessity, Johnston abhorred the people he was forced to live amongst, and Massingham clearly found him utterly objectionable. When Massingham and Johnston were briefly forced into an alliance against the rest of the East Enders on their street, Massingham felt that Johnston 'saw the two of us defending a solitary outpost of Empire against barbarian hordes, and the more the children jeered at us the happier he became. The other pale-face, however, found it less exhilarating'.[110] Referring to Johnston's unpopular tendency to brandish his public school tie in the company of his East End neighbours, Massingham noted that

> He was incapable of seeing that it made him despised and hated and that it was as much out of place in his present surroundings as if he had been living in the midst of barbarous and naked savages in some remote part of the Congo. Perhaps he felt that he was in the Congo. He hated everybody in the street and talked of them as if they were a savage tribe.[111]

Massingham constructed Johnston's anthropological attitude towards the residents of the East End in a particularly negative way. It is difficult to see precisely what role

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[107] Ibid., pp.12-13.
[108] Ibid., p.13.
[109] Ibid., pp.2-3.
[110] Ibid., p.122.
[111] Ibid., p.243.

anthropology played in Massingham's understanding of the East End. Whilst he clearly found it a useful metaphor for demonstrating the alien nature of the East End and perhaps found methodological inspiration in the discipline's fieldwork, he seemed to balk at the application of anthropological 'racial' terminology to the residents of the East End.

Massingham did not declare an affiliation to any trend in social research or any social scientific academic discipline, but he did construct his use of ethnographic methodology as being in opposition to the production of statistical data or 'theorising'. It was not that Massingham rejected undertaking research 'in a proper scientific spirit'; it was just that he constructed *experience* as being of central importance.[112] Massingham recognised that as a journalist he had been theorising about the poor for years, 'suggesting remedies for their standard of life', and had only recently realised his compulsion 'to know how they actually lived'.[113] He went on to reflect that

> I suppose that there was never a time when we knew more about the health of the people, the exact state of their poverty measured in pounds sterling, and the number of our slums…But it seemed to me…that we knew next to nothing of them as human beings, that they had become for us no more than numerals in our statistics. I wanted now to be able to refer my theories to a sure background of experience, to know what went on in the minds and hearts of people, and it was this desire that finally drove me from my friends and into the East End.[114]

After his first stint in the East End, he reflected:

> If I had failed to make any friends and to find those lasting contacts which I had imagined at one time were so easy to establish, I had at least succeeded in freeing myself of all those sentimental generalizations that had hitherto influenced my social and political outlook. The unemployed had no longer a place in my mind as a row of figures in an economic argument but were a vast army of living and suffering human beings.[115]

In other words, Massingham felt that through his experience of living in the East End and interacting with the poor, he had cured his 'mind of theorising, and at last was in a fit state of mind to observe and understand'.[116]

The concomitant of Massingham's rejection of the anonymity of statistical data was his desire to live with the poor and to share as much as possible in their way of life. Massingham recognised the difficulties involved in 'looking at life as they did, as distinct from observing the facts of their existence. To recognise that a man is in want is one thing; to know exactly what is going on in his mind when he asks you for a copper is quite another'. He also recognised that such an identification between researcher and researched

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[112] Ibid., pp.112-113.
[113] Ibid., p.2.
[114] Ibid., p.3.
[115] Ibid., p.131.
[116] Ibid., p.132.

could not be engineered and might never come about, no matter how extensive the researcher's experience or sympathy.[117] Significantly, Massingham felt that he did achieve this complete understanding of the population that he was researching, and deemed this event so momentous that it was used as the name of his book, *I Took Off My Tie*. Massingham felt he truly understood what it meant to be one of the poor following an incident which occurred whilst he was tramping the streets looking for work with a companion. Arriving at the City, he recalled that it 'happened to be the first day I did not put a tie on'. He and his companion stopped to survey the scene before them as clerks, typists and business men, intent on getting to work, passed them by. Massingham was struck by how familiar the whole scene seemed to him, and how easily he could slip back into that world when he noticed that a policeman, 'the familiar figure, the friend of us all', was watching them with 'suspicion and hostility'.[118] Massingham

> realised that the taking off of my collar and tie earlier in the morning had been an act of tremendous importance and had placed me on the wrong side of the fence. He was no longer my friend, but my enemy. I had unconsciously locked myself out of the world of busy preoccupied people, and for him I had become potentially a disturber of the peace, a breaker of safes, a dangerous being. In a flash I saw everything in a different light. I felt very small and alone.[119]

The notion of experience was central to Massingham's ethnographic project and his keenness to be 'at one' with the poor was matched by his dislike of representations of the middle classes in the East End. In this sense, Massingham's relationship with his research subjects can be compared to that of Stephen Reynolds and his deep sense of attachment to and respect for the fishing community he researched in 1909.[120]

Massingham's dislike of Johnston has already been mentioned, but here we can relate it to his status as a middle-class man living in the midst of poor working-class Londoners. Massingham characterised Johnston as a snob who rented two rooms which he could barely afford so that he did not have to live any closer than necessary to the local people to whom he referred as 'hooligans', 'hoboes' and 'human rats'. When Massingham came to call, Johnston revelled in the company of a fellow 'gentleman' and in the chance 'to hear educated speech'. Constantly referring to his public school education, Johnston was at pains to distance himself from his poor neighbours, and this did not sit well with Massingham.[121] The objectionable presence of Johnston in *I Took Off My Tie* emphasises

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[117] Ibid., pp.3-4.
[118] Ibid., pp.4-6.
[119] Ibid., pp.5-6.
[120] Stephen Reynolds, *A Poor Man's House*, (London: Bodley Head, 1909). See also: Christopher Scoble, *Fisherman's Friend: A Life of Stephen Reynolds*, (Tiverton: Halsgrove, 2001).
[121] Massingham, *I Took Off My Tie*, pp.99-106 & 183-184.

the extent to which Massingham's research involved the transgressing of class boundaries. As noted in the Introduction, the transgression of class boundaries has been described as an essential component of social exploration most associated with the late Victorian and Edwardian era.[122] Massingham's frequent and mostly hostile references to middle-class people and institutions in his account clearly aligned his work with the tradition of social exploration.[123]

Massingham's ethnographic text was written in a literary and informal style throughout. He arranged *I Took Off My Tie* into chapters, but they had no titles and he did not include an introductory or concluding chapter. At times, Massingham wrote with considerable literary flair and described some of his observations quite imaginatively. For example, after Massingham's first foray into the East End, a local man took him to a bus stop. As they walked along the streets that evening, Massingham noted that the only signs of life emanated from the public houses:

> If the doors were open, there would be a great beam of orange light lying across the street, and you could see the rain falling like arrows through it on to the pavement, the hats, and the coats of the crowd. Then suddenly you would be in the Magic Circle yourself, a great wave of stale beer would fall over you, and almost at once you would be out of it again and into the darkness.[124]

There is also evidence to suggest that Massingham used considerable artistic licence when translating his experience into text, even if he did have the use of his field notes. For example, Massingham discussed in detail, over six pages, his meeting with Mr Harrison, a solicitor. He included extensive quotations of both his own words and those of Harrison, along with detailed references to the expressions, mannerisms and character of the solicitor.[125] It is unlikely that Massingham managed to note accurately all the details he includes in his final text so, using his notes, his memory, and his imagination, he appears to have reconstructed the meeting with the solicitor.

Massingham's use of humour throughout his ethnographic text can be seen as in keeping with his overall novelistic style, but it also served more important functions.[126] For example, Massingham shared a humorous incident with the reader which occurred as

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[122] See above, pp.15-17; Kent, *A History*, p.37; Keating, *Into Unknown England*, p.13.

[123] As well as his dislike for Johnston, Massingham expressed hostility towards the middle-class landlord and his mother as well as his 'extreme prejudice' against the settlement movement and all other manifestations of middle-class philanthropy. Massingham, *I Took Off My Tie*, pp.13-14, 28-30 & 33.

[124] Ibid., p.10.

[125] Ibid., pp.33-39.

[126] In Chapter 1 it was suggested that Howard Goldsmid's use of humour in his covert account of common lodging houses in London emphasised the literary quality of his work, as opposed to any association with nascent social science. See above, pp.53-54. Howard J. Goldsmid, *Dottings of a Dosser, Being Revelations of the Inner Life of Low London Lodging-Houses*, (London: T. Fisher Unwin, 1886), pp.69-70.

he relaxed in the East End home of the Symonds family. Mr Symonds was showing one of his sons a picture of 'Jesus holding a lamb in His arms' and, even though Symonds appeared uncomfortable, Massingham encouraged him to explain the picture to his child:

> 'Well, nar,' said Symonds with the embarrassed air of a schoolboy who has been told to lecture to a class in front of the headmaster. 'Well, nar, Georgie. Yer sees this 'ere chap. 'E's the blessed Sivyer, 'E is. Nar in this 'ere pickcher, 'E's jus' 'iked up one young ship, an' t'other 'as gorn an' got unner 'Is feets. 'E ses, "Blarst yer, why don't yer kip from unner me feets? Carn't yer see? Do yer wants me ter fall dahn an' break me bloody neck?"'… 'Yer see,' continued Mr Symonds, 'if it warn't for that bloody stick in 'Is 'and, 'E'd be over like a shot, fer sartin. Nuffin'' (concluded Mr Symonds dramatically), 'nuffin' would sive 'Im.'[127]

Given Massingham's respect for the poor of the East End, it is unlikely that he shared this incident with the reader in order to poke fun at Symonds. Rather, it could be seen as a way of making the reader complicit in Massingham's ethnographic adventure. Certainly, Massingham was not concerned to make his text relentlessly dry and sober, and he frequently related comic incidents which centred on him. He recalled an incident where he threw a stone at a tenement window to try and gain access to the building. One woman became so irritated by his persistence that she showered him with a bucket full of vegetable scraps and fish bones from her open window.[128] On another occasion, having returned to his rented rooms in the East End, Massingham described himself as being 'frozen with fear' after finding beetles and rats roaming freely. He confided in the reader that he spent a sleepless night searching for more vermin, repeatedly getting up and stripping to scratch and examine himself for bugs before redressing and getting back into the bed.[129]

Massingham's tendency to share these slightly embarrassing incidents was perhaps calculated to disarm his readers and convince them of Massingham's trustworthiness as an author. Similarly, Massingham seemed willing to share his initial ineptness in the East End world with the reader and he candidly related several incidents that occurred in the field which could be construed as ethnographic failures on his part. For example, lost after his first trip to the East End, Massingham approached a boy who was lying along the top of a wall pretending to be an aeroplane bombing his friend below. Massingham recalled:

> I went up to him and asked him the way to the main street. He stopped making a noise through his lips and said: '---- off.' I was completely taken by surprise. I said: 'Can you tell me where I can catch a number 15 bus?' '---- off,' said the boy. He switched on his engine again, and soared ten thousand feet above the earth. I stood watching him helplessly…[130]

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[127] Massingham, *I Took Off My Tie*, p.179.
[128] Ibid., p.52.
[129] Ibid., pp.70-71.
[130] Ibid., p.9.

Massingham convincingly constructed himself as initially inept at conducting himself in the East End and his honesty could be understood as serving an important purpose. We must consider Massingham's account of his most significant failure in the field. As a result of a misunderstanding between Massingham and one of his tenants, Mr Shepherd, there was a physical confrontation and, ultimately, the residents of the East End street maintained a campaign of intimidation against the ethnographer.[131] In the pub, Massingham's pint was spilled on purpose, and the other customers, including Shepherd mimicked his Oxford accent, continuing to blame him for the near-eviction of Shepherd and his family. His neighbours began to break into his rooms, damaging and stealing his possessions, he was hustled off the pavement and had things thrown at him. Massingham was finally forced to leave his lodgings when he came home one day to find that someone had broken in and 'smashed the rest of my crockery and had then done his business in the frying-pan and left it in the middle of the table'.[132]

The ultimate purpose of sharing such preliminary disasters in the field was that it allowed Massingham to demonstrate the importance of experience to understanding and to establish his post-ethnographic authority. Once Massingham had been forced out of his lodgings, he reflected in hindsight that no-one could 'tell a second person how to conduct himself in the slums; the traveller can only find out by the painful process of trial and error'.[133] The sense of journey from ignorance to understanding, outlined in Parssinen's model of the ethnographic journey, is apparent in Massingham's work. Parssinen compares at the ethnographic texts composed by Victorian and Edwardian social explorers and those produced by social scientists and she notes the journey metaphor embedded in both types of ethnographic text. In the work of the social explorers, this notion of a journey involves both the researcher and the reader:

> the explorers actual movement in time and space; his corresponding development from ignorance to knowledge; and the reader's vicarious experience of the explorer's physical and educational journeys. The logic of chronology becomes the logic of causality in a voyage of discovery: The ways in which one sees become the products of what one has seen already.[134]

Parssinen suggests that in the texts produced by the social explorers, a narrative or chronological approach was adopted which mirrored the journey the social explorer had taken.

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[131] Ibid., pp.120-121.
[132] Ibid., pp.126-130.
[133] Ibid., p.131.
[134] Parssinen, 'Social', p.206.

One particular incident allowed Massingham to demonstrate his ethnographic authority clearly, and again, the notion of an ethnographic journey comes to the fore. Having fled the local pub in a distressed state after overhearing a conversation amongst his tenants about their suspicions of the new rent collector in their midst, he reflected:

> This simple explanation did not occur to me until long afterwards. At this stage of my journey, while I was still completely ignorant of slum conditions, I could not see that the appearance of a stranger must inevitably arouse suspicion. I did not know that the majority had something to conceal and that they must always be on their guard. Their alarm was very natural. A stranger from the middle-class suddenly appears in their midst and there seems to be no rational explanation for his presence. Well, then, is he perhaps a spy? Sent by the labour exchange to find out if Shepherd is earning a little money on the quiet? Or by the housing authorities to see how many children Mrs Symonds has? We do not know. We are poor unlettered people, ignorant of the laws and motives of these people who rule us. We must beware.[135]

It was only because Massingham had lived among the East Enders and understood their way of life that, retrospectively, he could comprehend their reaction to him that day in the pub in a way that would formerly have been impossible to him.

Perhaps the most striking aspect of Massingham's ethnographic text is the extent to which it is about his own personal journey through the East End and his own relationship with the observed rather than an account of the poor in the East End. Especially in the second half of *I Took Off My Tie*, Massingham devoted a good deal of time to reflecting on his emotional state and the degree to which he was at home amongst the poor of the East End. Feeling melancholy, Massingham reflected:

> For, although my old surroundings were only divided from me in point of time by a few hours and in point of geography by only a mile or two, they now seemed to be something I had known many years ago and to lie a great distance away…The East End had taken me in, but it had not digested me, and the feeling that I was in it and yet not part of it and that by coming down here I had somehow cut myself off from my own world made me feel as if I were acting in rather an absurd game of charades. This feeling of unreality was all the greater because everything was both strange and familiar at the same time. Had the cockney spoken a foreign language and had the East End itself been full of great temples of barbaric splendour, I should have found this society easier to study and been able to play the part of a detached observer to perfection.[136]

It was clearly important to Massingham to feel accepted by the people he was observing. The aim of his ethnographic project was not just about getting close enough to observe their behaviour; Massingham nursed a desire to be accepted by the East End community. His emotional state was related to his perceived closeness to the research subjects. Whilst he was at home with the Symonds family, for example, Massingham found that he 'was absurdly contented. For the first time I felt that I was home and that I could say what I

[135] Ibid., pp.68-69. Massingham's use of 'we' is reminiscent of Mary Higgs' use of 'we' to identify herself with the vagrant women she was observing, demonstrating her empathy with her research subjects. See above, p.56. Mary Higgs, *Glimpses into the Abyss*, (London, P.S. King and Son, 1906) p.172. Emphasis in original.
[136] Massingham, *I Took Off My Tie*, p.73.

wanted and act naturally. It was all very pleasant.'[137] Pondering his situation further, he remarked:

> Here I was in a very ugly room filled with shouting children, talking to a man who had not changed his shirt for a week and whose sweat you could smell every time you came near him. I was perspiring too, but I did not mind very much. I did not mind the noise or the smells, or the fact that Mrs Symonds had got the hiccoughs. I was thinking of only one thing: that I had at last – I hoped finally – made my peace with them.[138]

Massingham was apparently aware of the extent to which his yearning to be embraced by the people of the East End was superfluous to his original aims of his social research project:

> What more did I want? I had come down to the East End to study conditions, and I had gained a safe vantage point from which I could observe everything without any danger to myself. It seemed an ideal end to the adventure, and yet I was not satisfied…For my original purpose it was only necessary for me to watch its life as a detached spectator, but my interest had so increased that I now wanted to become part of it and to identify myself completely with the people who persisted in remaining aloof.[139]

Indeed, as the text draws to a close it becomes clearer that Massingham's goal of becoming absorbed into the East End had overtaken his aim of producing a piece of social research. The final chapters of the text are exclusively concerned with the illness, death and funeral of Johnston. It is of central importance that it was Massingham who co-ordinated the care of Johnston and made arrangements for his funeral.[140] The point is that by the end of the ethnographic text, Massingham had become a useful and integrated part of the East End community he set out to observe and *I Took Off My Tie* no longer reads like a piece of social research, but as a novel in which Massingham was the protagonist.

In the left-leaning periodical *Time and Tide* George Orwell reviewed Massingham's *I Took Off My Tie* alongside a more traditional example of travel literature under the suggestive title 'Travel Round and Down'.[141] The link between Massingham's work and travel literature was accentuated by the large advertisement on the same page as Orwell's review for Gordon Sinclair's *Khyber Caravan,* marketed as the travel book of the year for sheer thrills and fascination. Orwell's review of *I Took Off My Tie* was lukewarm at best. He suggested that Massingham's travel was 'vertical rather than horizontal', and that his claim that the East End of London was as unknown as the Trobriand Islands was 'an exaggeration, but not so far from the truth'. Orwell praised Massingham's method but

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[137] Ibid., p.181.
[138] Ibid., p.182.
[139] Ibid., p.217.
[140] Ibid., pp.249-284.
[141] George Orwell, 'Travel Round and Down', *Time and Tide*, Volume 17, Number 42, (October 17, 1936), p.1453.

suggested that the subject matter of the book was 'a good deal superior to the writing'.[142] Shortly before Orwell's review appeared, Massingham's publisher Heinemann's took out a full page advertisement in *Time and Tide* to promote their autumn publications. *I Took Off My Tie* was given pride of place in this advertisement and the praise from other reviewers included in the advertisement again added to the notion that this work of covert ethnography was thought of as travel literature. Roger Pippett of the *Daily Herald* wrote that Massingham, '[t]he author of this magnificent work…only ventured a mile or so east of Aldgate Pump. But he has written one of the most remarkable travel-stories of his generation'.[143]

In his discussion of social exploration in Victorian and Edwardian Britain, Raymond Kent distances this phenomenon from travel writing in general. He suggests that:

> Social exploration is more than just a travelogue, and account of a journey during which the range and variety of human life is displayed…It is more purposive, more geared to the discovery of the unknown and presupposes a rigid class structure in which a representative of one social class consciously sets out to explore, analyse and report upon the life of another class lower on the social scale.[144]

However, there seems to be some significance in the classification of *I Took Off My Tie* as travel literature. A glance through the pages of *Time and Tide* suggests that, at least in 1936 amongst the readership of this left-leaning periodical, travel literature was popular and frequently featured in reviews and book guides.[145] Titles such as *Strange Places and Strange Peoples* and *Leaves from the Jungle* are suggested as general reading by *Time and Tide*.[146] The anthropologist Raymond Firth's book *We, the Tikopia: A Sociological Study of Kinship in Primitive Polynesia* also appeared on the periodical's list of general reading in the book guide.[147] Furthermore, *Time and Tide* seemed to make a habit of having Orwell review travel literature.[148] This suggests that the interest in travel literature was linked to the academic practice of anthropology and that, at least to some extent, the work of social commentators such as Massingham and Orwell were considered to be anthropological takes on Britain, as travel literature concerning Britain rather than the wider world.

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[142] Orwell, 'Travel', p.1453.
[143] *Time and Tide*, Volume 17, Number 41, (October 10, 1936), p.1389.
[144] Kent, *A History*, p.37.
[145] *Time and Tide*, Volume 17, Number 28, (November 28, 1936), p.1678; *Time and Tide*, Volume 17, Number 43, (October 24, 1936), p.1486; *Time and Tide*, Volume 17, Number 31, (August 1, 1936), p.1109; *Time and Tide*, Volume 17, Number 48, (November 28, 1936), p.1678.
[146] *Time and Tide*, Volume 17, Number 43, p.1486.
[147] *Time and Tide*, Volume 17, Number 48, p.1678.
[148] George Orwell, 'Real Adventure', *Time and Tide*, Volume 17, Number 29, (July 18, 1936), pp.1041-1042; Orwell, 'Travel', p.1453; George Orwell, 'Desert and Islands', *Time and Tide*, Volume 17, Number 47, (November 21, 1936), pp.1643-1644.

Celia Fremlin and 'the Domestic Service Problem'

Born in 1914 in Middlesex, her father a medical doctor, Fremlin is best known as an accomplished writer of mystery fiction.[149] She enjoyed a long literary career, publishing her last novel in 1994 whilst her first novel, published in 1958, *The Hours before Dawn,* won a Mystery Writers of America Edgar Allan Poe award.[150] The daughter of a Hertfordshire doctor, Fremlin read classics at Oxford, graduating in 1936.[151] Whilst at Oxford, she had been a member of the Communist Party, although she admitted that it was not her strong political convictions which motivated her membership, it was that 'everybody who was anybody was in the Communist Party...that was where all the fun was'.[152] Motivated by her scepticism about Communist Party comrades' views on working-class life, Fremlin took on a series of jobs in domestic service between 1937 and 1938 to find out more, a move described by Fremlin's niece as 'unusual for a middle-class woman in those days'.[153]

Fremlin used her undercover experiences in domestic service as the basis for a covert ethnography about 'the domestic service problem' in Britain, in other words, the difficulty obtaining and retaining good domestic staff at a time when there were other employment opportunities for young women.[154] Fremlin had completed the field work for *The Seven Chars of Chelsea* before the outbreak of war, but it was not published until 1940. A review of *The Seven Chars of Chelsea* noted the unfortunate timing of the publication, suggesting that the problem of domestic service had a greatly reduced importance in wartime and that, of course, the war had somewhat altered the situation. Whilst many potential domestic servants had been drawn into the munitions factories, exacerbating the problems of supply, potential employers of servants were 'cutting down their expenditure on service'.[155] However, Fremlin related her overall research aim to the idea that

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[149] 'Celia Fremlin: Author who wrote The Hours Before Dawn', Obituary, *Times*, September 9th 2009, http://www.timesonline.co.uk/tol/comment/obituaries/article6826388.ece , [accessed 7th July 2010].
[150] Margaret Kettlewell, 'Celia Fremlin', Obituary, *Guardian*, Sunday 6th September 2009, http://www.guardian.co.uk/theguardian/2009/sep/06/celia-fremlin-obituary , [accessed 11th July 2010]; Celia Fremlin, *King of the World*, (Sutton: Severn House, 1994); Celia Fremlin, *The Hours before Dawn*, (London: Victor Gollancz, 1958).
[151] Judy Giles, '*"*A Little Strain with Servants": Gender, Modernity and Domesticity in Daphne Du Maurier's *Rebecca* and Celia Fremlin's *The Seven Chars of Chelsea*', *Literature and History*, Volume 12, Number 2, (2003), p.43.
[152] Giles, '"A Little Strain..."', p.43.
[153] Ibid.; Kettlewell, 'Celia Fremlin'.
[154] Fremlin, *The Seven Chars*, p.vi.
[155] Review of 'The Seven Chars of Chelsea', *Times Literary Supplement*, (August 3, 1940), p.379.

> the growing conviction among intelligent people that military victory in this war is not the only thing on which we must concentrate our energies; that on the home front there are battles to be fought as well as services to be rendered.[156]

Fremlin was fundamentally interested in the class structure of society and endeavoured to explore this from the angle of domestic service where 'class distinctions are most forced into prominence'.[157]

Fremlin offered no straightforward description of her methodology to the reader and this has to be pieced together from a careful reading of the text. Fremlin appears to have used a variety of research methodologies, including questionnaires and interviews, but her covert ethnography lay at the core of her account. The body of the text (exclusive of the introduction and conclusion) contained ten chapters and the first four of these were concerned with the discussion and analysis of her ethnographic fieldwork. The first of these chapters dealt with Fremlin's attempts to gain employment within the world of domestic service using agencies, following up adverts in shop windows and being interviewed by potential employers.[158] Another chapter focused on her experience as a live-in scullery maid in a household consisting of only 'her Ladyship' and a number of domestic servants.[159] In the subsequent chapter, Fremlin discussed her experience working as a live-in chambermaid in a rather rundown and squalid boarding house.[160] The last chapter in the book to be based on an ethnographic episode concerned Fremlin's time spent working as a charwoman in a large London hospital with seven other experienced charwomen.[161]

Fremlin did not go into detail about her preparations for immersing herself in the research context and, again, we have to piece this together using scraps of information scattered throughout the text. It is clear, for example, that Fremlin spent a considerable amount of time working in each of her three domestic service roles, but she did not specify exactly how long she remained with any of her employers and based her ethnographic accounts on detailed descriptions of her experience of one day in each job. Presumably, given that Fremlin was a graduate who moved in middle- and upper-class circles, she must have assumed a covert status to some extent in order to justify her apparent need for employment in domestic service. Yet, Fremlin did not make it clear how she managed to embed herself in the world of domestic service. There is evidence to suggest that Fremlin

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[156] Fremlin, *The Seven Chars*, p.v.
[157] Ibid., p.7.
[158] Ibid., pp.8-26.
[159] Ibid., pp.27-49.
[160] Ibid., pp.50-72.
[161] Ibid., pp.73-86.

occasionally altered her accent to fit in with her surroundings, but it was not part of a systematic disguise.[162] Furthermore, Fremlin appears to have adopted the pseudonym 'Margaret Peters' for the purpose of her ethnographic stints. She does not, however, go into any great depth about the use of a pseudonym or any other physical disguise apart from to note that the use of a pseudonym seemed rather superfluous, as potential employers invariably, and seemingly wilfully, got her name wrong anyway.[163]

Fremlin did devote considerably more attention, however, to exploring the phenomenon of ethnographic research in Britain more generally. Reflecting on her initial decision to undertake research into the world of domestic service, she recalled that

> When I first embarked on the researches which led to this book I did so in that spirit of arm-chair socialism which is so prevalent among my class and generation. I thought that by coming down from Oxford and taking a series of jobs as kitchen hand, charwoman, cook-general and so on I would get to 'know' the domestic servant class; would understand and appreciate their lives and conditions of work; would find out where the mistresses were 'wrong' and where the servants were 'right'.
> Needless to say, I did not succeed in doing anything of the kind.[164]

Fremlin goes on to explain, in the authoritative tone of an arm-chair socialist who has gone out to experience the reality for herself, that:

> By working and living for a time among a class other than one's own one may learn a lot; may make many friends. But one will not become a member of that class. Between oneself and them there will remain a barrier; thin and clear as glass, but impenetrable. However much one may will the contrary, one will remain essentially an outsider; everything one says or writes about one's experiences will, in the last analysis, be from the point of view of an outsider.[165]

Thus, Fremlin dismissed the possibility of actually 'becoming' a member of a different social group simply by walking in their shoes. Whilst the experience element of ethnographic research was important to Fremlin, she denied the possibility of researchers being able to escape entirely their own subjectivity. Fremlin's perspective, developed in the late 1930s, foreshadows the critique of ethnographic realism, as discussed in the introduction, which was not developed formally until much later in the twentieth century.[166]

The language and imagery which Fremlin used in her introductory passages to ironically justify the use of ethnography to explore sections of her own society was significant. She wrote:

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[162] Ibid., pp. 52 & 55.
[163] Ibid., pp.12 & 55.
[164] Ibid., p.v.
[165] Ibid., p.vi.
[166] See above, p.22.

> I can think of no more striking condemnation of present-day society than the fact that there is room in it for books like this. And there are coming to be a good many of them. It is becoming more and more the fashion for people like myself to come down from a university, or out of Mayfair, and go and work as charwomen, waitresses or whatnot, to see what it is 'like'. And when we have found out what it is 'like', we come back among our old friends and tell them about it.[167]

That such 'amateur job-crawlers' and 'dilettante adventurers' like Fremlin existed at all was a condemnation of the state of British society, that they were considered to 'have a real function as purveyors of information' was, according to Fremlin 'astounding' and 'sociologically horrifying'. The reason that Fremlin felt social researchers such as herself had a function at this time was because of the lack of free communication between the middle classes and the working classes. Belonging to different classes entailed speaking different languages, thinking different thoughts and essentially inhabiting different worlds. Thus, Fremlin and her fellow ethnographers can act to some extent as messengers and interpreters 'between the two worlds'. Fremlin recognised that they may be inefficient interpreters 'but at present we have no efficient competitors, so we enjoy a scarcity-value that we do not deserve'.[168]

Using terminology which is particularly evocative of the anthropological connotations of ethnographic research, Fremlin reflected on the alien nature of the charwoman and her world as far as the middle and upper classes were concerned:

> Most of your friends see, and probably speak to, at least one charwoman every day of their lives. Yet if you were Christopher Columbus retuning from the New World, Jules Verne from a voyage to the moon, you could scarcely be received with more amazement, more plied with wondering questions.[169]

She went on to suggest that

> The travellers' tales that the Athenians listened to of old; tales of fire-breathing monsters; of three-headed men; of ants as big as donkeys; these tales are as nothing compared with the travellers' tales some of your upper-class friends will expect you to bring back about the 'working classes'.[170]

Fremlin established further parallels between exploring the world of the British working classes and societies in distant lands by noting the tendency of her friends to refer to the working classes as a 'homogenous "them"', as if they were zoological specimens or natives of a lost tribe.[171] It would appear that Fremlin was trying to drive home the irony of having to use such anthropological methods to explore aspects of her own society.

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[167] Fremlin, *The Seven Chars*, p.1.
[168] Ibid., pp.1-2.
[169] Ibid., p.3.
[170] Ibid.
[171] Ibid., p.7.

The Seven Chars of Chelsea has been described as a somewhat directionless combination of social survey and fiction, resembling at one moment political polemic and comic anecdote the next and it is certainly true that no sharp division was made by Fremlin between field experience and analysis.[172] Her narrative accounts of episodes of ethnographic experience were interspersed with flurries of analysis and commentary on the situation in general.[173] The way in which Fremlin wrote her account was periodically reminiscent of a novel, perhaps unsurprising given her subsequent career as a noted writer of mystery fiction. She used the first person liberally, expressed her feelings, quoted direct speech and described things imaginatively. Fremlin's account of her first visit to a registry office in search of domestic work demonstrates her informal writing style:

> I arrived at nine o'clock in the morning, opened the door labelled 'Please walk in' and found myself in a dingy office which I can only describe as littered with furniture. In one corner a pale-faced, pimpled girl of about sixteen was typing with incredible slowness. Another, rather older, was cowering over a very small gas-fire and doing something to her nails. As I came in she jerked her head towards the gloomy little typist. The latter got up and picked her way through some particularly irrelevant pieces of furniture to the table in front.
> 'Yes, Miss?' she said, with a lack of enthusiasm which I have never seen equalled, even at the Labour Exchange.
> I explained that I wanted daily general work.
> 'No dailies, is there, Maisie?' she said.
> 'Sorry,' said Maisie to me. 'But Mrs Crow ain't here yet. You'd better sit down.'… 'No, I've nothing for you to-day I'm afraid,' she concluded, in a voice which said, as clearly as any words could have done: 'I can't imagine why you have come here wasting my time like this.' I crept out discomfited.[174]

Fremlin revealed her subjective involvement with the researched population. She positively enjoyed her time spent living and working with Lily and Mackie, other domestic servants with whom Fremlin lodged in the boarding house, revelling in the 'vitality', 'carefree gaiety' and 'freedom' that she enjoyed there.[175] Fremlin also noted her respect for one domestic servant in particular, who she sensed fought 'like a drowning man' against the 'nightmare boarding-school' scenario in which she found herself.[176] Fremlin's subjectivity resurfaced repeatedly in the text as she revealed the extent of the advocatory nature of her research endeavour.

Fremlin devoted nearly half of her book to a narrative account of fieldwork. It is difficult to categorise it as being more akin to the social exploration of the Victorian and Edwardian period or more in keeping with social scientific ethnography. Fremlin did separate her account of fieldwork from her analysis to some extent but the field work

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[172] Giles, "'A Little Strain...'", p.43.
[173] For example, Ibid., p.20.
[174] Ibid., pp.9-10.
[175] Ibid., pp.71-72.
[176] Ibid., p.40.

narrative occupied a place of central importance in the text; it was not simply about establishing an experiential authority and then retreating within it to focus on constructing an authoritative analysis.

For Fremlin, the world of domestic service presented problems which needed to be remedied, and she used her text not simply to explore and describe domestic service from the perspective of the domestic servant, but also to analyse what she perceived as the problems within this world to be, and proposed solutions for them.[177] As *The Seven Chars* progressed, and Fremlin gathered experience of domestic service, her advocatory statements regarding not just domestic service but also class became increasingly strident. Far from limiting herself to advice on employer-employee relations, she advocated the removal of a system of 'competitive capitalism' which compelled the employer to be a 'slave-driver' and the worker to be 'an obstinate saboteur'.[178]

It is in the analytical chapters in the second half of the text that issues about Fremlin's authoritativeness come to the fore. Having worked as a domestic servant herself, and having spoken to many employers, Fremlin felt that she was in a position of authority to make pronouncements on the world of domestic service. In the second half of her book, Fremlin's tone became increasingly that of someone lecturing employers about their cultural ignorance of their domestic staff.[179] And, despite her modest claims to be making only 'very tentative suggestions', Fremlin went on to comment on the psychology of young domestic servants with reference to their perceived wastefulness.[180] The point is that Fremlin, as a result of her time spent immersed in the world of the domestic servant, could claim to know and understand the behaviour of domestic servants better than domestic servants could themselves. Fremlin's covert experience of domestic service gave her the confidence to speak authoritatively about the matter.

Mass-Observation, Celia Fremlin and War Factory Work

After reviewing *The Seven Chars of Chelsea* favourably in the *News Chronicle*, Tom Harrisson invited Fremlin to become a Mass-Observer.[181] Three years after Fremlin published *The Seven Chars of Chelsea*, M-O published *War Factory*, an ethnographic

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177 Ibid., p.vi.
178 Ibid., p.175.
179 Ibid., pp.100-120.
180 Ibid., pp.121-126.
181 Giles, '"A Little Strain..."', p.50.

account of a war munitions factory in Wiltshire, which Fremlin had undertaken the covert fieldwork for. This ethnographic account is of particular interest for two of reasons. First, as a product of M-O, a social research institution which, as discussed earlier in this chapter, had a troublesome relationship with academic institutional social sciences, *War Factory* occupies a significant spot in the history of covert ethnography.[182] Second, only two years before *War factory* appeared, Jahoda had completed her ethnographic study, in a similar research context, yet she had firmly situated her account within the framework of the academic social sciences. Therefore, some useful comparisons can be made between Fremlin and Jahoda's work.

Dorothy Sheridan, M-O's archivist, has written that *War Factory* received a warm reception by the press when it was published in 1943, much more so than other M-O publications. On release, it was hailed by the *New Statesman* as the 'first coherent and serious study' of an industrial wartime setting.[183] The relative popularity of M-O's account of wartime factory life must have pleased Harrisson, who, in the midst of his attack on academic social science in general, had singled out Jahoda's report on factory life for specific criticism.[184] Furthermore, it has been praised by social historians, such as Summerfield, who referred to it as 'a superb picture of the responses of a group of women, mainly unaccustomed to industrial work, to the processes and work-place relationships of an engineering factory, and the prejudices with which they were confronted by men and management'.[185] It must be noted, however, that *War Factory* was far from typical of M-O's work in that it entailed one Mass-Observer, namely Fremlin, being covertly embedded in the context under observation for an extended and intensive period. The uncharacteristic nature of the method was recognised at the time by Harrisson, and by subsequent historical commentators, such as Calder.[186]

In her retrospective preface to *War Factory* written in 1987, Fremlin recalled that in 1943 she was called up and, as far as she was aware, had been sent to work as an unskilled hand in the machine shop of a factory based in Wiltshire at random. She suggested that wherever she had been sent, she would have determined to use it as a research opportunity for M-O, for whom she had worked since 1939. Unbeknown to Fremlin, however, Harrisson had 'pulled various strings' in order to have her set to work in a factory where

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[182] See above, pp.87-90.
[183] Fremlin, *The Seven Chars*, p.x.
[184] M-O, *War Factory*, p.10.
[185] Ibid., p.x; Summerfield, 'Mass-Observation', p.447.
[186] M-O, *War Factory*, pp.6-7; Calder, 'Mass-Observation', p.131.

the management was keen for M-O to investigate the business from the perspective of the shop floor.[187] It is unclear at what stage Fremlin learnt that the ethnographic research she was undertaking was, in fact, to be used to answer specific research aims, but the effective commissioning of the study by factory management undoubtedly affected the product of the field work, the ethnographic text. Summerfield has expressed concern about the circumstances which led to the production of *War Factory,* suggesting that it effectively amounted to M-O spying on a workforce on behalf of their management in order to improve productivity.[188]

The amount of time that Fremlin spent working in the factory is not specified, but it is clear that she was the only researcher involved in the fieldwork and that she assumed a covert research role, working alongside the other machine shop girls, living in similar circumstances and socialising with them to a certain extent. In short, she effectively immersed herself in their lifestyle for a certain period of time. During this time, she used her own 'personal brand of speedwriting', memorized 'lively bits of conversation' and established 'a reputation as an almost obsessional letter-writer' to record data secretly. The covert nature of the research was important, according to Fremlin, because if the other factory girls had been aware of her research agenda, 'all spontaneity would go out of their behaviour': some would have resented her as a management spy, whilst others would have been unable to resist the urge to show off. Without the element of covertness, Fremlin remarked that the research would have become 'stilted and artificial to the last degree'.[189]

The book is highly structured and the material was not related in a chronological narrative; rather it was organised into sections and chapters in order to address the research aims.[190] Fremlin did not just report her observations; she tried to synthesise them into a coherent explanation of the context she was studying. Fremlin may have undertaken her observations in an open-ended way, but the ethnographic text was constructed, possibly with the help of Harrisson, in response to the managers' concerns about the machine shop girls' productivity. For example, in one chapter concerned with the attitudes of the girls towards their work, Fremlin reported that it was the women's apathy towards the factory as a whole which was the 'biggest problem with which the authorities are faced'.[191] That the account was written to address the concerns of management had, as Summerfield

[187] M-O, *War Factory*, p.vii.
[188] Summerfield, 'Mass-Observation', p.447.
[189] M-O, *War Factory*, pp.vii-viii.
[190] Ibid., p.v.
[191] Ibid., p.43.

suggested, an impact on the construction of the report. For example, at one point, Fremlin suggested that the management might take more advantage of the machine shop girls' respect 'for the really high-ups' in encouraging them to improve their productivity.[192] Far from simply reflecting on the situation in the factory as experienced by Fremlin, *War Factory* was used to provide specific advice for factory management. There is one other point to note about the thematic structure of *War Factory*. A thematic organisation of research data, as opposed to a narrative account of field experience, has been defined as a hallmark of professional ethnography produced in the anthropological paradigm. However, the structural professionalism of *War Factory* was counterbalanced by other features of the book, such as the informal writing style which will be discussed in more detail.

In the opening chapter of the text, Fremlin described the Wiltshire town in which the factory was situated and how the influx of newcomers to work in the factory has affected the local population. The most striking feature of this opening section was the authoritative way in which Fremlin assumed the role of the all-seeing, all-knowing presence. She explained commandingly how each section of the community perceived the other, and stipulated why the viewpoint of each was skewed in its situatedness.[193] The stance which Fremlin adopted is reminiscent of the anthropologist who, from his or her 'outsider' vantage point, can comprehend the native's world utterly. However, Fremlin's authorship of this text was compromised on several fronts. First, clearly, there is the problem that Fremlin was part of M-O, a research collective, and although she was the only fieldworker, she may not have been the only individual involved in translating the field experience into a text. This lack of clarity is reflected in the fact that the author of *War Factory* was given as 'Mass-Observation' and that it was never clearly stated exactly what role Harrisson had in the construction of the text. In the final paragraph of the final chapter the reader is informed that '[t]hese are the facts as We saw them', further adding to the ambiguity of authorship.[194]

Second, members of the factory management were allowed to read and liberally annotate the ethnographic text. The labour manager usually limited himself to inserting factual information in footnotes that were relevant to the body of the text.[195] The works manager, however, commented extensively on the text and at times even contradicted

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[192] Ibid., p.68.
[193] Ibid., pp.13-17.
[194] Ibid., p.121.
[195] For example, Ibid., p.13.

Fremlin.[196] This affected the authoritativeness of the text as it undermined Fremlin's ethnographic authority. Fremlin included a description of an episode on the factory floor, during which of the machine shop girls was unhappy that she had been transferred to the assembly room, which she regarded as a punishment for bad behaviour. The girl returned to the machine shop cloakroom to air her grievances with her former colleagues, and to tell them about her encounter with the works manager which preceded the unpopular transfer. The works manager, via a footnote, contradicted his employee's version of events, referring to it as a 'perversion of the truth' and dismissed it as a show of 'bravado' intended to impress her workmates.[197] That the manager's attack on the integrity of one of his employees was allowed to be published raises questions over the extent to which the commissioners of the research controlled the ethnographic project.

There was a third way in which Fremlin's authority as the author of this ethnographic text was challenged. At times, Fremlin referred to herself in the first person, but at other times she used the third person: 'an investigator' or 'the observer'.[198] The uncertainty over the authorship of the text was emphasised when Fremlin referred to herself in the third and second person in quick succession writing '[l]et the investigator tell the tale: She was set to work…'[199] To heighten the confusion, Fremlin sometimes directly quoted her own speech and observations which she had, presumably, recorded in her field notes.[200] At other times, however, she appeared to rely on her memory to describe how she felt in the field, and included it in the body of the text:

> And certainly the time from two o'clock till six seems to go slowly. At about three o'clock one gets the feeling that the time will *never* pass; you think to yourself: after a whole hour, it will still only be four o'clock, and there will be two more hours to go after that…One begins to make idiotic bargains with oneself: if I drill a hundred of these holes without looking up, then by the time I do look up five minutes will have passed.[201]

Switching repeatedly between 'I', 'the observer' and 'she' makes it hard to pin down Fremlin in relation to the research context. 'I' suggests that Fremlin placed some importance on her own experience of the factory life. 'The observer' suggests that she was merely situating herself within the context in order to passively observe the behaviour of others. 'She', perhaps most confusingly, suggests that Fremlin was imagining herself into the role of a factory girl.

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[196] For example, Ibid., p.17.
[197] Ibid., pp.54-55, footnote number 1.
[198] For example, Ibid., pp.19 & 26.
[199] Ibid., p.56.
[200] For example, Ibid., pp.19-22 & 26.
[201] Ibid., p.30.

Stylistically, *War Factory* is rather incongruent. Seemingly at odds with the highly structured nature of the text and the drive towards organising the ethnographic material in such a way as to answer specific research aims, the text often reads, like Fremlin's *The Seven Chars of Chelsea*, in a novelistic way. Fremlin often included large sections of 'verbatim' accounts of the activity which she observed in the factory. These sections comprised direct speech and descriptive material which often covered two pages at a time.[202] For example, Fremlin related this passage in her text:

> 'Who's got my overall?' says a sulky dark-haired girl, pushing about among the chaos of coats and overalls hanging up. 'This is my peg, I left it hanging there last night. Who's got it?' 'You're the mug…'[203]

Fremlin also included phrases in italics in brackets after direct quotations, akin to stage directions. For example, she quoted one factory hand as saying '"You shouldn't leave it on the floor, then it wouldn't get trod on." (*General laugh from immediate neighbourhood.*)'[204] Fremlin's tendency to frequently include the direct speech of the research subjects and to use the present tense when conveying events to the reader lend *War Factory* a dramatic literary quality which, while present in texts such as Hugh Massingham's *I Took Off My Tie,* is at odds with other, more academic, manifestations of ethnography, such as Jahoda's social psychological analysis of factory life, also featured in this chapter.

As an ethnographic text, *War Factory* is quite unusual and it is difficult to apply the arguments of Parssinen and James Clifford about the nature of ethnography to it. Because Fremlin's authorship is diluted to such an extent, it is hard for any sense of ethnographic authority to be established, and the fact that there was no clear 'fable of rapport' exaggerated this lack of authority. The text assumed the topical structure normally associated with professional social scientific ethnography, yet, like Bakke's *The Unemployed Man*, which will be analysed subsequently, the inclusion of episodes and examples from field experience throughout the analytical sections of the account detract from the authoritativeness of the analysis. Of course, this does not mean that *War Factory* failed as an ethnographic account; it simply means that it is harder to categorise as belonging to the journalistic tradition of social exploration, such as Massingham's *I Took Off My Tie*, or the emergent genre of academic ethnography to which Jahoda and Bakke's accounts belong. M-O's project was complicated by trying to follow a remit set by the

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[202] Ibid., pp.24-25.
[203] Ibid., p.24.
[204] Ibid.

commissioners of the research, the factory management. Given Harrisson's hostility to academic social science and his criticisms of Jahoda's work, it is unlikely that he would have wanted M-O's account of factory life to read like Jahoda's.

M-O was not alone in its hostility towards the domination of interwar social research by academic sociologists. In 1931, Sydney Frank Hatton published an account of working-class male youths based on his experience of them called *London's Bad Boys*.[205] Although the account is not ethnographic, it reminds us of the extent of social commentary being produced outwith the boundaries of academia and often in response to academic research. Hatton worked with juveniles in the slums of London through the 'London Men's Junior Institute', which provided education and recreation.[206] Its significance lies in its emphasis on experience rather than research as the route to understanding, and in its rejection of the findings of academic research into juvenile delinquency. The *Times Literary Supplement* referred to Hatton's work as 'a first-rate book, based on first-hand observation'.[207] Hatton clearly aligned himself against academic books on working-class youth:

> There are hundreds of books which deal with adolescence from a psychological and physiological point of view, but there are few that are sufficiently "popular" in aspect and style to appeal to the "man in the street." You will soon discover that this book is neither highbrow, academic, nor pedantic…[208]

He then wrote:

> I make no apology for the personal, 'chatty' style in which the book is written. It is not for the grey-beards and the gas-bags who would learnedly dissect each characteristic of adolescence, it is a practical appeal on behalf of 'Youth'…[209]

The point is that, despite the ascendancy of academic social research, including academic covert ethnography, during the interwar period, the work of Massingham, Fremlin and, to an extent, M-O, demonstrates the continuation of an alternative, non-academic tradition of social research.

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[205] Sydney Frank Hatton, *London's Bad Boys*, (London: Chapman and Hall, 1931).
[206] Hatton, *London's Bad Boys*, pp.24, 27 & 95-119.
[207] Review of 'London's Bad Boys', *Times Literary Supplement*, Thursday 21st January 1932, p.47.
[208] Hatton, *London's Bad Boys*, p.9.
[209] Ibid., p.10.

Edward Wight Bakke and Unemployment in the 1930s

Born in Iowa, Edward Wight Bakke gained his undergraduate degree in philosophy before going on to study divinity and the social sciences at postgraduate level at Yale University.[210] In fact, his covert research into unemployment in Britain was undertaken as part of his fieldwork for his doctoral research about 'the effect of Unemployment Insurance on the willingness and ability of workers to support themselves'.[211] Bakke received his Ph.D. from Yale in 1932 and, in 1933, he published his covert ethnographic account of unemployment in London, *The Unemployed Man: A Social Study*, based on his doctoral research. Back at Yale, Bakke was the Director of Unemployment Studies at the Institute of Human Relations between 1934 and 1939, publishing two books on an eight-year study of unemployment in Connecticut in 1940.[212] Bakke's focus shifted from unemployment to industrial relations and workplace organisation. From 1944 until the late 1950s, he directed Yale's 'Labor and Management Center' and he was frequently asked to advise governmental commissions and academic organisations on matters of industrial relations.[213]

Bakke's covert research into unemployment in London can be situated in the mass of literature which was produced in the 1930s in response to the problem of long-term unemployment. In 1935, the Pilgrim Trust agreed to fund three years of research by Oeser and his team into the social psychology of unemployment in Dundee.[214] In the same year, Walter Brierley, an unemployed miner from Derbyshire, published his novel *Means Test Man,* which dramatised a week in the life of one family as they waited for the formidable means test man to visit.[215] In 1938, the year Oeser's team were due to complete their research, the Pilgrim Trust commissioned another research project into unemployment which resulted in the publication of *Men Without Work* within the year.[216] However, Bakke's research is of particular significance because of its covert ethnographic element.

The focus of Bakke's research was Greenwich in London and he described his plan in his preface:

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[210] Guide to the E. Wight Bakke Papers, 1932-1971, Kheel Center for Labor-Management Documentation and Archives, Cornell University Library, 'Biographical Note', http://rmc.library.cornell.edu/ead/htmldocs/KCL05522.html , [accessed 21st June 2010].
[211] Bakke, *The Unemployed*, p.xiii.
[212] Edward Wight Bakke, *The Unemployed Worker: A Study of the Task of Making a Living Without a Job*, (New Haven: Yale University Press, 1940); Edward Wight Bakke, *Citizens Without Work: A Study of the Effects of Unemployment upon the Worker's Social Relations and Practices* (New Haven: Yale University Press, 1940).
[213] Guide to the E. Wight Bakke Papers.
[214] Oeser, 'Methods', pp.343-363.
[215] Walter Brierley, *Means Test Man*, (London: Methuen, 1935).
[216] The Pilgrim Trust, *Men Without Work*.

> This is what I determined to do: to take lodgings with a working-class family, to share their life insofar as it was possible to do so, to join in their activities or to loaf on the streets or at factory gates as the occasion might require, to go with them to clubs and churches and 'pubs', to join the hunt for a job, and during the whole process find out all I could of the causes and consequences and adjustments involved in unemployment among the men and women who were unemployed.[217]

Thus, there was a definite ethnographic element in Bakke's methodology. However, he also used his ethnographic status as a means to an end. Whilst living in Greenwich, he conducted interviews with workers, mostly unemployed, and with other members of the Greenwich community. He also used relevant statistical material from *The New Survey of London Life and Labour,* which had not yet been published.[218] Bakke's use of covert ethnographic methodology, and the way in which his work was received, can be used to understand the status of ethnographic method within interwar academic social science.

Bakke was, in one sense, keen to highlight that his research involved the innovative use of ethnographic methods. He suggested that the method exemplified in his research was 'that of living among those who were the objects of study and sharing as far as possible their activities and their thoughts'.[219] In his conclusion, Bakke reflected that during his research he hoped in the back of his mind that his use of this method to study unemployment 'would be of some value in the development of a technique of sociological research', presumably the method of covert participant observation.[220] To a certain extent, Bakke contrasted his use of covert ethnography with the quantitative methods more readily associated with sociology at this time. He stressed that the kind of qualitative data generated by his intensive approach was just as important as the quantitative material generated by other methods:

> These qualitative factors are an integral part of the truth about social change and institutions. The discovery of such factors is often impossible by means of cold blooded research on the basis of carefully planned questionnaires or the tabulation of recorded statistics.[221]

Bakke's departure from 'cold blooded' research methods was, on the whole, received positively by the press and academic community.[222] One reviewer in the *Journal of Political Economy*, George Wheeler, found in Bakke's text 'a freedom from academic

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[217] Bakke, *The Unemployed Man*, p.xiv.
[218] Ibid., pp.xiii & xv.
[219] Ibid., p.296.
[220] Ibid., p.249.
[221] Ibid., p.298.
[222] Review of 'The Unemployed Man', *Times Literary Supplement*, (September 21st, 1933), p.619; George S. Wheeler, 'Review of E. Wight Bakke, *The Unemployed Man* and *Insurance or dole?*', *The Journal of Political Economy*, Volume 44, Number 5, (1936), pp.707-709; James M. Williams, 'Review of E. Wight Bakke, *The Unemployed Man*', *American Journal of Sociology*, Volume 40, Number 2, (1934), pp.271-272; George Sinclair Mitchell, 'Review of E. Wight Bakke, *The Unemployed Man*', *American Economic Review*, Volume 24, Number 3, (1934), pp.520-521.

pattern and stiffness'.[223] Reiterating the contrast between the results of using ethnographic methods as opposed to a quantitative approach, Wheeler pointed out:

> We have become hardened to tabular presentations of the 'problem of unemployment. The term 'subjective' is often indiscriminately used to imply disparagement, whereas a subjective approach, as used in Dr Bakke's reporting, is perhaps most successful in conveying the plight of human beings.[224]

Despite Bakke's evident determination to pioneer the use of ethnography within a sociological context, and the praise he received for doing so, there is evidence to suggest that Bakke lacked confidence in the academic validity of his methodology.

Bakke focused on outlining his interview methodology in much more detail than he did the covert ethnographic element of his research. He emphasised that his conclusions were largely based on the analysis of data he gained from interviews with over 150 individuals, and he stressed his use of 'tabulation' and the assignment of 'serial numbers' required for this rigorous interview process.[225] Furthermore, Bakke was keen to demonstrate the extent to which he had used existing statistical research to check against his 'more particular and human study', to guard against the possibility that his research subjects were not typical of the unemployed at large.[226] He referred to the statistical background of official analyses of unemployment against which he 'again and again placed…[his]…more human picture that…[he]…might be sure it had no serious distortions'.[227] This implies that Bakke had a lack of faith in the academic validity of his covert ethnographic material, or a lack of faith in the reception such material would receive. He needed recourse to a sound interview methodology and the support of statistical analyses of unemployment to make his account authoritative.

Using Clifford's ideas on twentieth century anthropological texts, we can develop further insights into Bakke's use of the ethnographic method. Clifford looked at the way anthropological ethnographers between roughly 1900 and 1960 derived authoritativeness from their field experience, from the notion that 'you are there, because I was there'.[228] During this period,

> a new conception of field research established itself as the norm for European and American anthropology. Intensive fieldwork, pursued by university trained specialists, emerged as a

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[223] Wheeler, 'Review', p.707.
[224] Ibid., p.708.
[225] Bakke, *The Unemployed*, pp.279-283.
[226] Ibid,, pp.48-49.
[227] Ibid., p.50.
[228] James Clifford, 'On Ethnographic Authority', *Representations*, Number 2, (1983), p.118.

> privileged, sanctioned source of data about exotic peoples…[B]y the mid-1930s the new style had been made popular, institutionalized, and embodied in specific textual practices.[229]

This new conception of field experience gave the ethnographer 'an authority both scientifically validated and based on a unique personal experience', and by the 1930s this style had come to dominate anthropological texts.[230] As we have already discussed, Malinowski was instrumental in establishing this new type of fieldwork and, as Clifford points out, *Argonauts* was 'archetypal of the generation of ethnographies that successfully established participant–observation's scientific validity'.[231] The advent of the professionally trained field worker brought into being a 'powerful new scientific and literary genre, the ethnography, a synthetic cultural description based on participant-observation'.[232] Thus, the use of intensive methods by anthropologists resulted in a new type of anthropological text appearing. As Clifford explains:

> Fieldwork was now centred on the *experience* of the participant-observing scholar. A sharp image, or narrative, made its appearance – that of an outsider entering a culture, undergoing a kind of initiation leading to "rapport"…Out of this experience emerged, in unspecified ways, a representational text authored by the participant-observer.[233]

In classic ethnographic accounts of this period, the stereotypical 'fable of rapport' narrated the way in which the ethnographer achieved full participant observer status within the field. Once this rapport was established, according to Clifford, the professional anthropological ethnographer disappeared within it. The quasi-invisibility of the participant observer was paradigmatic; it was 'an established convention for staging the attainment of ethnographic authority'.[234] Separating the field experience from the analysis has important implications for the text as a whole. As Clifford explains, in these authoritative and professional ethnographies produced by anthropologists from the 1920s onwards:

> it is important to note what has dropped out of sight. The research process is separated from the texts it generates and from the fictive world they are made to call up. The actuality of discursive situations and individual interlocutors are filtered out. But informants – along with field notes – are crucial intermediaries, typically excluded from authoritative ethnographies. The dialogical, situational aspects of ethnographic interpretation tend to be banished from the final representative text.[235]

When we apply the standards of academic authoritativeness which Clifford associates with anthropological ethnographic texts of this period, we can see how Bakke failed to achieve

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[229] Clifford, 'On Ethnographic Authority', p.120.
[230] Ibid., pp.118 & 120.
[231] Ibid., pp.123-124. See above, pp.90-95.
[232] Ibid., pp.124 & 127.
[233] Ibid. p.128. Emphasis in original.
[234] Ibid., pp.132-133.
[235] Ibid., p.132.

this authority and can understand why, as a result, he felt the need to bolster his covert ethnographic account of unemployment with recourse to the trappings of quantitative sociology.

Bakke did attempt to claim the kind of experiential authority over the unemployed in Greenwich that Clifford suggested was a necessary facet of the new type of professional and scientific ethnographic text. Bakke suggested that his ability to speak authoritatively about his research subjects came from his experience of their world gained through ethnographic research. Bakke demonstrated this when he spoke about the notion of workers having no control over their lives:

> This feeling of control has important consequences. It causes the worker to feel a minimum of responsibility for his own fate; for responsibility goes with control. No one who has not shared the life the worker can realise the number of points at which the ultimate decision as to his way of life rests with others.'[236]

The implication was that only Bakke, armed with his direct experience and understanding of the unemployed was capable of making this pronouncement. We can also clearly delineate a 'fable of rapport' in Bakke's text. In the opening pages of the first chapter, Bakke established his rapport with the unemployed men of Greenwich. He described the scene in a working men's club where he engaged local men, especially George Roberts, in conversation. Bakke marked out this conversation as being symbolic of his entry into the world of the unemployed:

> This conversation in a Greenwich working men's club was the beginning of an experience of intimate association with English workers and particularly with unemployed workers. I went to live in their midst in search of answers to several questions…Out of that experience came answers to those questions, answers which fell from the lips and stood revealed in the actions of George Roberts and hundreds of his workmates.[237]

Bakke began to lose the ethnographic authority that he had worked hard to establish when he failed to disappear within his text once rapport had been established. He did not retreat into a detached analysis of the unemployed in Greenwich and he allowed his 'fable of rapport' to spill into the rest of his text. Using the insights of Parssinen into ethnographic writing, we should be able to explain why the way in which Bakke wrote his text may have detracted from the professionalism and social-scientific status of his account.

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[236] Bakke, *The Unemployed*, p.10.
[237] Ibid., p.2. Bakke's establishment of rapport through a conversation in a working men's club can be compared to Massingham's account of taking off his tie. Massingham, *I Took Off My Tie*, pp.5-6. See above, p.105.

As discussed, Parssinen suggests that an ethnographic journey is at the root of all ethnographic research projects.[238] This notion of journey tends to be evident throughout ethnographic accounts associated with the tradition of the social explorers, such as Massingham's. However, using the anthropological paradigm, professional social scientists tend to present ethnographic texts in such a way that the 'journey', the fieldwork experience, is displaced. Field experience, and interpretation of this experience are inextricable from the product of research, yet professional social scientists are discouraged from portraying themselves as being at the centre, or even having any part in the written account, of their research. There is, in essence, a tension between the way in which social scientists are expected to communicate research findings to an audience and the narrative form which flows naturally from ethnographic experience. Often, professional ethnographers organise and present their material not chronologically but topically, sometimes telescoping the experiential aspect of their work into a preface or first chapter, or sometimes suppressing it entirely. As Parssinen explains, in ethnographic accounts where the material is presented topically, 'the journey may be suppressed altogether, and the fruits of induction wrenched from time into the ethnographic present, to become static, unarguable truths'.[239] She suggests that ethnographic texts which use a narrative structure and those which use a topical structure essentially form separate literary genres.[240]

The problem with Bakke's text is that it does not entirely fit into either of these genres. As professional social scientists tend to do, according to Parssinen, Bakke structured his account topically in relation to specific aspects of the life of the unemployed; it is not a straightforward narrative account of his time spent in the field such as a social explorer might produce.[241] However, he failed to maintain a rigid topical structure once he had established his ethnographic authority. Once Bakke had established his rapport with the research subjects, he did not disappear from view and focus on the analysis of his data. For example, to support his claim that there was a tendency among working-class men to blame machinery for the uncertainty of their work to some extent, Bakke quotes no fewer than four different men, at some length, in succession.[242] He did not have the confidence to make the statement without backing it up with evidence. He seemed unsure that his ethnographic status alone would imply sufficient authority to make such statements. Even in the body of the text, Bakke interrupted his analysis of aspects of unemployment to

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[238] See above, pp.24-25; Parssinen, 'Social', p.206.
[239] Parssinen, 'Social', p.206.
[240] Ibid., p.217.
[241] Bakke, *The Unemployed*, p.xi.
[242] Ibid., p.3.

include narrative accounts of field experience.[243] His consistent use of the first person throughout the text is the most obvious manifestation of Bakke's failure to construct a topical ethnographic account in which the ethnographer remains invisible.[244]

The point is that Bakke was trying to establish the use of ethnographic method as a valid sociological technique, but he failed to follow the textual conventions which made the ethnographic accounts of anthropologists legitimately and authoritatively scientific and professional. We will now move on to explore Jahoda's ethnographic text about women's factory work. By applying the same ideas about authority and structure to her work, the failings of Bakke's account in terms of achieving professional social scientific status will become even clearer.

Marie Jahoda and 'Some Socio-Psychological Problems of Factory Life'

When Lazarsfeld moved to America after their divorce in the mid-1930s, Jahoda became the director of the non-University research institute he had founded in Austria and continued to be a committed socialist. In 1936, the offices of the research institute were raided and Jahoda was arrested, spending eight months in prison, most of that time being spent in solitary confinement. Under pressure from British social scientists and the French prime minister, Jahoda was released on the condition that she left Austria.[245] In 1937, Jahoda travelled to England where she was associated with various projects including an Austrian refugee organisation, the London wartime social survey, a covert socialist radio station and, most significantly, undercover research into the psychology of war factory work.[246] In 1941, Jahoda used the method of ethnography, initially covertly, to research the socio-psychological problems experienced by women factory workers in Britain during the war.[247] With permission from the managers of the factory to carry out her research, Jahoda worked for several months. During this time, mutual home visits occurred between Jahoda and some other factory girls. For the first month, she worked covertly but then, for some unknown reason, the other factory workers were made aware of her research agenda.[248] Jahoda published her ethnographic text as a concise article in the *British Journal of Psychology*.

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[243] See, for example: Ibid., pp.66-67.
[244] See, for example: Ibid., pp.4,6,11 & 22.
[245] Lisl Klein, 'Jahoda, Marie (1907-2001)', *Oxford Dictionary of National Biography*, Oxford University Press, (2005), http://www.oxforddnb.com/view/article/75873 , [accessed 16th June 2010].
[246] Klein, 'Jahoda, Marie'.
[247] Jahoda, 'Some Socio-Psychological Problems', pp.191-206.
[248] Ibid., p.193.

Jahoda justified her use of ethnographic methodology in the first section of her article. The problem she intended to tackle was more methodological than practical. She suggested that a growing number of psychological investigations into factory life were being carried out but that they were inadequately acquainted with the necessary background knowledge. In addition, these projects had used interviews and questionnaires to research factory life, and Jahoda suggested that these methods were inappropriate. They worked well, she argued, when applied to 'educated and sophisticated' research populations such as undergraduate students, but they did not work so well, according to Jahoda, when applied without alteration to 'the worker'. Workers, 'when confronted with a scientist who generally is also a middle class person and an obvious outsider' asking questions, would give whatever answer they thought to be 'right'.[249] Therefore, an alternative method had to be used to bypass the problems associated with existing research on the subject.

Although Bakke and Jahoda were both using an ethnographic methodology and they were both researching and writing as academic social scientists, their ethnographic texts were noticeably different. Overall, Jahoda confidently employed ethnographic method in a flawlessly professional and social scientific way. Like the anthropological ethnographies which were dominant in the 1930s, Jahoda established her rapport and her experiential authority in a subtle, sparse way, and then disappeared within her text. She devoted just two paragraphs to discussing method and fieldwork experience; here she simply noted that 'a friendly contact had been established' with her fellow factory workers. Even in this tightly contained discussion of field work, Jahoda referred to herself in the third person as 'the investigator' and 'the observer' rather than 'I'.[250] Jahoda managed almost completely to suppress her ethnographic journey in the final text. She referred to herself in the first person or to events in the field which involved her directly only twice in the whole account. Once, for example, she recalled that she was reprimanded by a fellow worker for looking at her watch to early on in a shift.[251] When Jahoda was forced to illustrate a point of her analysis with an actual example from the field, she quoted from her field notes, carefully distinguishing these passages from the body of her text by presenting them in a smaller font. For example, to illustrate the lack of interest that the factory girls had in their work, Jahoda quoted her field notes, which were clearly distinguishable from

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[249] Ibid., pp.191-192.
[250] Ibid., p.193.
[251] Ibid., p.201.

her analysis: 'A group of eleven girls who were working for ten days on a special order concerned with an advertisement, were asked afterwards whether they knew what was advertised on each of the many thousands of objects they had been handling. None of them knew'.[252] Thus, Jahoda decentred her experience in the field to such an extent that she managed to be almost invisible in her own text.

Having established her experiential authority, Jahoda constructed a detached academic analysis of the problems of factory life, organising her account topically around issues such as 'social patterns in factory life', the 'time experience of factory girls' and the 'social conditioning of the personality of the factory girl'.[253] Her narrative or journey is almost completely subsumed by detached analysis. Jahoda successfully filtered out any trace of her fieldwork experience and her analysis consisted of 'unarguable truths' which had been extracted from their context in the field. For example, Jahoda made this claim on the basis of her experience but entirely alienated from it:

> The external sign which unites members in the horizontal dimension is the wearing of an overall. The 'headgirls' wear an overall different in colour and cut from those worn by the ordinary workers. This distinction corresponds to different functions, and is meant and understood as a symbol.[254]

Unlike Bakke with his recourse to statistics, Jahoda uncompromisingly asserted her authority and the validity of her ethnographic research by generalising her results. For example, she mostly referred to the researched in abstract terms as 'the factory girl', a population of which she claimed to have utter comprehension due to her experiential authority.[255] It appears that Jahoda managed to construct her ethnographic account in such a way that it appeared convincingly professional and social scientific. Yet, her research was criticised for its use of 'imposing vocabulary' and the trappings of 'learned' sociology by Harrisson, one of the founding members of M-O who was involved in the production of *War Factory*.[256]

Conclusion

This discussion has demonstrated the extent to which covert ethnography was experiencing a period of transition between 1914 and 1945. An analysis of the work of Massingham, Fremlin, M-O, Jahoda and Bakke has illustrated the state of flux in which

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[252] Ibid., p.197.
[253] Ibid., pp.191-206.
[254] Ibid., p.194.
[255] Ibid., p.195.
[256] M-O, *War Factory*, pp.10-11.

covert ethnography was used in this era. The key methodological development was the establishment of a clear model of professional ethnography in the discipline of anthropology and the influence of this model extended beyond academic ethnography. Whereas the work of Massingham can be quite firmly located in the extra-academic tradition of social exploration, his use of anthropological imagery demonstrates the continued influence of anthropological endeavour on social exploration. M-O's *War Factory* and Bakke's study of unemployment could be characterised as attempts to make use of this new anthropological model of ethnography, with limited success. M-O's identification with an anthropological perspective was at odds with its uneasy relationship with academia at a time when anthropology was becoming increasingly academicised. Bakke, on the other hand, undertook his covert ethnography from a firmly academic standpoint. However, lacking confidence in the validity of his ethnographic data, he struggled to conform to the model of anthropological ethnography, and had recourse to other, more firmly established academically legitimate methods and sources of data. Jahoda's distinctly academic covert ethnography, when compared to Massingham's covert social exploration, illustrates how the emergence of a strong professional model of ethnography had come to shape the research method between the wars.

Chapter 3: Academic Covert Ethnography
from 1946 to 1969

Introduction

This chapter explores the covert ethnographic research undertaken by academics between 1946 and 1969, a period when relatively few covert ethnographic projects appear to have been undertaken. Out of the three academic research projects discussed in this chapter, two focused on the covert observation of work groups. Pearl King undertook research into one very specific factory-based work role in order to delineate and articulate the skill involved in this task.[1] Enid Mumford worked in three canteens catering for Merseyside dock workers as part of her doctoral research.[2] John Spencer's research, however, was not based on his observations of work. Rather, he used covert observation to study the relationship between criminal behaviour and serving in the Armed Forces.[3] We will first of all reflect upon how the convergence of a number of factors related to the state of social research in the immediate post-war period shaped social research at this time. Once we have familiarised ourselves with the three examples of covert ethnography from this period, we will be in a position to explore how this convergence may have hindered the use of covert ethnographic methods between 1946 and 1969. We will then be able to explore the construction of these covert ethnographies and attempt to relate this to the context in which they were produced.

Post-war Social Research at the Crossroads

Given the relatively few covert ethnographic research projects undertaken by academics in the post-war era, it is important to consider the context of social research in Britain at this time and to evaluate how conducive a climate this period would have been to such study. At the beginning of the last chapter, we noted in a general sense that sociology had not developed in any particularly meaningful way as an academic discipline between 1914 and 1945. In the decade preceding the war, empirical social scientists had mostly been occupied with the production of regional social surveys and it was suggested that

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[1] King published her work in two parts: Pearl King, 'Task Perception and Interpersonal Relations in Industrial Training: The Development of a Training Project in the Hosiery Trade. Part I', *Human Relations*, Volume 1, Number 1, (1947), pp.121-130; Pearl King, 'Task Perception and Interpersonal Relations in Industrial Training: Part II', *Human Relations*, Volume 1, Number 3, (1948), pp.373-412.

[2] Enid Mumford, 'Social Behaviour in Small Work Groups', *Sociological Review*, Volume 7, Issue 2, (1959), pp.137-157.

[3] John Spencer, *Crime and the Services*, (London: Routledge and Kegan Paul, 1954).

during the war, although the quantity of social research increased, there were no advances in terms of the quality of social research in Britain.[4] Just after the war, in 1946, the eminent sociologist Thomas Marshall delivered his inaugural lecture at the London School of Economics entitled 'Sociology at the crossroads' and he posed two questions: 'Where does sociology stand today? and Along what road should it travel into the future?'[5] His lecture gave the impression that sociology continued to languish in the doldrums as it had done during the war. Marshall admitted that in the recent past sociology had 'not enjoyed too good a reputation in this country and that even now [in 1946] it is still regarded in some quarters with a certain amount of suspicion.'[6] However, Marshall's lecture was forward-looking and he pointed out the path he thought sociology should take and the aspects of the discipline which needed to change. Marshall's lecture proved prescient and many of the points he raised shaped the practice of sociology in important ways over the next few decades, especially in terms of the subjects researched and the methodologies used in the post-war era.

In his lecture, Marshall noted the extent to which sociologists were being called upon in the immediate aftermath of the war to assist with post-war planning and reconstruction.[7] This link between empirical sociology and post-war planning and reconstruction can be clearly seen in the number of publications from the 1950s and 60s dealing with housing and notions of neighbourhood and community.[8] He urged sociologists to shift their focus from theoretical abstraction and the search for universal laws to pursue subjects empirically which had a bearing on the practical realities and problems facing Britain at the time.[9] The discipline of sociology, he suggested, 'need not be ashamed of wishing to be useful'.[10] Marshall's desire that post-war sociology should be practical and applicable to societal problems seems to have become a reality.

Writing in the early 1950s, Thomas Simey noted that the social research which had been carried out at Liverpool University in the post-war years was orientated towards identifying social problems and devising solutions to them. He also noted the involvement of academic social science in training social workers and administrators.[11] The practical

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[4] See above, p.84.
[5] Thomas Marshall, *Sociology at the Crossroads and Other Essays*, (London: Heinemann, 1963), p.3.
[6] Marshall, *Sociology*, p.4.
[7] Ibid., pp.8 & 9.
[8] See, for example: Michael Young and Peter Willmott, *Family and Kinship in East London*, (London: Routledge, 1957); G. Duncan Mitchell et al, *Neighbourhood and Community: An Enquiry into Social Relationships on Housing Estates in Liverpool and Sheffield*, (Liverpool: University Press of Liverpool, 1954).
[9] Marshall, *Sociology*, p.11.
[10] Ibid., p.22.
[11] Thomas Simey, 'The Analysis of Social Problems', *Sociological Review*, Volume 1, Issue 1, (1953), p.73.

applicability of the sociological research being carried out by the Department of Social Science at the University of Liverpool was reiterated in 1960 when an article reviewing their post-war work appeared in the *Sociological Review*.[12] The work of the Department was guided by the principal that there was 'no necessary conflict in seeking to advance both "fundamental" or theoretical and "problem-centred" or practical aims in the same researches…'. Their research output was dominated by the production of empirical studies involving the direct investigation of '"problem-centred"' phenomena, just as Marshall had envisaged sociological research should be.[13]

King, Mumford and Spencer, the three academics discussed in this chapter, all appear to have readily embraced these notions of utility and problem-solving, to some extent, in their research projects. King's article was a report on a research project designed to solve a very specific industrial problem: the efficient training of linkers for the hosiery industry. King suggested that the university department to which she belonged 'was characterised not only by a special interest in industrial training, but by a wish, more strongly developed than is common, to undertake practical problems in the field and to relate experimental and operational research.'[14] Furthermore, Mumford used King's work as 'the best known example' of how participant observation had been used successfully by research workers

> whose principal aims were to discover the reasons for particular technical or administrative difficulties, so that during and on the completion of the research, therapy could be applied and changes made that would help to remedy these problems.[15]

In keeping with the tenets of post-war sociology, as set out by Marshall and Simey, Mumford's research also focused on a key subject area, industrial sociology, and she too emphasised the potential for the practical application of her results for industrial management 'from a training, production and human relations point of view'.[16] However, Mumford also used her research findings in a more traditionally academic sense to challenge and corroborate the findings of other academic social scientists. Mumford frequently compared her findings with those of other academic social scientists interested in the behaviour of small groups. For example, she compared her findings to those of Elton Mayo and Donald Roy.[17] Mayo's work involved the use of overt observation to study the

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[12] W.H. Scott and J.B. Mays, 'Research Report No.1: Department of Social Science, the University of Liverpool', *Sociological Review*, Volume 8, Issue 1, (1960), pp.109-117.
[13] Scott and Mays, 'Research', p.110.
[14] King, 'Task...Part II', p.410.
[15] Enid Mumford, 'An Evaluation of Participant Observation as a Research Method for the Study of Work Groups in Industry', (Liverpool University, 1951), p.27.
[16] Mumford, 'Social', p.156.
[17] Ibid., see for example pp.140, 141, 145 & 155.

behaviour of workers at the American Western Electric Company and Roy used covert participant observation to explore workplace behaviour in a steel processing plant, again in America.[18] One of Mumford's principal research aims when she began her doctoral career was to challenge the results Mayo achieved through overt observation by making covert observations instead.

Spencer also appeared to embrace some of the aspects Marshall and others promoted in post-war social research. For example, Hermann Mannheim, who was Spencer's doctoral supervisor, emphasized the practical application of Spencer's results in his preface to *Crime and the Services*, suggesting that they would be 'of equal value' to academics and to administrators in the Services and the penal system.[19] This would seem to suggest that Spencer was keen to take on board the notion promoted in the post-war period that sociologists should strive to be useful and to contribute to the alleviation of social problems. He was keen to demonstrate that his work conformed to the model of sociology which eminent figures, such as Marshall, were establishing in the post-war period by emphasising the empirical nature of his research and the usefulness of his results. Nevertheless, there was also a theoretical element to Spencer's research in that he made extensive use of an existing academic historiography relating to criminal behaviour and aspects of life in the Armed Forces. In his foreword to *Crime and the Services*, Mannheim suggested that, apart from some American research, no comprehensive sociological work had been done on 'Service life' apart from Spencer's and that his was certainly the 'first detailed empirical analysis' of the relationship between crime and the Armed Forces.[20] Spencer was not attempting to refute or support any particular academic stance in his work but he did relate his work to a broad range of social scientific, psychiatric and psychological literature.[21] All three academic ethnographers from this era, therefore, attempted to embrace in some way what sociology aspired to be at this time, the empirical study of social problems which would lead to solutions on a practical level.

The focus on post-war reconstruction and social problems meant that in the years immediately following the war, social research was dominated by work relating to notions of neighbourhood, community and family as social researchers attempted to assist in the

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[18] Fritz Roethlisberger, *Management and the Worker: An Account of a Research Program Conducted by the Western Electric Company, Hawthorne Works, Chicago*, (London: Harvard University Press, 1939); Donald Roy, 'Quota Restriction and Goldbricking in a Machine Shop', *The American Journal of Sociology*, Volume 57, Number 5, (1952), pp.427-442.
[19] Spencer, *Crime*, p.vi.
[20] Hermann Mannheim, 'Foreword', in Spencer, *Crime and the Services*, (London: Routledge and Kegan Paul, 1954), p.v.
[21] He listed an extensive range of academic articles and monographs in his bibliography. Spencer, *Crime*, pp.290-299.

planning of post-war society.[22] In addition to the extensive work carried out by the Institute of Community Studies from 1953 onwards, other social scientists also focused their empirical efforts on communities of different scales. In 1958, Madelaine Kerr completed *The People of Ship Street* and others, such as Isabel Emmet, focused their attention on specific towns or villages as communities.[23] In 1960, social research at the University of Liverpool could be categorised into two main groups: community studies, which continued to attract academic attention, and industrial studies.[24] As the sixties progressed, industrial relations and 'race' relations became increasingly important in British society and they therefore attracted the empirical attention of social scientists. There is nothing per se about most of these broad areas of research which precluded the use of covert participant observation. It would, presumably, have been relatively straightforward to covertly observe some communities, such as some workplaces or geographical areas. Clearly, however, it would be unfeasible to covertly observe other communities. For example, given the rarity of non-white social scientists in post-war Britain, it would be difficult for many covert studies of non-white immigrant communities to have been undertaken.[25] Apart from King and Mumford, it appears that few, if any, social scientists used this method between 1946 and 1969 to covertly study industrial work situations or any other communities.

The relative absence of covert participant observation studies is particularly striking given the growth of sociological research overall during the 1960s. In his survey of sociological research in Britain, Ernest Krausz noted that the rapid development of the teaching of sociology in British universities and colleges had led to a concomitant increase in the number of research projects being undertaken. More than twice as many research projects were carried out between 1961 and 1966 than had been completed between 1945 and 1960. Throughout the 1950s and 1960s, Krausz noted that sociologists undertaking empirical research in the field either used the survey technique, a form of participant observation, or a combination of the two. He found that the survey approach appeared to be 'much favoured in British sociological research' and that research projects involving some form of observational method were more scarce. Observational techniques, Krausz suggested, covered a wide range of methods including disguised observation and varying

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[22] Marshall, *Sociology*, p.8.
[23] Madelaine Kerr, *The People of Ship Street*, (London: Routledge, 1958); Isabel Emmet, *A North Wales Village: A Social Anthropological Study*, (London: Routledge and Kegan Paul, 1964). Margaret Lassell's *Wellington Road* focused on the intensive overt observation of one family and their immediate neighbours on a street in a post-war housing estate: Margaret Lassell, *Wellington Road*, (London: Routledge, 1962).
[24] Scott and Mays, 'Research', pp.112-113.
[25] Eyo Ndem was a Nigerian-born researcher who researched social differentiation amongst the black population of Manchester in the 1950s but it is not clear if he was systematically covert or overt. Eyo Ndem, 'The status of colored people in Britain', *The Phylon Quarterly*, Volume 18, No.1, (1957), pp.82-87. Ken Pryce used his identity as a black Jamaican to gain entrée into the society inhabited by fellow West Indians in the St Paul's area of Bristol. His research will be discussed in chapter 4. Ken Pryce, *Endless Pressure: A Study of West Indian Life-Styles in Bristol*, (Harmondsworth: Penguin, 1979).

degrees of participation, but he did not refer to a single example of a British sociological research project undertaken in the 1950s or 1960s which had made use of covert participant observation.[26] The covert research of King, Spencer and Mumford undertaken at this time was atypical given the continued dominance of survey methodology in British sociology.

Before we move on to discuss King, Spencer and Mumford's research projects, it is important to point out the significant ways in which the context of institutional sociology in Britain had changed. In 1947, discussions began about the prospect of a journal for the LSE and, in 1950, the Director of the LSE, Sir Alexander Carr Saunders, established the *British Sociological Journal* (*BJS*), and two professors of sociology at the LSE, including Marshall discussed above, acted as joint editors.[27] In 1951, the British Sociological Association (BSA) was founded by a group of individuals associated with the LSE and with Political and Economic Planning (PEP), an independent organisation interested in research linked to policy.[28] In 1967, the BSA began publishing *Sociology*, which, alongside the *Sociological Review* and the *BJS*, became the third important forum for the publication of sociological and related texts in Britain.[29] The BSA was intended to provide a 'professional body for sociologists' which could 'represent the discipline to the outside world'. In her account of the history of the BSA, Jennifer Platt suggests that although not every eligible person belongs to a learned society, they are nonetheless a vital part of the academic social structure which affects the practice of a discipline across the board.[30] King, Spencer and Mumford were undertaking their research in an era in which the professionalisation of sociology was gathering pace and, as we will discuss below, this had important consequences for the practice of covert research.

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[26] Ernest Krausz, *Sociology in Britain: A Survey of Research*, (B.T. Batsford Limited: London, 1969), pp.2, 67 & 77-84.
[27] Frances Heidensohn and Richard Wright, 'The *British Journal of Sociology* at Sixty', *British Journal of Sociology*, Volume 61, Issue s1, (2010), Special Issue: The BJS: Shaping Sociology over Sixty Years, http://www.bjsshapingsociology.com/view/0/index.html , [accessed 28th July 2010], p.2.
[28] Jennifer Platt, *The British Sociological Association: A Sociological History*, (Durham: sociologypress, 2003), p.18.
[29] Platt, *The British Sociological Association*, pp.60-61.
[30] Ibid., pp.1 & 18.

Post-War Covert Academic Ethnography

Pearl King and the Manufacture of Hosiery

Born in 1918 in Surrey in the midst of a Zeppelin raid, King spent her early childhood in East Africa with her missionary parents.[31] In 1941, King graduated with a degree in psychology, with sociology and anthropology as subsidiary subjects. She then went to Edinburgh University to train in personnel management and for three years she worked as a Labour Officer in a Royal Ordinance factory.[32] As a student, King was politically active, describing herself as being on 'the socialist side'. By the time King was a Labour Officer, she described herself as a communist, promoting egalitarian and democratic relationships between herself and the women workers she supervised.[33] In the mid-1940s, King was seconded to London University to research the training of assembly-line workers and it was at this time that King became involved in covert research in the hosiery industry.[34] King began her training as a psychoanalyst in 1946, publishing her covert ethnographic accounts of the hosiery industry in 1947 and 1948. She went on to become not just a clinical psychoanalyst and psychoanalytic educator, but also a leading organiser of psychoanalytic politics at national and international level. In the 1980s, King was the first President of the British Psychoanalytic Society not to have medical training and has also made a significant contribution to documenting the history of the discipline of psychoanalysis.[35]

Using the method of covert participant observation, King was the undercover fieldworker in therapeutic problem-centred research undertaken in collaboration with an anonymous large British hosiery firm, an unnamed university department and 'a firm of industrial consultants…who specialised in the installation of psychologically designed training schemes'.[36] Towards the end of the war, it became evident that the manufacture of hosiery in Britain was suffering from a shortage of 'linkers' described as 'a skilled group of hosiery operatives concerned with the machine process which finishes stockings and

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[31] Women Psychoanalysts in Great Britain, Pearl King, http://www.psychoanalytikerinnen.de/index.html?greatbritain_biographies.html , [accessed 21st June 2010]; Ken Robinson, 'Introduction to Pearl King and her Work', in Pearl King, *Time Past and Time Present: Selected Papers of Pearl King*, (London: Karnac, 2005), p.1.
[32] Robinson, 'Introduction', p.2.
[33] Ibid., pp.2-3.
[34] Ibid., p.2.
[35] Ibid., p.2; Brett Kahr, 'Foreword', in Pearl King, *Time Past and Time Present: Selected Papers of Pearl King*, (London: Karnac, 2005), pp.xiii-xv.
[36] King, 'Task...Part I', p.129. Presumably, the industrial consultancy firm involved in the project was the Tavistock Institute of Human Relations since it was in their journal, *Human relations*, that the findings were published: King, 'Task …Part I', pp.121-130; Pearl King, 'Task...Part II', pp.373-412.

socks'.[37] It was discovered that before the war, linkers had been recruited and trained using a local cultural pattern that was impossible to replicate post-war. The problem was that none of the linkers could articulate clearly the skill of linking or break down the intricate processes involved so that it could be taught to a new generation of hosiery operatives quickly and efficiently.[38] With the on-site assistance of the Tavistock Institute, the hosiery firm set up a training centre where the idea of systematic industrial training could be put into practice. The training centre was successful from the start in establishing systematic training methods for other aspects of work at the hosiery firm, but it continued to prove impossible to systematically train linkers.[39]

It was at this point that it was decided to use the method of covert participant observation to break down the job of linking because, King noted, the resident consultant at the hosiery firm had become 'convinced that no method of observing the job from the outside would yield the "secret of the skill"'. As a psychologist, King was asked to suspend her University studies and learn the job of linking 'subjectively, from the inside'.[40] It was hoped that, as a psychologist, she would be able to acquire and then articulate the skill of linking which the true linkers had proved incapable of. The covert nature of the research was an essential component of the project. As King explained:

> It was decided that the writer should not appear directly as a psychologist, or indeed as officially connected in any way with either…[the hosiery firm or the firm of industrial consultants]. However, the disturbing effects of an additional investigator in an explicitly technical role were judged as likely to be doubtful in securing the necessary degree of acceptance in the trainee-group.[41]

Thus, King took on what she described as an 'operational role' as a 'trainee-instructor' in order to observe the other linkers at work covertly and to learn the skill herself. She was represented as an employee of a London firm which was considering moving into hosiery production after the war. Her cover story was that this London firm had seconded her to the training centre in order to train as a linker in preparation for her role as an instructor when she returned to London.[42]

King thought of herself as having a dual identity, as a trainee-instructor on the one hand, and as a psychologist on the other. As a trainee-instructor, she was able to become part of the training group and experience for herself 'the technical and emotional

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[37] King, 'Task...Part II', pp.373 & 374.
[38] Ibid., p.374.
[39] Ibid., pp.375, 380 & 385.
[40] Ibid., p.385.
[41] Ibid., p.386.
[42] Ibid.

components' of learning to link. As a psychologist, she was able to make observations of this experience and to check her findings against the experiences of other trainees and skilled linkers.[43] For King, posing as a trainee-instructor allowed her to observe the other linkers, but it also gave her the opportunity to experience the learning process herself and King made use of both of these sources of information.[44] King enjoyed success with this strategy and on her twenty-sixth day at the training centre she found she could link, slowly but accurately, and that 'from the articulation of this internal scheme a conception of the content of the skill could be built up'.[45]

King was in no doubt that it was because she had made a covert 'psychological analysis' 'from inside the task' that the skill of linking was successfully broken down.[46] When the trainees, including King, had finally developed the skill of linking, King described it as a result of 'a qualitative change arising out of the combination of intellectual, kinaesthetic, tactile, visual and emotional experience, and its integration into a total and new relationship between the individual and the task'. The trainees themselves found it extremely difficult to describe this process of acquiring the skill of linking and the 'articulation and communication of these change-points was the task of the psychological investigation'. The documentation of these change-points 'was the basis for reshaping the training programme in terms of the psychological reality of the learning process'.[47] King's academic status, as well as her practical experience of industry gained as a personnel manager would her shaped her ethnographic project.

John Spencer and the Relationship between Crime and Service in the Forces

Born in 1915, Spencer read classics at Oxford's Balliol College before moving to the LSE where he studied social sciences. After leaving the LSE, Spencer demonstrated his interest in crime by working with the probation service for Surrey County Council, a position which he returned to after the war. During the war, Spencer served with the Royal Artillery in France, Belgium, India and Burma and, on his return to Britain, he returned to the LSE where he became assistant lecturer and lecturer in social science. Spencer built his

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[43] Ibid., pp.386-387.

[44] For example, King suggested that feeling relaxed and maintaining good interpersonal relations were essential components of being able to link successfully. She made this claim on the basis of her own experience that she found it easier to link when she was engaged in conversation with the other trainees. Her observations of the other trainees confirmed her own experience – they also found that chatting to people with whom they had a good rapport enabled them to link more successfully. King, 'Task...Part II', pp.392-393.

[45] Ibid., p.387. The training model developed on the basis of King's findings proved successful and linkers were trained in a shorter period of time. Ibid., p.399.

[46] Ibid., p.388.

[47] Ibid., p.389.

doctoral research at the LSE on his practical experience in the probationary sector and military service, publishing the findings of his research in 1954. After leaving the LSE in 1953, Spencer pursued his academic interest in many aspects of social policy in Britain and at the University of Toronto. In 1967, Spencer became the first Chair of Social Administration at Edinburgh University and, reflecting his continued interest in the effect of military service, he was the Chairman of the Army Welfare Inquiry Committee between 1974 and 1975.[48]

The intention of Spencer's doctoral research was to explore the influence of life in the Armed Forces on criminal behaviour and he produced his book, *Crime and the Services*, in 1954 based on his doctoral research into this subject.[49] Using a combination of documentary analysis and covert observation and interviews, he undertook research at three institutions: Dartmoor prison, Maidstone prison and Sherwood borstal.[50] Although *Crime and the Services* was based on Spencer's research at all three penal institutions, Dartmoor required the most extensive use of covert participant observation as it proved 'immeasurably the more difficult' of the two prisons he visited to access. This was because a prison is, as Spencer noted, 'a closed community' par excellence and, given the geographical isolation of Dartmoor, it would not be possible for him to visit the institution without having a ready explanation for his presence.[51]

Spencer clearly understood that it was essential for the prisoners to identify him with the unofficial element in the prison; otherwise conversation between them would be inhibited.[52] Spencer acknowledged that 'it may be argued that the most sensible plan would have been for me to have laid all my cards on the table and not to have attempted to disguise the nature and object of my research.' Spencer countered this suggestion by saying that he did not at any point in his fieldwork attempt to conceal his interest in Service experience, which was the main purpose of his visit. Furthermore, Spencer said that it seemed 'undesirable that the prisoners should see themselves either as the object of study or the basis for an experiment.'[53]

The ex-Service prisoners at Dartmoor assigned a variety of identities to Spencer including that of the new chaplain, the new Deputy Governor, the psychologist, the

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[48] 'John C. Spencer, 1915-1978', *British Journal of Criminology*, Volume 19, Number 1, (1979), pp.1-2.
[49] Spencer, *Crime*, pp.v & ix.
[50] Ibid., p.1.
[51] Ibid., p.275.
[52] Ibid., p.276.
[53] Ibid., p.277.

psychiatrist, an official from the War Office or the Prison Commission and Spencer noted that 'the prisoner's story varied according to his opinion of my identity'.[54] He found that the role of chaplain at Dartmoor was particularly conducive to the sharing of 'personal information' by the prisoners and finally he settled on an identity associated with the prison chaplain. After discussion with the Governor at Dartmoor, Spencer decided that he would explain his arrival to the prisoners by saying that he 'had come to help the Chaplain in the running of his general-knowledge class'.[55] Once he had identified a role for himself, Spencer was able to undertake his research at Dartmoor in 1948.[56]

Spencer's research consisted of a study of one hundred ex-service convicts incarcerated in the prison and his information was obtained almost exclusively from two sources: prison records and 'individual interviews in the cells'.[57] Based on his analysis of the one hundred prison records, Spencer compiled various quantitative breakdowns of the prisoners in terms of their type of crime, their length of service and their levels of recidivism.[58] He then went on to 'interview' forty two of the one hundred prisoners whose records he had used. These 'interviews' were conducted under the pretext that Spencer was assisting the prison chaplain with a class for the inmates, not on the basis that he was a sociologist interested in the relationship between crime and service. The 'interviews' lasted between half an hour to an hour and a half, 'depending on the extent to which the man was prepared to talk freely about himself' and, in some cases, a second interview took place.[59] Given that Spencer had told the prisoners that his 'main work whilst staying at the prison was the organization of a General Knowledge Class', he had to be careful not to 'appear unduly anxious to question the men on matters of a fairly personal nature'. His technique for gaining access to the information he wanted was to discover a topic of conversation in which the prisoner being 'interviewed' was interested and then to 'put the problem of the effect of Service life in as detached and as general a way as possible'.[60]

Enid Mumford and Canteen Work at the Liverpool Docks

Mumford was born in Cheshire in 1924 and graduated from Liverpool University in 1946 with a degree in social psychology. Like King, Mumford also gained some practical

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[54] Spencer, *Crime*, p.275.
[55] Ibid., p.276.
[56] Ibid., p.101.
[57] Ibid., p.100.
[58] Ibid., pp.105-115.
[59] Ibid., p.100.
[60] Ibid., p.101.

experience in industry which informed her later academic research, working first as a personnel manager for an aeronautical engineering firm developing industrial relations strategy for a large female workforce and then as a production manager for a Liverpool clock and watch manufacturer. In 1948, Mumford returned to Liverpool University as a research associate in the social sciences department, working alongside some of the key figures in industrial sociology, such as Tom Lupton. Mumford was particularly interested in the effect of technical change in industry and her ideas were greatly influenced by the socio-technical approach of the Tavistock Institute, with whom King also worked.[61]

Mumford's experience of working in industry shaped her approach to academic research and she believed it was important to immerse yourself in the workplace being studied. It was whilst she was with Liverpool University that Mumford spent months in coal mines talking to miners and their supervisors about the effect of technology on their job, and went undercover as a catering assistant at the Liverpool docks. Subsequently, she took a lectureship at Liverpool University and completed her PhD at Manchester University. Mumford went on to have a long career at the Manchester Business School and also became a council member of the Tavistock Institute, continuing to research in a very practical way how technology could best be used to human advantage in industry, eventually becoming Emeritus Professor.[62]

Early on in her academic career, Mumford demonstrated a particular interest in the method of participant observation, and in 1951 at Liverpool University, she completed her thesis, 'An Evaluation of Participant Observation as a Research Method for the Study of Work Groups in Industry'.[63] Initially, Mumford had set out to study the structure and organisation of working groups using the method of covert participant observation, but her thesis evolved into a research project in which the primary interest was the research method itself.[64] Mumford recognised that although the term 'participant observation' was new in the 1950s, the technique implied by it, 'obtaining information by means of role play', had a history stretching back over many hundreds of years.[65] Mumford found examples of the use of role playing in mythology, folk tales, in the work of Shakespeare, in journalism, in novels and even in the world of espionage.[66] Her point was that the method

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[61] Frank Land, 'Mumford , Enid Mary (1924–2006)', *Oxford Dictionary of National Biography*, Oxford University Press, http://www.oxforddnb.com/view/article/97841, [accessed 1 July 2010].
[62] Land, 'Mumford'.
[63] Mumford, 'An Evaluation'.
[64] Ibid., p.2.
[65] Ibid., p.16.
[66] Ibid., pp.16-23.

of participant observation was 'not something that has been entirely discovered by the social scientist'.[67] Mumford's discussion of participant observation as a research method in the social sciences included a reference to Beatrice Webb's work in London sweatshops, discussed in chapter 1, as an historical example of the method in use.[68] Although Mumford suggested that there were many other examples of the use of participant observation in the modern sociological field, all three examples she chose to present were produced by academics based in America, not Britain.[69]

Whilst she was undertaking her research at Liverpool University, a team of researchers from the Department of Social Science were working on *The Dock Worker*, a collaborative exploration of Merseyside dock work. Under the auspices of this project, Mumford took the opportunity to become a participant observer, working in three dock canteens.[70] Mumford's covert work in the dock canteens was undertaken as the practical fieldwork associated with her thesis, which she completed in 1951, but it was not until 1959 that Mumford published an article about the norms of work group behaviour based on the data gathered from her covert research.[71] In her 1959 article in the *Sociological Review*, 'Social Behaviour in Small Work Groups', Mumford described her research aim as to 'observe, analyse and compare group norms of behaviour operating in a number of similar work situations' and, to this end, the method of 'concealed participant observation' was used to study three dock canteens in the port of Liverpool, as well as a factory in France.[72] Mumford provided a rationale for her use of this particular method: 'This method was selected as it was considered that its use would not significantly disturb group behaviour and attitudes, and data could be obtained through participation in the groups' day-to-day experiences'.[73]

Due to her departmental responsibilities at the University, Mumford spent no more than a month at any one canteen.[74] At each canteen, Mumford assumed a different role to disguise her true identity as an academic undertaking research on those around her. In the first canteen, Mumford posed as a trainee canteen supervisor – this gave her unlimited access to information about work life within the canteens but made it difficult to access

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[67] Ibid., p.18.
[68] See above, pp.36-38.
[69] Mumford, 'An Evaluation', p.35.
[70] University of Liverpool, Department of Social Science, *The Dock Worker: An Analysis of the Conditions of Employment in the Port of Manchester*, (Liverpool: University Press of Liverpool, 1954).
[71] Mumford, 'Social'.
[72] Mumford, 'Social', p.137. We will focus exclusively on Mumford's ethnographic material relating to the English ports as her fieldwork in France is beyond the remit of this thesis. Her covert work in France was undertaken before the opportunity arose to work covertly at the docks.
[73] Ibid., p.137.
[74] Mumford, 'An Evaluation', p.158.

more general information about problems at the docks. In the second canteen, Mumford pretended to be 'a writer'. This allowed her to extract lots of information about work life at the docks in general but this role emphasised the 'social distance' between Mumford and the other canteen workers and was therefore not conducive to good working relationships. In the third canteen, Mumford gave herself the vague identity of a student doing vacation work and allowed the work group to evolve a more specific role for her over time. She found this to be the most successful of her three assumed identities.[75] Only her role as a writer allowed Mumford to make notes openly. In her other roles, she made notes during the day, in the cloakroom, and wrote up the day's experiences in detail in the evening after work.[76] Mumford observed the canteen workers as three examples of industrial work groups and, based on her observation and participation in their work life, she suggested a series of norms which shaped the behaviour of small groups of industrial workers.

The Structure of Covert Academic Ethnography from 1946 to 1969

In Mumford and Spencer's texts, the ethnographic material was presented in a formal, circumscribed and non-narrative way associated with professional ethnographies of this era.[77] The bulk of Spencer's text on crime and service in the armed forces focused on his analysis of documentary sources. His ethnographic data, derived from the covert interviews with prisoners in Dartmoor, was relatively self-contained within his book. Isolated from his documentary analysis, Spencer presented his covertly gathered ethnographic material in the format of case histories presented in a non-narrative format. Within these case studies, there is very little sense of interaction between Spencer and his research subjects, and he has managed to completely divorce the data generated from his experience in the field.[78] The way in which Spencer chose to present his covert ethnographic findings would appear to have wider implications regarding the status of such covert research which will be explored in more detail below.

Like Spencer, Mumford's published ethnographic work was not narrative in structure and she digested and reorganised her narrative experience of working in the three dockyard canteens before presenting it to her academic readership. Her article, 'Social Behaviour in Small Work Groups', was tightly structured around the premise that the behaviour Mumford observed in the dock canteens suggested that group norms of

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[75] Ibid., p.155.
[76] Ibid., p.158.
[77] See above, pp.23-24; James Clifford, 'On Ethnographic Authority', *Representations*, No.2, (1983), p.120.
[78] Spencer, *Crime*, pp.121-135.

behaviour emerged to fulfil particular objectives of individuals within the work group or objectives of the group itself.[79] She then discussed the behaviour of the canteen workers as it related to five objectives she had identified under subheadings, comparing and contrasting her results with the findings of other academic studies of small group behaviour.[80]

In contrast to the work of Spencer and Mumford, King's article was dominated by a narrative account of her experience in the field. First, King explained why, and how, the project between the Tavistock Institute and the hosiery factory was established. She then detailed the research that was undertaken before she became involved in the project and why it had not been successful in deconstructing the skill of linking. It was at this point in her text that King presented her ethnographic data in a narrative structure, explaining her covert role and entry into the field and her experiences there. Finally, she related how her findings were applied to the training of linkers. King's own journey from ineptitude to skill in linking was central to the research project so it is not surprising that this narrative dominates the text. Perhaps also because King was part of a wider collaborative process, she had to signpost her role in the process quite clearly. Nonetheless, the clear narrative thread running through King's article and the transparency of her ethnographic journey from ignorance to understanding, associated with the tradition of social exploration, distinguishes her work from the majority of other British academic covert ethnographies published prior to 1980.

Researcher and the Researched in Post-War Covert Academic Ethnography

According to Carol Ann Parssinen, professional academic ethnographers of this era tended to suppress the fieldwork experience in their final written work.[81] In terms of structure, as discussed above, this suppression of the fieldwork experience could manifest itself in the use of a non-narrative format. In terms of writing style, the suppression of the fieldwork experience was maintained through use of formal and detached language, such as that used by Mumford in her ethnographic article on the behaviour of small work groups. For example, she referred to the sites of her research as simply Canteen A, Canteen B and Canteen C. Clearly, there may have been issues of anonymity which encouraged

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[79] Mumford, 'Social', p.137.
[80] Ibid., pp.137-151. Mumford also analysed the role of informal group leaders in enforcing norms of behaviour as well as the sanctions that were imposed on norm breakers: Ibid., pp.151-154.
[81] Carol Ann Parssinen, 'Social Explorers and Social Scientists: The Dark Continent of Victorian Ethnography', in Jay Ruby (ed.), *A Crack in the Mirror: Reflexive Perspectives in Anthropology*, (Philadelphia: University of Pennsylvania Press, 1982), p.206;

Mumford to disguise the real identity of the canteens observed but it is noticeable that she chose to identify them in such a neutral manner rather than, perhaps, inventing pseudonyms for each canteen.[82] No account of the fieldwork process was given in this article and, in fact, it was left to the reader to assume that Mumford undertook the covert ethnographic research herself, as this was not stated explicitly.[83]

It is only with reference to extra-textual materials that we can get a much more detailed and reflexive picture of Mumford's experience of covert ethnographic fieldwork. In her unpublished thesis, Mumford had written in a suitably academic tone, but she had made some important concessions to the norms of academic presentation, which she defended in her Preface. For the benefit of the examiners, she pointed out that it had been impossible to write parts of her account without using the first person singular and that she had 'deliberately introduced into the text many of the expressions and colloquialisms used in the canteens' as omission of these would result in the loss of 'local colour'.[84] This contrast would suggest that Mumford had been careful to remove such non-academic elements from her published work. Perhaps an example of this would be her frugal use of quotes in her published work. In her thesis, she had criticised another covert participant observer, Celia Fremlin, for presenting copious amounts of dialogue in her text which Mumford doubted was genuine:

> It is interesting to note that records of actual conversations are given in Mass Observation's report. In the present piece of research this was found impossible, as there was never sufficient privacy or spare time to make notes during working hours and any attempt to do so always aroused comment.[85]

In her own published ethnographic account, Mumford included dialogue from the field sparingly. She quoted the canteen supervisors as prone to saying '"We have to work from 7 in the morning to 6 at night, but you dock workers can 'welt' (i.e., work an hour and rest an hour) all day"'. She also wrote that 'If a man complained of dirty food he was told he was imagining it or it was his own fault. The girls said "what right have they to complain when most of them are used to eating like pigs at home"'.[86] It would appear that Mumford intended these quotes to represent typical examples of what the canteen workers would say in a specific situation given Mumford's experience of their behaviour.

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[82] Mumford, 'Social', p.138. Mumford further categorised the canteen workers into numbered sub-groups: Mumford, 'Social', p.138. In her thesis, Mumford referred to the canteens as North Hornby Canteen, Toxteth Canteen and Birkenhead Canteen. Mumford, 'An Evaluation', Table of Contents.
[83] Mumford, 'Social', pp.137-157.
[84] Mumford, 'An Evaluation', Preface.
[85] The Mass Observation report Mumford was referring to was *War Factory* and the covert participant observer was Celia Fremlin. Mumford, 'An Evaluation', p.33.
[86] Mumford, 'Social', p.143.

The extent to which Mumford, Spencer and King reflected on their role in the field varied between the three texts. Spencer offered very little in the way of reflection upon his covert experiences interviewing prisoners in Dartmoor. He showed no sign of emotional investment in either the research project or the research subjects as individuals. At the beginning of his text, he reflected in a very limited way on how his research strategy might be observed by his readers but, beyond this, Spencer wrote in an objective and passive manner.[87]

King, on the other hand, reflected in a circumscribed manner on how she coped in the field. For example, she suggested that her slightly elevated position as a trainee-instructor in the hosiery factory, not just a mere trainee, and the fact that she was the first in her group to master the skill of linking seamless fine hose meant the group looked to her for leadership, even in personal matters. King wrote that she found herself 'forced into a counsellor role' and found it difficult in these situations not to use her skills as a psychologist, which would compromise her cover story.[88] Unlike Spencer, she revealed her own emotional investment in the research field suggesting that during her training it was unavoidable that she would occasionally become as despondent as the rest of the trainees about her apparent lack of progress.[89] King managed to include a degree of subjectivity in her ethnographic account without compromising the formality of her work. In her role as a psychologist reporting back to the hosiery firm management, she felt there was a limit to what she could say about her experience in the training centre because of 'her feelings of loyalty to her fellow trainees and the risk of damaging her relationship to them'.[90] She refrained from referring to herself in the first person, preferring to use terms such as 'the writer' and 'the investigator' instead.[91]

Within her ethnographic article, Mumford referred to her presence in the field of research very rarely and, when she did, it was in a detached and passive way, the same as King.[92] Like Spencer, Mumford kept self-reflection to an absolute minimum in her ethnographic text and it is only with reference to Mumford's other work, her thesis and other published article, that we gain any insight into her subjective experience of the canteen work. In an article published shortly before her ethnographic text, Mumford

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[87] Spencer, *Crime*, p.277.
[88] King, 'Task...Part II', p.393. King also found that she could give advice on linking to fellow trainees but that she could not back such advice with her 'scientific data and prestige' which would have given her advice more force as this would compromise her role as a trainee-instructor. King, 'Task...Part II', p.409.
[89] Ibid., p.408.
[90] King, 'Task...Part II', p.409.
[91] Ibid., p.392. See, for example: Ibid., p.390.
[92] Mumford referred to herself on three occasions as 'the research worker'. Mumford, 'Social', pp.150 & 151.

evaluated the method of participant observation based on her own experience of the method and it was here that she discussed her subjectivity in terms of the psychological effects of participant observation on the researcher.[93] In two out of the three dock canteens, Mumford said she readily gained acceptance and experienced 'feelings of security and a sense of "belonging"…and there was reluctance to leave the situation.' In the canteen where she had posed as a writer and had not been accepted by the group as a fellow worker, she felt unhappy and insecure.

Although Mumford said her ethnographic experience allowed her to develop her social skills, her assumption of the role of a canteen worker caused her problems in her real life. She noted that, as a result of the emphasis on discipline in canteen life, 'a very real fear of supervision developed. On returning to academic life, this was carried over and some time elapsed before it was possible to reassume appropriate relationships with higher-ranking members of the faculty'.[94] Although Mumford recognised that merging her life with the lives of those under observation had the potential to become a serious personal problem for the participant observer, she did not see this subjectivity as detrimental to the research process.[95]

Whereas other methods such as surveys and experimentation facilitate the maintenance of objectivity on the part of the researcher, Mumford pointed out that the opposite was the case with participant observation.[96] She implied that a loss of objectivity was indeed a desirable and necessary step in being a participant observer explaining that

> the observer must immerse himself deeply in his role and assume the group's attitudes, values and behaviours, if he is to come to understand and interpret its characteristics. A participant observer who fails to identify with the group under investigation may misinterpret or ignore valuable data because he is not operating in terms of his own emotional experience.[97]

Experience was clearly an important element in Mumford's ethnographic fieldwork. It was her personal 'involvement' in the dock canteen research which 'enabled data to be obtained in terms of personal experience, for the participant observer was able to observe herself as a member of the group she was studying'. Although her engagement with the research subjects and environment resulted in the loss of what Mumford referred to as her '"absolute" objectivity', she retained her '"personal" objectivity' and, she argued, this improved the quality of the material obtained as 'it was possible to evaluate conclusions by

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[93] Mumford, 'An Evaluation', pp.159-160.
[94] Ibid., p.160.
[95] Ibid., p.157.
[96] Ibid., pp.157-158.
[97] Mumford, 'An Evaluation', p.158.

appealing to direct emotional experience'.[98] Her senior colleague at Liverpool University, Simey, agreed. Using methods such as attitude surveying and experimentation, Simey argued, it was relatively easy for the researcher to maintain some degree of objectivity and detachment. A participant observer, on the other hand, 'who succeeds in remaining objective and mentally detached from his group will be unlikely to succeed in his research as he will not himself be fully sharing the experiences of the group'. Simey noted that Mumford was able to share in every aspect of the group life of canteen workers meaning that she was able 'to obtain data in terms of her own experience for she was able to observe herself as a member of the group she was studying'.[99]

For King, the experience element of covert participant observation proved to be absolutely essential. As she made clear in her article, the observation of linking had not yielded any results and it was King's own experience of the learning process and articulation of it that was central to the success of the project and to the text. King did, however, check her experiences against those of the other trainee linkers. She wrote, for example, that her 'introspections' about three stages of stitch recognition were 'generally agreed by other trainees'.[100] Although observation of the experience of others was used to corroborate her own experience, it was secondary. For Spencer, however, the notion of experience was not important in the same way. In the preface to *Crime and the Services* Mannheim wrote that Spencer's blend of scholarly knowledge and practical experience was an essential prerequisite to the production of such a study.[101] Spencer, as well as having been a Probation Officer and London Juvenile Court Magistrate, had served in the British Army during the Second World War.[102] Spencer had prior experience of dealing with criminals and serving in the armed forces, yet he did not have direct experience of the phenomenon under observation, criminal behaviour whilst in service.

Methods and the Anthropological Paradigm

After the war, Marshall indicated that sociology had to make methodological changes. As well as warning against remaining too philosophical and theoretical, Marshall also warned against the temptation to waste energy collecting a multitude of facts without

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[98] Ibid., p.158. Simey also thought that the quality of Mumford's data 'was improved because she was able to evaluate her conclusions in the light of her own experience.' Simey, 'The Analysis', p.83.
[99] Simey, 'The Analysis', p.83.
[100] King, 'Task...Part II', p.389.
[101] Mannheim, 'Foreword', p.v.
[102] Ibid., pp.v-vi.

an adequate sense of purpose.[103] He linked this tendency with 'the ambition to bring the social sciences more into line with the natural sciences by making greater use of quantitative methods'.[104] Just because something could be measured, Marshall argued, this did not mean it was worth measuring, and he chided those sociologists who insisted on using 'figures and diagrams' even when these were not the best ways of expressing their findings.[105] Marshall called for the use of more 'qualitative study' but he did not offer much more detail on the type of qualitative methods sociologists should use.[106]

Less than a decade later, Simey seemed to advocate a similar move away from quantitative methods associated with the natural sciences towards more qualitative approaches. He wrote that social research could hitherto be divided into two classes: 'those in which the researcher endeavours to refrain from compromising his objectivity in any way' and 'those in which he deliberately surrenders it in exchange for a deeper insight into the human problems involved in the situation he studies'.[107] Strictly objective methods could not, said Simey, answer the questions 'how' and 'why' of social situations which the social scientist needed to know. As a result, he suggested that many social scientists, including those at Liverpool, had 'turned their attention to intensive methods of enquiry which…[had] brought them into closer relations with the subjects of their researches'.[108] Thus, although neither Marshall nor Simey offered very specific guidance on the type of qualitative methods sociologists should use, the shift away from quantitative methods and a desire to study research subjects intensively should, if anything, have promoted the use of covert participant observation amongst the social research community. However, Michael Banton offers an explanation for why it may have taken a while for sociologists to begin using qualitative methods on a larger scale despite the suggestion that this was the direction in which sociology should be headed.

Banton described a paradigm as a shared idea about what constitutes an appropriate topic of research and the method to use to study that topic.[109] He suggested that, at any one point in time, sociologists are affected by a 'paradigm of what sociology should look like' and that such paradigms can exert a hold upon the sociologist's views.[110] Writing in 1964, Banton suggested that since the war, sociology had been affected by intolerance and

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[103] Marshall, *Sociology*, p.15.
[104] Ibid.
[105] Ibid., pp.15 & 19.
[106] Ibid., p.19.
[107] Simey, 'The Analysis', p.71.
[108] Ibid., p.72.
[109] Michael Banton, 'Anthropological Perspectives in Sociology', *British Journal of Sociology*, Volume 15, Number 2, (1964), p.97.
[110] Banton, 'Anthropological', pp.97 & 98.

dogmatism which came from the dominance of particular paradigms which 'as if for their adherents they were the only true sociology'.[111] He explained that

> [m]uch of the strength of academic scholarship derives from the utilization of paradigms. A community of scholars trained in a particular mode of analysis can exploit its possibilities and develop a powerful intellectual tradition, but they will be to some extent the prisoners of their own paradigm, unable to appreciate the virtues of other approaches. Reform from within is difficult and it is often the amateur…or the stranger from a sister discipline…who opens up new lines of work.[112]

We can see clearly that eminent sociologists such as Marshall and Simey were advocating a post-war shift from one sociological paradigm to another, although they were not particularly precise about what they wanted to move away from and what they wanted to move towards. According to Banton, it was the paradigm of the social survey that had had a strong hold upon 'the public mind' since before the Second World War in Britain and the success of this paradigm delayed the launching of new lines of enquiry, presenting an obstacle to the establishment of an alternative paradigm.[113] Even after the war, sociology floundered because there was no intermediate between sociological theorising and the social survey. In her thesis completed in 1951, Mumford noted that in terms of sociological research methods, the attitude survey had 'come into vogue in the last thirty years' in the U.S.A. and Britain, indicating that the social survey paradigm was still dominant.[114] In post-war Britain 'the conception of the survey was distinct and widely understood' and the assumption amongst the British public was for a long time that 'any social research worker is engaged in a 'survey' and that this is properly the collection of information about a social problem in logical categories comprehended by laymen and intended to support proposals for reform'. In the middle of the twentieth century, the social survey paradigm began to lose its commanding place because, as Marshall and Simey had suggested, it was an 'unsuitable…mode of enquiry for the new problems that were attracting attention' in the post-war years.[115] Yet, it proved difficult to establish an alternative sociological paradigm.

We have already learned that sociologists such as Marshall were pointing out the subjects they thought deserved empirical sociological attention yet Banton suggests that in the 1940s, 'research problems were ill-defined and all else suffered' as a result. This was a turbulent time for would-be sociologists and many struggled to use the existing social survey paradigm to accommodate the sociological issues they were interested in. In the 1930s and 40s, in the wake of successful studies in America, there was much talk and

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[111] Ibid., p.98.
[112] Ibid., p.101.
[113] Ibid., p.102.
[114] Mumford, 'An Evaluation', p.5
[115] Banton, 'Anthropological', p.103.

speculation about the application of anthropological methods to sociological problems in Britain. It was not until the later 1940s, however, that Banton suggests a new tradition of British sociological research began to emerge. The methods of social anthropologists helped to formulate a new sociological paradigm in Britain which was more applicable to the problem-centred, intensive style of research that Marshall had predicted would be so important. Banton singled out the sociologist Edward Shils as being particularly active and effective in challenging scholarly conventions at this time, convincing students that sociology was a subject still under construction and still capable of being shaped.[116]

The import of the anthropological paradigm allowed sociologists to find an alternative to the social survey paradigm but it was not a case of simply shifting unproblematically from one to the other.[117] As Banton suggested, it proved difficult for sociologists to release themselves completely from the grip of the survey paradigm even as they were actively trying to embrace the anthropological paradigm. In Kenneth Little's 1940s study of race relations, *Negroes in Britain*, for example, Banton found

> compromises between the social survey approach and the direct study of a sociological…problem. Re-reading the book and bearing in mind the literature of its day one can almost see the author struggling to break with an unduly restricted conception of the social scientists' task.[118]

Nevertheless, the anthropological paradigm had a marked effect on post-war sociology in Britain, most particularly with respect to community studies in the 1950s and 1960s, such as Norman Dennis, Fernando Henriques and Clifford Slaughter's *Coal is our Life*.[119] In practical methodological terms, the application of an anthropological paradigm meant using some form of participant observation and by the 1960s it was being used confidently in an overt context by social researchers in Britain. Isabel Emmet, for example, married a resident of the fictitiously named Welsh village Llan and lived there for a number of years before publishing *A North Wales Village: A Social Anthropological Study* based on overt participant observation.[120] Emmet clearly embraced the anthropological paradigm, aligning her work with that of classic social anthropologists who had worked outside their home countries such as Evans-Pritchard on the Azande and Richards in Northern Rhodesia.[121] Her work was also notable for the absence of questionnaires, surveys, charts and figures as tools of research. In contrast, she strove to understand behaviour within one particular

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[116] Ibid., pp.103-104.
[117] Ibid., p.103.
[118] Ibid.
[119] Banton, 'Anthropological', p.105. Norman Dennis, Fernando Henriques and Clifford Slaughter, *Coal is our Life: An Analysis of a Yorkshire Mining Community*, (London: Eyre & Spottiswoode, 1956).
[120] Emmet, *A North*, pp.ix & xiii.
[121] Ibid., p.xii.

human group by studying it intensively.[122] Thus, Emmet was rejecting the social survey paradigm in favour of a more intense and qualitative anthropological approach.

In his account of the anthropological influence on the practice of post-war empirical sociology, Banton did not refer to any studies which made use of covert participant observation after 1945. His description of sociologists working within the anthropological paradigm, however, would certainly have encompassed them as he described them bringing 'back reports to the educated public as from another country, quoting the words and describing the actions of the strange inhabitants. Above all, they let their subjects speak for themselves instead of pressing their stories into the mould of middle-class judgement'.[123] Writing in the mid 1960s, Banton suggested that many social scientists had already learned of the 'very great limitations' of applying the anthropological model of participant observation to industrial society. The results were disappointing, according to Banton, when compared with earlier ethnographic projects in industrial societies, such as Webb's work on East End tailoring, which we discussed in the first chapter.

We can see traces of the influence of the anthropological paradigm in Spencer's *Crime and the Services*, one of the few academic covert ethnographies published between 1946 and 1969. He indicated in his text that he was keen to make use of the anthropological paradigm which Banton said was beginning to make an impression on British empirical sociology at this time. Spencer used the term 'field-work' to describe his activities at the three institutions and he noted that he had borrowed this word from social anthropology, emphasizing the importance of work carried out in the field on a day-to-day basis rather than 'in libraries and archives'.[124] Spencer stressed that the 'most interesting evidence' was found in the field, not in the records, reflecting Marshall's call for post-war sociologists to focus on empirical findings rather than theory and abstraction.[125] On more than one occasion, Spencer compared the methodological difficulties he faced with those experienced by social anthropologists working in industrial societies, such as Kenneth Little in Britain.[126]

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[122] Ibid., p.ix.
[123] Banton, 'Anthropological', p.103.
[124] Spencer, *Crime*, p.2. Spencer said that he was not suggesting that such records were unimportant but that his 'primary emphasis' was 'on the behaviour of people in social situations.'
[125] Ibid., p.8.
[126] Ibid., pp.274 & 275. Kenneth Little, *Negroes in Britain: A Study of Racial Relations in English Society*, (London: Kegan Paul, 1948).

Just as Banton felt Little's struggle to break free from the survey paradigm was evident on the pages of his book *Negroes in Britain*, Spencer seemed almost uncomfortable with the qualitative and personal nature of his 'interview' material. It would appear that Spencer primarily used his observational data to generate case studies of the prisoners interviewed. He completed forty two interviews and, in his thesis, case studies of all forty two prisoners were included.[127] In *Crime and the Services*, Spencer could only accommodate seven of these case studies. In each of these seven case studies, Spencer, using pseudonyms for all of the prisoners, described the upbringing, the Service history, the criminal history, his description of their personality and his socio-criminal prognosis for each.[128] The case studies provided illuminating glimpses into the lives of some ex-Service men in Dartmoor and, when *Crime and the Services* was reviewed in 1955, the reviewer was particularly taken by Spencer's brilliant 'pen-sketches'.[129]

However, Spencer's decision to use a brief case study format may suggest he was unsure how to make use of his covertly gathered qualitative data. Whereas he was able to confidently analyse and synthesise the written records of one hundred prisoners, once he had generated the interview data he stopped short of synthesising it into a coherent account of what it demonstrated about the relationship between crime and service in the Armed Forces. In fact, Spencer felt compelled to adopt a classification system to provide him with a framework for arranging the case-histories in a specific order.[130] This suggests that Spencer was uncomfortable with his qualitative and relatively unstructured data and that classification somehow added to the authoritativeness of the material.

It would seem that methodological developments within the discipline of sociology after the Second World War and through the 1960s should have encouraged rather than have hindered the use of covert participant observation. Although Marshall's lecture in 1946 suggested a sense of unease and a lack of methodological confidence amongst sociologists, by the 1950s the anthropological paradigm had become quite firmly established, opening up the method of overt participant observation to legitimised sociological use. Going covert would only have represented a small modification of the overt method so it is striking that so few social scientists chose to do so prior to 1969. We

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[127] Spencer, *Crime*, p.121.
[128] Ibid., pp.124-135.
[129] Hugh Klare, 'Review of *Crime and the Services* by John Spencer', *Sociological Review,* Volume 3, Issue 1, (1955), p.144.
[130] Spencer, *Crime*, p.121.

can find some explanation for this dry spell if we take a closer look at the issues of research ethics and social sanctions on research in the post-war period.

Research Ethics, Social Sanctions and Covertness

There is considerable evidence to suggest that in the 1950s and 60s there was growing concern about the ethics of social scientific research. According to one social scientist writing in the 1950s, the notion of social sanctions on social scientific research was frequently raised in discussion between social scientists at all stages in the research process but that 'public information' on the topic was difficult to find.[131] John Barnes has suggested that professional social scientists in Britain were much slower than their American counterparts to respond to the ethical issues surrounding social research and that their responses were much more equivocal.[132] Nonetheless, some British social scientists did discuss their growing concern over ethics in print.[133] In the mid 1950s, Simey linked the need for ethically sound research to the problem-centred nature of much post-war sociological research. If their research was to benefit the research subjects at all, then the sociologist must preserve their integrity by engaging with the research subjects in an attempt to empower the community under study to overcome social problems.[134] He suggested that it was the chief merit of '"problem-centred" research' that it 'reintroduces into the scope of scientific and administrative work the human motivations and values which are the significant factors in all kinds of social and political behaviour.'[135]

Writing later in the same decade, Alexander Wilson pointed out that 'the research worker in any branch of scientific work cannot avoid some degree of social responsibility for his activities and for their social consequences'.[136] He expressed how important it was for any research project to be socially sanctioned by the community being studied. Wilson maintained that 'social research needs public co-operation' and that the public had a minimal tolerance for research without adequate sanction.[137] He pointed out that a failure to achieve the support of the researched could result in the withdrawal of co-operation in future research and he emphasised the negative effect such a breakdown in

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[131] Alexander Wilson, 'A Note on the Social Sanctions of Social Research', *Sociological Review*, Volume 3, Issue 1, (1955), p.109.
[132] John Barnes, *Who Should Know What? Social Science, Privacy and Ethics*, (Cambridge: Cambridge University Press, 1979), p.161.
[133] See, for example: Wilson, 'A Note'; Simey, 'The Analysis'; Joseph Henry Fichter and W.L. Kolb, 'Ethical Limitation on Sociological Reporting', in J.H. Fichter, *Social Relations in the Urban Parish*, (Chicago: Chicago University Press, 1954), pp.218-232; Margaret Stacey, Correspondence on 'Professional Ethics', *Sociology*, Volume 2, (1968), p.353.
[134] Simey, 'The Analysis', p.74.
[135] Ibid., p.85.
[136] Wilson, 'A Note', p.110.
[137] Ibid., p.115.

communications would have on the public opinion of social research in general.[138] Furthermore, he suggested that 'although the matter is seldom discussed from this point of view, the law actually provides sanctions *against* observation of behaviour, and *against* communication of such observation in certain circumstances' and that the relevance of such laws to social research was not always realised.[139]

As Barnes suggested, throughout the 1950s and especially in the 1960s, there was much more discussion about the ethics of covert research amongst American professional social scientists. In 1953, Joseph Fichter and William Kolb wrote about the 'Ethical Limitations on Sociological Reporting' for the *American Sociological Review* and they highlighted the absence of a systematic ethical framework for sociologists. If the American sociologist was to become more scientific, according to Fichter and Kolb, 'he must …become increasingly sensitized to the rights, feelings, and needs of the people he studies'.[140] The use of covert participant observation to explore the training programme undertaken by recruits to the American Air Force provoked some comment.[141] Lewis Coser wrote to the *American Sociological Review* suggesting that the researchers involved had shown a complete 'disregard for professional ethics'.[142] Another correspondent in the same edition of the *American Sociological Review*, Julius Roth, was more concerned that the researchers had implied that the use of undercover participant observation was both rare and difficult. On the contrary, Roth suggested, using this method was relatively common and easy, and potential users of the method should not be put off.[143]

In 1959, the respected American sociologist Edward Shils made his unequivocal opinion regarding the ethical status of incognito social research clear. According to Shils, this method, 'which was once highly esteemed' was

> susceptible to considerable ethical abuse. It is wrong for an inquirer ostensibly to take up membership in a community with the intention of conducting a sociological inquiry there without making it plain that that is what he is doing. His self-disclosure might occasionally hamper research he is conducting, but the degree of injury suffered does not justify the deviation from straightforwardness implied by withholding his true intentions.[144]

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[138] Ibid., pp.110-111.
[139] Ibid., p.112. Emphasis in original.
[140] Fichter and Kolb, 'Ethical', pp.544-550.
[141] Mortimer Sullivan, Jr., Stuart Queen and Ralph Patrick, Jr., 'Participant Observation as Employed in the Study of a Military Training Program', *American Sociological Review*, (1958), Volume 23, Number 6, pp.660-667.
[142] Lewis Coser, 'A Question of Professional Ethics?', *American Sociological Review*, (1959), Volume 24, Issue 3, pp.397-400.
[143] Julius Roth, 'Dangerous and Difficult Enterprise?', *American Sociological Review*, (1959), Volume 24, Issue 3, p.398.
[144] Edward Shils, 'Social Inquiry and the Autonomy of the Individual', in Daniel Lerner (ed.), *The Human Meaning of Social Science*, (New York: Meridian Books, 1959), p.128.

In 1961, the anthropologist and social commentator Margaret Mead stated her equally unambiguous position on the ethical status of covert research in the American magazine, *Science*. She suggested that such covert participant observation constituted a 'deception' which violated conventions of privacy and dignity as well as casting social scientists 'in the role of spies, intelligence agents, Peeping Toms, and versions of Big Brother'. Overall, such a method damaged social scientific endeavour by destroying trust between researchers and the researched.[145] It was also around this time that another example of American covert ethnography provoked controversy. Six male sociology graduate students posed as alcoholics at meetings of the Alcoholics Anonymous (A.A.) support group around New York in order to observe the behaviour of other alcoholics, without their knowledge.[146] In a letter to the journal, *Social Problems*, Fred Davis expressed similar distaste for such methods as Shils and Mead had done, describing the infiltration of the A.A. by social scientists as 'repugnant'.[147]

In 1962, Roth once again came to the defence of covert ethnographers suggesting that there was no simplistic dichotomy between 'secret' and 'non-secret' sociological research. Rather, he suggested there was a continuum between the poles of secret and non-secret research which could not be untangled from each other, thus it was impossible to sanction against any particular form of research.[148] Five years later, whilst agreeing with Roth's notion of a continuum between secret and non-secret research, Kai Erikson suggested that certain fundamental rules should be adhered to even when working in such a 'hazy territory'. He suggested the following 'negative sanctions': 'that it is unethical for a sociologist to *deliberately misrepresent* his identity for the purpose of entering a private domain *to which he is not otherwise eligible*' and 'that it is unethical for a sociologist to *deliberately misrepresent* the character of the research in which he is engaged'.[149]

In 1968, Norman Denzin recognised that most of the comments made regarding the ethics of disguised observation had followed the path of condemnation which Shils had laid out in 1959. Acknowledging the fact that his views might be representative of the minority of professional sociologists at the time, Denzin nonetheless wished to argue that the use of disguise by sociologists was not unethical. He argued that sociologists had the

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[145] Margaret Mead, 'The Human Study of Human Beings', *Science*, 20th January 1961, Volume 133, Number 3447.
[146] John Lofland and Robert Lejeune, 'Initial Interaction of Newcomers in Alcoholics Anonymous: A Field Experiment in Class Symbols and Socialization', *Social Problems*, (1960), Volume 8, Number 2, pp.102-111.
[147] Fred Davis, 'Comment on "Initial Interaction of Newcomers in Alcoholics Anonymous"', *Social Problems*, (1961), Volume 8, Number 4, pp.364-365.
[148] Julius Roth, 'Comments on "Secret Observation"', *Social Problems*, (1962), Volume 9, Number 3, pp.283-284.
[149] Kai Erikson, 'A Comment on Disguised Observation in Sociology', *Social Problems*, (1967), Volume 14, Number 4, pp.366-373. Emphasis in original.

right to make observations of any setting 'to the extent that he does so with scientific intents and purposes'. The goal of sociology, in common with other sciences, was 'the advancement of knowledge and explanation' and any method that facilitated achieving this goal, without wilfully harming the research subjects or the sociology profession, was justifiable. He was adamant that 'no areas of observation are in an a priori fashion closed to the sociologist, nor are any research methods in an a priori fashion defined as unethical'.[150]

The preceding few paragraphs have demonstrated the extent to which the ethical status of covert research was being questioned in America in the 1950s and 1960s. Perhaps there was such forthright discussion of the ethics of covertness in America at this time because there were several controversial covert ethnographies undertaken around this time for American professional sociologists to react against. In addition to the covert studies of the American Air Force and Alcoholics Anonymous mentioned above, disguised observation had also been used to investigate a psychiatric hospital from the perspective of a mental patient and a religious cult in 1950s America.[151] Furthermore, ethical controversy followed in the wake of the abandonment of 'Project Camelot' in the 1960s.[152] Presumably, professional sociologists in Britain would have been aware of the growing concern surrounding research practices across the Atlantic. However, British debate surrounding research ethics appears to have been somewhat less widespread and forthright than the American example.

As suggested above, the post-war institutionalisation and calls for professionalisation of sociology in Britain provided an important context for the growing concern regarding the ethics of sociological research. As the historian Kent has suggested, institutionalisation in the form of journals and learned societies can be associated with the development of intellectual continuity.[153] In 1953 and 1955 respectively, noted sociologists Simey and Wilson used the forum of the *Sociological Review* to highlight the particularly acute ethical dilemma surrounding the use of covert research methods. For Wilson, the greatest difficulties arose over social sanction when the subject of observation did not

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[150] Norman Denzin, 'On the Ethics of Disguised Observation', *Social Problems*, (1968), Volume 15, Number 4, pp.502-504. Kai replied to Denzin's comments in the same edition of this journal: Kai Erikson, 'A Reply to Denzin', *Social Problems*, (1968), Volume 15, Number 4, pp.505-506.

[151] William Caudill et al, 'Social Structure and Interaction Processes on a Psychiatric Ward', *American Journal of Orthopsychiatry*, (1952), Volume 22, pp.314-334; Leon Festinger, Henry Riecken and Stanley Schacter, *When Prophecy Fails*, (Minneapolis: University of Minneapolis Press, 1956).

[152] This was a project which many feared represented the use of social scientists as state spies, working to strengthen the position of the American government. See: Irving Horowitz, *The Rise and Fall of Project Camelot: Studies in the Relationship between Social Science and Practical Politics*, (Cambridge, Massachusetts: Massachusetts Institute of Technology, 1974).

[153] Kent, *A History*, pp.189-190.

know they were being studied and he suggested that examples of 'unsanctioned observation and communication have certainly proved costly to research in general', although he did not give any specific examples. [154] Simey was more concerned with the effect that covertness would have on the utility of research carried out in this way and he explicitly contrasted the therapeutic aims of problem-centred research with the nature of covert participant observation. For Simey, the 'essence of participant observation as a research technique is that it involves a concealment of the role of the observer as a researcher, and possibly even misrepresentations about it'.[155] Such a state of affairs could prove disastrous if, for instance, the researcher's true identity was discovered, research would break down and co-operation for further research was unlikely to be obtained. Even more importantly, however, Simey claimed that it was very difficult to publish and therefore make practical use of data gathered in this way.[156]

In 1963 in the *British Journal of Sociology*, John Barnes, who is now Emeritus professor of sociology at Cambridge, wrote about ethical issues in fieldwork and, although he focused on anthropological fieldwork overseas, he did touch upon the issue of covert research in Western societies. Significantly, Barnes' clear condemnation of undercover research was associated with the professional status of social researchers. Such a method was 'simply dishonest' in Barnes' view and, as a profession, British social scientists

> should say that we will not undertake covert research. If we wish to enjoy public support as a responsible profession we must not only avoid acting as spies even in the best causes; we must make it clear in advance that we will not act in this way.[157]

As professional sociologists and anthropologists, Barnes suggested that 'we have an abiding interest in seeing that we are regarded as responsible professionals by all those we work with' and that the 'interests of the profession outlast' those of any specific investigator or research project. He described the professional ethics of the discipline as being 'ill-defined' and, whilst he accepted that a professional code of ethics would not make difficult ethical decisions any easier, 'it might at least remind ethnographers that these problems do have to be solved and cannot be ignored'.[158]

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[154] Wilson, 'A Note', pp.114-115.
[155] Simey, 'The Analysis', p.81.
[156] Ibid., p.81. Simey was right to point out that problems would arise when it came to publishing covertly gathered material. In her thesis, which Mumford had conducted the covert research in the canteens for, she wrote: 'In the dock canteens the girls became my friends and, in consequence, I would be reluctant to publish the material contained in this thesis in case it ever came to their knowledge.' Mumford, 'An Evaluation', p.246.
[157] John Barnes, 'Some Ethical Problems in Modern Fieldwork', *British Journal of Sociology*, (1963), Volume 14, Number 2, pp.118-134.
[158] Barnes, 'Some Ethical Problems', pp.123 & 133.

It was not, however, until 1970 that the BSA published a 'Statement of Ethical Principles and their Application to Sociological Practice' in their journal, *Sociology*.[159] When the BSA was founded in 1951, it was recognised that, '"professional" sociology was to all intents and purposes non-existent in Britain'.[160] Although the BSA was originally conceived as a meeting point for all of the social sciences and the terms sociology and sociologist were defined very broadly, there was a minority, even at the inaugural meeting, who wished to use the BSA as a vehicle for promoting sociology as a specialism and a profession.[161] Within the BSA, the Teachers' section has been recognised as 'the most effective pressure group...for the professional point of view'.[162] Set up following the BSA AGM in 1963, the Teachers' section was exclusively for professional sociologists to 'sustain a conception of sociology as a serious discipline for which training was required'.[163] Furthermore, in 1965, the Teachers' section developed a *Register of Professional Sociologists* which was inspired by the view that 'almost anyone...could pose as a sociologist because the BSA was not generally regarded as an organisation which only the properly qualified could join...'.[164] According to Platt, it was members of the Teachers' section that were the 'prime movers' in the establishment of *Sociology*, the BSA's journal, in 1967.[165] A journal was thought to be desirable following conversations with American sociologists who 'stressed the extent to which a journal responsible to the profession could be an instrument for the propagation of satisfactory scholarly standards in the discipline.'[166]

This drive for professionalisation within the BSA in the 1960s is significant because whenever ethical standards were discussed in the pages of *Sociology*, or in other BSA publications, they were frequently associated with the professional status of the discipline. In 1968 in the Correspondence pages of *Sociology*, the Institute of Community Studies reproduced the guidelines used for survey interviewers and their obligations to informants in the hope that they would 'stimulate discussion about the ethical aspects of survey research'.[167] The following year, Margaret Stacey, the general secretary of the BSA, again used the Correspondence pages of *Sociology* to welcome the contribution made by the Institute of Community Studies regarding ethics. She noted that in 1967 a BSA sub-

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[159] V.L. Allen, Correspondence on 'Statement of Ethical Principles and their Application to Sociological Practice', *Sociology*, (1970), Volume 4, pp.114-117.
[160] J.A. Banks, 'The British Sociological Association – The First Fifteen Years', *Sociology*, Volume 1, Number 1, (1967), p.1.
[161] Banks, 'The British Sociological Association', p.1; Platt, *The British Sociological Association*, pp.19, 21 & 23.
[162] Banks, 'The British Sociological Association', p.1.
[163] Platt, *The British Sociological Association*, pp.34-35.
[164] Ibid., p.35.
[165] Ibid., p.36.
[166] Banks, 'The British Sociological Association', p.6.
[167] Ann Cartwright and Peter Willmott, Correspondence on 'Research Interviewing and Ethics', *Sociology*, Volume 2, Number 1, (1968), pp.91-93.

committee on professional ethics had been established as there were many areas of professional practice in which sociologists had ethical concerns. The sub-committee had generated a report on research ethics which was being circulated amongst members. Furthermore, Stacey reported that the BSA had established a Standing Committee on Professional Ethics to consider reactions to the report and to consider introducing a 'code of ethics to which sociologists should be expected to adhere'.[168]

As the 'Statement of Ethical Principles and their Application to Sociological Practice' was not published until 1970, a full discussion of its contents in relation to covert research will be confined to the next chapter covering the period between 1970 and 1980. However, it is significant to note here that the 'Statement' covered a range of ethical issues, of which covert research was just one. It is interesting to note that despite invitations to discuss professional ethics by the Institute of Community Studies and Stacey, there appeared to be no further discussion of the prospect of ethical guidelines in the pages of *Sociology*. Perhaps this is indicative of a lack of interest in ethics on the part of most sociologists, besides those passionate about pushing for the professionalisation of the discipline. Of course, it could be that debate was generated on the subject but that it was not published.[169] The point to note is that there was growing concern regarding research ethics and this was associated with the increasing professionalisation of sociology.

Ethical dilemmas and issues over social sanctioning were important considerations for social researchers, even if they had been open about their research and their intention to publish work based on that research. In the mid-nineteen-fifties, Norman Dennis conducted what he described as 'an interview and participant-observation study' of an English city and wrote an academic article based on this research about the community association movement in the urban fringes.[170] He lived with his wife on two of the Bristol estates whilst conducting his research. He participated fully in the community life as a researcher and as a resident. Dennis' work highlights the grey area between overt and covert participant observation, echoing Roth's idea of a continuum between secret and non-secret or covert and overt research. Dennis made no consistent or deliberate attempt to conceal his research status. Rather, he 'made it clear who I was whenever ordinary courtesy would indicate that [it] would be a violation of privacy not to have done so – that people would

[168] Margaret Stacey, Correspondence on 'Professional Ethics', *Sociology*, Volume 2, (1968), p.353.
[169] The BSA's newsletter for members, *Network*, described by Platt as a forum for the discussion of controversial issues, BSA policy and professional gossip, was not established until 1975. Platt, *The British Sociological Association*, p.40.
[170] Norman Dennis, 'Changes in Function and Leadership Renewal: A Study of the Community Association Movement and Problems of Voluntary Small Groups in the Urban Locality', *Sociological Review*, Volume 9, Issue 1, (1961), pp.55-84.

feel deceived if I had said nothing and they found out later'.[171] When the events he was participating in were fully public, Dennis felt under no obligation to publicise his research status. On the other hand, he was not particularly concerned if those under observation knew he was a researcher.[172] Thus, his decision to be at least a partially overt participant observer was a matter of courtesy and protecting the privacy of the research subjects. However, even with the best of intentions, overt ethnographers have become embroiled in scandals over the miscommunication of their observations to research subjects.[173]

It would appear that some British social researchers were so concerned about the ethical debate surrounding covert research that they were careful to stress the overt nature of their research. In the 1960s for example, under the auspices of Manchester University, Sheila Cunnison used the method of overt participant observation to research how social factors influenced the output and pay of workers in a garment factory.[174] Cunnison spent six months working on the shop floor of a garment factory but she was keen to stress the overt status of her research and her willingness to share her research agenda with the subjects of her research. She wrote 'I made no secret of my research interests: before I started work I met managers in the industry, the secretary of the waterproof union, and a few workers from… [the factory floor]. I tried to explain what my purposes in taking a job at… [the factory] were; and, when I actually began work, I again did my best to explain my presence to the people I worked near and to anyone else who was interested'. In a footnote, she again stressed that the research technique she used was referred to as '"open" participant observation to distinguish it from cases where research interests are not disclosed'.[175]

There is certainly evidence to suggest that ethical considerations did affect the way in which the method of covert participant observation was used by British academics the 1950s and 60s. Spencer, writing in the mid-1950s, appeared reluctant to dwell on the covert nature of his fieldwork in his text. There are a few clues which can be found in Spencer's text which suggest he was uncomfortable with the notion of covertness, even though he had explained why he felt it necessary to take such an approach to his research subjects. Spencer did not once use the term 'covert' to describe his research method. The

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[171] Personal correspondence.
[172] Personal correspondence.
[173] See, for example D.H.J. Morgan, 'The British Association Scandal: The Effect of Publicity on a Sociological Investigation', *Sociological Review*, Volume 20, Issue 2, (1972), pp.185-206.
[174] Sheila Cunnison, *Wages and Work Allocation: A Study of Social Relations in a Garment Workshop*, (London: Tavistock, 1966).
[175] Cunnison, *Wages*, p.xix.

closest he came to acknowledging the deceitfulness of his methodology was when he used the terms 'disguise' and 'pretext' and these somehow do not capture the full extent of the façade Spencer employed whilst in Dartmoor.[176] Furthermore, Spencer used the term 'interviews' to refer to the discussions he had with the prisoners whilst they were under the impression that he was an assistant to the chaplain, not a social scientist.[177] The term 'interview' implies a degree of explicit consent on the part of the interviewee to discuss a specific topic. The prisoners were not really being interviewed in the conventional sense of the word: they did not know Spencer's real identity; they did not know they were being interviewed, and they were certainly not aware of the interviewer's agenda.

Finally, Spencer focused on the functionality of his research role rather than its covertness. He described his method as 'the functional approach'.[178] He suggested that without taking on 'a clearly defined role, rumour would spread quickly around the institution resulting in a fictitious and perhaps damaging role being attributed to the researcher, especially being associated with the disciplinary staff.[179] Spencer found support for such a functional approach in the work of social scientists based in America, including Norman Polansky.[180] It is, however, possible to adopt a functional approach without being covert about your research. Tom Lupton, for example, used an overt functional approach to study industrial relations. In *On the Shop Floor*, Lupton explained that he participated fully in the work on the shop floor but he informed his fellow workers about his academic status and his research agenda.[181] Spencer's 'functional approach' is reminiscent of the method of 'functional penetration' used by social psychologists Oeser and Jahoda in the interwar period.[182] Whilst there is, of course, nothing wrong with Spencer emphasising the functional aspect of his role in the field, he seemed to do so at the expense of dwelling on the covertness of his role. Spencer's apparent discomfort with the covert and qualitative nature of his 'interview' material suggests that some social scientists may have been struggling to work outside the survey paradigm and that, at least behind the scenes, ethical questions were being raised about the use of covert research methods.

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[176] Spencer, *Crime*, pp.276 & 277.
[177] Ibid., p.100.
[178] Ibid., p.277.
[179] Spencer, *Crime*, pp.277-278.
[180] Ibid., p.277. Norman Polansky et al, 'Problems of Interpersonal Relations in Research on Groups', *Human Relations*, Volume 2, (1949), pp.281-291.
[181] A.J.M. Sykes, 'Review of *On the Shop Floor* by Tom Lupton and *Coal and Conflict* by W.H. Scott et al', *Sociological Review*, Volume 11, Issue 3, (1963), p.371. Tom Lupton, *On the Shop Floor: Two Studies of Workshop Organization and Output*, (Pergamon, 1963).
[182] See above, pp.97-99.

As with Spencer's work, there is evidence to suggest that the covert nature of Mumford's research proved to be problematic. It would appear that Mumford was, to some extent, aware of the difficulties using covert methodology could bring about for academic social researchers. In her thesis, Mumford commented on the predicament which King had found herself in having completed her covert research in the hosiery factory. In her role as a trainee-instructor, King felt a loyalty to her fellow workers and they often confided in her. King was reluctant to betray these confidences to the management of the firm or to do anything that might damage her friendships on the shop floor.[183] Mumford noted that King was eventually able to publish her interpretations and conclusions some time after her investigations were finished.[184] Also in her thesis, Mumford clearly recognised the negative connotations associated with covert research. She suggested that the role adopted by a covert participant observer should 'fit the participant observer's personality, age and sex. If possible, it should not be too far removed from the truth so that the necessity for lies and evasions is reduced to a minimum'.[185] She recognised the effect these connotations had on the use of the method within the social sciences in Britain: 'The unfortunate association of participant observation with the anti-social function of spying is one of the reasons why it is condemned as a research method by a number of sociologists'.[186]

In practice, Mumford appeared to find being covert quite challenging. In the canteens, she had to imitate the appearance and behaviour of the other canteen workers. With some practice, she was able to pick up the local accent and to overcome her initial revulsion to dressing in what she described as the dirty and untidy manner favoured by the other canteen workers. Other aspects of being covert were, however, more troublesome for Mumford. It was only 'with difficulty' that she acquired the highly valued skill amongst the canteen workers for repartee with the dockers and to 'retaliate in kind to a cheeky or suggestive remark'.[187] There is evidence to suggest that she struggled to maintain her cover stories whilst working covertly in the canteens. In Canteen A, when she was posing as a trainee canteen supervisor, Mumford's true identity was almost revealed when she 'was detected using an unusual brand of lipstick'.[188] Furthermore, she found it much easier to stay in character, as it were, in Canteen C where she described herself simply as a student

[183] Mumford, 'An Evaluation', pp.30-31; King, 'Task...Part II', p.409.
[184] Mumford, 'An Evaluation', p.31.
[185] Ibid., p.237.
[186] Ibid., p.24.
[187] Ibid., p.156.
[188] Simey, 'The Analysis', p.82.

doing holiday work as this role was easier to assume and portray given its proximity to the truth and did not require such blatant misrepresentation in terms of her dress and accent.[189]

It was not just on a practical level in the field that Mumford appeared to struggle to some extent with her covertness: it also caused her a degree of emotional turmoil. In her discussion of the merits of covert participant observation as a method for studying industrial work groups, Mumford noted that the participant observer may experience feelings of guilt given 'the necessity for deceit' when they are being offered sincere friendship by members of the group under observation who are unaware of their true identity and research agenda. Clearly reflecting on her own experiences in the dock canteens, Mumford explained that

> Because of…[the covert participant observer's] personal identification with the groups observed he must make a difficult ethical decision as to how he can make use of the information he has obtained. If he continues to conceal his identity it may be difficult for him to publish his results in any general way, since he would be unwilling to have members of the groups suddenly exposed to publicity made possible only by their involuntary participation in the research. But, unless the participant observer reveals his identity, it is impossible for him to forewarn the groups of the consequences of his association with them.[190]

It could be the case that Mumford spent some time mulling over this dilemma, attempting to achieve a balance between making use of the research data she had gathered and protecting her research subjects by avoiding what she termed 'irresponsible publication'.[191] Mumford must have undertaken her covert research in the dock canteens prior to 1954 when *The Dock Worker* was published yet it was not until 1958 and 1959 that she published her articles based on this episode of fieldwork. Simey, who was also based at the Department of Social Science in Liverpool University, suggested as much when he noted that, as a consequence of Mumford becoming friendly with the canteen girls, Mumford 'found herself very reluctant to publish in Liverpool the data she obtained there in case it ever came to their knowledge and they were led to feel that she had betrayed them.'[192] If data cannot be published then it was unlikely to have any practical bearing on social problems thus covertness was an obstacle to realising the usefulness of social research. Mumford's own misgivings about the covert nature of her research may have contributed to the response her work received from other social scientists.

Simey, Mumford's distinguished colleague at Liverpool, wrote an article on the merits of participant observation as a method of analysing social problems and he was

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[189] Mumford, 'An Evaluation', pp.155-156.
[190] Ibid., p.159.
[191] Mumford, 'An Evaluation', p.161.
[192] Simey, 'The Analysis', p.83.

quick to point out the flaws in Mumford's work and, in general, seemed to have reservations about the applicability and utility of such methods suggesting that it could best be used to 'obtain general information as a preliminary for a more penetrating study'.[193] Mumford echoed Simey's sentiment suggesting that if participant observation was used to obtain only 'general information preparatory to a more penetrating study' the covertness of the research would be less problematic as it would be less likely that the research agenda would 'reach the ears' of the research subjects through publication.[194]

Mumford's covert work at the dock canteens appears to have been marginalised to some extent in the historiography of industrial sociology at Liverpool. We already know that Mumford considered the primary objective of her dock canteen research to be the evaluation of participant observation as a research method. However, the material on which her articles were based had been collected as part of *The Dock Worker* enquiry carried out by the Department of Social Science at Liverpool University.[195] Although both Mumford and Simey have emphasised the use of participant observation in the early stages of research, Mumford's data is conspicuous by its absence in *The Dock Worker*. Her material relating to canteens hardly features at all in the publication.[196] The other researchers involved analysed different aspects of working at the docks and unlike Mumford they did not use covert research techniques.[197]

In the mid-1960s, Richard Brown of the University of Leicester addressed the Conference of University Teachers of Sociology in Liverpool and took the opportunity to review Liverpool's Department of Social Science's work on industrial sociology.[198] He noted that, in general, the Department had stuck to orthodox research techniques using statistics and interviews to gain most data.[199] He did note that participant observation had been used 'by at least one member of the Department but not directly' for any of the studies featured in his speech, including *The Dock Worker*.[200] He was, of course, referring to Mumford's covert work.[201] In their review of sociological research at Liverpool since 1945, Scott and Mays marginalised Mumford's work to the extent of appearing to deny its existence. In a noticeably incongruous passage, they wrote that

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[193] Ibid., p.84.
[194] Mumford, 'An Evaluation', p.159 & 161.
[195] Mumford, 'Social', p.137.
[196] University of Liverpool, *The Dock.*
[197] Ibid.
[198] Richard Brown, 'Participation, Conflict and Change in Industry: A Review of Research in Industrial Sociology at the Department of Social Science, University of Liverpool', *Sociological Review*, Volume 13, Issue 3, (1965), p.273.
[199] Brown, 'Participation', p.276.
[200] Ibid., pp.276-277.
[201] Ibid., p.293.

> One final general point should perhaps be mentioned. We [members of the Department of Social Science at Liverpool University] have always acted on the assumption that a project should not proceed unless there is an understanding and acceptance of its aims on the part of the 'subjects' involved. This is not only a moral issue, but also in our experience a pragmatic necessity, at least in the long run.[202]

Of course, Mumford was a covert researcher and therefore had undertaken research under the auspices of the Liverpool Department of Social Science without the explicit understanding and acceptance of the researched. If we take Scott and Mays' comments to their logical conclusion, covert research such as Mumford's was not only impractical but also immoral. It is noticeable that Scott and Mays referenced *The Dock Worker* as a part of the Department's contribution to industrial sociology but they did not acknowledge Mumford's articles based on covert research at all, even though her piece on social norms in the work place had been published the previous year in the same journal in which Scott and Mays' review appeared.[203]

It seems, therefore, that the growing concern about research ethics and the social sanctioning of research in the decades following the war may have inhibited researchers from considering the use of covert methods, including covert ethnography. It was the covertness of Spencer and Mumford's respective research projects which in each case appeared to cause problems. Mumford struggled with her conscience in the field; Spencer struggled to accommodate the ethnographic material in his text. Whereas Spencer seemed almost unwilling to acknowledge the covertness of his own research, Mumford's work was marginalised as an immoral anomaly, a black mark on the record of Liverpool's industrial sociology. Although there was general support for an increased use of qualitative methods in the post-war period and the import of the anthropological paradigm which legitimised open participant observation, it seems that ethical concerns may have prevented social scientific researchers from undertaking covert ethnography.

Conclusion

In the previous chapter, the emergence of a professional anthropological model for ethnography was discussed. In this chapter, it has been established that between 1946 and 1969, the anthropological paradigm, essentially the ethnographic method, was adopted by

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[202] Scott and Mays, 'Research', p.111.
[203] Ibid., pp.116-117. It is interesting to note that when Mumford used her research at the docks as a case study in a much later publication concerning implementing changes in the workplace, she emphasised the participatory aspect of her fieldwork, but failed to mention the covert element. Enid Mumford, *Redesigning Human Systems*, (London: Information Science Publishing, 2003), pp.52-69.

the academic discipline of sociology. The emphasis on post-war empiricism, taken together with the import of the ethnographic method, would, in theory, have fostered the use of participant observation between 1946 and 1969. However, there were two factors which inhibited and shaped the use of covert ethnography in post-war Britain, despite these developments. First, as Banton explained, the shift from the survey paradigm to the anthropological paradigm, which legitimised the ethnographic method, did not go smoothly and post-war sociology continued to be dominated by the production of surveys. With reference to Spencer's covert ethnography, it was discovered that even when researchers actively tried to embrace the ethnographic method, the survey paradigm continued to shape their work. Second, although the adoption of the anthropological paradigm paved the way for the academically legitimate use overt ethnography, the emerging debate concerning the ethics of covert research within a professional discipline of sociology actively discouraged the use of undercover research methods. The marginalisation of Mumford's covert research was symptomatic of academic sociology's discomfiture with the use of subterfuge, which some commentators suggested undermined the professionalism of the discipline. In the next chapter, it will become clear that the concern surrounding research ethics would continue to have significant implications for the use of covert ethnography by professional sociologists.

Chapter 4: Academic Covert Ethnography in the 1970s

Introduction

The 1970s was a much more prolific episode in the history of covert ethnography when compared to the post-war era and in this chapter, we will analyse eight examples of British academic research which involved the use of covert participant observation. These research projects were undertaken against a backdrop of considerable debate about the ethics of professional sociological research and this context will be established before the ethnographic projects are described. As we will discuss in this chapter, the subject matter of these research projects focused on the key social research issues of the 1970s: work cultures, deviancy, ethnicity and subcultures. During the 1970s, for example, covert ethnography was used by academics to study aspects of gambling, crime in the workplace and Pentecostalism.[1] Once these research projects have been summarised, we will explore the construction of these 1970s academic covert ethnographies in some detail, comparing and contrasting them with other examples of covert ethnography. Then, given the growing significance of ethical research standards during this era, the ethical implications of the featured ethnographies will be discussed. Furthermore, the concern expressed regarding the ethical status of covert research fed into a wider debate about the reputation of professional sociology and this will be discussed briefly towards the end of the chapter.

Sociology and Research Ethics in the 1970s

In the previous chapter, it was explained that ethical concerns surrounding social research may have dissuaded social scientists from using the method of covert participant observation since the end of the Second World War and throughout the 1950s and 1960s. It was suggested that this could explain the scarcity of covert ethnographies produced by British academics in this period. Despite the rapid growth experienced by the discipline of sociology in Britain in the late 1960s and early 1970s, there appears to have been only a

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[1] On gambling, see: David Oldman, 'Chance and Skill: A Study of Roulette', *Sociology*, Volume 8, Issue 3, (1974), pp.407-426; David Oldman, 'Compulsive Gamblers', *Sociological Review*, Volume 26, Issue 2, (1978), pp.349-371. On crime in the workplace see: Gerald Mars, 'Hotel Pilferage: A Case Study in Occupational Theft' in Malcolm Warner (ed.), *The Sociology of the Workplace: An Interdisciplinary Approach*, (London: Allen and Unwin, 1973), pp.200-210; Jason Ditton, 'Baking Time', *Sociological Review*, Volume 27, Issue 1, (1979), pp.157-167; Jason Ditton, *Part-Time Crime: An Ethnography of Fiddling and Pilferage*, (London: Macmillan, 1977). On Pentecostals see: Andrew Walker and James Atherton, 'An Easter Pentecostal Convention: The Successful Management of a "Time of Blessing"', *Sociological Review*, Volume 19, (1971), pp.367-387; Roger Homan, 'Interpersonal Communication in Pentecostal Meetings', *Sociological Review*, Volume 26, Issue 3, (1978), pp.499-518; Ken Pryce, *Endless Pressure: A Study of West Indian Life-Styles in Bristol*, (Harmondsworth: Penguin, 1979).

modest increase in the number of covert ethnographies published in the 1970s. Even though a greater number of sociologists appear to have made use of covert fieldwork methods in the 1970s than had done so previously, ethical concerns may have continued to hinder the uptake of such incognito research on a larger scale.

It was in 1970 that the British Sociological Association (BSA) published their 'Statement of Ethical Principles and their Application to Sociological Practice' which they hoped would act as a useful guide for sociological research workers.[2] The guide aimed to set out the 'responsibilities of the sociologist acting in his professional capacity: (i) to his discipline and (ii) to the subjects of his study' and, although not specifically targeted at the use of covert methods, many of the recommendations could nevertheless be applied to such research.[3] According to the BSA, it was the duty of the professional sociologist

> to maintain the independence and integrity of sociology as a discipline, the freedom to research and to study, publish and disseminate the results of sociological research, saving that in the pursuit of these ends he should remember at all times his responsibility to safeguard the proper interests of those studied or affected.[4]

As a professional, the sociologist had a responsibility 'to explain as fully as possible and in terms meaningful to the subjects what his research is about, who is undertaking and financing it and why it is being undertaken'.[5] A note specifically pertaining to the use of covert research methods was included in the 1970 'Statement of Ethical Principles', demonstrating the scale of ethical unease associated with such fieldwork in Britain at this time. The BSA recommended that the professional sociologist

> be aware of the ethical issues involved in observation or experimental manipulation of subjects without their knowledge, a form of research enquiry which should be resorted to only where it is not possible to use other methods to obtain essential data. These methods should only be used when it is possible to safeguard completely the interests and anonymity of the subjects.[6]

Effectively, the BSA's 'Statement' formalised the concerns expressed by Simey and others in the 1950s that researchers had a responsibility to publish their research findings and to consider the effect that their fieldwork could have upon the conduct of further research in that area in the future.[7] When the 'Statement' was published, it was noted that the code had been developed with the aid of questionnaire responses from BSA members, with reference to the ethical guidelines of overseas social science associations and had been

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[2] V.L. Allen, Correspondence on 'Statement of Ethical Principles and their Application to Sociological Practice', *Sociology*, (1970), Volume 4, pp.114-117.
[3] Allen, Correspondence, p.114.
[4] Ibid.
[5] Ibid., p.115.
[6] Ibid.
[7] Ibid. See above, p.158.

modified in light of discussion at the 1969 BSA AGM.[8] It would seem safe to assume that the 'Statement' enjoyed broad support in the sociological community given the opportunities for input BSA members were given in the development of the code and the absence of any comment in subsequent issues of *Sociology*, although perhaps reactions were expressed in another, unpublished, forum.

Nonetheless, despite the BSA's clear attempt to curtail the use of covert research methods by professional social scientists in Britain, other commentators in the 1970s were more supportive of such clandestine fieldwork. Jack Douglas, an American sociologist of deviance, for example, was aware of the ethical debate which had surrounded the use of covert observation. However, he maintained that, in some research contexts, it would be the only feasible way to access certain types of information.[9] He did not deny that serious moral questions were at stake but he did suggest that the ethical objections made against covert research by social scientists involved 'considerable academic posing and "priggishness"'. Douglas was keen to avoid what he described as 'methodological puritanism' and suggested that in 'the search for the truth', covert research could be justified.[10] Admittedly, Douglas was not saying anything that radically differed from the BSA's ethical stance on fieldwork. Rather, it is a question of emphasis. The BSA described covert research as a last resort for the retrieval of 'essential' data, although the term 'essential' remained undefined. Douglas, on the other hand, maintained that 'in the search for truth', there were whole categories of information in certain social contexts that could only be accessed through covert research.[11] The debate hinges on what exactly constitutes 'essential' data and the notion of all sociological 'truths' being of equal importance.

John Barnes, a British sociologist, provided a much more nuanced discussion of the ethical acceptability of covert fieldwork towards the end of the 1970s. He recognised that the attitudes of his fellow social scientists towards covert research varied 'from unqualified disapproval to easy or enthusiastic acceptance'.[12] Some were of the view that all covert research was categorically ethically wrong and that if information could not be acquired by social scientists operating openly, then it should simply not be sought at all. Some even suggested that the use of deception and lying by social scientists ultimately undermined the

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[8] Ibid., p.114.
[9] Jack Douglas, 'Observing Deviance', in Jack Douglas (ed.), *Research on Deviance*, (Random House: New York, 1972), pp.5-10.
[10] Douglas, 'Observing', p.8.
[11] Ibid.
[12] John Barnes, *Who Should Know What?: Social Science, Privacy and Ethics*, (Cambridge: Cambridge University Press, 1980), p.121.

just social order by eroding trust.[13] However, Barnes balanced out this rather prohibitively rigid condemnation of covert research with a couple of arguments aimed at normalising the inherent deceitfulness of such fieldwork. In the first place, Barnes suggested that deception was a normal feature of everyday life and there was therefore no reason why the social researcher should have to eradicate all traces of deception from their research. According to Barnes, the anthropologist Gerald Berreman argued that deception was 'an integral part of all social relations, and that the relation between scientist and citizen cannot be an exception to this rule'. Using the ideas of Erving Goffman to support his position, Barnes noted that all people, scientists and citizens alike, shared the notion of 'back and front regions of social space'. Everyone undertakes secret preparation in the back region for the social roles they enact in the front region, in public. What the social scientist was trying to do was look behind his/her research subject's mask, to see into their back region and discern what was really going on behind the performance of everyday roles. As the research subject was presenting a mask, a performance, to the social scientist, there was nothing wrong with the social scientist using a mask too. To be absolutely honest about the research agenda was, according to Berreman, 'ethically unnecessary and methodologically unsound'.[14]

Secondly, Barnes suggested that the partial concealment of truth was in fact 'a pervasive concomitant of field research', not something exclusively associated with covert ethnographic research.[15] It could be argued that those using other methods of social research, such as the survey, may be equally prone to concealing elements of the truth from their research subjects by, for example, burying the 'real' questions amongst some red herrings in order to disguise the true focus of the research project. Of course, Barnes noted that covert research could take many forms, some clearly more objectionable than others.[16] For him, a scientist who studied a workforce covertly but with the connivance of management was much more open to criticism on ethical grounds than the completely covert researcher.[17] Barnes' normalisation of deception in everyday life and in social research acts to counterbalance the perspective that covert research was inherently ethically unacceptable. However, some undercover research projects have been undertaken which stretch this notion to its very limits.

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[13] Barnes, *Who Should Know What*, pp.122-123.

[14] Ibid., p.106; Gerald Berreman, *Behind Many Masks: Ethnography and Impression Management in a Himalayan Village*, (Indianapolis: Bobbs-Merrill, 1962); Erving Goffman, *The Presentation of Self in Everyday Life*, (London: Penguin, 1990).

[15] Barnes, *Who Should Know What*, p.104.

[16] Ibid., p.121.

[17] Ibid., p.123.

In Montreal in the 1970s, two Canadian anthropologists undertook covert research in a palliative care unit for the dying.[18] One of the medical anthropologists assumed the role of a patient with terminal pancreatic carcinoma. Key gatekeepers at the medical establishments he stayed in were aware of his pseudo-patient status. The vast majority of the staff he came into contact with, all of the other terminally ill patients and their visitors, however, were unaware of his research status. To simulate the features of his assumed illness, the researcher underwent procedures such as 'supraclavicular incision' and learned to mimic the symptoms by closely observing fellow patients dying of the condition.[19] An editorial paper written by some of the researchers involved in this project made it clear that they did take ethical considerations seriously and that, as a result of the research, the covert observer himself suffered physically and psychologically.[20] However, the researchers manifestly failed to address the implications their covert research had for the terminally ill patients on the ward who empathised with the researcher, and their family and friends, with whom the researcher interacted. It is not revealed, for example, how the researcher affected an exit from his research role. The possibility that, in collusion with the key gatekeepers, the researcher allowed his companions on the palliative care unit to believe that he had finally succumbed to his illness and died was not ruled out. Perhaps, on the other hand, the researcher made his real identity public towards the end of the project, just before leaving the field. In this case, many of the other patients suffering from terminal pancreatic carcinoma may have died before he had the chance to explain that he had simply been faking the illness of which they were dying. Such an example of covert participant research, even though it is not British and extreme as it is, highlights the ethically murky territory of undercover social research. Historically, however, even relatively 'innocuous' examples of covert research based on eavesdropping on conversations in public places attracted comment concerning the invasion of privacy.[21]

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[18] Ibid., p.126; W.B. Murray and R.W. Buckingham, 'Implications of Participant Observation in Medical Studies', *Canadian Medical Association Journal*, (1976), Volume 115, Number 12, pp.1187-1190.
[19] R.W. Buckingham, S.A. Lack, B.M. Mount, L.D. MacLean and J.T. Collins, 'Living with the Dying: Use of the Technique of Participant Observation', *Canadian Medical Association Journal*, (1976), Volume 115, Number 12, pp.1211-1215.
[20] Murray and Buckingham, 'Implications', pp.1187-1190.
[21] Barnes, *Who Should Know What*, p.121. Here, Barnes was referring to the research carried out by Carney Landis on national differences in conversation conducted in America: Carney Landis, 'National Differences in Conversations', *Journal of Abnormal and Social Psychology*, (1927), Volume 21, pp.354-357.

Academic Covert Ethnography in the 1970s

Covert Observation of Workplaces and Workplace Deviancy

In the 1970s, at least three social scientists published work based on the use of covert methods to research issues related to workplace behaviour, particularly deviant or social behaviour in the workplace: Stuart Timperley; Gerald Mars and Jason Ditton. Although the findings were not published until 1970, it was in 1966 that Timperley, based at the University of Liverpool, spent twelve weeks working covertly as a general hand in a newly opened municipal airport somewhere in Britain.[22] As part of one of four shifts of general hands, Timperley worked alternately as a crew member, a baggage handler and a porter.[23] The purpose of his research was to explore the 'emergence of a social organization' amongst the workers in this newly established workplace.[24] Rather than focusing on the work tasks assigned to the general hands, Timperley was more interested in how the airport workers organised their social time and activities in the workplace. He was concerned with issues seemingly peripheral to the work role such as the emergence of a system for the fair distribution of tips amongst general hands and the social rules governing the gambling of basic wages as opposed to money earned through overtime and tips by married men.[25] Timperley's ethnographic article appeared in the *Sociological Review* and his subsequent publications demonstrated his continued interest in the sociology of the workplace.[26]

Gerald Mars, an industrial sociologist born in Manchester in 1933 and educated at Cambridge, the London School of Economics and in Newfoundland, produced a short covert ethnographic account of pilferage in the hotel industry.[27] His research appeared as a chapter in an edited collection entitled *The Sociology of the Workplace* in 1973. Mars described his work as an exploration of 'institutionalized pilferage' in the hotel industry using the method of participant observation.[28] Mars gathered, or generated, the data on

[22] Stuart Timperley, 'A Study of a Self-Governing Work Group', *Sociological Review*, Volume 18, Issue 2, (1970), pp.259 & 279. Timperley also carried out a number of interviews once the 'participational' phase of his research was finished: Timperley, 'A Study', pp.259 & 279.
[23] Ibid., p.261.
[24] Ibid., p.259.
[25] Ibid., pp.266-268 & 267.
[26] See: Stuart Timperley, *Personnel Planning and Occupational Choice*, (London: Allen and Unwin, 1974); Dan Ondrack and Stuart Timperley (eds.), *The Humanisation of Work: a European Perspective*, (London: Alan Armstrong and Associates, 1982). Later in his career, Timperley was Professor of Strategic Management and Organizational Behaviour at the London Business School for a number of years and, demonstrating his commitment to the practical realisation of his academic work, Timperley was a founding partner and chairman of Slatter Timperley Associates, an organisation involved with strategic consulting and management education. The British Theatre Guide, 'ACE [Arts Council England] announced the new council', http://www.britishtheatreguide.info/news/ACE-newcouncil.htm, [accessed 4th August 2008].
[27] Paul Rock and Mary McIntosh (eds.), *Deviance and Social Control*, (London: Tavistock, 1974), pp.viii-ix.
[28] Mars, 'Hotel', p.200.

which the chapter was based by working as a waiter in a hotel in the seaside town of Blackpool which employed twenty waiting staff in the dining room and was open to the public.[29]

In his account, Mars described how waiting staff used various 'fiddles' to supplement their wages. One such 'fiddle' which Mars observed was described at length. Say a customer ordered two coffees. If the waiter was 'on the fiddle', he would order only one coffee from the kitchen and then, using hidden crockery, he would split this one cup of coffee between two cups. Now the waiter can charge the customer for two coffees but only pass half of the payment on to the hotel, pocketing the other half for himself. Such scams were made easier, according to Mars, if you had an accomplice in the kitchen or, in large dining rooms, a 'checker' willing to turn a blind eye. Mars also detailed how waiting staff justified their pilferage with reference to their paltry wages and how they distinguished between such accepted institutionalized forms of pilferage directed at the employing establishment and actual theft from guests, which was considered truly deviant behaviour.[30]

Barry Turner reviewed the edited collection in which Mars' work on hotel pilferage appeared and, despite finding the collection on the whole disappointing, he did describe Mars' contribution as 'a modest piece of reporting in the spirit of the Chicago School', and 'deserving of mention'.[31] Mars' account of pilferage in the hotel industry certainly does make fascinating reading, yet, there are reasons to suspect that this is the kind of research into deviant behaviour which Richard Sparks dismissed as 'participant observation of the ""What I did on my summer vacation" variety'.[32] Mars completed his doctoral research into dock pilferage in Newfoundland using a research method approximating participant observation in 1972.[33] His account of hotel pilferage appeared just one year later in 1973. In this account, Mars revealed that his observations of and participation in hotel pilferage 'were by-products of an anthropologist's modified field approach or through working in a number of jobs before receiving formal anthropological training – what can be termed "retrospective participant observation"'.[34] Furthermore, Mars described his method of data collection as 'unsystematic', suggesting that at the time he worked in the hotel, he had no

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[29] Ibid., p.202.
[30] Ibid., pp.202 204.
[31] Barry Turner, 'Review of *The Sociology of the Workplace: An Interdisciplinary Approach* by Malcolm Warner (ed.)', *Sociological Review*, (1974), Volume 22, Issue 2, p.282.
[32] Richard Sparks, 'Review of *Images of Deviance*, Stanley Cohen (ed.)', *Sociology*, Volume 7, (1973), pp.148-149.
[33] Gerald Mars, 'Dock Pilferage', in Paul Rock and Mary McIntosh (eds.), *Deviance and Social Control*, (London: Tavistock, 1974), pp.209-228.
[34] Mars, 'Hotel', p.200.

conception of using his experience of hotel work as the basis of an academic discussion of pilferage.[35]

It would seem reasonable to suggest that Mars took on work as a waiter in Blackpool, perhaps during the summer break, when he was an undergraduate student. It may have only been later, after his use of participatory methods to explore dock pilferage for his doctorate, that Mars decided to retrospectively consider his time as a waiter as an episode of research. This possibility raises issues about the status of participant observation as a research method. To what extent can sociologists or other writers consider recalled episodes from their 'real' lives as periods of research? Does this suggest that all of the ethical and personal turmoil experienced by academic covert observers, all of their efforts to professionalise and make scientific their research, are unwarranted? Nevertheless, Mars' work on hotel pilferage, considered alongside the research of Timperley and Ditton, fits in well with other sociological explanations of workplace behaviour in the 1970s. He demonstrated an enduring interest in the sociology of workplace deviancy, being involved with the publication of more texts on this issue in the 1980s and into the twenty-first century.[36]

As a doctoral student at the University of Durham, Jason Ditton undertook covert participatory research into deviant behaviour at a medium-sized factory-production bakery in the south-east of England, fictitiously named the Wellbread Bakery.[37] He was spurred on to research this area as a result of Stanley Cohen's lectures on deviance and he was also supported by a noted American industrial sociologist, Donald Roy, who had used covert observation to study non-productive behaviour in the workplace.[38] Ditton produced a number of articles based on this episode of covert research before and after his book, *Part-Time Crime: an Ethnography of Fiddling and Pilferage*, was published in 1977.[39] For the purposes of this chapter, however, we will focus exclusively on the book, Ditton's fullest account of his covert research project.

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[35] Ibid., p.202.
[36] See, for example: Gerald Mars, *Cheats at Work: An Anthropology of Workplace Crime*, (London: Allen and Unwin, 1982); Gerald Mars, *Occupational Crime*, (Aldershot: Ashgate, 2001); Gerald Mars, *Work Place Sabotage*, (Aldershot: Ashgate, 2001).
[37] Ditton, *Part-Time*, p.1.
[38] Ibid., Preface. See, for example: Stanley Cohen (ed.), *Images of Deviance*, (Harmondsworth: Penguin, 1971); Donald Roy, 'Quota Restriction and Goldbricking in a Machine Shop', *The American Journal of Sociology*, Volume 57, Number 5, (1952), pp.427-442, Donald Roy, 'Banana Time: Job Satisfaction and Informal Interaction', *Human Organization*, (1959), Volume 18, pp.158-168.
[39] See, for example: Jason Ditton, 'The Fiddling Salesman: Connivance and Corruption', *New Society*, November 1974, pp.535-537; Jason Ditton, 'Alibis and Aliases: Some Notes on the "Motives" of Fiddling Bread Salesmen', *Sociology*, Volume 11, Number 1, (1977), pp.233-255; Ditton, 'Baking'.

In 1971, Ditton's proposal to complete a PhD based on a period of 'relatively unstructured participant observation' was accepted by Durham University. As an undergraduate student, Ditton had worked at the Wellbread Bakery during vacations and he decided to use this workplace as the scene of his research.[40] Although Ditton did not begin receiving his graduate grant until the October of 1971, he worked during the summer of that year 'as a plant worker, *né* undercover participant observer' at Wellbread's.[41] Between May and December 1972, keen to get back into the thick of it, Ditton returned once again to Wellbread's and worked this time as a despatch operative.[42] Between January and June in 1973, Ditton interviewed his fellow despatch workers and then, in June 1973, he took work in the sales department in order to observe his fellow salesmen. In September 1973, Ditton stopped working as a bread salesman and spent the rest of the year interviewing salesmen.[43]

The fact that Ditton had previously worked at Wellbread's does, of course, bring in to question the extent to which his research was truly covert. Ditton recognised how his research differed in this respect from other participant observers: he already 'knew most of the men, and had none of the usual problems of getting permission, getting into the field, getting accepted, or getting going'.[44] In addition, in the early stages of his fieldwork, Ditton was obliged to reveal his status as a researcher to his co-workers at Wellbread's. Naturally, Ditton found it impossible to remember everything that happened during the course of a working day at Wellbread's and needed to make rough notes during his shifts. Ditton recalled that

> Eventually the wheeze of using innocently provided lavatory cubicles occurred to me. Looking back, all my notes for that third summer were on Bronco toilet paper! Apart from the awkward tendency for pencilled notes to be self-erasing from hard toilet paper…, my frequent requests for 'time out' after interesting happenings or conversations in the bakehouse and the amount of time that I was spending in the lavatory began to get noticed. I had to pacify some genuinely concerned work-mates, give up totally undercover operations, and 'come out' as an observer – albeit in a limited way.[45]

Afterwards, Ditton was able to take notes more openly, but he was careful to remain coy and vague when questioned about his note-taking being linked to his university studies.[46]

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[40] Ditton, *Part-Time*, pp.4-5.
[41] Ibid., p.5. The term '*né*' does not seem appropriate but it is used in the original.
[42] Ibid., pp.5-6.
[43] Ditton, *Part-Time*, pp.6-8.
[44] Ibid., p.5.
[45] Ibid.
[46] Ibid.

Ditton's periods of participant observation were interrupted by episodes of much more intrusive and overt research using questionnaires and interviews to elicit information from co-workers. The questionnaires were intended by Ditton to be nothing more than 'a blind' to distract the research subjects from his real research undertaken as a participant observer.[47] However, there is evidence to suggest that Ditton's presence in the bakery remained conspicuous. Towards the end of his project, Ditton noted that one salesman said he had 'stood out' and 'didn't look like a bakery person'.[48] Ditton's experiences in the field again highlight how fuzzy the boundaries can be between real life and research on the one hand and between overt and covert research on the other. Like Mars, Ditton used a place where he worked as the setting for a research project. Unlike Mars, however, Ditton made a clear distinction between periods of straightforward paid employment at the bakery and episodes of fieldwork. Some of Ditton's research subjects were aware that a research project was underway, but they were misled regarding the specifics. The bakery workers did not know that Ditton was researching workplace crime and, because Ditton used questionnaires and interviews to divert attention away from his observational research, they were probably unaware that their day-to-day interaction with Ditton was, in fact, at the core of his research.[49]

Ditton had not set out with the intention of studying the topic of workplace crime at the bakery but, by the time he was working with the sales team, he admitted that although the participatory research was intended to be unstructured with no specific topic delineated, that he 'almost knew' he was 'going to study "the fiddle"'. Ever since Ditton had worked at Wellbread's, the topic of 'fiddling' had been 'veiled in analogy, vagueness, mystification, allusion, euphemism and ambiguity' and this was the subject which therefore attracted Ditton's 'analytic curiosity'.[50] By the time Ditton's fieldwork had come to an end, he had spent more than 4,560 hours as a participant observer and had thirty-four interviews transcribed. Overwhelmed with the amount of information he had, it was clear that Ditton would have to focus on one aspect of life at Wellbread's and he decided to pursue his interest in fiddling and limited his analysis to the sales department.[51]

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[47] Ibid., p.6.

[48] Ibid., p.11. Two other covert participant observers researching work cultures were informed by research subjects that they were somehow different to the rest of the workforce: Beatrice Potter, 'Pages from a Work-Girl's Diary' *Nineteenth Century*, Volume 24, Number 139, (1888), pp.302 & 313; Polly Toynbee, *A Working Life*, (Harmondsworth: Penguin, 1973), pp.91-92; See above, p.60. See below, p.231. Ditton was, however, an employee at the bakery before he embarked on his research project, unlike Webb and Toynbee.

[49] Ditton, *Part-Time*, p.6.

[50] Ibid., p.7.

[51] Ibid., p.9.

Ditton described his work as 'a theoretical and not an empirical analysis of fiddling and, only secondarily, an analysis of the bread salesman'. Even though he believed that he had produced empirical novelty and had, in a way, 'provided a complete "recipe" of how bread salesmen may successfully go about fiddling', he was keen to stress the ultimately theoretical nature of his work. His aim was to explain how 'fiddling' could be simultaneously a criminal activity and yet be practically and psychologically trifling. He wanted to convey the ordinariness of fiddling amongst the bread salesmen, to communicate how fiddling was normal within the bread salesman's life.[52] The reviews of *Part-Time Crime* were positive and Ditton's work was referred to as the 'first full-length "insider" investigation' of hidden crime and as 'meticulously done and honestly reported.'[53] Gerald Mars, whose assistance Ditton acknowledged in *Part-Time Crime* and who wrote about fiddling in hotel restaurants, did however refer to Ditton's assertion that nothing had previously been written about workplace fiddles from a sociological viewpoint before as a blemish.[54]

Non-Work-Related Deviancy: Gambling, Alcoholism and Vagrancy

In the second half of the 1970s, two British sociologists, David Oldman and Peter Archard, published work on aspects of non-work-related deviancy that made use of participatory research which could be considered covert to some extent. David Oldman was educated at Cambridge and, after a spell as a sociology lecturer at the University of Sussex, he became a senior lecturer in sociology at the University of Aberdeen in 1971.[55] It was whilst he was a professional sociologist in Aberdeen that Oldman worked for two years as a part-time croupier in a small local casino in order to observe the behaviour and conversations of gamblers. Oldman published two articles based on his participatory research in the casino. The first, 'Chance and Skill: A Study of Roulette', was published after one year of field research in 1974, and the second, 'Compulsive Gamblers', was published some time after the episode of fieldwork had been concluded, in 1978.[56]

Oldman suggested that attitudes were polarized between those who gambled regularly and those who did not, with the former often being labelled deviant. In his first article, Oldman used his observations of one of the games played at the casino, roulette, to

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[52] Ibid., p.11.

[53] Gerald Mars, 'Review of *Part-Time Crime: An Ethnography of Fiddling and Pilferage* by Jason Ditton', *British Journal of Sociology*, (1980), Volume 31, Number 1, p.136; Stuart Henry, 'Review of *Part-Time Crime: An Ethnography of Fiddling and Pilferage* by Jason Ditton', *Sociological Review*, Volume 26, Issue 1, (1978), p.162.

[54] Ditton, *Part-Time*, Preface; Mars, 'Review', p.136.

[55] Oldman, 'Chance', p.426.

[56] Ibid., pp.407-426; Oldman, 'Compulsive', pp.349-371.

demonstrate how gambling was a 'rational activity' for gamblers.[57] The crux of the issue for Oldman was that roulette was a game of chance yet the gamblers he observed treated it as a game of skill. Whereas the existing literature on gambling stressed the passivity of gamblers when faced with unpredictable outcomes, Oldman's observations suggested that those playing roulette did not submit passively to the outcome but engaged in theorizing about, and attempting to influence or judge, the outcome of each spin of the roulette wheel.[58] In his second article, Oldman used his direct experience in the casino to challenge the view held by lay people, sociologists and gambling experts concerning the existence of compulsive gamblers. Within 'the world of the casino', Oldman insisted, there was 'no comparable concept' and the elusive compulsive gambler simply did not exist.[59] Rather, his observations had demonstrated that habitual gambling may indeed lead to economic crisis and that, subsequently, the gambler would adopt and espouse a 'rhetoric of compulsion' to retrospectively explain their actions.[60]

There are two aspects of Oldman's work which make considering it an example of academic covert ethnography problematic. First of all, at no point did Oldman use the term 'covert' to describe his research and, furthermore, there is no evidence to suggest that he employed any sort of cover story to explain his presence in the casino. From reading his articles, it is clear that he worked part-time at the casino whilst he was employed as a professional sociologist. Oldman noted that approximately sixteen part-time croupiers were employed by the casino to work evenings and they were drawn from a variety of day-time occupations. In this respect, Oldman was similar to the rest of the part-time croupiers. However, he went on to mention that the majority of his part-time colleagues had low-paid day-time occupations classified in the Registrar General's social class III.[61] In this respect, Oldman, as a relatively well-paid professional, did not fit the profile of the typical croupier. In his second article, Oldman reflected that

> After working at the club for a few months, in spite of the fact that my class and regional and occupational identities were alien to other staff and punters, I found that to enter the club was to escape immediately and gratefully from all the other problems of my existence into an entirely controllable world.[62]

This remark suggests that Oldman did not disguise his identity as a Cambridge-educated professional sociologist whilst he was working at the casino. Oldman did not mention

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[57] Oldman, 'Chance', p.407.
[58] Ibid., pp.408-412.
[59] Oldman, 'Compulsive', pp.349-350.
[60] Ibid., p.350.
[61] Oldman, 'Chance', p.412.
[62] Oldman, 'Compulsive', p.366.

whether any of his colleagues or clients had questioned his presence at the casino or if they had any idea that he was undertaking research.

Secondly, it could be suggested that Oldman's method of research was not, strictly speaking, participant observation (overt or covert), because he was using the role of croupier to observe gamblers.[63] Of course, there would have been considerable practical barriers preventing Oldman from living the life of a habitual gambler, most pertinently the unlikelihood of funding being forthcoming for such expensive field research. At least as a croupier, Oldman would have earned money. However, the phenomenon discussed in Oldman's first article, defining skill as a factor in a game of chance, did in fact involve the croupier quite significantly. Oldman suggested that many of the casino's clients attributed to the spinner 'a causal effectiveness in the outcome of the spin' thus roulette effectively became a contest between the 'punter' and, on behalf of the casino, the spinner. He noted that requests to place bets were sometimes whispered to the scraper lest the spinner overhear and deliberately avoid that number. Similarly, many punters waited until the ball had been released before placing their bets.[64] The croupiers also played a role in maintaining this contest. Placing and stacking chips was the most laborious and boring aspect of being a croupier and Oldman noted that, in an attempt to dissuade punters from flooding the table, croupiers would try to persuade punters of their spinning dexterity. Even the casino management colluded in this strange notion that skill was important in roulette, a game of chance. If a table was losing, the spinner was changed, often to a more senior member of staff.[65] Whereas Oldman observed the relatively safe and contained world of casino gambling, another sociologist, Peter Archard, immersed himself in an altogether more threatening and unpredictable social circle of homeless alcoholics.

In 1979, Archard published an ethnographic account of non-work-related deviancy entitled *Vagrancy, Alcoholism and Social Control*. Archard was a research fellow at the Polytechnic of the South Bank in London and he was supported in his research by sociologists based in other academic institutions in London as well as through attending meetings of the National Deviancy Conference.[66] In addition to these associations with the world of academic sociology, Archard also worked in a research capacity for the Alcoholics Recovery Project, a London-based charity which continues to operate today to

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[63] This is similar to John Spencer's covert ethnography of the relationship between crime and service in the forces. He adopted the role of assistant to the prison Chaplain rather than joining the forces and becoming a criminal. John Spencer, *Crime and the Services*, (London: Routledge and Kegan Paul, 1954), p.276; See above, pp.143-144.
[64] Oldman, 'Chance', p.419.
[65] Ibid., p.421.
[66] Peter Archard, *Vagrancy, Alcoholism and Social Control*, (London: Macmillan, 1979), pp.xiii-xiv & back cover.

reduce the harm caused by problem alcohol and drug use.[67] In an attempt to understand the relationship between vagrancy, alcoholism and the agencies and institutions designed to control these social phenomena, Archard undertook participatory research into a 'drinking school' in London.

According to the back cover of the paperback edition of *Vagrancy*, Archard spent fifteen months living on 'skid row'.[68] However, a more detailed program of empirical research was delineated by Archard inside the book. His research process consisted of first reading the relevant literature and then visiting 'skid row' institutions such as soup kitchens and hostels in the company of 'a recovered skid row alcoholic'.[69] Only then did Archard undertake the main participatory phase of his research. He had decided that 'gaining entrée into a drinking school' would offer him 'the best point of departure for obtaining an insider's view of the skid row world'. Initial contact was made with a drunk through a mutual acquaintance and Archard cultivated this relationship over a period of three months, often supplying money for alcohol, until he was eventually accepted by him into his drinking circle.[70] At its most intensive, Archard's fieldwork required him to spend several hours on a daily basis in a south London park with his 'original skid row contact' and his drinking friends. After approximately six months of participant observation with the drinking school, Archard moved on to researching the institutions which the men of 'skid row' frequented, such as common lodging houses and police courts.[71]

Archard's drinking circle were aware of his research agenda and they accepted him into their company in exchange for contributing drinking money and participating in other daily chores such as begging, keeping an eye out for the police, and doing 'the run' to an off-licence. Archard made it clear that this process of slowly and overtly gaining entrée into and the acceptance of the group being studied was 'in keeping with the recommendations of sociologists experienced in fieldwork'. At no point in his discussion of methodology did he refer to the possibility of researching skid row covertly, yet there were instances during the fieldwork when Archard did conceal his research status from those not immediately associated with his drinking school.[72] As the following passage reveals, Archard did, in fact, switch between overt and covert status in the field:

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[67] Archard, *Vagrancy*, p.xxi; Alcoholics Recovery Project, www.arp-uk.org, [accessed 26th August 2008]. Archard suggested that his association with this charity worked to his advantage during his fieldwork in that he was 'able to use the credentials of the agency as a passport' to other agencies of social control he wished to research. Archard, *Vagrancy*, p.214.
[68] Ibid., back cover.
[69] Ibid., pp.206-207.
[70] Ibid., p.207.
[71] Ibid., p.209.
[72] Ibid., pp.206-209.

> Each new observational setting demanded a renewed attempt to establish myself as a participant observer, either by divulging my true research role, or successfully blending into specific social milieus by 'passing' as a skid row man, for instance in common lodging houses, certain parks, open spaces, railway stations, police stations, magistrate's courts, and other locales.[73]

Archard justified his occasional covertness by suggesting that he may have been vulnerable to physical attack if his identity as a social researcher was known in certain contexts. Being occasionally covert was a strategy adopted by Archard to preserve his own safety whilst attempting to explore and understand the events unfolding around him at any given moment.[74] Significantly, Archard did not attempt to justify covertness in methodological terms.

Both Archard and Oldman conducted ethnographic fieldwork, but it is difficult to classify their projects conclusively as covert ethnographies. None the less, their apparent flirtation with covertness merits their inclusion in this discussion. Neither Oldman nor Archard's work appears to have attracted much critical attention from within academic sociology. Jerry Shepperd reviewed Archard's work for the journal *Contemporary Sociology* describing it as an important contribution to the literature on alcoholism and homelessness, but without drawing particular attention to his methodology.[75] However, in an article published in America in the 1990s, Archard's work was described as offering 'little insight into homelessness as a lived experience' when compared with the 'rich ethnographies' of homelessness and alcoholism in America by Nels Anderson and James Spradley.[76]

Ethnography and Religion: Covert Observations of Pentecostal Believers

During the 1970s, three academic accounts of aspects of Pentecostalism in Britain were published which made use of covert research. The first was an article by Andrew Walker and James Atherton entitled 'An Easter Pentecostal Convention: The Successful Management of a "Time of Blessing"' which appeared in the *Sociological Review* in

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[73] Ibid., p.224. This is similar to Norman Dennis' approach to fieldwork in the 1950s where no deliberate attempt was made to conceal his researcher status but neither was it disclosed unless necessary. Personal Correspondence; Norman Dennis, 'Changes in Function and Leadership Renewal: A Study of the Community Association Movement and Problems of Voluntary Small Groups in the Urban Locality', *Sociological Review*, Volume 9, Issue 1, (1961), pp.55-84.

[74] Ibid.

[75] Jerry Shepperd, 'Review of *Vagrancy, Alcoholism and Social Control* by Peter Archard and *Alcohol, Reform and Society: The Liquor Issue in Social Context* by Jack Blocker (ed.)', *Contemporary Sociology*, Volume 10, Number 3, (1981), pp.380-381.

[76] Ruth Pinder, 'Turning Points and Adaptations: One Man's Journey into Chronic Homelessness', *Ethos*, Volume 22, Number 2, (1994), pp.211-212; Nels Anderson, *The Hobo: The Sociology of the Homeless Man*, (London: University of Chicago Press, 1923); James Spradley, *You Owe Yourself a Drunk: An Ethnography of Urban Nomads*, (London: University Press of America, 1988).

1971.[77] Now Professor of Theology and Education at King's College London, Walker was a lecturer in sociology and social administration at Chiswick Polytechnic when this article was published. It was his first publication of many to date relating to Christian belief and practice.[78] James Atherton, who was an assistant lecturer in 'liberal studies' at Salford Technical College in 1971, regards the work he completed with Walker to be his 'one respectable sociological article'.[79]

Walker and Atherton used the method of participant observation to study a Pentecostal convention which took place in 'a Northern city' over four days, from Good Friday to Easter Monday. Although they noted that attendance at the convention was not restricted to Pentecostal believers, they decided to '"play" specific roles in relation to Pentecostalism'. One of the researchers posed as a committed believer in Pentecostalism throughout the duration of the convention, whilst the other pretended to be an evangelical who was interested but sceptical about Pentecostalism itself.[80] Despite suggesting that non-Pentecostals and even non-Christians would not be excluded from such conventions, Walker and Atherton justified their covertness in a footnote. They suggested that

> [t]he outsider is at a peculiar disadvantage in Pentecostal circles. He is treated either as an object of distrust or more likely as one in need of salvation. It would be impossible for him therefore to enter fully into areas of the services and informal gatherings designed for believers.[81]

One woman at the convention, a member of a local Pentecostal church, knew the professional identity of Walker and Atherton, and acted as their informant.[82] Walker and Atherton were able to tape record the services with impunity as this was often done by believers wishing to 'take the "blessing" to others unable to attend' and they also took notes which they secreted in their bibles.[83]

The Easter convention was composed of twelve major Christian services over the course of four days, other specialist services provided in anterooms at the back of the church and informal recesses at lunch and dinner.[84] It seems that Walker and Atherton's attempts to blend in with the believers were successful. They noted that conformity of dress was one of the 'recognition signals' used by believers to identify each other and the convention was 'very much a best dress and suit occasion'. Even more important than

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[77] Walker and Atherton, 'An Easter'.
[78] Professor Andrew Walker, Kings College London, www.kcl.ac.uk/schools/sspp/education/staff/awalker.html , [accessed 27th August 2008].
[79] James Atherton, James Atherton's Home Page, www.doceo.co.uk/me.htm , [accessed 27th August 2008].
[80] Walker and Atherton, 'An Easter', pp.367-368, 371.
[81] Ibid., pp.385-386.
[82] Ibid., p.368.
[83] Ibid., pp.368 & 379.
[84] Ibid., pp.371-372.

dress, however, were accessories which displayed commitment to Pentecostalism. The researchers noted that nearly everyone carried 'a copy of the Authorised Version of the Bible, usually bound in black Morocco leather' and that commitment to Jesus could be measured by the extent to which the Bible was well-thumbed. Other accessories included badges from organisations such as the Scripture Union, and one man sported a tie embroidered with the message 'Jesus Saves' in gold lettering. Walker and Atherton also noted that it was important that one 'looked "saved"' in order to blend in. During the recesses, the researchers found that their faith was assumed rather than questioned, and they supplied the expected responses when spoken to. When a believer asked the researchers, for example, 'Isn't Jesus wonderful?' one of them would respond 'Yes, praise his Name!'.[85]

Walker and Atherton used their observations to suggest how such a large event could be successfully managed. They suggested that the apparent spontaneity of the congregation remarked upon by previous researchers was in fact carefully managed by Pentecostal authority figures, most notably the pastors.[86] Walker and Atherton's paper based on covert observation of a religious gathering did not provoke much of a reaction from their fellow sociologists or the public in general. This is surprising given the hostile response that even non-covert studies of religious groupings received in the 1970s. For example, Roy Wallis' research into the Church of Scientology in Britain, based largely on documentary and interview data, was only published after 'lengthy and expensive litigation'.[87] Roger Homan's covert research into Pentecostal meetings in Britain, published seven years after Walker and Atherton's article, sparked considerable and relatively forthright debate amongst sociologists about the ethics of such fieldwork.

Homan, currently Professor of Religious Studies at the University of Brighton, undertook his research into Pentecostalism in Britain in the late 1970s when he was with the same institution, then called Brighton Polytechnic. Homan's article entitled 'Interpersonal Communication in Pentecostal Meetings' was published in the *Sociological Review*.[88] In this article, Homan's purpose was to survey interpersonal communication in Pentecostal assemblies and his empirical research was extensive. As an overt observer and interviewer, he visited fifty-eight Pentecostal institutions across Britain, Canada and the

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[85] Ibid., pp.378-379.
[86] Ibid., p.372.
[87] Roy Wallis, *The Road to Total Freedom: A Sociological Analysis of Scientology*, (London: Heinemann, 1976), pp.v-vii.
[88] Homan, 'Interpersonal'.

United States of America.[89] In addition, Homan felt it was necessary to undertake a substantial period of covert participant observation by 'fellowshipping' with a Pentecostal assembly in Britain on a regular basis for over a year. During this phase of his research, none of Homan's fellow Pentecostal believers were aware of his research interest.[90]

Homan chose to use covert research techniques because of the way he thought his research subjects, Pentecostal believers, would react to him if he declared his real intentions as a non-believer and, even more significantly, as a social researcher. Like Walker and Atherton, Homan was concerned that as a non-Pentecostal at Pentecostal gatherings, he would be subjected to the concentrated 'evangelism' efforts of believers. Pretending to be a believer was therefore 'desirable' and 'expedient' as it would allow Homan to conduct his research 'unhindered'.[91] As well as misleading his research subjects about his faith, Homan also concealed his research agenda from the other Pentecostals during this phase of research. As a result of his experiences as a non-participant observer in several 'old-time pentecostal' assemblies, Homan judged the 'old-time pentecostal' movement to be 'highly disfavourable to the conduct of overt sociological research'. He came to this conclusion because he observed 'a consistent denigration by preachers of education in schools and universities' as part of a 'more general iconoclasm'. He noted that 'Of all the people in universities, the sociologist is for old-time pentecostals the real bogy-man [sic]… [It] transpired that old-timers hold a generalized view of sociology as "communist inspired" or "atheistic"'.[92] As such, it was only by undertaking his research covertly that Homan would have the opportunity to observe 'the normal language-behaviour' of his research subjects.[93]

Like Walker and Atherton, Homan was also able to use a tape recorder occasionally as meetings were sometimes recorded in order to share them with those who could not attend. Similarly, on occasions when the preacher suggested to the congregation that they might like to take a note of the point he was about to make, Homan took the opportunity to make written notes for his research.[94] He was a completely covert participant observer in that he 'conformed his outward behaviour in all possible respects with the norm' that existed amongst the Pentecostal congregation. He moved to accommodation close to the assembly he had chosen to infiltrate and, when he attended their meetings, he carried a

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[89] Ibid., p.500.
[90] Homan, 'Interpersonal', p.501; Roger Homan, 'The Ethics of Covert Methods', *British Journal of Sociology*, (1980), Volume 31, Number 1, p.46.
[91] Homan, 'Interpersonal', p.500.
[92] Homan, 'The Ethics', p.48.
[93] Ibid., p.49.
[94] Ibid., p.50.

black leather bible, used 'praise phrases', exchanged 'sacred greetings' and even opened a prayer meeting when requested to do so.[95] He also participated in the 'breaking of bread', although this did cause him a 'crisis of conscience'. He did not, however, attempt to speak in tongues during an assembly, partly for ethical reasons but also partly because he feared that his performance may have been unconvincing and that his research status would have been discovered. Similarly, Homan did not offer himself for baptism as this seemed to him to be 'inordinately fraudulent'.[96] Unlike Walker and Atherton's work, Homan's covert research into Pentecostalism in Britain provoked quite vociferous debate amongst British sociologists about the ethics of such research. A third ethnographic study of Pentecostalism, written in 1979 by Ken Pryce, also attracted criticism regarding ethics.

Pryce's *Endless Pressure: A Study of West Indian Life-Styles in Bristol*, was based on his doctoral research conducted between 1969 and 1974 under the supervision of sociologist Huw Benyon and Professor of Anthropology Michael Banton.[97] Pryce described his main method of research as participant observation, but the vast majority of his observations were conducted overtly or at least semi-overtly. Using the data he generated, Pryce distinguished six lifestyle types amidst the West Indian population of Bristol which could be grouped into one of two 'major "life-orientations"'. The three lifestyles belonging to the 'expressive-disreputable' life orientation included 'hustlers', 'teenyboppers' and 'in-betweeners'. Those belonging to the 'stable law-abiding' life orientation, on the other hand, had lifestyles categorised by Pryce as 'mainliners', 'proletarian respectables' and 'saints'.[98] It was Pryce's research into the 'saints' lifestyle group and their Pentecostal churches in the St Paul's area of Bristol which required his most sustained attempts at covert participant observation.

Ideally, Pryce would have liked to have been able to move freely between the congregations of all the West Indian churches in St Paul's but, to begin with, he limited himself to attending just two which he visited on alternate Sundays and also on some week nights. However, due to 'practical necessity', Pryce was forced to concentrate his research efforts on just one church. He found that the 'doctrinal and sectional' differences between congregations meant that being known to be a member of more than one would be running the risk of being regarded as a 'spy' by all. Even maintaining membership of two churches, the Calvary and the Church of God, proved too difficult for Pryce. Two members of the

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[95] Ibid., pp.49 & 50.
[96] Ibid., p.53.
[97] Pryce, *Endless*, pp.ix & xi.
[98] Ibid., p.xii.

Church of God sect attended a wedding at the Calvary church and were taking tea with the pastor of the Calvary congregation when Pryce appeared. Much to the silent surprise of the couple from the Church of God, the pastor unwittingly introduced Pryce to them as his most recent convert. Pryce's embarrassment was so acute that he abruptly stopped attending the Church of God and continued to visit only the Calvary church.[99]

Pryce noted that although he had initially been accepted into the Calvary congregation, there was still considerable suspicion surrounding his motivation for joining the church. Pryce gave the partially true response that, despite having a very religious background, his struggle to receive an education had led him away from the church and now that he was in Bristol, he 'just felt like going to church again'. The congregation were, however, unaware of his research agenda.[100] Pryce suggested that he was 'highly conscious of the ethical implications' of his approach and he eventually revealed his status as a university researcher into West Indian social problems to the Calvary pastor. In exchange for concealing Pryce's research interests from the rank and file members of the congregation, the pastor wanted Pryce's baptism and permanent membership of the church.[101] For a while, Pryce managed to resist the pastor's attempts to coerce him into baptism but, after a while, he became aware that his un-baptised and un-saved status was alienating him from the rest of the congregation. Pryce wrote:

> What I was discovering was that to learn more about the church from the standpoint of the ordinary members, I needed to be on the inside as a fully fledged believer. I had no choice therefore but to give in one Sunday morning when I and other unsaved persons like myself were called to the altar and asked if we were ready to be baptized. With hands laid on our heads, we were prayed for and cajoled into accepting baptism, which we all did.[102]

Now that he was himself a 'saint', Pryce was treated by the rest of the Calvary congregation with a new candour and often invited to their homes to meet their children and join in discussion about West Indian life in Bristol.

Although he found it to be the most interesting aspect of his project, Pryce noted that his research into the 'saints' lifestyle was by far the most time-consuming aspect of his fieldwork and he spent twelve months 'singing, praying, eating, feasting, travelling, talking, discussing, visiting and attending services with saints'. Half of this time was spent simply 'trying to stay on good terms' with the 'saints'.[103] He found his covert fieldwork

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[99] Ibid., pp.282-283.
[100] Ibid., p.284.
[101] Ibid., pp.284-285.
[102] Ibid., p.285.
[103] Ibid., p.286.

demanding, requiring 'long, arduous months of repetition and routine'. The day-long services at the Pentecostal churches in St Paul's were so 'wearisome' and 'physically exhausting' for Pryce that he often returned home 'groggy' and 'mentally incapacitated'. He found that taking notes in the field aroused anxiety and suspicion and his attempts at tape recording proceedings failed. In the end, he had to borrow recordings made by other members of the congregation and this extended the period he had to spend in the field.[104]

Despite his lengthy episode of incognito social research, Pryce's covertly gathered material on the 'saints' of St Paul's translated into just one small twenty-page chapter of his book about the West Indians in Bristol.[105] The majority of his discussion of the 'saints' does not make use of his covert ethnographic research findings. On the contrary, most of Pryce's discussion of the 'saints' of Bristol focused on the origins of Pentecostalism and the key features of this sect and its organisation that appear to have been gleamed from other, more prosaic documentary sources. In fact, in the chapter dealing with the 'saints', Pryce only referred to his experience in the field three times. He wrote about his baptism into the faith, a particular sermon he witnessed and he included an example from his 'notebook' of the culture of economic solidarity to which the 'saints' adhered.[106]

The response to Pryce's work was rather lukewarm. One reviewer, Howard Parker, described Pryce's monograph as having had enormous potential to become a seminal work worthy of comparison with respected American ethnographies such as William Foote Whyte's *Street Corner Society* and Elliot Liebow's *Tally's Corner*. However, Parker was ultimately rather disappointed with Pryce's work.[107] He commented that 'Pryce clearly had a difficult time…deciding exactly how to capitalize on his social science training and his blackness vis a vis a study of some aspect of West Indian life in Bristol'. He was especially critical when it came to considering Pryce's covert research into Pentecostal worship amongst West Indians in Bristol. He suggested that Pryce had been 'sucked into the complex world of the city's Pentecostal Churches eventually emerging with very little to show for his efforts'.[108]

Pryce's inadequate use of the method of participant observation essentially lay at the root of Parker's disappointment with *Endless Pressure*. He questioned why Pryce, as a

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[104] Ibid., pp.286-287.
[105] Ibid., pp.198-218.
[106] Ibid*.,* pp.203-205, 208-209 & 217.
[107] Howard Parker, 'Review of *Endless Pressure* by Ken Pryce', *British Journal of Sociology*, Volume 32, Number 1, (1981), p.145. William Foote Whyte, *Street Corner Society: The Social Structure of an Italian Slum*, (Chicago; London: Chicago University Press, 1955); Elliot Liebow, *Tally's Corner: A Study of Negro Streetcorner Men*, (London: Routledge, 1967).
[108] Parker, Review, p.145.

graduate student, was allowed to 'stumble' into his participatory fieldwork and how, despite the fact that that there had only been a 'handful of participant observation studies' carried out in Britain, Pryce had nonetheless managed to ignore their instructions regarding entrée into the field. For Parker, Pryce's work failed because he 'never really moved into his subject's world but merely peered in from the edges'. One of the reasons Parker came to this conclusion was because Pryce 'was not able to be completely honest with his subjects about his research role.'[109] Thus Pryce's covertness was in itself deemed a failure.

Pryce's work has received much more vigorous retrospective criticism at the hands of Peter Ratcliffe who, writing in 2001, argued that Pryce's work had damaged the West Indian community in Bristol. There is no suggestion that Pryce intentionally set out to harm his research subjects. In fact, Pryce was clearly proud of his West Indian identity and was keen to use it to facilitate his research. He felt an affinity with the West Indians in Bristol and remarked that many of the people he met there had become his closest friends. In his acknowledgements, Pryce also expressed the hope that his study did 'not bring harm to anyone in Bristol' and, to this end, he had used pseudonyms throughout the account and changed some minor biographical facts to protect the anonymity of his research subjects.[110] Nevertheless, Ratcliffe suggested that Pryce's claim to have a common identity with the West Indians in the St Paul's area of Bristol was 'distinctly spurious'. Pryce was a middle-class university-educated Jamaican brought up in the Caribbean studying mostly working-class black Bristolians, many of whom had been born in Britain or had arrived as migrants in their childhood.

Ratcliffe suggests that Pryce's status as an outsider came across clearly in the way he failed to challenge white racist 'common-sense' stereotypes of West Indians living in Britain.[111] For Ratcliffe, *Endless Pressure* was built upon 'cultural essentialisms and stereotypes' which associated the West Indian population of Bristol with practices such as drug-taking and pimping.[112] According to Ratcliffe, Pryce's published work was 'read as providing a justification for the use of "hard" policing methods' in the locale and, by angering large sections of the local black population, made it considerably more difficult for future researchers to gain their confidence. This is exactly the scenario that Simey,

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[109] Ibid.
[110] Pryce, *Endless*, pp.ix-xii.
[111] Peter Ratcliffe, 'Sociology, the State and Social Change: Theoretical Considerations', Peter Ratcliffe (ed.), *The Politics of Social Science Research: 'Race', Ethnicity and Social Change*, (Basingstoke: Palgrave, 2001), p.10.
[112] Ratcliffe, 'Sociology' , p.5.

writing in the 1950s, had feared the use of covert methodology might bring about.[113] Referring to Pryce's use of his identity to gain enhanced access to research subjects, Ratcliffe suggested that Pryce 'betrayed both his subjects and his professional colleagues'.[114] In this case, it does not appear to have been the covert nature of Pryce's sociological fieldwork which incurred the wrath of fellow social scientists, but rather his unwitting perpetuation of negative cultural stereotypes and the effect his research had on the researched community.

Empirical Research with Practical Application?

In the previous chapter, it was noted that the covert ethnographic work of Pearl King, Mumford and Spencer appeared to be influenced by the push to make sociological research empirical and research findings applicable to social problems and policy making. In the 1970s, academic sociologists appear to have continued to emphasise the importance of conducting empirical rather than abstract research. For example, in his concluding paragraph, Timperley suggested that his research was an attempt to 'extend knowledge about what goes on in industry' and that he was heeding the call of a fellow sociologist for 'a greater body of fact, of carefully observed studies of what actually goes on'.[115] Similarly, Mars was reassured by the appearance of Ditton's ethnographic work as he hoped 'it might encourage more sociologists to get up off their bottoms; to concentrate less exclusively on their questionnaires and to become personally involved in their field of enquiry as anthropologists do'.[116] In his account of West Indians living in Bristol, Pryce noted his intention to avoid the 'academic abstractions' associated with 'race'-relations theory and instead provide an 'interpretive understanding' of West Indian lifestyles. Rather than focusing on his research subjects as 'mere categories or statistics', Pryce was attempting to view them 'as human beings'.[117]

However, the emphasis on the practical applicability of research results appears to have been lost. In the 1970s, only the work of Archard on alcoholism and vagrancy and, to a lesser extent, Oldman's work on casino gambling could be viewed as focusing on social problems in need of practical solutions. Furthermore, the extent to which Oldman and Archard attempted to offer solutions to the problems of gambling and alcoholic vagrancy

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[113] Thomas Simey, 'The Analysis of Social Problems', *Sociological Review*, Volume 1, Issue 1, (1953), p.81; See above, p.158.
[114] Ratcliffe, 'Sociology', p.11.
[115] Timperley, 'A Study', p.279; A.J.M. Sykes, Book Review, *Sociology*, Volume 2, Number 1, (1968), p.100.
[116] Mars, 'Review', p.136. Although, as already mentioned, Ditton did emphasise the theoretical rather than the practical implications of his research. Nevertheless, it was empirical research. Ditton, *Part-Time*, p.11; See above, p.182.
[117] Pryce, *Endless*, p.xii.

respectively, based on their empirical research, was limited. In his second article based on his ethnographic fieldwork, Oldman tackled 'Compulsive Gamblers', the problematic aspect of gambling. Although his work made reference to the 'therapeutic literature' on problem gambling, his aim was to demonstrate that, whilst the rhetoric of compulsion was present in such literature, it had no basis in reality, according to his covert observations.[118]

Similarly, although Archard's research focused to a certain extent on the institutions of social control purportedly designed to help the homeless alcoholic, and that he himself was part of one such organisation, he did not advance any solutions based on his research. In a very short section of his book entitled 'Skid Row Drinking: A Collective Solution to a Social Problem', Archard suggested that social policy towards homeless alcoholics was based on individualising the problem and finding individualised solutions. Based on his research, he suggested that a more collective solution based on a better understanding of the wider culture of skid row was more appropriate. However, he did not suggest how, in any practical sense, this shift in the conceptualisation of the problem might manifest itself in policy changes.[119]

The Structure of 1970s Covert Academic Ethnography

Careful reading of the covert ethnographic accounts produced by academics in the 1970s reveals that all of the researchers featured in this chapter chose to present their research findings in a thematically structured account. For example, after some initial descriptions of the airport and role of the general hand, Timperley's discussion of his observations was organised thematically.[120] Under sub-headings, he described what he thought were the most salient manifestations of workplace organisation that he observed, such as the development of a system for the distribution of tips received by porters amongst all the general hands.[121] Similarly, Ditton's analysis of his findings in the bakery were presented over five chapters which were structured thematically to cover such matters as the relationship between the fiddling salesman and customers and the motivations for indulging in workplace crime.[122] In this sense, the covert ethnographic accounts produced by academics in the 1970s conformed to a thematic structure with which they were most readily associated. However, in his introductory chapter, Ditton defended his decision to

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[118] Oldman, 'Compulsive', p.368.
[119] Archard, *Vagrancy*, pp.182-184.
[120] Initial descriptive material: Timperley, 'A Study', pp.260-266. Thematic description of observations: Timperley, 'A Study', pp.266-275.
[121] Ibid., pp.266-268.
[122] Ditton, *Part-Time*, pp.16-172.

present his research findings in this thematic and theoretically informed way, describing his work as a 'consciously theorised ethnography'. He rejected the idea that a narrative account would represent a more 'pure ethnography' that was somehow free from theoretical assumptions.[123]

In such thematically organised ethnographies, the ethnographic journey of the fieldwork experience tends to be hidden, buried beneath learned analysis of the observations. However, despite a prevailing tendency towards thematic structure, the fieldwork experience was more prominent in some examples of 1970s covert academic ethnographies than others. For example, in Oldman's account of roulette, he frequently presented anecdotes and specific examples from his experience as evidence to support his argument. At one point, for example, he noted that a very few punters attempted to influence the outcome of the spin: one woman by kissing the chips before she placed them on the board, another by insisting the spinner smile at her before spinning the ball.[124] It is only when Oldman began his 'concluding remarks' that he retreated into a more generalised and detached summary of his findings.[125] Similarly, although the bulk of Walker and Atherton's analysis of the Pentecostal convention was presented thematically, there was a stronger sense of narrative to their account compared to others. For example, at the beginning of their article, they outlined their previous research and the reasoning behind their decision to use an ethnographic methodology and to study the particular event they chose.[126] Walker and Atherton also described their preparation for fieldwork, such as reading Pentecostal magazines and attending services, in order to familiarise themselves with the necessary vocabulary to blend in at the convention.[127] This set the scene for their analysis of material gathered covertly at the convention.

Other 1970s academic ethnographers, however, appeared to struggle to accommodate their ethnographically sourced material in their work. In three places, Pryce included what appeared to be extended passages from his field notes and he presented these in a smaller font than the rest of the text.[128] The way in which Pryce delineated his ethnographic material from the rest of the text suggests that Pryce may have been suffering from the same problem which afflicted the author of *Crime and the Services,* Spencer. As discussed in the previous chapter, Spencer struggled to synthesize his covertly gathered

[123] Ditton, *Part-Time*, p.12.
[124] Oldman, 'Chance', p.417.
[125] Ibid., pp.424-426.
[126] Walker and Atherton, 'An Easter', pp.367-368.
[127] Ibid., p.368.
[128] Pryce, *Endless*, pp.203-205, 208-209 & 217.

material with his other findings.[129] It would appear that, although Pryce spent an extended period of time conducting research covertly, like Spencer, he struggled to incorporate the material generated or gathered in this way into his published text. However, in an appendix at the very end of *Endless Pressure*, Pryce gave some insight into his methodology and fieldwork.[130] This practice of carefully restricting discussion of fieldwork to carefully delineated areas of the ethnographic text was evident in other examples of 1970s academic covert ethnography.

There is little sense of the ethnographic experience in the analytical body of Archard's book about *Vagrancy, Alcoholism and Social Control*. However, in his introductory chapter, he noted that 'too often the substantive product of research work' gets published without the theoretical underpinnings or the 'research techniques' being explained in any detail.[131] Archard's final chapter on 'Research Perspective and Method' was designed to remedy this shortfall. Most of the chapter was devoted to a discussion of his theoretical perspective, symbolic interactionism, but it was in this section that he also discussed his research method and experience in the field candidly and reflexively.[132] In the same way, Ditton carefully contained his discussion of his fieldwork experience in his first chapter, before moving on to analyse his findings thematically. In this chapter, Ditton initially described the bakery he worked in and the sales team.[133] Then, under the sub-heading 'A Research Diary: Getting to Know about the "Fiddle"', he provided a detailed and reflexive account of his research experience before tackling the ethical implications of his research and discussing the theoretical basis of his account.[134] In Homan's article about 'Interpersonal Communication in Pentecostal Meetings', beyond including a brief section on 'Research Method' in which he outlined his use of both overt and covert participant observation over an extended period of time, he made little reference to his fieldwork.[135] As with Mumford's research, as discussed in the previous chapter, most insight into his methodology and fieldwork experience is gained from reading a separate article published by Homan two years after his ethnographic article in response to criticism of the ethical status of his work.[136]

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[129] See above, pp.147 & 157.
[130] Pryce, *Endless*, pp.279-297.
[131] Archard, *Vagrancy*, p.xvii.
[132] Ibid., pp.185-237.
[133] Ditton, *Part-Time*, pp.1-4.
[134] Ibid., pp.4-15.
[135] Homan, 'Interpersonal', pp.500-501.
[136] See above, p.149; Homan, 'The Ethics'.

Researcher and the Researched in 1970s Covert Academic Ethnography

In the previous chapter, we discovered that post-war academic ethnographers tended towards a formal and detached writing style. The extent to which our 1970s academic ethnographers conformed to this expected style varied. Some, such as Homan and Timperley, referred to themselves throughout their respective accounts in a consistently detached way as 'the researcher', 'the researcher as participant observer', 'the researcher as participant' and 'the author'.[137] Other covert ethnographers did refer to themselves in a less detached manner, but only in certain parts of their ethnographic texts. Pryce, for example, referred to himself in the first person frequently in his introduction and appendix on 'Method of Study'.[138] However, in his chapter based on covert ethnographic research, he referred to himself as 'I' only once, and otherwise wrote in a passive and academic tone.[139] Similarly, in the body of his text, Archard wrote in a formal and impersonal tone. However, this detachment was somewhat mitigated by the more personal tone adopted in some of the endnotes in which he discussed his fieldwork experience.[140]

In two of the ethnographic accounts analysed for this chapter, a more informal tone was achieved by the ethnographers referring to themselves in a more personal frame of reference throughout. For example, Oldman frequently referred to himself as 'I' in both of his ethnographic articles, even when it would have been perfectly feasible to write in the passive voice usually adopted in academic texts.[141] Similarly, Walker and Atherton frequently used the first person in their study of Pentecostalism. Furthermore, they occasionally revealed their emotional responses to events in the field. For example, they were initially embarrassed by the 'constant emotional *volte-face* from seriousness to raucous laughter' during the sermons and described themselves as 'puzzled' by the vague and banal interpretations made of the speaking in tongues which occurred at the convention.[142]

All the ethnographers whose work has been analysed for this chapter included references to the direct speech of their research subjects as observed during the course of

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[137] See, for example: Homan, 'Interpersonal', pp.510, 514 & 515; Timperley, 'A Study', p.259.
[138] Pryce, *Endless*, pp.xi-xiii & 279-297. Mars and Ditton also used 'I' almost exclusively in the introductory passages of their ethnographic text before retreating into a more formal and passive tone for the analysis of data in the body of the text: Mars, 'Hotel', pp.200 & 202; Ditton, *Part-Time*, pp.1-15.
[139] Pryce, *Endless*, p.218.
[140] Note, for example, Archard's use of 'I' in endnote number 11 for Chapter 5: Archard, *Vagrancy*, p.253.
[141] For example, Oldman wrote 'These examples illustrate in piecemeal fashion what I consider to be the important questions in studying predictive theorizing in games of chance.' Oldman, 'Chance', p.410; see, for example: Oldman, 'Compulsive', p.360.
[142] Walker and Atherton, 'An Easter', pp.375 & 387 (endnote number 19). Emphasis in original.

their fieldwork. The extent and way in which they made use of such data varied between ethnographers. For example, Homan used direct examples and quotations from the field sparingly, and such quotations were set apart from the body of the text, and presented with no trace of local dialect.[143] In their accounts of Pentecostalism, Walker and Atherton and Pryce also quoted from sermons they listened to covertly and other members of the congregations.[144] Pryce noted that the West Indian 'saints' he encountered spoke in a 'Jamaican folk idiom' but the only reflection of this accent in Pryce's quotes is the use of 'Gawd' rather than 'God'.[145] Timperley referred to his co-workers in consistently passive terms such as 'the men' or 'the general hands'. However, two co-workers were mentioned by name – Tom and Phil – because they assumed prominent roles within the organisational structure of the general hands.[146] Only once in his lengthy article did Timperley quote one of the general hands and he did so without reflecting any regional accent or colloquialisms.[147]

However, Oldman, Ditton and Archard made use of the direct speech of their research subjects in a different way. On no fewer than six occasions, Oldman presented lists of phrases representative of what roulette players in the casino in Aberdeen would typically say in a given situation.[148] Some of these phrases, such as 'He's spinning chocolates tonight', make use of terms specific to the game of roulette whilst others, such as 'Och – he's nae use tonight – he's a wandering willie – all over the wheel', contain approximations of the Aberdonian accent.[149] Ditton's use of quotations was noted by commentators to be a source of humour in his book. In his review of Ditton's work, for example, Mars noted that his use of quotes from men working in the bakery 'enlivened' the account, some of which were 'hilarious'.[150] Another reviewer referred to the rich evidence Ditton had managed to gather from the 'incredibly witty characters' he met in the bakery.[151] Ditton certainly made extensive use of quotations gathered during the ethnographic phase of his research. For example, the issue of off-loading stale bread on to customers was discussed and Ditton used evidence from no fewer than six bakery workers

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[143] See, for example: Homan, 'Interpersonal', pp.505 & 509.

[144] See, for example: Walker and Atherton, 'An Easter', p.370; See above, p.185; Pryce, *Endless*, pp.287, 210-211, 214-215, 215-216 & 216. Pryce also quoted from a letter sent to him from a pastor in the Pentecostal church: Pryce, *Endless*, p.215. Mars also occasionally quoted waiting staff and hotel managers in his account of workplace theft without reflecting any regional dialect: Mars, 'Hotel', pp.202, 204 & 207.

[145] Pryce, *Endless*, pp.209 & 215.

[146] Tom is first mentioned on p.266 and Phil on p.271. Timperley, 'A Study', pp.266 & 271.

[147] Timperley, 'A Study', p.274. He wrote 'the general feeling can best be summed up by the remark of one general hand: "I don't mind cleaning toilets if I'm told I'm a toilet cleaner."'

[148] Oldman, 'Chance', pp.415, 416, 419, 422 & 424.

[149] Ibid., pp.416 & 419.

[150] Mars, 'Review', p.136.

[151] Henry, Review, p.163.

on the topic.[152] When Ditton relayed a customer complaint about their bread being stale to a manager, he was told 'Look, there's no such thing as stale bread at this bakery, so go back and tell them that. There's only fresh bread ... and bread which isn't quite as fresh as it normally is'.[153] Archard frequently quoted the alcoholic vagrants he had encountered in the field, sometimes at considerable length, and the following quotation is fairly representative of those used in Archard's book *Vagrancy*:

> I would never go back to the spike [government reception centre] again because it's full of mental cases...nutters. Then there's the heavy gang [the porters] – they're always fucking you about, telling you what to do. I like my freedom...to do a skipper gives me freedom.[154]

Typically, Archard included swearing but any regional accent the research subjects might have spoken with was not reflected in the quoted speech.

Of course, many of the communities explored by covert ethnographers had a specialised vocabulary to some extent, not just the homeless. Timperley, for example, in his otherwise quite formally written account of working as a general hand in an airport, had to accommodate some of the key vernacular terms used by his research subjects. Timperley described the initial social interaction between the general hands taking 'the form of card playing, visits to the nearby canteen, newspaper reading, backing horses, or simply "brewing up" in the "bothy"'. [155] 'Bothy', according to Timperley's definition provided in a footnote, 'appears to be a common name for rest-room or mess-room'.[156] The next time Timperley used the term 'bothy' in his account he presented it in quotation marks and provided a definition in parenthesis.[157] Subsequently, the term was used without quotation marks and without definition.[158] As mentioned above, in his account of gambling in Aberdeen, Oldman used terminology specific to the game of roulette, some of it specific to roulette played in the local area. More generally, Oldman noted that 'players' were known locally as 'punters' and he adopted this term, using it often in the rest of his account.[159] In his second article, Oldman used the term 'punters' without any explanation of its colloquial status.[160] This gradual adoption of colloquial terms used in the specific research context suggests that ethnographers such as Timperley and Oldman became fully immersed in and fluent with the particular cultures they were exploring covertly. Arguably, it is also a useful

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[152] Ditton, *Part-Time*, pp.56-58.
[153] Ibid., p.56.
[154] Archard, *Vagrancy*, p.54.
[155] Timperley, 'A Study', p.263.
[156] Ibid., p.280.
[157] Ibid., p.264.
[158] For example, Ibid..
[159] Oldman, 'Chance', pp.412 and, for example, pp.413, 414 & 415.
[160] Oldman, 'Compulsive', p.361. In his account of workplace theft in hotels, Mars initially used the terms 'fiddle' and 'punter' in quotation marks and explained their meaning. Towards the end of his account, he used these colloquial terms unselfconsciously: Mars, 'Hotel', pp.202 & 204.

tool for demonstrating the affinity of the researcher with the researched, therefore strengthening the authoritativeness of their ethnographic work.

Covert Academic Ethnography and Journalism in the 1970s

Many of the academics featured in this chapter were careful to distinguish their work from that produced by journalists on the same subject matter, and to locate their accounts firmly in the sociological literature. For example, Archard's account of alcoholic vagrants was very much situated in the sociological literature on deviancy and he noted that 'from a sociological perspective the study of the British skid row scene as a social phenomenon has for the most part remained unexplored. Most accounts of down-and-outs have remained at a purely journalistic level'.[161] Similarly, Mars made a distinction between the 'considerable amount of journalism' on the subject of workplace theft and the few academic, sociological accounts that had been produced on the subject.[162] Occasionally, throughout the course of Timperley's thematic account of the significant episodes of his ethnographic research in an airport, he would relate his findings to the work of other academic sociologists.[163] However, it was in his 'discussion' at the end of the article that Timperley clearly situated his findings in the wider historiography of industrial and organisational sociology.[164] Ditton's work was criticised for 'some dreadful lapses into sociological pretentiousness' which added little, according to the reviewer, to the account of workplace deviancy.[165] Certainly, Ditton's analysis is heavily embedded in an understanding of the sociological literature pertaining to deviance and the workplace. However, Ditton did also make reference to non-academic ethnographic material, such as George Orwell's observations in *Down and Out in Paris and London*.[166]

In the previous chapter, we explored the establishment of an anthropological paradigm in British sociological research which helped to establish ethnographic research as a legitimate, scientific and academic practice.[167] In Archard's work, we find evidence of an academic covert ethnographer working in the 1970s using this paradigm to re-affirm the legitimacy of his research methodology. He was keen to emphasise the academic roots of ethnographic fieldwork in 'anthropological field studies' undertaken at the 'turn of the

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[161] Archard, *Vagrancy*, p.xv.
[162] Mars, 'Hotel', p.200.
[163] For example, Timperley related his findings about work role ambiguity to the work of other industrial sociologists: Timperley, 'A Study', p.274.
[164] Ibid., pp.275-279.
[165] Henry, Review, pp.163-164.
[166] Ditton, *Part-Time*, pp.47, 58 & 69. George Orwell, *Down and Out in Paris and London*, (London: Victor Gollancz, 1933).
[167] See above, pp.152-158.

[twentieth] century' and its status as a rigorous research strategy demanding 'systematic qualitative analytic descriptions and explanations'.[168] Using the academic literature that had accumulated on sociological research methods, Archard was able to locate his work in apparently well-established conventions and rules regarding the use of the method. For example, Archard adopted an ethnographic realist perspective writing that 'participant observation demands of the observer that he immerses himself in the phenomenon under study to the point where any preconceptions are suspended in favour of grasping the perspective, the central meanings, constructed by his subjects'.[169] The disadvantages of a realist perspective have already been discussed in the course of this thesis.[170] What is important here is the way in which the pre-existing sociological literature on ethnographic methodology allowed Archard to present this statement in such a way that it appears to be a well-established rule of sociological ethnographic research.

Incognito Research and Ethics in 1970s Academic Covert Ethnography

In the previous chapter, we explored the way in which King, Mumford and Spencer dealt with the covert nature of their research in their publications. We discovered that, whilst King and Mumford were relatively explicit about the covert nature of their fieldwork and the implications of this, Spencer appeared to be uncomfortable with the covert nature of his fieldwork and attempted to diminish or even mask this aspect of his work.[171] In contrast, it will be shown in the next chapter that some of the non-academic covert ethnographers working in the 1970s were keen to emphasise, and even exaggerate, the covert nature of their research.[172] In our examples of covert academic ethnography published in the 1970s, we find that whilst some sociologists were quite forthcoming about the covert nature of their research, others were relatively reticent. Of course, this may be linked to the extent to which the different research projects were based on covert observations. For some researchers, such as Pryce and Archard, covert observation was just one of many methods they used in the course of their research projects. Nonetheless, the different ways in which they discussed the covert aspect of their work is notable. On the one hand, Homan and Walker and Atherton, for example, made the covert nature of their fieldwork quite clear.[173] On the other hand, Timperley never explicitly mentioned the covert nature of his research and only once, in his opening sentence, did he refer to his core

[168] Archard, *Vagrancy*, p.203.
[169] Ibid., pp.202-203.
[170] See above, p.22.
[171] See above, pp.158-170.
[172] See below, pp.237-240.
[173] Walker and Atherton, 'An Easter', p.368; Homan, 'Interpersonal', pp.500-501.

research method as participant observation.[174] However, it is Archard who appeared to struggle the most with the covert nature of elements of his fieldwork.

In his discussion of research methodology, Archard effectively prohibited the legitimate use of covert participant observation by academic sociologists. He described successfully gaining entrée as an overt observer as one of the 'assumptions' which 'underscore the technique'.[175]

> The student must attempt to persuade those he is studying to accept him...On the whole the observer should avoid wherever possible presenting himself as something he is not; in particular any attempt to pass as a deviant or as someone concerned with their correction should be resisted, especially when the researcher is interested in developing and sustaining a close yet simultaneously distanced relationship with his subjects...He has his role as observer sanctioned by the subjects studied...[176]

Much of Archard's ethnographic fieldwork was conducted overtly in that his key informant in the drinking school was aware of his research agenda and, after a while, the whole of his close-knit group of homeless alcoholics were informed of his real identity. [177] Crucially, however, Archard did undertake aspects of his research covertly and, although he suggested that passing himself off as a homeless alcoholic was occasionally necessary for his own safety, this does not account for the full extent of his covert research.[178]

Although Archard stressed the overt aspects of his participatory fieldwork, he quietly acknowledged the benefits of covert observation. Archard noted that some of the methodological problems he encountered in the field revealed how the researcher needed to 'modify his approach' and sometimes shift his role in the field.[179] For example, Archard admitted to a passively covert role when he pointed out in an endnote that being described as 'a good man' by alcoholics who were aware of his identity as a social researcher allowed him to 'avoid explaining my research purpose to everyone whom I came into contact with on the row'.[180] However, Archard also entered into situations in the field in which he actively colluded in other people's perception of him as a homeless alcoholic. In his research into the institutions of social control, Archard noted that he did not attempt to pass himself off as an alcoholic vagrant. However, he revealed in an endnote that

> There were two exceptions to this general rule. On the occasion when I was arrested for public drunkenness with a number of alcoholics I did not divulge my research identity to the police authorities. Consequently I was processed through the police station and magistrate's court.

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[174] Timperley, 'A Study', p.259.
[175] Archard, *Vagrancy*, p.205.
[176] Ibid., pp.205-206.
[177] Archard, *Vagrancy*, pp.208-209.
[178] Ibid., p.224.
[179] Ibid., p. 210.
[180] Ibid., p.267.

> Undergoing the process confirmed much of what I had been told by alcoholics about the police and lower courts. It also served the purpose of raising my credibility as a researcher among alcoholics who knew of my research role. The second occasion consisted of presenting myself at an out-patients [sic] psychiatric unit as an [sic] homeless alcoholic. The central criticism of medically staffed psychiatric units was also confirmed, namely that when it comes to homeless alcoholics psychiatrists are unable to offer any substantial assistance, merely referring patients to the government Reception Centre...[181]

It is perhaps understandable that Archard, when confronted with the prospect of being arrested along with his drinking school companions, chose to allow the police to believe he was a natural member of the group. Had Archard revealed his identity as a sociologist conducting research in an attempt to avoid arrest, this could have elicited a negative response from his research subjects and ultimately result in the premature termination of that phase of his research. However, when he chose to visit a psychiatric unit and present himself as a homeless alcoholic, Archard appeared to actively adopt a covert role when there was no compulsion to do so.[182] The impression gained from Archard's attitude towards the covert elements of his research is that it was a useful aspect of his fieldwork but not one that he wanted to emphasise. Archard's reluctance to draw attention to the covert aspects of his methodology may have been related to the continuing ethical debate surrounding such clandestine research.

The majority of British academic covert ethnographers who published their work in the 1970s received little or no criticism of their research ethics that can be found in print. Timperley's research into work groups in a new airport, Oldman's work on casino gambling, Archard's study of alcoholic vagrants and Walker and Atherton's account of one day spent at a Pentecostal convention all appear to have escaped published ethical criticism. In his account of occupational theft in the hotel industry, Mars did not reflect on his own ethical research standards. However, he did suggest that the reason there were so few sociological accounts of occupational theft could be linked to the reluctance of social researchers to disclose information which they may have received in confidence on ethical grounds.[183] Pryce's use of covert methods to infiltrate Pentecostal congregations in Bristol was criticised in passing by Martin Bulmer in his appraisal of Homan's research ethics, which we will discuss shortly. Bulmer described Pryce's insincere membership of the

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[181] Ibid., pp.269-270.

[182] There was another occasion when Archard adopted a covert role when it was not strictly necessary to do so. When exploring an unfamiliar area of 'skid row', Archard was approached by the police and he told them he was homeless. He only admitted his researcher status when the police uncovered evidence to the contrary upon searching him: See endnote number 11 for Chapter 5. Archard, *Vagrancy*, p.253.

[183] Mars, 'Hotel', pp.200-201.

church congregation as representing a 'betrayal of trust' by basing personal relationships upon falsehood.[184]

Although Ditton's covert research of pilferage in a commercial bakery does not appear to have attracted much, if any, outside criticism on the grounds of research ethics, the ethics of covertness were something which Ditton himself addressed in his work. In his preface, Ditton acknowledged that having revealed many of his colleague's tricks in detail, he did not 'expect that many of the men at Wellbread's will look too kindly on the cut in real wages that this work may mean to them' and that his 'bakery self would agree with them'.[185] Ditton accepted that his research was covert in that, although he had been partially open about his research agenda, he had never fully declared his interest in workplace crime. Nevertheless, Ditton suggested that he had 'nothing very significant to add to the ever expanding sociological literature on the experiences, morals, ethics and practices of the participant observer'.[186] Despite his protests that he had nothing much to add to the ethical debate, however, Ditton went on to defend his use of covert research methods.

Participant observation, according to Ditton, was 'inevitably unethical by virtue of being interactionally deceitful' and that it did not become ethical merely because this deceit was 'openly practised'. Like Barnes, Ditton invoked Goffman to argue that, in a way, all observation broke the hidden rules structuring interaction, as the researcher was 'conspicuously concerned to an improper degree with the way that the interaction, qua interaction, is proceeding, instead of becoming spontaneously involved in the official topic of conversation'. Just as Berreman had argued that deception was a part of everyday social life, Ditton suggested that observation was not an activity restricted to sociologists and sociological research was 'more of an exaggeration of conventional social activities than something separately constructed and separately justified'.[187]

As a researcher into deviant behaviour, Ditton declared that the 'end serves as the justification' as far as covertness was concerned because, as Polsky had suggested, how else could crime be studied in its natural surroundings? Put another way, Ditton suggested that '[w]ithout reliance on some form of subterfuge the practices of subterfuge will not be open to analysis'. Ditton also attempted to justify his covertness by suggesting that,

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[184] Martin Bulmer, 'Comment on "The Ethics of Covert Methods"', *British Journal of Sociology*, (1980), Volume 31, Number 1, pp.60-61.
[185] Ditton, *Part-Time*, Preface.
[186] Ibid., p.9.
[187] Ditton, *Part-Time*, p.10. See above, p.175.

because of the researcher's honesty, another investigation of fiddling in a bakery failed to report any significant findings. He also resorted to the justification that he was not doing anything that had not already been done by other researchers. Furthermore, he claimed his 'ethical offence' was mitigated because he had sought to protect the identity of the bakery and the individual workers.[188]

Despite his defence of his methodology, Ditton appeared to receive little, if any, criticism of his covertness in print following the publication of *Part-Time Crime*. Homan, on the other hand, was censured robustly for using covert methods to study Pentecostalism in Britain in the later 1970s. In 1978, the publication of Homan's article 'Interpersonal Communication in Pentecostal Meetings' in the *Sociological Review* prompted Bob Dingwall from the Centre for Socio-Legal Studies at Wolfson College, Oxford, to write a letter of complaint to the then editor of the journal, Ronald Frankenberg. Extracts of the correspondence between Dingwall and Frankenberg were reproduced in *Network*, the newsletter of the BSA. In his letter to Frankenberg, Dingwall wrote of his concern that such a reputable journal as the *Sociological Review* would be 'prepared to associate itself with the ethically dubious practice of covert fieldwork' which Dingwall had hoped, as a result of the critiques of Roth and Davis which were discussed above, had been rendered 'professionally unacceptable'.[189]

Although Dingwall suggested that such covert methods may be justifiable in a very small range of extreme situations, Homan's research did not meet this criterion. Dingwall was disturbed that the journal's referees had not insisted that Homan include an analysis of the ethical issues at stake and that the *Sociological Review* had participated at all in the dissemination of the products of 'unethical work'. Frankenberg responded, noting that he agreed strongly with Dingwall's perspective and had hesitated greatly before publishing Homan's paper. It was also revealed in this edition of *Network* that the decision to publish Homan's work was retrospectively discussed at a full meeting of the *Sociological Review*'s editorial board but they decided against laying down any general rules prohibiting the publication of such articles.[190]

Following the publication of the findings of his covert research into Pentecostalism in Britain and Dingwall's pointed criticism of his work and the decision to publish it,

[188] Ditton, *Part-Time*, pp.9-10. Ned Polsky, *Hustlers, Beats and Others*, (Harmondsworth: Penguin, 1967).
[189] Bulmer, 'Comment', pp.60-61.
[190] Dingwall and Frankenberg, Correspondence, 'Covert Observation', p.7.

Homan wrote an article reflecting on his experience as a covert observer. Notably, this article appeared in a different journal, the *British Journal of Sociology,* and Homan began by writing:

> This paper is a confession: it is a critical reflection on the use by its writer of the method of covert participant observation, of which covert interviewing is regarded to be a part. I used such a method with commitment over a period of about eighteen months but now wish to express serious reservations on its adoption.[191]

Far from capitulating to the disapproval of Dingwall and Frankenberg, however, Homan asserted in this article that his use of covert methods had not significantly harmed his research subjects or the standing of sociology as a profession. He wished to express serious reservations about the use of such methods as a result of the effect covertness had had upon *himself*.[192] During his covert research, Homan took the line that in conversation with Pentecostal believers, it was acceptable to lead them to believe he shared their world view because, not only was non-reactivity of the observed desirable, but also that 'the truth often hurts'. Homan felt that it was 'spiritually' kinder to his research subjects to lie about his lack of genuine faith in their religion.[193] Afterwards, however, Homan was troubled and felt guilty that he had been dishonest in concealing his 'ulterior motive' from old-time Pentecostal believers who had invited him to tea and showered him with Christmas cards. It was this lingering sense of guilt combined with the effect such surreptitiousness appeared to have had on Homan's own personality which led him to dissuade others from undertaking covert research. The method had an effect on him as a person and he noted that formerly close friends remarked that he had become more analytical. Undertaking covert research, according Homan, undermined the researcher's subsequent development of open and honest relationships.[194]

Nonetheless, Homan did address the wider ethical debate surrounding the use of covert observation as a method of social research. He recognised the fact that there was a 'substantial body of opinion' within the British profession of sociology which would have disapproved of his method and, in particular, that it ran contrary to the principles laid out by the BSA. Homan respected Edward Shils' discussion of the ethical issues surrounding participant observation 'as the most considered and scholarly analysis to date', but he maintained that his methods were defensible.[195] Homan seemed to find comfort in the fact

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[191] Homan, 'The Ethics', p.46.
[192] Ibid., p.57.
[193] Ibid., p.54.
[194] Homan, 'The Ethics', pp.54-55. Mumford noted the negative effect her use of covert research techniques had had on her relationships and attitude in 'real life'. Enid Mumford, 'Participant', p.160; See above, p.151.
[195] Ibid., p.52; Edward Shils, 'Social Inquiry and the Autonomy of the Individual', in Daniel Lerner (ed.), *The Human Meaning of Social Science*, (New York: Meridian Books, 1959), pp.114-157.

that, according to him, other covert sociologists had used methods 'even more clandestine and dishonest' than his own. Here, he referred to a string of mostly American examples, including some of those discussed above and other notorious examples, such as Laud Humphreys' covert observation of cottaging.[196] Homan argued that, in a way, covert research was more 'considerate and sensitive' than overt observation of religious practice as the knowledge that they were being observed may cause the character and quality of the rituals to be affected and, ultimately, it would become a 'less sincere' activity for the observed.[197]

Significantly, Homan associated the disapproval of covert research methods with what he considered to be a perennial objection to disguises running through the literature concerning research ethics.[198] However, the work of the historian Angus McLaren suggests that the significance of the notion of disguise has changed over time. He suggests that notions of identity and disguise fascinated late nineteenth and early twentieth-century English society, and were not necessarily automatically condemned. He argued that a 'society made anxious by shifting class, gender, and racial relationships was naturally preoccupied by dress and role playing, by visual codes and clues'. McLaren suggested that disguises, including those adopted by middle- and upper-class 'slummers' around the turn of the twentieth century, such as Jack London and Beatrice Webb, had the ability to empower, frighten and amuse.[199] The alleged shift from a societal ambivalence towards disguises described by McLaren at the turn of the twentieth century, to an outright objection noted by Homan in the late twentieth century, may help contribute towards an explanation of the declining use of covert research methods in Britain between these two eras.

Homan's reflection upon his use of covert research methods elicited a response from Martin Bulmer, who remarked that the debate surrounding the ethics of covert participant observation had gone on 'for at least a quarter of a century'.[200] Bulmer described Homan's recognition of the effect of covert methods on the researcher as one of the most refreshing features of his account, but insisted that although this display of self-awareness was 'a good defence against cynicism', it was 'not a rebuttal of the criticism' Bulmer was levelling at him. He found Homan's defence of such methods of 'doubtful

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[196] Ibid., pp.50-51; Laud Humphreys, *Tearoom Trade: A Study of Homosexual Encounters in Public Places*, (London: Duckworth, 1970).
[197] Homan, 'The Ethics', p.53.
[198] Ibid., p.52.
[199] Angus McLaren, 'Smoke and Mirrors: Willy Clarkson and the Role of Disguises in Inter-War England', *Journal of Social History*, Volume 40, Number 3, (2007), pp.597-598.
[200] Bulmer, 'Comment', p.59.

validity'.[201] In this article, Bulmer reiterated one of the criticisms that had been made of covert research from the 1950s onwards; that the use of covert methods was likely to make future research in that area difficult as the subjects of such research would react adversely if or when they found out. Bulmer expanded this point to suggest that if sociologists were to adopt covert methods on a large scale, all sociological research would become more difficult, as this would reinforce the image of sociologists as 'sly tricksters' which already existed in some social circles.[202] He suggested that 'a world too full of pseudo-converts, pseudo-patients, pseudo-students, pseudo-party-members and others playing pseudo-roles' would 'not promote a healthy climate for social science'.[203] Just as Dingwall's letter to Frankenberg had echoed the ethical recommendations set out by the BSA in the 1970s, Homan was not suggesting that covert observation should never be used as a method of research but that the use of it should be 'highly exceptional' and that the decision to use 'deception' required careful justification.[204]

Sociology in Post-War Britain: Public Reputation and Self-Image

At the beginning of the previous chapter, we noted that in 1946 Thomas Marshall regarded sociology as having a poor reputation and that it was in fact regarded by some 'with a certain amount of suspicion'.[205] This vaguely negative public persona of the sociologist was given more weight by Homan when he suggested that although academia in general was dismissed as iconoclastic by the Pentecostal movement, it was the sociologist in particular that was the real 'bogy-man [sic]' and his discipline was associated with Communism and godlessness.[206] In the 1950s, Simey suggested that one way in which sociology could preserve its integrity was to openly engage with research subjects and empower the researched community to overcome their social problems.[207] In some ways, the practice of covert research was the antithesis to this approach and it could be suggested that the use of covert methods brought the whole discipline of sociology into disrepute.

In fact, this is exactly the criticism which was levelled at post-war covert ethnographers by other professional sociologists.[208] In the case of Pryce, it was suggested

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[201] Ibid., pp. 59 & 62.
[202] Ibid., p.63.
[203] Ibid., p.61.
[204] Bulmer, 'Comment', p.64. See above, pp.206-207.
[205] Marshall, *Sociology*, p.4. See above, p.135.
[206] Homan, 'The Ethics', p.48. See above, p.189.
[207] Simey, 'The Analysis', p.85.
[208] Bulmer, 'Comment', p.63.

that the publication of his work on the West Indian community in Bristol attracted negative press coverage, angering large sections of the local black population thus making it less likely that they would co-operate with social researchers in the future. By bringing sociology into disrepute in this way, one of Pryce's harshest critics, Ratcliffe, suggested that Pryce had betrayed his 'professional colleagues'.[209] In a more detached analysis, Barnes noted the historical dimension to the use of secrecy by social scientists suggesting that, for example, in the community studies of the 1950s, the social scientist could have revealed inessential aims and subsidiary findings to research subjects to give an impression of openness and honesty without revealing the core research aims, as this might jeopardise the results. Neither this technique, nor using vagueness and flattery to sidestep gatekeepers, would work in the context of the 1970s, according to Barnes. At this time in history, Barnes suggested, citizens were generally too familiar with social science to be fooled by such approaches. In 1979, Barnes was able to write that social inquiry had become a 'recognised recurrent activity'.[210]

Of course, as is clear from the discussion above, professional sociologists in Britain were aware, to some extent, of the public reputation of their discipline and how covert research impinged upon it, but we must also consider the self-image of sociology during this era. In previous chapters, we have witnessed the professionalisation of sociology as an academic discipline in Britain and it is worth pointing out that the BSA's ethical statement of 1970 included a re-affirmation of the professional status of sociologists, suggesting 'that only persons properly trained or skilled should undertake social research'.[211] In the early 1970s Douglas, a sociologist of deviance, argued that sociologists in general, in line with other professionals such as doctors and lawyers, had to 'carve out a special *moral niche*, or to construct a *situational morality*, for their research activities'.[212] When 'the research need was clear', Douglas argued, there were grounds to break social rules such as those on privacy. Similarly, Douglas suggested that the legal protection given to lawyers and doctors against being prosecuted as accessories to crimes should, and probably would, be extended to sociologists working in the field of deviance.[213]

In contrast to Douglas' assertions, Homan recognised that 'while the detective is fortified by social sanctions in many of his activities, the sociologist does not draw the

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[209] Ratcliffe, 'Sociology', p.11.
[210] Barnes, *Who Should Know What*, pp.110-111.
[211] B.S.A., 'Correspondence', p.114.
[212] Douglas, 'Observing', pp.8-9. Emphasis in original.
[213] Ibid. p.9.

same support from the values he espouses'.[214] Homan noted that, even within the strict guidelines set out by the BSA, there remained the possibility that the ends justified the means in that covert methods were allowed if it was not possible to use other methods to obtain essential data. This could be interpreted, however, as 'an arrogant claim about the significance of research findings'.[215] Covert researchers in particular had to be content that their research aims were significant enough and that their data was essential enough that they were justified in breaking social rules to pursue their research.

Furthermore, there is another way in which the use of covert research methods can be seen as an indicator of the apparent arrogance or superiority of the covert researcher. As Erikson has pointed out, covert research by impersonation is attempted only by scientists looking down the social ladder rather than up, masquerading as workers but not as managers, as privates rather than generals. In this sense, covert research conformed to a nineteenth-century model in which the elite scientist gathered data in the slums rather than on a model based on negotiation with citizens.[216] Useful parallels can be drawn with the world of literature and with historical examples of anthropological fieldwork amongst non-European people. As McLaren has noted, the masquerading depicted in the work of Rider Haggard and Rudyard Kipling, amongst others, was also used

> by whites to penetrate non-European cultures. Here again the complacent English reader assumed that the white man could successfully pass as native, whereas the native's attempts to cross the racial barrier were always doomed to fail.[217]

There are significant lessons to be learned here about the power dynamic implicit between the professional researcher and the researched in covert ethnographic fieldwork.

Conclusion

With a couple of notable exceptions, all of the 1970s academic covert ethnographers discussed in this chapter produced confident and authoritative ethnographies which can be clearly located in the anthropological paradigm of sociological research as established in the post-war era. All eight covert ethnographies were, for example, structured thematically, in keeping with the professional model of ethnography. Other stylistic features, such as the use of the first person and the inclusion of quotations and dialect were shown, in most cases, to have been carefully managed by the ethnographers in

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[214] Homan, 'The Ethics', p.55.
[215] Ibid., p.57.
[216] Barnes, *Who Should Know What*, p.123.
[217] McLaren, 'Smoke and Mirrors', p.597.

such a way as to maintain the impression of academic professionalism. Pryce, however, like Spencer in the 1950s, and even Edward Wight Bakke in the 1930s, struggled to accommodate his covert ethnographic material in his account of West Indians in Bristol, even though the anthropological paradigm had been well established by the time Pryce produced his account. Archard's account of vagrancy and alcoholism did conform to the established model of sociological ethnography. However, like Spencer before him, he seemed uncomfortable with the undercover element of his research. Archard's reticence regarding his covertness should be considered in the context of the growing controversy surrounding the use of covert methods by professional sociologists. As the response to Homan's covert ethnography of Pentecostal believers demonstrated, there was concern that not only was covert participant observation unethical, but that it would undermine the professional reputation of academic sociology. Although the establishment of the anthropological paradigm in sociology legitimised the use of ethnographic method to study British society, the concern surrounding the ethics of covertness meant that academic covert ethnographers operated on the margins of their academic discipline.

Chapter 5: Non-Academic Covert Ethnography from 1946 to 1980

Introduction

In the previous two chapters, we have discussed the covert ethnography produced by British academics between 1946 and 1980. In this chapter, we will discuss the much more modest amount of non-academic covert ethnography to emerge during this same era. Surprisingly, compared to the number of academic covert ethnography produced between the end of the Second World War and 1980, especially in the 1970s, relatively few examples of post-war non-academic covert ethnography have been located. We will focus on four of these which were all published in the early 1970s.[1] The first, Polly Toynbee's *A Working Life*, appeared in 1971 and detailed episodes of covert research she undertook in various workplaces.[2] Two of the four texts focused on the experience of homelessness in Britain: Jeremy Sandford's *Down and Out in Britain* first published in 1971 and Robin Page's *Down among the Dossers* which appeared in 1973.[3] The fourth example to be considered is James Patrick's *A Glasgow Gang Observed*, which, as the title suggests, made use of ethnography to explore Glasgow's gang culture.[4]

We will first of all discuss why so little non-academic ethnography emerged in Britain between the end of World War II and the beginning of the 1970s. Subsequently, we will look in a little more depth at the context and content of the key primary sources for this chapter; the work of Toynbee, Sandford, Page and Patrick. As in previous chapters, some time will be spent picking apart the structure and other features of these late-twentieth-century examples of non-academic covert ethnography, comparing and contrasting them with each other and with other earlier examples. Notably, Patrick's ethnographic project stands out as an example which arguably straddled the boundary between academic and non-academic research and this is an issue which will be explored in more depth. In the previous two chapters, we have discovered that the ethics of research

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[1] It should be noted that James Patrick undertook his fieldwork in 1966 and 1967 although he did not publish his account until 1973. James Patrick, *A Glasgow Gang Observed*, (London: Eyre Methuen, 1973), p.9.

[2] Polly Toynbee, *A Working Life*, (Harmondsworth: Penguin, 1973). First published in 1971 by Hodder and Stoughton.

[3] Jeremy Sandford, *Down and Out in Britain*, (London: New English Library, 1972). First published in 1971; Robin Page, *Down among the Dossers*, (London: Davis-Poynter, 1973).

[4] Patrick, *A Glasgow Gang*. Philip O'Connor's 1963 publication, *Britain in the Sixties: Vagrancy*, also drew upon some covert ethnographic material to explore homelessness. However, O'Connor had personal experience of being homeless during an earlier period of his life and his account was an attempt to delineate the 'ethos of vagrancy' and to understand its 'ideological and ethical pattern' rather than an account of the experience of vagrancy. For this reason, O'Connor's text will not be considered a key source in this chapter. Philip O'Connor, *Britain in the Sixties: Vagrancy*, (Harmondsworth: Penguin, 1963).

became an increasingly prominent issue amongst academic researchers and in this Chapter we will explore the extent to which ethical concerns shaped the work of 1970s non-academic covert ethnography.

An Absence of Non-Academic Covert Ethnography from 1946 to 1970

It is difficult to explain why there was such an apparent dearth of non-academic covert research carried out between the end of the Second World War and 1980. It is particularly striking that no non-academic covert ethnography appears to have been published in Britain for the two-and-a-half decades between 1946 and 1970. Before the work of Toynbee, Sandford, Page and Patrick emerged in the first half of the 1970s, the last non-academic covert ethnographers to be discussed were Hugh Massingham, Celia Fremlin, Marie Jahoda and Edward Bakke. The economic upheaval of the 1930s, mass unemployment and the sense of social unrest provided ready subject matter, such as poverty and the experience of work, for Massingham and Bakke, as well as the academic researchers of the day. The special circumstances of the war provided material which early 1940s covert researchers could fruitfully explore, such as Jahoda and Mass-Observer Fremlin's undercover accounts of war work. Throughout the 1950s and the early and mid 1960s, Britain enjoyed relative economic stability and, there were arguably few pressing issues for social researchers to tackle using covert methods. It was not until the receding optimism about the state of society in the late 1960s and the increasing tensions of the early 1970s that we see covert research re-emerge as a useful tool for the social researcher. The mid 1960s witnessed the 'rediscovery of poverty'.[5] Julie Rugg suggests that, as part of this 'rediscovery', poverty and unemployment became increasingly pressing social problems throughout the 1970s and into the 1980s.[6]

In fact, if we look at the rationale presented by the four covert ethnographers featured in this chapter for undertaking their research, we can see how important the social and economic context was in shaping their research agenda. Patrick was the first of the four covert ethnographers to embark on his fieldwork and his decision to conduct covert research into gang culture was based on two main factors. First of all, his job at an approved school allowed Patrick to come into contact with gang members, including Tim,

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[5] See, for example: Brian Abel-Smith, *The Poor and the Poorest: A New Analysis of the Ministry of Labour's 'Family Expenditure Surveys' of 1953-54 and 1960*, (London: Bell, 1965); Peter Townsend, *Poverty, Socialism and Labour in Power*, (Fabian Society, 1967); Peter Townsend, *Poverty in the United Kingdom: A Survey of Household Resources and Standards of Living*, (London: Allen Lane, 1979).
[6] Julie Rugg, 'Poverty and social exclusion', in Francesca Carnevale and Julie-Marie Strange (eds.), *Twentieth Century Britain: Economic , Cultural and Social Change*, 2nd Edition, (Harlow: Pearson-Longman, 2007), pp.310-314.

his key informant, and therefore the opportunity to conduct such research was open to him.[7] Secondly, Patrick explained that although there had been press coverage and public concern about gang violence in Glasgow throughout the 1950s and early 1960s, it was in 1965 that 'the trickle of news items on gangs began to swell into a flood' and, by May 1966, when Patrick began his undercover research, Glasgow's association with gang violence was making the national news.[8]

In *A Working Life*, Toynbee did not go into any depth about her rationale for researching the experience of work in Britain in the early 1970s. Providing context for her research, she presented a list of statistics concerning the working population including working hours and average pay but, beyond this, she did not explain why she had chosen to research the world of work.[9] In a recent interview with *Society Now* magazine, it is noted that just prior to undertaking her covert research, Toynbee had been working for the *Observer* newspaper reporting on 'big strikes' and it was suggested that she write a book on a related topic. Her covert research fed into her future interest in labour relations.[10] So, on a personal level, the world of manual work was something that concerned Toynbee. On a wider level, British society in the early 1970s was concerned with industrial disputes and, therefore, the experience of work which gave rise to these disputes. Like Patrick, Toynbee was able to apply covert research methods to an issue which was deemed to be a current social problem.

Both Sandford and Page used covert methods to study homelessness and the British state in the early 1970s and, again, it can be argued that this was an emergent social issue at the time. In the 1970s, poverty and unemployment became increasingly prominent social problems and Rugg has suggested that this was a time during which the meaning of poverty was contested. Post-war, the welfare reforms had accepted statutory responsibility for the poor and their welfare. However, in the 1970s, the unemployed and the poor were increasingly said to be responsible for their own situation and the image of the 'benefit scrounger' became a regular feature in the popular press.[11] Essentially, Sandford and Page's covert accounts of homelessness were comments made from opposing sides of this contemporary social debate. By and large, Sandford concluded that the poor, homeless and vulnerable in Britain were not responsible for their situation and that the state had failed to protect and support them as it should. Page, however, reached the opposite conclusion

[7] Patrick, *A Glasgow Gang*, p.13.
[8] Ibid., p.20.
[9] Toynbee, *A Working Life*, pp.9-10.
[10] Sophie Goodchild, 'The Social Vocalist', Interview with Polly Toynbee, *Society Now,* Issue 4, 2009, p.23.
[11] Rugg, 'Poverty', pp.312 & 314.

finding the vast majority of homeless people he met were capable of fending for themselves, and that the state system of benefits was generous and open to abuse.

Thus, the little covert ethnography which did emerge between the end of the Second World War and the beginning of the 1980s was associated with some of the major social and political issues of the time: gang violence in Glasgow, the experience of work and industrial discontent as well as poverty and the state. These were the issues with which wider society, including our covert ethnographers, was concerned and to which covert research methods could be fruitfully applied.

Non-Academic Covert Ethnography in Early the 1970s

Polly Toynbee and some 'Stupid Boring' Jobs[12]

Polly Toynbee, daughter of writer and newspaper editor Philip Toynbee, and great niece of prominent economic historian and philanthropist Arnold Toynbee, has written two books based on covert research into low-paid, unskilled labour. Her first account, *A Working Life*, was published in 1971 and her second, *Hard Work: Life in Low-Pay Britain*, was published in 2003. Given her intervening high-profile career, it is unsurprising that *Hard Work* has received so much more attention than *A Working Life*.[13] Described as 'the queen of the leftist journalists' with an 'immodest air of authority' which has 'often infuriated the right', Toynbee has been a notable columnist for the *Observer,* the *Guardian* and the *Independent* as well as social affairs editor for the BBC between 1988 and 1995 and is currently President of the British Humanist Association in addition to being a member of numerous public bodies.[14] Toynbee has won various accolades for her journalism including the Orwell Prize in 1998 and the British Press Awards' 'Columnist of the Year' in 2007. As a vocally disgruntled supporter of Labour's rule, Toynbee continues to be subjected to scornful and vitriolic attacks at the hands of the right-wing press, accused of hypocrisy for championing the poor whilst indulging in the private education of her children and owning property in Italy. They credit her with immense power within

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[12] Toynbee, *A Working Life*, p.38. Whilst employed in the cake factory, Toynbee discovered that 'If the work is of minimal interest, so the thoughts and preoccupations of the mind will match it exactly. A stupid boring job makes a stupid boring mind.' Margaret Lassell also used this quote to headline her review of Toynbee's account in the *Times*. Margaret Lassell, 'A stupid boring job makes a stupid boring mind' A Working Life by Polly Toynbee, Review, the *Times*, July 26th 1971, p.8.

[13] Toynbee's *A Working Life* was reviewed positively by Lassell, a social scientist who had used overt participant observation in her own research in the early 1960s: Lassell, 'A stupid boring job', p.8. Lassell's research using overt participant observation: Margaret Lassell, *Wellington Road*, (London: Routledge, 1962).

[14] Andy McSmith, 'Polly Toynbee: Re-born as a Lady of the Right', *The Independent*, 26th November 2006, http://www.independent.co.uk/news/people/profiles/polly-toynbee-reborn-as-a-lady-of-the-right-425833.html , [accessed 20th Dec 2009].

Britain's liberal intelligentsia and some even fear 'the creeping Pollyfication of the world at large.'[15]

In *Hard Work*, Toynbee used the metaphor of a desert caravan to describe British society, with the rich at the head of the caravan and the poor at the end, stretching further and further apart to the point where they could no longer be said to be travelling together at all.[16] This image was recently borrowed and used in a speech by the Conservative leader David Cameron. This apparent attempt to make Toynbee the new poster-girl for the centre-right in British politics provoked a strong reaction from Boris Johnson, the Conservative mayor of London, who described Toynbee as the 'fairy godmother' of New Labour who

> incarnates all the nannying, high-taxing, high-spending, schoolmarminess of Blair's Britain. She is the defender and friend of... every gay and lesbian outreach worker, every clipboard-toter and pen-pusher and form-filler whose function has been generated by mindless regulation. Polly is the high priestess of our paranoid, mollycoddled, risk-averse, airbagged, booster-seated culture of political correctness.[17]

Toynbee's first ethnographic account of labour published in 1971, *A Working Life*, did not attract as much politically motivated attention but it was nevertheless an interesting example of 'an old journalistic standard'.[18]

Having dropped out of Oxford after winning a scholarship to read history, Toynbee took on a couple of menial jobs whilst she wrote and had her first novel published.[19] Soon after, she joined the *Observer* and was encouraged to take on more work roles, some covertly, in the spirit of investigative journalism, and develop her experiences into *A Working Life*. In retrospect, Toynbee described this project as 'a personal exploration of a world of manual work I knew nothing about' where she 'had travelled the country taking jobs as they came, describing the lives of people, many just getting by, with hardship lurking around the corner.'[20] *A Working Life* was based on observational research conducted by Toynbee but not all of it was participatory or covert. For example, the chapters about 'Youth', 'Labour' and 'Old Age' were based on overt observations made with the co-operation of the Department of Employment in some instances. Similarly, Toynbee's chapters on 'Coal' and 'Steel' were based on overt observations made on visits arranged by the National Coal Board and the Steel Corporation.

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[15] William Langley, 'Profile: Polly Toynbee', *The Telegraph,* 26th November 2006, http://www.telegraph.co.uk/comment/personal-view/3634626/Profile-Polly-Toynbee.html , [accessed 27th December 2009].
[16] Polly Toynbee, *Hard Work: Life in Low-Pay Britain*, (London: Bloomsbury, 2003), pp.3-4.
[17] McSmith, 'Polly Toynbee'.
[18] JoAnn Wypijewski, 'Winners and Losers', A Review of *Hard Work* by Polly Toynbee, *The Guardian*, 15th February 2003, http://www.guardian.co.uk/books/2003/feb/15/highereducation.news , [accessed 20th December 2009].
[19] Polly Toynbee, *Leftovers*, (London: Weidenfeld & Nicolson, 1966).
[20] Toynbee, *Hard Work*, p.3.

The chapters which we are most interested in here are 'Cakes', 'Maternity Ward', 'Car Parts', 'Army' and 'Soap' as these detailed episodes of Toynbee's research which were either completely or almost completely covert. In her 'Note' at the end of her monograph, Toynbee wrote that some of the jobs she did 'needed the co-operation of the management as they were not the sort of places that employed casual labour.'[21] Lever Brothers and Joseph Lucas gave Toynbee their full co-operation and, to the best of her knowledge, 'only the top management and personnel officers' were aware of her researcher status.[22]

In 'Cakes', Toynbee discussed her time spent working incognito in a large cake factory. Her first job was to slip a cardboard disc under each cake as it passed by on an assembly line. Despite initially having to work at a frenzied pace to catch up with missed cakes, Toynbee got to grips with this job quickly only to be moved to the 'nastiest job' in the department. Here, cakes exited the machine sliced in three horizontally and the woman ahead of Toynbee on the line controlled the flow of cream onto the bottom and middle layers of the cakes and placed the top layer onto the middle one. Toynbee's job was to stack these layers on to the bottom layer and straighten the whole cake, not easy when the 'cakes are heavy and the cream is slippery'.[23] Nothing short of psychological warfare developed between Toynbee and the worker ahead of her on the line. Toynbee accused her of holding onto the cake for an extra moment, putting the whole process out of synch and forcing Toynbee to stretch to reach the cake, or even get up and chase it down the line.[24]

Characteristics of all of Toynbee's accounts of covert research are established in her chapter about 'Cakes'. She is repeatedly critical of assembly-line work, describing it as 'deathly'.[25] Entering the cake factory, she described it in terms redolent of William Blake's 'dark Satanic Mills' with 'fearful, threatening and relentless' noise, like a 'horrible symphony orchestra', although the comparison is somewhat mitigated by the presence of so much sponge cake, cream and jam in Toynbee's case.[26] As with most of her accounts of covert research, it is not entirely clear how much time Toynbee spent working in the cake factory. Toynbee's account focused on how the work made her feel, on short descriptions

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[21] Toynbee, *A Working Life*, p.153. There were some companies which refused to let Toynbee work and write about them including Bird's Eye in Grimsby, Ford in Dagenham and the GPO telephone service.
[22] Ibid.
[23] Ibid., p.35.
[24] Ibid., pp.37-38.
[25] Ibid., p.35.
[26] Ibid., p.34. William Blake, 'Jerusalem' (from 'Milton'), Poetry Archive, http://www.poetry-archive.com/b/jerusalem.html , [accessed 31st March 2010].

of her fellow workers and their socio-economic circumstances and on workplace grievances and disputes. In 'Cakes', for example, Toynbee was 'horrified' and 'bored to the point of desperation' by the work which she found so 'depressing' that she could do nothing in the evenings but watch television, rather than make notes as she had intended.[27] She described some of her co-workers, including Jenny, an impoverished West Indian in her forties, who invited Toynbee home for tea and to meet her children.[28] Toynbee's interest in labour relations comes through clearly in *A Working Life* and in her chapter about 'Cakes', she noted that none of the workers, except some skilled male engineers, were unionised and that, had there been a union, the practice of being made to take official breaks at inappropriate times, such as almost immediately after lunch, would have been resolved quickly.[29]

In 'Maternity Ward', Toynbee described her experience of being a ward orderly in the maternity ward of 'St Mathilda's', giving her 'a worm's eye view' of what hospital meant for the working classes.[30] As a ward orderly, Toynbee worked a basic forty hour week with compulsory overtime on Saturdays and Sundays and found the work hard and relentless, but not difficult. Toynbee thought it a 'dreadful job' which was 'regimented down to the last detail' where she was 'shouted at all the time by tired cross people', but she did not discuss the actual day-to-day work in much detail.[31] In this brief chapter, she was more preoccupied with describing the hierarchy and working dynamic between all grades of medical and non-medical staff and lamenting the state of the hospital system.

In 'Car Parts', Toynbee worked in the finishing shop on the ground floor of one of Lucas' eleven car plants in Birmingham, manufacturing small bulb holders. Here, her job involved three stages but, nevertheless, the days dragged.[32] During this episode of ethnographic research, Toynbee lodged with a Polish family in a rough part of town.[33] As far as factories go, Toynbee described the car plant as 'a good one' but quickly added that 'as factories [in general] are terrible and deadening places to work in, so was this'.[34] Similar to her chapter on 'Cakes', Toynbee presented some short biographies of co-workers in the car plant and concerned herself with workplace dynamics between different

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[27] Ibid., pp.40-41
[28] Ibid., pp.43-44
[29] Ibid., p.36.
[30] Ibid., pp.48 & 53.
[31] Ibid., p.53.
[32] Ibid., pp.59 & 61-62.
[33] Ibid., pp.77-78.
[34] Ibid., p.59.

ranks of worker.[35] Whilst Toynbee was working at the plant, there was an unofficial strike by some foremen and charge-hands to restore pay differentials between themselves and the shop floor workers under their supervision.[36]

The chapter entitled 'Army' detailed Toynbee's time with the Women's Royal Army Corps (WRAC). After visiting the WRAC recruiting office in London and undergoing tests and interviews, Toynbee signed up for six years with the option of getting out after six weeks and she was sent to the WRAC barracks in Guildford for training.[37] Toynbee spent two weeks training and socialising with the rest of her thirty-two strong platoon of WRACs. After one week, Toynbee and eleven others wanted to leave but the sergeant would not consider their resignations until the end of the second week.[38] After two weeks, Toynbee was 'desperate to get out' and, in a WRAC English exam, wrote an acerbic attack on her time spent in WRAC training, was duly summoned to see the Company Commander, and discharged.[39] In her account of WRAC life, Toynbee was primarily concerned with presenting short biographies of fellow recruits and their reasons for signing up, and with criticising the strict regime of discipline enforced in the training camp.[40]

In 'Soap', the final chapter of *A Working Life* based on covert ethnographic research, Toynbee discussed her work for Lever Brothers. During this episode of research, Toynbee had lodgings in the village of Port Sunlight, described by Toynbee as the 'grand insane fantasy' of Lord Leverhulme, the paternalistic founder of the business and attached village.[41] In the huge Lever Brothers factory, Toynbee worked in the 'Scourers Department' where Dot toilet cleaner and Vim scouring powder were packed. Again, as in the cake factory, Toynbee was engaged in assembly line work, this time watching canisters of Vim emerge from a machine putting the lids on, removing the faulty ones, putting the lids on manually, and returning them to the belt.[42] Again, Toynbee described the work as 'grim' and the conditions as 'wretched' but, nevertheless, she was 'excited by the noise and the speed' at which everything happened.[43] Just as in previous chapters, Toynbee presented short biographies of her co-workers and their relationship with their work.[44]

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[35] Ibid., pp.62-22, 66 & 69.
[36] Ibid., pp.67-74.
[37] Ibid., p.79.
[38] Ibid., p.90.
[39] Ibid., pp.91-92.
[40] Ibid., pp.83 & 84.
[41] Ibid., pp.93 & 94.
[42] Ibid., p.95.
[43] Ibid.
[44] Ibid., pp.99-106.

Again, a good deal of Toynbee's account of working in the soap factory was focused on a brewing workplace dispute.[45]

Jeremy Sandford and Homelessness

Jeremy Sandford was educated at Eton and read English at Oxford before becoming a freelance writer for the BBC and a range of newspapers and journals. Along with his wife Nell Dunn, writer and heiress, Sandford moved from upmarket Chelsea in 1959 to a workman's cottage in Battersea as they were anxious to live and work alongside 'real people'. Sandford is best remembered as the writer of *Cathy Come Home*, a television drama about homelessness directed by Ken Loach aired in 1966, which shocked the nation and hastened the organisation of the housing and homelessness charity Shelter.[46] Sandford's second television drama, aired in 1971, about alcoholism and vagrancy, *Edna, the Inebriate Woman*, did not garner as much public support for alcoholics as *Cathy Come Home* had done for the sober homeless but it nevertheless won awards.[47] Elements of Sandford's research for these television dramas had been conducted covertly and he used this as the basis for his book, *Down and Out in Britain*, which was first published in 1971.

Sandford attracted a substantial amount of press attention in the early 1970s but very little of it was directly related to his partially ethnographic text, *Down and Out in Britain.* In one newspaper article written in 1973 it was suggested that not since Dickens had 'anyone supported the poor and underprivileged with such verve' as Sandford had and he described himself as 'a kind of public relations officer in reverse for the people at the bottom'.[48] In her review of *Down and Out in Britain* for the *Times Literary Supplement,* Phyllis Willmott agreed that Sandford had articulated his message concerning the plight of the poor clearly. However, she was decidedly unimpressed with his publication as a whole concluding that

> Judged by the standard of either social research or literature, this book is a bad one. Yet it probably has some kind of value in increasing awareness that there are still some shadowy and unsavoury

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[45] The machines were routinely over-staffed and a spare worker would take the place of another allowing them to go to the cloakroom for a chat and a smoke. According to Toynbee, this system was unofficial but universally recognized and the management accepted and catered for it. However, during the course of Toynbee's research the old cloakroom (with seats and a much-loved cloakroom assistant) was replaced with a new non-smoking seat-free cloakroom overseen by an unpopular new attendant. There were mutterings of discontent and talk of approaching the union. Toynbee, *A Working Life*, pp.96-97.

[46] Anthony Hayward, Obituary of Jeremy Sandford, *Independent*, 15th May 2003, http://news.independent.co.uk/people/obituaries/article36532.ece , [accessed 8th January 2008].

[47] D. S. Kenrick, 'Sandford, (Christopher) Jeremy (1930–2003)', *Oxford Dictionary of National Biography*, Oxford University Press, (2007), http://www.oxforddnb.com/view/article/90003 , [accessed 29 March 2010].

[48] Tim Devlin, 'Jeremy goes home to the gypsies', *Times*, 14th July 1973; Clifford Longley, 'No High Life for Author who Exposes Low Life', *Times*, 21st October 1971.

corners to the welfare state. Mr Sandford's 'new broom' may not sweep them clean, but it will no doubt stir up the dust.[49]

Willmott found 'minor factual inaccuracies and some misleading generalizations' in Sandford's account and described it as a 'scrapbook of quotations from a wide, though largely ill-documented, range of sources'. In addition, she made one especially damning observation given our focus on covert research: 'One has the feeling that the actual "down-and-out" days must have been fairly brief'. Willmott suggested that this was no doubt why Sandford had found his material on homelessness insufficient to sustain a book on its own and needed to cover other groups of disadvantaged people in society, such as unmarried mothers.[50]

In fact, only parts one and three of Sandford's account focus exclusively on the homeless whilst parts two, four and five all focus on other vulnerable groups and the state apparatus for dealing with them. Only part one appears to be based on covert research so we will restrict our analysis to this section of the book, along with the introduction and part six, Sandford's overall conclusions.[51] Even within Sandford's discussion of homelessness in part one of his book, the extent to which his account was based on his own covert ethnographic research is difficult to determine. His account covered several different episodes of research into homelessness, some of which were covert and others which involved Sandford visiting relevant institutions and engaging in conversations with the homeless and those caring for them overtly. These episodes are not clearly demarcated from each other and, given the lack of a clear narrative structure (or any other discernible structure), it is difficult to untangle the material based on covert research from that based on other methods of primary research.

Sandford does, nonetheless, appear to have undertaken at least three discernible episodes of covert research into homelessness in 1970s Britain. On the first occasion, Sandford visited St Botolph's Church in Aldgate, London, where the crypt was kept open throughout the night to provide services and shelter for the homeless. In disguise, he joined over one hundred other homeless people as they waited in the churchyard for the doors to be opened. He queued along with them, received his tea and buttered bread and sat in the vaults of the church, talking and listening to his companions.[52]

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[49] Phyllis Willmott, 'Bottom people', Review of *Down and Out in Britain* by Jeremy Sandford, *Times*, 10th December 1971.
[50] Willmott, 'Bottom people'.
[51] In the revised edition, Sandford has included some 'Notes' which detail his response to criticisms of *Down* and his play 'Edna, the Inebriate Woman' and a section reprinted from the Vagrancy Act. Sandford, *Down*, pp.161-174.
[52] Ibid., pp.19-23.

Like many covert social explorers before him (and Page subsequently), Sandford spent a night in a workhouse disguised as a homeless man.[53] Despite not having his National Insurance card, Sandford was eventually granted access to the workhouse by a surly official before having his clothes inspected and disinfected and ordered into a tepid shower before being fed watery soup and bread. Sandford was then provided with blankets and rough ill-fitting clothing before spending the night in a malodorous room housing over eighty men. After breakfast with the other inmates, Sandford appeared to relinquish his covert status and spent some time overtly observing a workhouse doctor and his assistant as they interviewed some of the homeless men who has spent the night in the 'Spike'.[54] This suggests that, even though Sandford spent the night covertly in the 'Spike', he must have done so with the agreement of the relevant authorities.

The third covert episode Sandford appears to have undertaken involved spending the night in a common lodging house. Sandford had been searching unsuccessfully for a bed for some time when he was approached by a Polish man who had already acquired a bed in a lodging house for the night. The Pole, once a steamboat skipper, had a plan: he sneaked Sandford into the common lodging house and, after proudly showing him pictures of his wife and car, allowed him to sleep under his bed. During the night, Sandford was caught and forced to leave the lodging house.[55] These three episodes appear to be the extent of Sandford's covert research.

Leaving Willmott's scathing review aside, Sandford's *Down and Out in Britain* elicited a mixed response from other commentators. D.A.N. Jones of the *New Statesman* and *Radio Times* described the book as 'primarily a sermon...illustrated by properly lurid anecdotes about cases of hardship' whilst Geoffrey Parkinson wrote in *New Society* that Sandford had a tendency to 'either slightly fictionalise problems or over-idealise people with problems'.[56] One reviewer believed the book to be a work of fiction.[57] Others were more positive about Sandford's work describing it as a 'frightening personal dossier' which demonstrated his 'detailed knowledge of the milieu' explored.[58] Sandford's best review, written in *Ink* by Jim Donovan, described *Down and Out in Britain* as 'a serious piece of social documentation'. He went on:

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[53] Ibid., pp.37-39.
[54] Ibid., pp.39-40.
[55] Ibid., pp.54-56.
[56] Ibid., pp.162 &166.
[57] Ibid., p.161.
[58] Ibid., pp.161 & 163.

> If the phrase 'descending into the bilges of our society' sounds a little too heroic for those readers who distrust the antics of middle class slummers and other social skin divers, then I would ask them to be patient; Mr Sandford's tales...may raise eyebrows around the dinner tables of suburbia, but let us hope that his probe into the causes behind these symptoms will raise some action.[59]

Donovan's review touched on two issues relating to Sandford's ethnographic research which will be explored in more depth in this chapter. First of all, the talk of heroic 'middle class slummers' travelling downwards into an abyss of poverty resonates with other historical examples of covert research into homelessness since the late 1800s which we have explored in earlier chapters. Certainly, there are some striking similarities between Sandford's late-twentieth-century account of a night in a workhouse, and the beginning of a night in a common lodging house, and those provided by, for example, Howard Goldsmid, Mary Higgs and J. R. Widdup. Secondly, Donovan expressed hope that Sandford's research would prompt action. One of the strongest features of Sandford's work is the insistent campaigning tone which, again, associates it strongly with the early tradition of covert social exploration in contrast to the academic covert ethnographers who had come to dominate the method by the time Sandford used it.

James Patrick and Glasgow's Gang Culture

Using the pseudonym 'James Patrick' for reasons of 'personal safety', an approved school teacher covertly researched a Glasgow gang in the late 1960s and published his findings more than half a decade later in 1973 under the title *A Glasgow Gang Observed*.[60] Whilst he was a student at the University of Glasgow and Jordanhill College of Education, a teacher training facility, Patrick had been working during the vacation at an approved school. It was during this time that Patrick met and developed a friendship with Tim, a pupil at the school who would become Patrick's key informant. After graduating, Patrick then applied for and got a job as a full-time teacher at the school, starting in August 1966. According to Patrick, the research project emerged spontaneously from a confrontation between himself and Tim during lunchtime one day at the school. Patrick had criticised boys who got into trouble during leave from the school and Tim had reacted angrily, asking Patrick what he knew about boys on leave and how they spent their time. As lunchtime ended and the other boys drifted away, Tim asked Patrick to come and see for himself. Patrick saw this as an invitation and a challenge.[61]

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[59] Ibid., pp.165-166.
[60] Patrick, *A Glasgow Gang*, p.9.
[61] Ibid., p.13.

To begin with, Tim had suggested an overt programme of research, that Patrick be introduced to his gang as an approved school teacher. However, Patrick decided that covert participant observation was the best option, despite the difficulties this methodology entailed.[62] Patrick's cover story was that he was Tim's best friend in the approved school and that, having no relatives nearby, during the holidays he had been befriended by Tim. Although never challenged about his age, Patrick planned to pass himself off as seventeen.[63] Tim was the front man of a small gang known as the Young Team which was based in the Maryhill area of Glasgow in the north-west of the city. The Young Team were affiliated with the much larger Maryhill Fleet gang composed of older boys and young men.[64] Patrick met with the Young Team on twelve occasions between October 1966 and January 1967 and spent a total of just under one hundred and twenty hours in the field. As Patrick's 'involvement with the gang deepened, so the hours lengthened' and during his final month of fieldwork he was in the continuous company of the gang for a period of around thirty-five hours.[65]

By his own admission, Patrick spent most of his time with the gang doing nothing very much at all. A great deal of time was spent by the Young Team simply standing on corners of their territory engaged in 'desultory conversation' and 'indiscriminate grumbling' which induced in Patrick feelings of 'unending boredom' and 'crushing tedium'.[66] However, Tim and the Young Team did occasionally spring into action and Patrick was able to participate covertly in a number of activities. On their first meeting, Patrick accompanied the Young Team to a pub in Glasgow's city centre where Pat, one of Tim's friends, started a fight with two labourers in which Tim participated before the group fled the pub. They then headed for a dance hall where once again Pat started a violent confrontation with the bouncers and Patrick sneaked out, fearing the bouncers would turn on him, and went home.[67] Patrick also participated in the trashing of a local public library, went to a cinema where the film was disrupted by gang members, took part in a game of football between the Young Team and the Maryhill Fleet, and attended a house party where recreational drugs and stolen alcohol were consumed. It was Patrick's reluctance to carry a weapon and failure to take an active role in the Young Team's violent confrontations with other gangs that eventually brought about his exit from the field.[68]

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[62] Ibid., p.14.
[63] Ibid., p.15.
[64] Ibid., pp.21, 35 & 37.
[65] Ibid., p.9.
[66] Ibid., p.80.
[67] Ibid., pp.30-32.
[68] Ibid., pp.56, 92-93, 135-139.

Given the nature of the activities in which the gang participated and the covert nature of his research, it is understandable that Patrick gave everyone featured in his account a fictitious name and altered or omitted details that would lead to their identification. Furthermore, following some 'legal advice', Patrick made some 'abridgements' to his account prior to publication.[69] He described his work as a 'descriptive account of a participant observation study of one...gang' which in an 'unashamedly exploratory' way provided a 'brief glimpse of the reality' which engaged the members of Glasgow's gangs.[70]

Upon publication of *A Glasgow Gang Observed*, Patrick's work attracted a good deal of publicity and he appeared anonymously on two television shows to discuss and defend his research.[71] Perhaps predictably, given that Patrick's research had added weight to the association between Glasgow and gang violence, and that he had accused the police of violent assault, he provoked a hostile reaction from the city's officials.[72] After hectoring Patrick on television, William Gray, the Provost of Glasgow, later described Patrick's research as '"a rather dubious project"' which had resulted in '"a most unsatisfactory book"'.[73] According to William Ratcliffe, the assistant chief constable in Glasgow at the time, it was '"nonsense"' that a member of staff at an approved school '"should run about with a gang"' and that *A Glasgow Gang Observed* had '"not added one iota of information to the sum total of knowledge about gangs in the city"'.[74]

Letters sent to the editor of the *Glasgow Herald* suggested that the public reaction to Patrick's work was more favourable than that of the city's officials. One lady from Dunoon wrote that having heard Patrick discuss his research on television, she believed his intentions to have been 'honourable if a shade foolhardy'.[75] A Glaswegian man wrote in to express his astonishment and dismay at the way Patrick had been 'pilloried' on television for, in his opinion, daring to suggest that delinquents were 'not born but spewed forth by a slum...environment'.[76]

[69] Ibid., p.16.
[70] Patrick, *A Glasgow Gang*, p.9.
[71] 'Profits on gang book offered to corporation', *Glasgow Herald*, Friday February 9th 1973, p.5. Patrick appeared on BBC's Current Account and on Scottish television's Dateline on February 8th 1973.
[72] Patrick claimed to have been 'hit on the shoulder by a baton' as he was bundled into a police van, despite offering no resistance, and was later punched and kicked by police after questioning at the station, again in an unprovoked attack. Patrick, *A Glasgow Gang*, pp.56-58.
[73] 'Gang book profit to help research', *Glasgow Herald*, Saturday 10th February 1973, p.14.
[74] 'Gang book profits', p.14; 'Profits on gang book', p.5.
[75] Jean Fisher, 'Letters to the editor', *Glasgow Herald*, Wednesday February 14th 1973, p.10.
[76] Alex Fraser, 'Letters to the editor', *Glasgow Herald*, Wednesday February 14th 1973, p.10.

Robin Page and 'Voluntary Vagrancy'[77]

Born into a farming family in 1943 in Cambridgeshire, Robin Page has been described as 'the townie's favourite yokel' and is best known for presenting the iconic BBC television show 'One Man and His Dog' in the 1990s.[78] Since then, Page has parted ways with the BBC, having accused them of following a 'metrocentric' agenda which deliberately marginalised rural-orientated programming.[79] Today, Page is noted for his political outspokenness on issues such as hunting and Britain's membership of the European Union. As a rural conservationist, Page writes a regular column, 'Country Diary', for the *Telegraph*, and has written many books lamenting the decline of the British countryside, as well as some children's fiction based on the rural environment.[80] Page's association with the countryside is worth noting as it constitutes a key theme in his ethnographic writing.[81]

In addition to his career as a journalist and writer, Page has flirted considerably with party politics. As well as being an Independent councillor in Cambridgeshire for thirty-six years, Page has stood for Parliament for the Conservatives, the Referendum Party and UKIP (the United Kingdom Independence Party). Most recently, Page has broken away from UKIP and established his own political party, United Kingdom First (UKF).[82] Page is on the right of the political spectrum and, as a covert ethnographer, this makes him unusual. The majority of the non-academic covert ethnographers discussed in the course of this thesis have been on the left of the political spectrum and have undertaken their research into marginalised social groups in a spirit of compassion. Page, in contrast, undertook his ethnographic research from a very different starting point.

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[77] This is how Page referred to his covert research. Page, *Down*, p.143.

[78] Julia Stuart, 'One Man and his Dog: the Robin Page Story', *Independent*, 24th March 2001, http://www.independent.co.uk/news/media/one-man-and-his-dog-the-robin-page-story-688730.html , [accessed 13th January 2010].

[79] Robin Page, 'Come bye and see us – sheep are back on TV', *Daily Mail*, 8th December 2007, http://www.dailymail.co.uk/tvshowbiz/article-500578/Come-bye--sheep-TV.html , [accessed 13th January 2010]. The magazine 'Country Living' also parted ways with Page over fears that he might alienate sections of their readership. Julia, 'One Man'.

[80] Robin Page, 'Country Diary', http://www.telegraph.co.uk/earth/earthcomment/country-diary/ , [accessed 13th January 2010]. Examples of Page's writing on the countryside: Robin Page, *A Peasant's Diary*, (London: Robinson, 1993); Robin Page, *The Decline of an English Village*, (London: Davis-Poynter, 1974). Examples of Page's children's fiction: Robin Page, *How the Fox got its Pointed Nose*, (Bird's Farm Books, 1989); Robin Page, *How the Heron got Long Legs*, (Bird's Farm Books, 1989).

[81] In 1977, Page used overt participant observation to explore the country pursuit of hunting. Despite his rural roots, Page had little prior experience of hunting and, for the purposes of research he undertook a variety of activities including fox hunting, hare-coursing, fishing and even cockfighting. Robin Page, *The Hunter and the Hunted: A Countryman's View of Blood Sports*, (London: Davis-Poynter, 1977).

[82] United Kingdom First party, 'About Us', http://www.ukfp.org/about-us , [accessed 13th January 2010].

Unable to stay on the family farm, Page became a 'social security special investigator' and was responsible for 'tracking down social security fiddlers and seeing them prosecuted'. Page recalled:

> I got the sack from that job for complaining about the cover-up of benefit fraud by our crooked MPs at the time. I only broke the Official Secrets Act to expose the cover-up, which for a naïve and innocent 26-year-old was quite nerve-racking.[83]

Shortly after leaving the civil service, in 1971, Page published *The Benefits Racket*, an account of benefit fraud which made extensive use of his experiences dealing directly with benefit claimants.[84] Two years later, in 1973, Page published his covert ethnographic account of homelessness, *Down among the Dossers*. Page's work with the vulnerably housed when he was a civil servant had fuelled his interest in this social group. As a part of the machinery of the Welfare State, designed to safeguard people from cradle to grave, Page could not understand why or how someone could become homeless. As a civil servant, he saw the homeless frequently, 'from the official side of the counter as they pleaded for money' but was unable to understand their way of life fully.[85] Page explained:

> Because of the many imponderables that arise when discussing down and outs...my dealings with them as a civil servant and the added difficulty of the unemployment situation, I decided recently that the only way to find out the facts was to become a down and out myself. In this way I hoped that I would find out why so many people live in abject poverty and discover whether it is they who have rejected society, or whether it is society that has rejected them.[86]

In this sense, Page's rationale for conducting undercover research amongst the homeless was much more confrontational than that of the other non-academic researchers we have discussed. Page's research was not open-ended and exploratory and fuelled by compassion and the desire to help; he wanted to use his covertness to determine whether his fellow vagrants were genuinely in need or if they were cheating the system.

There can be no doubt about the comprehensiveness of Page's covert research into homelessness. In *Down among the Dossers,* Page presented a clear narrative account of sixteen consecutive nights spent dressed as a vagrant, either sleeping rough or in charitable, commercial or state accommodation for the homeless as he tramped from Brighton to London.[87] Travelling on foot and by hitch-hiking from Brighton through Eastbourne, Hastings, Wadhurst and Sevenoaks to London in the summer of 1971, Page

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[83] Robin Page, 'Robin Page: Trying to Derail the Gravy Train', *Telegraph,* 21st May 2009, http://www.telegraph.co.uk/earth/earthcomment/country-diary/5357413/Robin-Page-Trying-to-derail-the-gravy-train.html , [accessed 13th January 2010].
[84] Robin Page, *The Benefits Racket*, (London: Tom Stacey, 1971).
[85] Page, *Down*, p.8.
[86] Ibid., p.9.
[87] A short chapter towards the end of *Down among the Dossers* was based on a separate stint of research into the recent phenomenon of music festivals but this will not be discussed here as it does not constitute covert research into a relatively closed social group. Ibid., pp.86-108.

adopted the pseudonym 'James Grieve', a type of English apple.[88] There were only two occasions when Page, in a sense, suspended his covertness, and accepted the hospitality of 'real life' friends. On his thirteenth day as a vagrant, Page 'stopped briefly to scrounge some food from...a photographer friend, and then continued towards central London'.[89] On his penultimate day, Page was on London's Fleet Street when he met a journalist with whom he had been working shortly before beginning his undercover research. He was so shocked by Page's appearance that he offered to buy him coffee and a roll, which Page gratefully accepted, as well as a complete meal and a pound note, which Page refused.[90]

During his time as an undercover tramp, Page 'only kept a brief diary, to avoid causing suspicion' but suggested that events in the field had made such an impression on him that his account was 'quite accurate'.[91] Given the extent of Page's ethnographic research and the detailed nature of his account, it is difficult to summarise his work. Nonetheless, some of Page's pre-occupations in his account of homelessness can be discerned. Page's over-riding concern was, as suggested above, to determine whether the vagrants he came into contact with were in genuine need of help and support or whether they were, in his opinion, capable of providing for themselves but homeless through choice, taking advantage of the help on offer. On his journey from Brighton to London, Page did encounter vagrants whom he judged to be genuinely inadequate but these deserving poor were thin on the ground, as far as Page was concerned.[92]

The vast majority of Page's fellow vagrants, he suggested, did not want to work and were content to survive by begging and claiming benefits.[93] He vented his frustration at those providing sustenance indiscriminately to the homeless, most of whom were 'taking advantage...being quite capable of fending for...[themselves] and without food and money through...[their] own actions'.[94] He noted with disapproval the way in which the homeless spent their benefits and provided examples of homeless people feigning disabilities in order to solicit sympathy and money from unsuspecting members of the public.[95] Furthermore, Page made a point of finding work, not just to alleviate his hunger and

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[88] Ibid., p.42.
[89] Ibid, p.62.
[90] Ibid., pp.76-77.
[91] Ibid., p.9.
[92] Page met one young 'sickly-looking' man in Brighton who had been kicked out of home after leaving school and had been homeless ever since. Page described him as a 'genuinely inadequate' individual who had 'not been caught in the net of the welfare state'. Ibid., pp.15-16. In Camberwell reception centre, Page suggested that it was quite easy to distinguish the 'inadequate, disabled and mentally ill' from the 'alcoholics' and 'wasters who wanted a free bed'. Ibid., pp.79-80.
[93] Ibid., p.16.
[94] Ibid., pp.36-37 & 72.
[95] In Brighton, Page noted the 'spectacular results' of his fellow dossers having received their benefits that day and having apparently spent the vast majority of it on alcohol. Page, *Down*, p.19. At Euston Station in London, Page observed a vagrant with a white stick purposefully colliding with passers-by and asking them for money. Ibid., p.72.

boredom, but also to demonstrate that it was possible. On a casual short-term basis, Page found work as a dishwasher and a kitchen porter through the Labour Exchange and, although the pay was poor, proved to his satisfaction that 'there was no need for anybody to be penniless or roofless' and that there was ample opportunity to 'climb back into society' for those who chose to take it.[96]

Perhaps unsurprisingly given Page's attempts to publicise the extent of benefit fraud just a couple of years earlier, he was anxious to highlight the administrative lapses made by state officials he encountered on his ethnographic journey. He expressed surprise at the extent of 'itinerant fraud' and the ease with which a false identity could be adopted.[97] When he visited the Labour Exchange in Brighton, for example, he noted that the clerk 'made no effort' to confirm Page's identity or National Insurance number, and neither did the employer when he started work.[98] He expressed similar concern at the laxity shown by gatekeepers at the reception centres in Brighton and Camberwell regarding identity.[99]

Having personally experienced the array of accommodation on offer to the early 1970s vagrant, Page felt qualified to criticise almost all efforts to help the homeless. He suggested that the system of state hostels and reception centres he had encountered had changed little since the days of the workhouse and that they 'could even be helping to perpetuate the problems rather than eradicating them'.[100] The charitable sector did not escape his wrath either. Visiting a soup run, he reflected that although he appreciated the 'sincerity and desire to help' of the volunteers, he could not understand what they hoped to achieve through the indiscriminate dishing out of soup.[101] Page spent his thirteenth night as a tramp in a Salvation Army hostel in London and, although he had previously had the greatest respect for the organisation, he vowed never to stay in a 'sally' again.[102]

> Because of the state of the toilet facilities I did not wash and because of the threat of lice I climbed into bed fully clothed. It was obvious that I would get little sleep. If I turned one way I was faced with the fetid breath of the Irishman and if I turned the other I had the scratching of the grey-haired man. As a result I lay in an uncomfortable supine posture, looking at the ceiling and conscious of the hordes of TB germs invading from one side and a column of marching lice advancing from the other. The situation was aggravated still further by the added fact that men were passing wind with gay abandon and every so often a slanging match would break out.[103]

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[96] Ibid., pp.18-19, 45, 51-54 & 55.
[97] Ibid., p.136.
[98] Ibid., p.19.
[99] Ibid., pp.32 & 78.
[100] Ibid., p.84. Higgs expressed the same sentiment about Edwardian workhouse casual wards which she described as '*national tramp manufactories*'. Mary Higgs, *Glimpses into the Abyss*, (London: P.S. King and Son, 1906), p.179; See above, p.59. Emphasis in original.
[101] Page, *Down*, p.74.
[102] Ibid., pp.66-70.
[103] Ibid., p.68.

Given the 'abysmally low standards' at the hostel, Page did not see the point of maintaining such institutions without the provision of rehabilitation. On the other hand, Page was most complimentary about the 'courteous and helpful', 'civil and sympathetic' policemen he encountered during his research. They gave him advice on where to find work and a good cup of tea and left him with 5p and 'a friendly smile and a wave'.[104] Similarly, Page praised those who had helped him when he had asked, including some nuns in a convent, a woman who gave him tea and a sandwich at her cottage, and a farmer who allowed Page to spend the night in one of his outbuildings, spreading fresh straw on the floor, and giving him use of the workmen's washing facilities.[105]

When Page's ethnographic journey finally came to an end, he was 'greatly relieved' that he was 'heading for a bath, a change of clothing, laughter and conversation' and that he would be able 'to climb back into society once more.'[106] Surprisingly, given the recent publicity that Sandford's play 'Cathy Come Home' and the subsequent establishment of the charity Shelter had drawn to the issue of homelessness, the publication of Page's *Down among the Dossers* seems to have attracted very little attention. Norman Shrapnel reviewed Page's 'dossing adventures' for the *Times Literary Supplement,* noting the clear distinction that Page made between 'the true dropout and the pretender'. Shrapnel also questioned the extent to which Page had truly experienced the life of a vagrant, suggesting that Page 'knew he was coming back' and had been fortunate to have tramped during a particularly warm summer, a point which Page himself acknowledged.[107]

The Structure of Non-Academic Covert Ethnography in the 1970s

Clifford and Parssinen's ideas about structure and textual authority have been used in previous chapters and they can, once again, be usefully applied to our 1970's non-academic ethnographies. To recap, the structure most readily associated with non-social-scientific late Victorian and Edwardian examples of covert ethnography was a narrative structure which mirrored the ethnographer's fieldwork. The use of a narrative structure implied that the ethnographer was claiming experiential ethnographic authority, meaning that it was their experience in the field which legitimated their contribution to the discourse. It was only as the ethnographic method became increasingly academicized that ethnographers sought ethnographic authority in alternative textual structures. The emergent

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[104] Ibid., pp.42-43.
[105] Ibid., pp.57-58 & 59.
[106] Ibid., p.85.
[107] Norman Shrapnel, 'Just Visiting', A Review of *Down among the Dossers* by Robin Page, *Times Literary Supplement*, 6th April 1973, p.372. Page, *Down*, p.144.

academic ethnography was distinguishable from earlier, non-academic, ethnography in that it tended to be structured in a non-narrative way, deriving authority from other sources and strategies.[108]

As with earlier examples of non-academic covert ethnography explored in previous chapters, it appears that the structures used by our 1970's non-academic covert ethnographers cannot be neatly categorised as purely narrative. We have already noted that Sandford's account of the homeless and other socially vulnerable groups had no obvious structure, resembling a dossier of material collated from a wide range of ill-documented sources. There was no sense of accumulated understanding evident in Sandford's account and little more can be said about it in terms of structure. The ethnographic texts produced by Toynbee, Patrick and Page, however, more closely resembled the narrative structure associated with social exploration, though with some significant divergences. Page's account of his 'voluntary vagrancy' had the most straightforward narrative structure of the three.

Page's *Down among the Dossers* was split into seven chapters without headings or a separate introduction or conclusion. At the beginning of his account, Page spent almost three pages outlining his reasons for undertaking his research before launching into a sustained narrative description of his impressive sixteen nights as an undercover vagrant which filled the rest of chapter one and chapters two, three and four.[109] We can see, then, that Page put a descriptive account of his fieldwork at the heart of his text, demonstrating that it was his experience of vagrancy which made his account authoritative. However, there are other strong sources of authority present in Page's work which are reminiscent of the ethnographic accounts produced by Goldsmid and Higgs in 1886 and 1906 respectively. Goldsmid, Higgs and Page are notable for their extensive covert research, often involving repetition of certain experiences, in comparison to other British ethnographers and this gives their research an extra dimension of experiential authority which is embedded in their texts.[110] Whereas Greenwood spent just one night in a workhouse casual ward and Toynbee worked in just one cake factory, for example, Page spent over two weeks disguised as a vagrant and stayed in two different reception centres

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[108] See above, pp.23-24; James Clifford, 'On Ethnographic Authority', *Representations*, Number 2, (1983), pp.119-120; Carol Ann Parssinen, 'Social Explorers and Social Scientists: The Dark Continent of Victorian Ethnography', in Jay Ruby (ed.), *A Crack in the Mirror: Reflexive Perspectives in Anthropology*, (Philadelphia: University of Pennsylvania Press, 1982), p.206.
[109] Chapter 5 was a narrative account of Page's visit to a music festival with a friend which is not being analysed.
[110] See above, pp.43 & 46.

during that time.[111] The repetition of his experience adds to the authoritativeness of his account and this repetition is emphasised by the narrative structure of the text.

Towards the end of his account, in chapters six and seven of *Down among the Dossers*, Page presented his reflections on the plight of the homeless, the institutional response to it and his conclusions. More importantly, he also made use of several sources of information on homelessness in Britain besides his own covert experience.[112] As well as his covert participatory research, Page made a point of speaking to homeless people about their situation whenever he met them and he overtly visited facilities providing accommodation for the homeless.[113] Furthermore, he was clearly familiar with local government reports on homelessness, made use of figures collated by non-governmental organisations such as the National Association of Voluntary Hostels, and quoted relevant legislation.[114] The range and depth of Page's knowledge about the homelessness problem in Britain only added to the authoritativeness of his account.

In Toynbee's *A Working Life*, the emphasis was again on description rather than analysis of her covert fieldwork. However, the book did not conform to the same basic narrative structure which Page had used. Toynbee's ethnography consisted of ten chapters which were prefaced with a list of facts relating to rates of employment and unemployment, rates of pay, pension statistics taken from official sources and described as 'Background'.[115] The first chapter, 'Youth', detailed the time Toynbee spent sitting in on interviews at the Youth Employment Bureaux but in the final paragraph of this chapter, we discover that Toynbee undertook this stage of her research after having completed her covert observations of at least some of the workplaces covered in *A Working Life*.[116] Each of the following six chapters discussed an episode of covert research in a different working environment. The following two chapters detailed Toynbee's overt observations of a coal mine and steel plant respectively and in the final chapter, 'Old Age', Toynbee wrote about her overt research with retired people. However, the book is not organised in the order in which Toynbee undertook her episodes of research and neither is each chapter organised in a chronologically accurate narrative.

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[111] Page visited the reception centres in Brighton and Camberwell. Page, *Down*, pp.31-39 & 77-85.
[112] Ibid., pp.109-146. Page was also aware of other examples of qualitative non-academic research into homelessness but this will be discussed further in the section *'Sandford and Page: Ethnographies of Homelessness since the 1880s'*.
[113] For example, Page recalled meeting a 'highbrow' tramp on his visit to the Tower Bridge Hotel, a private hostel at which Page wanted to speak to the manager. Page, *Down*, p.113.
[114] Ibid., pp.121-125.
[115] Toynbee, *A Working Life*, pp.9-10.
[116] Ibid., p.33.

The second chapter about 'Cakes', for example, was not structured chronologically as we learn about the recruitment process after Toynbee has described her experience of the work.[117] It is important to note, however, that Toynbee did not decide to avoid a narrative account in order to provide an analytical account organised thematically, as associated with academic covert ethnographers. Rather, Toynbee's concern appears to have been to provide a compelling and fluent account, regardless of chronology or analysis. There is no conclusion to *A Working Life* although the structure of the book, with accounts of working lives sandwiched in between the first and final chapters, 'Youth' and 'Old Age' respectively, reflects the stages of life.

Despite Toynbee's decision to forgo a strictly narrative structure, the journey metaphor, which is at the root of all ethnography but particularly evident in Victorian and Edwardian examples of social exploration, still emerged. In the first chapter detailing her observations of interviews at the Youth Employment Bureaux, she was disheartened when a young man, having been set up with an apprenticeship by the Employment Officer, appeared to leave the interview contented. Toynbee wrote:

> As he was going out of the room I wanted to call him back and take him with me to see the factories and offices I had seen and worked in. I should like to have shown him the cake bakery, the scourers department of Lever Brothers, the shop floor at Luca's. Wouldn't he then change his mind?[118]

Again, in her chapter about the cake factory, Toynbee made use of the metaphor of participatory research as a journey from a position of ignorance to understanding. Having settled into her role as a cake layer stacker, Toynbee reflected:

> I had often wondered what people thought about, working on assembly lines all day. The answer is nothing. The work needs just enough concentration to keep the mind occupied. I had thought it would be a time when I could sit and think for hours on end. But my mind was blank.[119]

Toynbee's account demonstrates that the structure of covert ethnography can be very complex. *A Working Life* is dominated by a sense of narrative but the account is not organised in a strictly narrative way. The sense of journey from ignorance to understanding is evident, without being overwhelming and simplistic.

Sandford, Page and Toynbee quite clearly produced their ethnographic accounts from the standpoint of non-academic ethnographers and, although we located some divergences, all three based their accounts on the narrative structure associated with non-academic ethnography. Patrick, however, is harder to categorise unequivocally as either an

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[117] Ibid., pp.34-47.
[118] Ibid., p.33.
[119] Ibid., pp.35-36.

academic or non-academic covert ethnographer, as we will explore in more detail shortly. This had important implications regarding the structure of Patrick's research. He could present his ethnographic material in the narrative style associated with non-academic ethnographers, or he could choose to adopt the anthropological paradigm of ethnography. As we have discussed in earlier chapters, academic ethnographers tend to demonstrate their ethnographic authority by establishing a 'fable of rapport' with the research subjects before retreating into a thematic structure written in an impersonal style, removed from the field experience.

A Glasgow Gang Observed was organised into twenty-one chapters plus a preface, glossary, bibliography and index. Patrick did indeed establish his rapport with the research subjects early on in his account. He did this by describing, right at the start of the first chapter, his appearance in detail as he met Tim and other members of the Young Team in his covert role for the first time:

> I was dressed in a midnight-blue suit, with a twelve-inch middle vent, three-inch flaps over the side pockets and a light blue handkerchief with a white polka dot (to match my tie) in the top pocket. My hair, which I had allowed to grow long, was newly washed and combed into a parting just to the left of centre. My nails I had cut down as far as possible, leaving them ragged and dirty. I approached the gang of boys standing outside the pub and Tim, my contact, came forward to meet me, his cheeks red with embarrassment. 'Hello, sur, Ah never thoat ye wid come.' Fortunately, the others had not heard the slip which almost ruined all my preparations.[120]

Significantly, however, this 'fable of rapport' is mirrored in decidedly non-academic examples of 1970s covert ethnography, such as Page's account of homelessness.[121] In *Down among the Dossers*, Page also described his disguise in detail before noting that:

> With four day's stubble on my chin I looked scruffy, and felt scruffier, and already respectable middle-class women out shopping were giving me disapproving sidelong glances as I passed them. However, the strange affinity which seems to bind down and outs together was quick to show itself, for as I passed a shelter containing two long-coated, red-faced cider drinkers, they smiled friendly alcoholic smiles at me, which seemed to indicate that I was already accepted as one of their own kind.[122]

It would appear that establishing a successful transition from one social world to another, from the world of the researcher into that of the researched, is a significant component of some academic and non-academic ethnographic accounts.

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[120] Patrick, *A Glasgow Gang*, p.13.
[121] In Chapter 2, we discussed the 'fables of rapport' used by Massingham, writing in the tradition of the social explorer, and Bakke, writing from an academic perspective; See above, pp.105 & 127-128; Edward Wight Bakke, *The Unemployed Man: A Social Study*, (London: Nisbet, 1933), p.2; Hugh Massingham, *I Took Off My Tie*, (London: William Heinemann Ltd., 1936), pp.5-6.
[122] Page, *Down*, pp.9-10.

Once Patrick had demonstrated his successful entry into the ethnographic field, however, he did not retreat into a sterile thematic account of gangs in Glasgow. In fact, Patrick initially embarked on a narrative account of his fieldwork, explaining that, having met the gang on twelve separate occasions,

> [t]he first few will be recounted in detail. Then, instead of a blow-for-blow account in strict chronological order of every time I met the gang, key events and representative situations will be described.[123]

Patrick's account was a mixture of narrative reconstructed from notes made during the period he was undertaking his covert research, recollections and reflections made at the time of writing. Even his chapters based on non-narrative accounts of fieldwork tended to be much more descriptive than analytical. The way in which Patrick switched in the midst of his ethnographic text from a straightforward narrative structure to a thematically structured account seems odd. However, unlike Bakke and Spencer, two mid-twentieth-century academic ethnographers who appeared to struggle to accommodate their ethnographic material in their accounts, Patrick's emphasis on description, and the foregrounding of his ethnographic findings, appear to have been intentional.[124] He noted near the end of his account that he had 'sought to describe, and to a lesser extent interpret' his observations of Tim and the rest of the gang.[125] He suggested that earlier research on the subject of delinquency had been criticised because it failed to communicate 'what it feels like to be a young delinquent in Glasgow' and that '"added information"' was needed so that '"these youngsters who are treated as facts and figures come alive to imagination as well as to intellectual understanding"'. Patrick described his book as

> an attempt to present the daily lives of a group of adolescent delinquents with appreciation and empathy;...an attempt to 'place upon the bare bones of statistics the flesh and blood and spirit of recognisable humanity'.[126]

Patrick handled his ethnographic material with confidence, if a little inconsistently, giving it pride of place in his account of Glasgow gang life in the late 1960s.

The initial narrative chapters of Patrick's account and his sustained focus on his ethnographic material demonstrated his attainment of experiential authority. However, Patrick also demonstrated his depth of knowledge of other, non-ethnographic, material relating to gangs in Glasgow. The concluding chapters of *A Glasgow Gang Observed*

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[123] Patrick, *A Glasgow Gang*, p.16.
[124] See above, pp.126-130 & 157-158.
[125] Patrick, *A Glasgow Gang*, p.229.
[126] Ibid. Patrick appears to have been quoting John Barron Mays. John Barron Mays, *Growing Up in the City: A Study of Juvenile Delinquency in an Urban Neighbourhood*, (Liverpool: Liverpool University Press, 1954), p.22.

included an extended discussion of Glasgow's housing problem, which Patrick felt was a significant contributory factor to gang culture, as well as two chapters dealing with the historiography of British and American research on juvenile delinquency respectively.

Covertness in 1970s Non-Academic Ethnography

The prominence which the covert nature of our 1970s non-academic ethnographic fieldwork was given varied. Surprisingly, given the apparently small amount of covert fieldwork actually undertaken by Sandford, the undercover nature of his research was repeatedly emphasised not only in his book *Down and out in Britain* but also in subsequent reviews. One journalist described Sandford walking 'unshaven and in tattered clothes among the down-and-outs' in order to research his television dramas and another described him as 'come back from the derelict building and midnight park bench to reproach the rest of us for our inhumanity.'[127] In one newspaper report about Sandford's fictionalised account of 'Edna', it was noted that in preparation he had 'devoted many days and nights to experiencing the life lived in lodging houses and reception centres, and slept rough in some of the frequent haunts of the destitute'.[128] On the back cover of the revised paperback edition, it was written that '[w]earing a tattered old great coat and a beard Jeremy Sandford recently took a journey into "the bilges of society". He stayed in seedy common lodging houses, kip houses and mingled with down-and-outs'. In his introduction, Sandford again emphasised his use of ethnographic research:

> I wanted to meet and talk with down and outs, dossers, tramps, the inhabitants of kiphouses, derries, Spikes. I wanted to see what life is like at the bottom, for those who have failed, those whom the Welfare State has failed to reach.[129]

The most blatant exaggeration of the extent of Sandford's covert research was made by John Hill, reviewing Sandford's book for *Social Action*, the Simon Community's in-house journal. He described Sandford as 'garbed in shabby greatcoat and bearded, lived in the filthy heart of Skid Row for the time needed to form his compelling book'.[130]

In contrast, although Toynbee spent a great deal more time undercover than Sandford did, the covert nature of her research was not emphasised. On the back cover of the 1973 paperback edition of *A Working Life* and in the editor's preface, the participatory

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[127] Devlin, 'Jeremy'; Longley, 'No High Life'.
[128] Longley, 'No High Life'.
[129] Sandford, *Down and Out*, p.9.
[130] Sandford, *Down and Out*, p.167.

nature of Toynbee's research was noted, though the covert nature of the research was not mentioned. On the back cover, we learn that Toynbee

> set out to learn about people in the best possible way. She went and worked with them, lived with them, and talked to them. She assembled cars, made soap, decorated cakes, joined the army and scrubbed hospital floors, and discovered at first hand all about people at work in Britain today.[131]

Similarly, the editor of *A Working Life* described how Toynbee had 'spent months actually living this book'.[132] Her incognito status, however, did not feature prominently in her account. In order to gain access to the car plant, Toynbee had approached the management and asked for permission to work there for research purposes 'as it was not the sort of place that you could wander into and get a job straight away'. Toynbee explained that because of this, her

> presence was thought a little strange by the other women, but they accepted my explanation that I was a student doing holiday work. The foremen and charge hands were not told either but they may have suspected that I was there to do some sort of research.[133]

When Toynbee was a WRAC, she thought that her incognito had been a success, noting that no-one appeared to have guessed what she was doing and registered her surprise that 'the girls didn't think me as odd as they might have done'.[134] However, when she was summoned to speak to the Company Commander after writing her critique of life in the camp, she was accused of 'stirring up discontent' amongst her fellow recruits. The Commander said

> 'Always happens when you have officer material in the ranks,'...to my surprise. (All I had done was to write an explicit essay. Apart from my rather noticeable but unavoidable incompetence, I had remained anonymous.)[135]

This incident is reminiscent of an episode in Webb's account of covert research into the sweated tailoring industry in the 1880s. In chapter 1 it was noted that Webb appeared to be engaged in a paradoxical project of assimilating herself into the culture of the sweatshop whilst simultaneously highlighting her social distance from her working-class research subjects. Webb had been keen to point out that the sewing mistress thought her a cut above the other workers in terms of social class.[136] Similarly, it could be argued that Toynbee was, even subconsciously, pleased that her assimilation into the working-class ranks of the WRACs had not been overly successful. Despite her attempts at covertness, she had been ear-marked as 'officer material'.

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[131] Toynbee, *A Working Life*, back cover.
[132] Ibid., p.1.
[133] Ibid., p.77.
[134] Ibid., p.91.
[135] Ibid., pp.91-92.
[136] See above, p.60.

In Wohl's account of late Victorian and Edwardian social exploration of the slums, he dismissed covertness as simply another 'dramatic device' in the armoury of the social explorer. Clearly, covertness is more than just a dramatic device, but it is true that the undercover nature of research can be used as a source of drama, as it was in three out of four of our 1970s non-academic covert ethnographies. Perhaps it is not surprising that Toynbee did not wring the same amount of tension from her incognito as Sandford, Patrick and Page given the subject matter of her research. Whereas Sandford, Patrick and Page were covertly exploring the relatively dark, deviant and occasionally criminal, subcultures of the homeless and gangs, Toynbee was undercover in the mundane world of work.[137] At the end of his first chapter, Patrick heightened the sense of danger and anticipation surrounding his fieldwork, noting that on the eve of his first meeting with the gang in disguise, Tim's delight and amusement at his acceptance of his invitation had transformed into incredulity that he intended to continue. Patrick ended with the ominous sentence 'I now understand why'.[138] In a similarly dramatic tone, Sandford wrote in his introduction that once, 'some years ago, I attempted a similar book, and abandoned it. This time I carried through'.[139]

One particularly fruitful source of drama was the notion that the true identity of the covert ethnographer could be revealed, leaving them exposed and vulnerable in a dangerous and potentially hostile environment. For example, after going to a local dance hall with the Young Team, Patrick was teaching at the approved school when

> one pupil asked me: 'Sur, wir you doon the Granada oan Setirday?' That other boys from the same approved school...should go to the same dance-hall, I had thought of but dismissed from my mind as too coincidental. The 'you-must-be-joking, down-*where*?' type of answer seemed to satisfy my questioner, but in future I would have to be far more careful.[140]

At one point, the time and venue where Tim and Patrick were to meet changed, increasing the risk of Patrick being identified and he heightened the tension by noting that he

> came very close to being spotted. On the very afternoon I chose to meet Tim, the approved school bus…happened to make its way through Maryhill. Luckily for me Tim recognized the bus as it approached and shouted out greetings to the occupants. I was given sufficient time to turn my back on the main road and walk down a side street.[141]

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[137] However, we did note in Chapter 1 that Webb used her covertness as a source of tension in her research into a work group. See above, p.57.
[138] Patrick, *A Glasgow Gang*, p.17.
[139] Sandford, *Down*, p.9.
[140] Patrick, *A Glasgow Gang*, pp.41-42. Emphasis in original.
[141] Ibid., p.59.

Page also recalled occasions when he felt vulnerable to exposure. In a Social Security office in Hastings, for example, a member of staff greeted Page with the phrase 'I know you' and, since he 'had been involved in several broadcasts shortly before starting out' on tramp, Page worried that he had been recognised but he managed to throw the officer off the scent.[142] Even more dramatic was the occasion in a Salvation Army hostel in London, when a 'silver-haired, red-faced dosser' with 'cold grey eyes' appeared to recognise Page but could not place him. Page recognised the man from having dealt with him in his capacity as a benefits officer two or three years previously.[143]

Language and Imagery in 1970s Non-Academic Covert Ethnography

The language used by 1970s non-academic covert ethnographers was significant in three ways. First, we can look at the way the ethnographers represented themselves in their accounts of covert research. Second, we can explore how the ethnographers accommodated the speech and vocabulary used by their research subjects in their accounts. Third, we can investigate the imagery used by ethnographers to describe the social settings they researched. All four covert ethnographers, Sandford, Toynbee, Patrick and Page, referred to themselves in the first person throughout their ethnographic accounts. This contrasts to the way in which their 1970s academic counterparts referred to themselves in their work. It was demonstrated that, although academic ethnographers used the first person, they did so in a carefully managed way, generally concentrating the use of 'I' or 'we' in demarcated sections of their text. In the conceptual heart of their work, the analytical account of the subject matter, the academic ethnographers tended to retreat into an impersonal style of writing which we do not find in the non-academic ethnographies of the 1970s.

The way in which the speech and vocabulary of research subjects is accommodated in an ethnographic account is significant because it maps out, in a sense, the social distance between the researcher and the researched. The extent to which the researched require translation is symbolic of their 'otherness' and representative of the extent to which their world is alien to that of the researcher. In fact, language proved to be one of Patrick's biggest stumbling blocks during the course of his research. Despite being a Glaswegian and having had experience of working with approved schoolboys, he initially found it very difficult to follow the Young Team's conversations as a covert participant observer and frequently had to seek clarification from Tim during teaching time regarding the meaning

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[142] Page, *Down*, pp.46-47.
[143] Ibid., p.65.

of certain terms.[144] One issue which Patrick highlighted was the difficulty he experienced in mirroring the language and accent used by his research subjects.[145] To distract attention from any mistakes he might make, Patrick 'took to swearing' and his rule of thumb became '[w]hen in doubt, say "fuck"'.[146] Nevertheless, mistakes occurred.[147]

Patrick was acutely aware of the difficulty that the vocabulary used amongst Glasgow gang members might pose for his readers and he provided a substantial glossary to allow translation into English.[148] Sandford and Page did not go so far as to include glossaries but they did translate certain key terms used by the homeless for the benefit of their readers. For example, Sandford footnoted his first paragraph in order to provide definitions of such terms as 'kiphouse', 'spike' and 'dosser'.[149] It is interesting to note that terms which Page had initially translated for his readership are later used unselfconsciously in the text, without translation, such as the term 'a straight', referring to a normal cigarette as opposed to 'a joint', a cigarette made with cannabis.[150] Toynbee, however, saw no need to provide such translations, perhaps because the work cultures she explored had less specialised vocabularies than existed amongst the homeless and Glaswegian gang members.

Toynbee, Patrick and Page all quoted the direct speech of their research subjects fairly frequently during the course of their ethnographic accounts. Toynbee quoted her research subjects and included their use of swear words but not dialect or regional accent.[151] Page, on the other hand, noted that he edited out much of the swearing from his account of his fellow dossers' speech but, like Toynbee, his supposed direct quotes contained no trace of regional accent or dialect.[152] For example, Page recorded this conversation with 'Frenchy', a tramp in Brighton:

> 'How do you manage here,' I asked him, 'and where do you sleep?' 'Underneath the boats on the other side of the pier,' he replied. 'They keep you dry and are quite warm. We've got a great scene going on here: plenty of food, plenty of women and plenty of acid [LSD] and shit [cannabis] if you want it. There's even dope [hard drugs such as heroin, cocaine and morphine] down here

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[144] Patrick, *A Glasgow Gang*, pp.15 & 32-33, 37.

[145] Ibid., p.27.

[146] Ibid., pp.51-52.

[147] For example, when Patrick and the rest of the Young Team are on their way to meet some girls before heading to a dance hall, they encounter some less than charming policemen. The police asked Patrick if he was with the others and he replied "yes" to the annoyance of Tim who informed Patrick that only snobs say "yes" and that "aye" would have been the culturally appropriate answer. Patrick, *A Glasgow Gang*, pp.52-53.

[148] Ibid., pp.230-237. One reviewer who found Patrick's account of gang life generally depressing wrote that it was enlivened with some humour, notably 'a useful small glossary of Glaswegian.' Terence Morris, 'Poverty culture', a review of *A Glasgow Gang Observed* by James Patrick, *Times Literary Supplement*, May 18th 1973, Issue 3715, p.547.

[149] Sandford, *Down*, p.9.

[150] Page, *Down*, pp.16 & 24.

[151] For example, Toynbee quoted a fellow WRAC who she described as a 'short and violent' Scottish woman swearing, but did not indicate a Scottish accent in the quote. Toynbee, *A Working Life*, p.84.

[152] Page, *Down*, p.9.

sometimes.' 'What do you do for money?' 'That's no problem. If you don't work you beg and if you don't beg you steal and some get assistance [Supplementary Benefit], it's all pretty easy. If you get depressed go up to the spike [Brighton Reception Centre] where you can get food and a good clean up. I went there last week.' It all sounded so simple.[153]

It is noticeable that both Page and Frenchy's speech are free from dialect but that Page has translated terms some of his readers might struggle with in square brackets. Patrick, on the other hand, committed to allowing his research subjects to speak for themselves, but noted that this presented issues of obscenity and unintelligibility. He also recognised that reflecting the use of a Glaswegian accent and gang member vocabulary in his quotations of direct speech may have made whole pages of his account appear to be 'extracts from "Oor Wullie" or "The Broons"...but such is the dialect'.[154] Important issues about the researcher's conceptualisation of the researched are raised by the researcher's decision to reflect the regional accent of their research subjects or not. It could be argued that including the regional accent used by research subjects preserves the authenticity and legitimacy of the ethnographic encounter and simply reflects the social distance and otherness of the researched from the researcher and, by extension, the reader. On the other hand, it could be suggested that presenting the dialogue of the researched in regional dialect serves to exaggerate and even exacerbate the social distance between the researched and researcher and even, if taken to extremes, portrays the researched as comic figures.

In chapter 1, we noted that those who have studied the Victorian and Edwardian phenomenon of social exploration suggested that the social explorer's use of imagery and rhetoric was one of the defining features of the genre. We went on to explore how such imagery manifested itself in our examples of late Victorian and Edwardian covert ethnography, especially those which dealt with vagrancy, and commented on its significance. In Page and Sandford's 1970's covert ethnographic accounts of homelessness, this imagery appears to resurface. Page described the tramps he encounters inhabiting a 'wilderness' and a 'vacuum' which existed in 'the shadows of our own' culture.[155] We also witness a re-emergence of the vocabulary of a downward journey being taken by the ethnographer, which is particularly reminiscent of late Victorian and Edwardian social exploration. The name of Page's account, *Down among the Dossers*, is significant given the connotations of a downward journey it contains and he later referred to his research project into vagrancy as a 'journey into the seamy sub-strata of society'.[156] Similarly, in his introduction Sandford explained how he had 'descended into the bilges of

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[153] Ibid., pp.13-14.
[154] Patrick, *A Glasgow Gang*, p.16.
[155] Page, *Down*, pp.9 &40.
[156] Ibid., p.144.

society' and described the world he observed, sometimes covertly, as being 'down in the abyss' or the 'nether regions'.[157] The similarities in imagery are just one of the ways in which covert accounts of homelessness produced between the 1880s and the 1970s can be compared and a more detailed discussion of this will follow.

Advocacy and Empathy in 1970s Non-Academic Covert Ethnography

It has been established that reformism was thought to be characteristic of amateur social research and may have been a barrier to the development of academic sociology in Britain.[158] Although Marshall's attempt to promote empirical and problem-centred research amongst post-war sociologists met with some success, the academic distaste for advocacy continued.[159] Patrick objected to the 'aloofness of most researchers to action programmes' whilst recognising that it was 'unscientific to move directly from descriptive to prescriptive writing'.[160] Nevertheless, he wrote:

> The constant pressure for solutions from people still working with delinquents, and the growing demand for a greater social return from academic research have forced me to add some ideas, however unoriginal and banal, on prevention and treatment.[161]

It should be remembered that Patrick worked in the approved school system before, during and after his covert research project into gang culture, a factor which affected the circumstances and behaviour of many of the pupils in his care. It is therefore unsurprising that Patrick would apply lessons learned in the field to his other role as a teacher.[162] In fact, despite his awareness of the un-academic nature of reformism, Patrick devoted his final chapter to 'treatment and prevention' and other advocatory sentiments were expressed elsewhere in his account.[163] Patrick abhorred the violence indulged in by the gang members but he was empathetic towards them and even identified with them to an extent.[164] He had set out to present their lives in a spirit of 'appreciation and empathy' and, during the course of his covert research, he became 'quite fond' of Tim, his main contact in the gang, and regretted that they had not stayed in touch when Tim left the school.[165]

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[157] Sandford, *Down*, p.9.
[158] See above, pp.68-75. Raymond Kent, *A History of British Empirical Sociology*, (Aldershot: Gower, 1981), pp.8-9; Mark Freeman, "Journeys into Poverty Kingdom': Complete Participation and the British Vagrant, 1866-1914', *History Workshop Journal*, Number 52, (2001), pp.103-104.
[159] See above, p.135.
[160] Patrick, *A Glasgow Gang*, p.209.
[161] Ibid..
[162] In the light of Patrick's covert research, the system of leave was changed for pupils known to be active gang members at the school where he worked. Ibid., p.144.
[163] Ibid., pp.208-229. At the end of his chapter about the historiography of British research into juvenile delinquency, Patrick called for 'a radical new policy of regional development' to bring jobs, investment, housing and other facilities to west central Scotland. Patrick, *A Glasgow Gang*, p.171.
[164] Patrick, *A Glasgow Gang*, p.14.
[165] Ibid., pp.141-142.

Page was nowhere near as 'fond' of his research subjects as Patrick was, but he was even more insistently advocatory regarding their treatment. Page was equally critical of the state and of 'empire-building' homelessness charities, which he viewed as having a vested interest in the problem of homelessness.[166] As a result of his 'voluntary vagrancy', Page advocated a modest upgrading of the facilities available to the homeless.[167] He suggested the state take the lead in quantifying the problem before providing specialised care for those who needed and deserved it. Meanwhile, Page advocated compulsory withdrawal from alcohol and drugs as a precondition of state support and a harsher stance to be taken against the 'workshy' homeless.[168]

As noted earlier, Page's research was not undertaken in the same empathetic spirit usually associated with non-academic covert ethnographers. Nevertheless, his covert experience of homelessness did appear to have an emotional legacy in that when he subsequently encountered homeless men, he wondered if they felt 'depressed and isolated' as he had done.[169] Since experiencing the hunger and insecurity concomitant with homelessness himself, Page had begun giving to those openly begging who he believed to be in genuine need - something he would never have done previously.[170]

However, Page remained adamant that not every dosser was 'a downtrodden victim of the capitalist system and a reject of the affluent society' and he was venomously critical of those writers who portrayed the situation in such a light:

> These do-gooders seem to write with one eye on their subject and the other on posterity, as if they wished to be remembered as compassionate and farsighted intellectuals, rather than recorders of the actual facts.[171]

Page made no direct mention of Sandford in his account at all, despite the fact that Sandford had published *Down and Out in Britain* just two years before Page published *Down among the Dossers*, and in the same year Page had been conducting his fieldwork. Nonetheless, given their markedly contrasting perspectives on the homeless, Page's objection to posterity-fixated 'do-gooders' could clearly have been aimed at Sandford. Sandford appeared to have an unwavering admiration for all of the homeless people he encountered. He clearly felt a spiritual affinity with the homeless and, in a sense, revered their way of life, arguing that in many ways they 'follow the Christian tenets on which our

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[166] Page, *Down*, pp.109-110.
[167] Ibid., p.127.
[168] Ibid., pp.135, 138, 138-140 & 142.
[169] Ibid., p.111.
[170] Ibid., p.144.
[171] Ibid., p.111.

society is supposed to be based better than us, the "successful" ones'.[172] When Sandford and a companion went to a derelict house occupied by homeless people, he found, 'huddled by the light of a candle and the dancing flames, grimed, seamy, basic people, the sort that Rowlandson painted', demonstrating the extent to which he romanticised their plight.[173]

One reviewer commented on the 'polemical' and 'preachy' tone of Sandford's work and there is no doubt that he hoped to achieve some good through his publication, noting in his conclusion that

> [t]here is need for informative and campaigning journalism in these times. Shelley used that fine phrase of writers, 'The unacknowledged legislators of mankind', and I have always felt that in these words were contained perhaps the highest aspirations that a writer can have.[174]

His ethnographic account was unapologetically reformist and he argued that his book, and his plays, had been intended as 'social not literary documents' that were primarily intended to spur the audience 'into thinking and action'.[175] Sandford's connection with and support of charitable institutions dealing with the homeless, including the Cyrenians, Shelter and the Simon Community, was clear.[176] Sandford advocated the provision of more small and permissive hostels, each 'with a resident father figure' and with minimal, if any, state input.[177] He encouraged readers who agreed with his philosophy to take direct action, to contact one of the charities approved and listed in his book and to offer their help.[178]

Toynbee's position in relation to reformism is more ambiguous than that of the other 1970s non-academic ethnographers. This could, of course, be because Toynbee was exploring work cultures rather than more emotive issues; she was encountering and writing about very ordinary people rather than a vulnerable section of society. Nevertheless, *A Working Life* was written in a tone which suggested Toynbee was troubled by the nature of the work so many people were engaged in and the editor suggested that Toynbee's account made the reader 'stop and look at a way of life we seem to accept without question'.[179] Toynbee did make some specific criticisms of the maternity ward she worked in related to

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[172] Sandford, *Down*, p.32.
[173] Ibid., p.24.
[174] Chris Dunkley, 'Edna, the Inebriate Woman', *Times*, 22nd October 1971, http://callisto10.ggimg.com/doc/LT/WrapPDF=contentSet=LT=recordID=0FFO-1971-OCT22-009-F.pdf [accessed 31st March 2010] ; Sandford, *Down*, p.149. Shelley was referring specifically to poets. Percy Bysshe Shelley, *A Defence of Poetry*, Edited by Albert Cook, (Boston, Massachussetts: 1891), p.46.
[175] Sandford, *Down*, p.165.
[176] Ibid., pp.5, 8 & 54.
[177] Ibid., pp. 152 & 153.
[178] Ibid., pp.158 & 174.
[179] Toynbee, *A Working Life*, p.1.

the standards of care, the facilities and staffing.[180] However, she did not discuss any generalised problems or offer any solutions relating to the nature of work in 1970s Britain.

What is surprising given Toynbee's evident fondness of some of her research subjects is the condescending manner in which she couched the social distance between them and herself. Carla, a co-worker in the car plant with whom Toynbee became quite friendly, was aware that Toynbee was a student at Oxford but, according to Toynbee, Carla did not fully grasp the significance of this. She wrote

> In describing my life to her I was, I suppose, anxious not to rub in the differences, but without actually lying I could see my words as they passed from my lips and entered her ears being translated from one class to another.[181]

While working in the soap factory, Toynbee told fellow workers that she was a student from Oxford doing holiday work and noted that they

> had not the remotest conception of what university was. They knew it was important, and almost the only way to better one's way of life...But beyond this fact people tended to imagine university in any way that pleased them.[182]

Dora was one of Toynbee's colleagues in the soap factory and, at the age of fifty, she had only once ventured further than Liverpool from her council house in Birkenhead. In the summer, she liked to visit the park in Birkenhead:

> 'Sometimes in the summer I sit in the park...and the birds are singing and the grass is green and the sun is shining and I think to myself, "Isn't it beautiful?" Now what's the point of going off somewhere else to see the same things?' If she had lived in a cardboard box she'd have said the same thing about the pinhole of light that came through a crack. I stopped worrying about whether I was judging other people's lives by own, not necessarily superior, set of values. Birkenhead park![183]

It is evident that three of the four of the non-academic ethnographers discussed in this chapter had a clear reformist agenda running through their work, a feature which differentiates their accounts from the academic covert ethnographers working during the same era. Toynbee's ethnographic account of 1970s work culture is a notable exception to the apparent rule that non-academic covert ethnographers use their research findings as a platform from which to suggest reform. However, in chapter 1 it was noted that Beatrice Webb did not use her covert ethnography of tailoring sweatshops to promote reform, even though she was engaged in other reformist ventures.[184]

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[180] Ibid., pp.53-54.
[181] Ibid., p.78.
[182] Ibid., pp.100-101.
[183] Toynbee, *A Working Life*, p.100.
[184] See above, pp.68-75.

Sandford and Page: Covert Ethnography of Homelessness since the 1880s

In this thesis, the unifying characteristic of the social research we have explored has been the use of covert participant observation to explore many social groups and contexts in Britain since the 1880s. However, there is no doubt that the covert exploration of vagrancy has a particularly rich heritage and that Page and Sandford were following in a well-established tradition when they decided to become homeless in order to understand homelessness. There are some striking similarities between the accounts of Sandford and Page and earlier covert researchers of homelessness. First, there was the significance of the physical transformation, the disguise used to blend in with the researched, which was particularly dramatic in the case of those researching the destitute. Goldsmid wrote in 1888 that

> My face...is absolutely repulsive by reason of the dirt that covers it. My shirt matches my face, and my coat accords with the appearance of both. Waistcoat and shirt-collar have alike been discarded, and a particularly unclean neckcloth...has taken the place of both. My boots are broken and patched. My hat is a frowsy looking specimen...[185]

In 1971, Sandford wrote

> He gave me heavy and filthy boots, tied with string. He gave me shapeless hideous trousers, and the sort of shirt that has no collar. He gave me a succession of other tattered shirts to wear under this, and string to go round my trousers below the knee to keep out the cold. He helped me to wrap newspaper round myself above the lowest layer of clothing but below the next, explaining that this would serve as an insulation through the nights. He gave me a hole-filled cap. Finally, and most important, he gave me a great and amorphous great coat, and told me to fill its pockets with odds and ends and wear it at all times, even when it was hot. To complete the disguise I let my beard go long, and didn't wash my hands so that, soon, dirt congregated under my nails.[186]

Goldsmid produced his covert account of homelessness eighty-three years before Sandford published his, yet, placing their descriptions of their disguises side by side brings home the stylistic similarities between their texts.

It is also noticeable that covert ethnographers of homelessness, on their numerous visits to workhouses and common lodging houses over the past century, have tended to

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[185] Howard J. Goldsmid, *Dottings of a Dosser, Being Revelations of the Inner Life of Low London Lodging-Houses*, (London: T. Fisher Unwin, 1886), p.12. C.W. Craven, who spent the night in a workhouse in Keighley in 1887 disguised as a tramp, also described his outfit near the beginning of his ethnographic account, including a 'dilapidated coat, specially slashed for the occasion, and a greasy nebbed cap.' C.W. Craven, 'A Night in the Workhouse' in Mark Freeman and Gillian Nelson (eds.), *Vicarious Vagrants: Incognito Social Explorers and the Homeless in England, 1860-1910*, (Lambertville, New Jersey: The True Bill Press, 2008), p.181. See also, for example, Everard Wyrall, *The Spike*, in Mark Freeman and Gillian Nelson (eds.), *Vicarious Vagrants: Incognito Social Explorers and the Homeless in England, 1860-1910*, (Lambertville, New Jersey: The True Bill Press, 2008), p. 232.
[186] Sandford, *Down and Out*, p.24. Page also included a detailed description of the clothes he wore in order to pass as a tramp. Page, *Down*, pp.9-10.

comment on the same aspects of their experience. After his second consecutive night in Camberwell reception centre, Page commented that

> The...procedure followed unchanged: soup, television and the noisy dormitory. Apart from the television it seemed that...the conditions in which dossers are housed and the treatment they receive have hardly changed since workhouse days.[187]

When Page visited the reception centre in Brighton, he was not asked to bathe nor have his clothes disinfected although a sign on the wall informed him that it may be a condition of entry. He was given an 'Oliver Twist style' supper of soup, bread and butter, cheese and tea with which he was not impressed. He described the soup as 'tasteless, like thick hot water, the bread was dry and the tea was sweet and sickly'.[188]

Page also referred to the 'disinfected and strange smelling...pyjamas' provided and the sparsely furnished and dirty appearance of the dormitory. As so often appears to have been the case in reception centres across Britain since the 1880s, Page did not find the atmosphere of 'filth, fetid breath and sweating feet' conducive to sleep, the situation only being aggravated by uninhibited belching, flatulence and coughing throughout the night, as well as some shouting and groaning.[189] In discussing his experience of Camberwell, Page reflected on the same issues that he had discussed in relation to the Brighton reception centre, and that earlier covert ethnographers of the workhouse had also addressed: the encounters with the officials, the inspection of clothes, the bath or shower, the awful food, the sleeping accommodation and the disturbances during the night.[190] He even referred to the work task, a staple feature of the Victorian and Edwardian stay in a workhouse, although he avoided participation in it.[191] Sandford provided similarly familiar recollections of staying in a reception centre.[192]

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[187] Page, *Down*, p.84.

[188] Ibid., p.35. Sandford also described the soup he received as 'watery'. Sandford, *Down*, p.38. Craven could not bring himself to eat the soup in Keighley workhouse in 1887. Craven, 'A Night', p.188. When F.G. Wallace-Goodbody made a covert visit to a workhouse casual ward in 1883, he was given bread which, judging by its taste, had been made from 'sawdust' and was 'singularly provocative of thirst'. F.G. Wallace-Goodbody, 'The Tramp's Haven', in Mark Freeman and Gillian Nelson (eds.), *Vicarious Vagrants: Incognito Social Explorers and the Homeless in England, 1860-1910*, (Lambertville, New Jersey: The True Bill Press, 2008), p.131.

[189] Page, *Down*, p.35. Widdup did not sleep well in Burnley workhouse casual ward in 1894 on account of, amongst other factors, the smell caused by the vomit on the straw which he had been given as bedding and 'the violent coughing of the casuals'. J.R. Widdup, *The Casual Ward System: Its Horrors and Atrocities. Being an Account of a Night in the Burnley Casual Ward, Disguised as a Tramp*, in Mark Freeman and Gillian Nelson (eds.), *Vicarious Vagrants: Incognito Social Explorers and the Homeless in England, 1860-1910*, (Lambertville, New Jersey: The True Bill Press, 2008), pp. 195-197. In 1906, Higgs also spent a restless night in 'A Northern Tramp Ward'. Higgs, *Glimpses*, pp.146-149.

[190] Page, *Down*, pp.77, 80-81, 85.

[191] Ibid., p.35. Higgs was not so lucky and in one workhouse casual ward she was required to pick oakum. Higgs, *Glimpses*, pp.154-155. Widdup was made to saw firewood with his fellow casuals after his night in Burnley workhouse. Widdup, *The Casual*, pp.201-202.

[192] Sandford, *Down*, p.38. Goldsmid, Page and Sandford also produced comparable accounts of nights spent in common lodging houses and it is particularly noticeable that both Goldsmid and Page took the time to describe the sleeping arrangements in detail, including the proximity of the beds to each other and the ineffective partitioning of the sleeping areas. Goldsmid, *Dottings*, chapters 2, 3, 5, 6, 8 & 9; Page, *Down*, p.53; Sandford, *Down*, pp.55-56.

There is some evidence to suggest that Page and Sandford were conscious that they were contributing to a substantial body of work on homelessness, and that they were aware of the parallels between their work and that of earlier generations of covert ethnographers. When Page was criticising the tendency of 'pamphleteers' to play down the number of undeserving vagrants amongst the homeless population, he invoked examples from earlier research into homelessness, such as George Orwell's *Down and Out in Paris and London* and Laurie Lee's *As I Walked Out One Midsummer Morning*, noting that these both recorded the existence of 'workshy' vagrants and 'professional tramps'.[193] Sandford also referred to Orwell's first-hand experience of accommodation for the homeless noting that the boarding houses Orwell had described in the 1930s 'where a rope is hung across the wall and the men lean across it, sleeping, and in the morning it is dropped and the men fall to the floor' would have suited the tramps he encountered in the 1970s very well.[194] Sandford's connection with an earlier generation of social research was heightened by his reference to William Booth and *In Darkest England and the Way Out*. Reflecting on the scene before him in 1970s London, Sandford wrote:

> In the nineteenth century General Booth rode over Vauxhall Bridge and was horrified to find men sleeping out in the open to the South of the river. This was one of the things which led him to found the Salvation Army. Now he would not need to cross the river to see them.[195]

Even the language used by Sandford is occasionally reminiscent of that used in much earlier Victorian observational accounts of homelessness. For example, the full title of the first part of *Down and Out in Britain* was 'Sleeping Rough, and Days and Nights in the Kiphouse: My introduction to the World of the Dosser and the Skipper'. Similarly, his conclusion was dramatically entitled 'The Violence of the State, and is there Any Solution?' These verbose titles were similar in style to the titles and chapter headings used in late Victorian and Edwardian covert ethnographies. For example, when J.R. Widdup produced an account of his covert research undertaken in the 1890s, he gave it the title *The Casual Ward System: Its Horrors and Atrocities; Being an Account of a Night in the Burnley Casual Ward, Disguised as a Tramp*.[196]

Despite his apparent awareness of the literature about homelessness in Britain, Page persisted in using his covert research as an opportunity to track down what he referred to as 'the old variety of romantic tramp', 'the true tramp' or the 'gentleman of the road'. Page was convinced that such a sub-set of the homeless had existed up until just a few years

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[193] Page, *Down*, pp.110-111. George Orwell, *Down and Out in Paris and London*, (London: Victor Gollancz, 1933); Laurie Lee, *As I Walked Out one Midsummer's Morning*, (Harmondsworth: Penguin, 1971).
[194] Sandford, *Down*, p.33.
[195] Ibid., p.16.
[196] J.R. Widdup, *The Casual*, pp.189-205.

previously, 'walking through the countryside at his own pace, surrounded by nature, peace and tranquillity'.[197] Although Page had encountered a few 'highbrow' tramps who had rejected society consciously on political grounds, they had been isolated examples and had not been suitably rural.[198] Page concluded that

> The freedom of the traditional tramp was...shown to have vanished; lost in an earlier generation and buried by time and change.
> The country lanes, through which to wander, have mostly gone, replaced by modern arterial roads, lacerating the countryside: the pulsating life-lines of industry, spilling over with noise, speed and often chaos.
> Brewing up tea by the roadside on a small wood fire, sinking into the welcoming subaqueous depths of cool sweet smelling grass, to sleep on a hot day, or just sitting on a fence watching the world go by, are all virtually unknown.
> The tramp of the seventies, has, like the rest of society, been caught up in a modern day rush to nowhere in particular. Hectically hitch-hiking from one reception centre to the next, from one squalid shelter to another, or hurrying on aching feet to beat the soup van to its next stopping point.[199]

For Page, the death of the 'gentleman of the road' was bound up with ever-encroaching modernity and what he saw as the destruction of his beloved rural environment. What is significant, however, is that this romantic figure of the 'gentleman of the road' finding solace in a rural haven does not resonate with other, earlier accounts of homelessness based on covert participant observation. In the 1880s, the well-known covert ethnographer Greenwood undertook a tramp in search of

> …free rovers, who resent and despise the trammels of civilisation and the responsibilities thereto pertaining…taking, just as it may happen, the lodging under a roof house and in a bed, or beneath a hedge, with the dewy grass for a cold counterpane, and all for the pleasure of indulging in unlimited liberty…[200]

However, none of the characters Greenwood encountered on his tramp through town and countryside approximated a 'free rover' or a 'gentleman of the road'.[201] Page's nostalgia appears to be based on a mythical past, on an ideal of romantic tramping which did not exist.

Blurring the Boundaries: Patrick's Research and Academia

Previous chapters have demonstrated some of the key differences between no-academic and academic examples of covert ethnography. Patrick's *A Glasgow Gang Observed* provides us with an example of a covert ethnography which arguably straddles this boundary between academic and non-academic research. On the one hand, Patrick

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[197] Page, *Down*, p.53.
[198] Ibid., pp.113-117.
[199] Ibid., pp.145-146.
[200] James Greenwood, *On Tramp*, in Mark Freeman and Gillian Nelson (eds.), *Vicarious Vagrants: Incognito Social Explorers and the Homeless in England, 1860-1910*, (Lambertville, New Jersey: The True Bill Press, 2008), pp.141-142.
[201] Greenwood, *On Tramp*, pp.141-180.

demonstrated his keen awareness of academic research into gangs and juvenile delinquency as well as an understanding of the research methodology from an academic perspective. On the other, Patrick constructed his work in such a way as to keep his account of his experience in the field quite undiluted by academic entanglements, he was critical of traditional academic research into gangs and his account was full of the drama and humour we would associate with the work of some of the earlier social explorers, such as Howard Goldsmid and Hugh Massingham.

The Academic Aspects of Patrick's Research

Despite its doubtful academic status, Patrick's work on gangs has been referred to by a number of academics in their own work.[202] It is revealed in the preface to *A Glasgow Gang Observed* that Stan Cohen and David Downes, two respected British sociologists of deviance, had read and commented upon Patrick's work prior to publication.[203] Furthermore, Patrick demonstrated a considerable engagement with the academic discourse surrounding gangs and delinquency. For example, early on in his account, Patrick referred to an American academic theory of delinquency which suggested that gang welfare was most likely to arise in disorganised slums populated by transient low-level criminals.[204] On the contrary, Patrick found Maryhill to be an integrated and cohesive community and cited other examples of empirical academic research into gangs and slums which formed part of the local social structure.[205]

As well as his engagement with the academic literature on gangs on a theoretical level, Patrick also noted similarities between his covert observations of incidents in the field and those witnessed by other academic researchers studying gang behaviour. For example, one day in the pub, the Young Team reminisced about an occasion when the gang had wrestled the trousers off of one member and thrown them from boy to boy in the street. After recounting this anecdote, Patrick mentioned that he had read about a similar incident in the American academic Walter Bernstein's 'The Cherubs are Rumbling', a late 1960s account of gang subculture.[206] Similarly, Patrick noticed that prowess on the pitch during the football match between the Young Team and the Maryhill Fleet distorted the

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[202] See, for example: Tom Kitwood, 'Cognition and emotion in the psychology of human values', *Oxford Review of Education*, Volume 10, Number 3, (1984), pp.293-301; Michael Smith, 'Hockey violence: a test of the violent subculture hypothesis', *Social Problems,* Volume 27, Number 2, (1979), pp.235-247.
[203] Patrick, *A Glasgow Gang*, p.10.
[204] Ibid., p.25; Richard Cloward, *Delinquency and Opportunity: a Theory of Delinquent Gangs*, (London: Routledge, 1961).
[205] Patrick, *A Glasgow Gang*, p.25; Gerald Suttles, *The Social Order of the Slum: Ethnicity and Territory in the Inner City*, (London: University of Chicago Press, 1968).
[206] Patrick, *A Glasgow Gang*, p.51; Walter Bernstein, 'The Cherubs are Rumbling', James Short (ed.), *Gang Delinquency and Delinquent Subcultures*, (Chicago: Harper and Row, 1968), pp.22-52.

hierarchy of the gang members established off the pitch. He compared this to Lewis Yablonsky's academic research which revealed that involving a New York juvenile gang in baseball altered the gang's dynamic.[207] In addition to his engagement with the academic literature on the subject matter of his research, Patrick also signalled his awareness of the academic literature on his chosen research methodology, recognising participant observation as a social scientific research method.[208]

The Non-Academic Aspects of Patrick's Research

The extent to which Patrick's ethnographic work should be classified as academic is difficult to decide. Features of Patrick's work which align it with other non-academic examples of covert ethnography have been discussed above such as his advocacy of reform and his focus on narrative. There are, however, some other points to note which add weight to the notion that Patrick's work should not be classified as an academic example of covert ethnography. The first point to note is that Patrick was not a sociologist or a social scientist; he described himself as having qualifications in the Classics.[209]

Secondly, commentators on Patrick's work have tended to marginalise the academic status of his work, especially the content of his monograph which was based on his covert research. The academic reviewers of *A Glasgow Gang Observed* did not appear to be impressed with Patrick's fieldwork and analysis of his experiences with the Young Team. They were, however, much more enthusiastic about his review of the academic literature. For example, in the *Probation Journal*, it was noted rather dismissively that the book arose 'out of the author's weekends over a period of four months spent "in disguise" with a particular gang' and was, as a result, 'full of flavour' and made 'interesting reading'. However, it was Patrick's discussion of earlier literature on gangs which the reviewer found 'useful'.[210] Similarly, Mary Wilson reviewed *A Glasgow Gang Observed* for the *British Journal of Criminology* and described the text based on his undercover fieldwork as 'a very lively description of his encounters' but was disappointed that Patrick had not gone beyond 'the traditional concerns of other writers'. Again, it was Patrick's 'concise review of the research literature and some interesting and original comments on the history of Glasgow gangs' which attracted Wilson's praise.[211] In an article concerning Glasgow gangs, the social historian Andrew Davies suggested that the only previous academic

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[207] Patrick, *A Glasgow Gang*, p.62; Lewis Yablonsky, *The Violent Gang*, (Harmondsworth: Penguin, 1967).
[208] Patrick, *A Glasgow Gang*, p.14.
[209] Ibid., p.213.
[210] D.A.M., Review of *A Glasgow Gang Observed* by James Patrick, *Probation Journal*, Volume 20, Number 3, (1973), p.92.
[211] Wilson, Review, p.411.

treatment of the subject had been Murray's *The Old Firm*. Davies mentioned Patrick's work in a footnote, but it was listed as an example of '[f]urther accounts', and not an academic treatment of the gang issue.[212]

Patrick himself suggested that *A Glasgow Gang Observed* was aimed at the 'general reader' rather than an academic readership as the 'problem described' was of 'more than academic importance'.[213] Professor Terence Morris, writing in the *Times Literary Supplement,* agreed, describing *A Glasgow Gang Observed* as a 'very much worthwhile' book from which much could be learnt 'not so much by sociologists who will already be familiar with the academic literature to which Mr Patrick refers as by the general reader who may be unaware of what gang life is really like'.[214] Patrick's impatience with and criticisms of academic sociology suggest that he wanted distance himself from the academic genre. For example, he described and then disagreed with the idea promoted by the established sociologist Stanley Cohen that delinquent theft was non-utilitarian. In contrast, Patrick noted that his Young Team indulged in utilitarian and non-utilitarian theft, concluding that they 'committed thefts as the need and mood of the moment took them without regard for the neat classifications of sociologists'.[215]

Patrick was especially critical of those academic accounts which were not based on any observational data. He suggested that the strongest impression he gained from his review of the academic literature on gang delinquency after he had completed his fieldwork was that 'many theoretical propositions have the stamp and outlook of the outsider, especially of the middle-class observer, who classifies delinquent behaviour from a safe distance'.[216] Other, more definitively non-academic, 1970s ethnographers were also critical of professional sociologists. Toynbee, for example, after explaining how depressed, tired and bored she had become after only a few days of working in the cake factory, criticised sociologists. She wrote that

> [s]ociologists who examine the leisure problem might find the answer lies in work. What will people do with more leisure time? Look how they waste it now in bingo halls, the palais, the pub, and down the bowling alley...Such William Morris thoughts about the worker who loves to weave and paint of an evening are a long way from how things really happen.[217]

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[212] Andrew Davies, 'Street gangs, crime and policing in Glasgow during the 1930s: the case of the Beehive Boys', *Social History*, Volume 23, Number 3, (1998), p.251. Bill Murray, *The Old Firm: Sectarianism, Sport and Society in Scotland*, (Edinburgh: J. Donald Publishers, 1984).
[213] Patrick, *A Glasgow Gang*, p.9.
[214] Morris, 'Poverty', p.547.
[215] Patrick, *A Glasgow Gang*, pp.199-200.
[216] Ibid., p.197.
[217] Toynbee, *A Working Life*, p.40.

Page was also critical of the 'sociological jargon' used in the discussion of homelessness but he did not appear to associate such 'jargon' with university-based academics. When Page vented his frustration at the truth of the matter, as he saw it, being distorted 'by a haze of jargon and sociological claptrap', he seems to have been referring to the accounts produced by homelessness pressure groups and those sympathetic to the plight of the homeless.[218] Page noted that a pamphlet published by the Cyrenians, the homelessness charity so beloved of Sandford, was 'full of trendy sociological jargon, which when analysed is virtually meaningless'.[219] It is difficult to know what to make of this conflation of academic sociology and the work of pressure groups. Perhaps we can explain it using a point made by Roger Homan when reflecting on his academic sociological covert investigation of Pentecostalism. Homan suggested that sociology was the academic discipline most readily associated with the political left.[220] It is possible that Page made the same connection, associating the language used by left-leaning charities with academic sociology.

Ethics and Non-Academic Covert Research in the 1970s

In chapter 3, we discussed the emergence of ethical concern in academic circles regarding the use of covert research methods and how covert ethnographers, such as Spencer and Mumford, dealt with ethical issues in their work.[221] In chapter 4, we continued our discussion of ethics, noting the formal stance taken by the British Sociological Association regarding the use of covertness in 1970 and the censure some academic covert ethnographers, such as Homan, were subjected to.[222] The covert ethnographers we have focused on in this chapter were, except in the case of Patrick, clearly working outwith the boundaries of academia. However, ethical problems associated with covertness did feature in the ethnographic accounts of Toynbee and Page. In Sandford's *Down and Out in Britain*, however, there was no mention of the ethical problems associated with undercover research. This could be because Sandford felt such an affinity for the social group he was researching covertly, and was so focused on his humanitarian motivations, that he did not consider his covertness to pose any significant ethical questions.

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[218] Page, *Down*, p.111.
[219] Ibid., p.133. Again in his concluding chapter, Page associated 'trendy verbiage' with 'sociologists and psychiatrists'. Page, *Down*, p.142.
[220] Roger Homan, 'The Ethics of Covert Methods', *British Journal of Sociology*, (1980), Volume 31, Number 1, p.48; See above, p.183.
[221] See above, pp.158-170.
[222] See above, pp.172-177 & 202-209.

When she was working in the car plant, Toynbee became especially close to Carla, a young woman who worked in the paint shop, and they spent their breaks and any spare time they had together.[223] Carla was especially concerned that Toynbee was lodging in such a poverty-stricken area of the city and brought her sandwiches.[224] When Toynbee was leaving the car plant, Carla made her promise to write but Toynbee felt too guilty to do so. She wrote:

> While I was there, working in the factory, and living in digs, it was easy to believe my own account of myself and to make friends. But when I got back to my own life, I couldn't write to Carla and tell her I had lied, and had been talking to her only so as to write about her. I couldn't tell her that I had only been working there so as to write about it, that I hadn't needed to work there at all, that I had only been doing out of interest what she was forced to do for the whole of her life.[225]

In her review of Toynbee's *A Working Life*, Margaret Lassell noted the ethical dilemma that Toynbee faced as a covert researcher and that the use of deception for the purposes of research troubled her. Toynbee's work raised 'the question of how far an observer is justified in pretending to be something different, in order to get closer to those she is studying'. More than this, however, Lassell sympathised with Toynbee, claiming to have had a similar experience in her role as an academic researcher and noted that 'the feeling of guilt is never quite argued away'.[226] Toynbee's concerns echoed those of other earlier academic covert ethnographers, such as King and Mumford, who both regretted having to deceive their research subjects with whom they became friendly.[227] Nevertheless, Lassell concluded that 'a book as excellent as *A Working Life* is a powerful justification of this method of research'.[228]

Page's awareness of the ethically problematic nature of covert research was not as well developed as that of Toynbee. However, he did mention a few issues which related the difficult ethical situations covert researchers can find themselves in. Page was reluctant to lie for the sake of his research and reflected that he had managed to avoid doing so as far as possible. At Camberwell reception centre, under the questioning of an official, Page noted that

> I was reluctant to give the name and address of my next of kin because I had no wish to make a false statement, for apart from my false name, most of what I had been saying contained quite a large proportion of truth, but was given in such a way as to be misleading.[229]

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[223] Toynbee, *A Working Life*, p.66.
[224] Ibid., pp.77-78.
[225] Ibid., p.78.
[226] Margaret Lassell used the research method of overt participant observation. She lodged with a family for an extended period of time and kept a journal which formed the basis of her publication. Margaret Lassell, *Wellington Road,* (London: Routledge, 1962), p.5.
[227] See above, pp.150-151.
[228] Margaret Lassell, 'A Stupid Boring Job Makes a Stupid Boring Mind' Review of *A Working Life* by Polly Toynbee, the *Times*, July 26th 1971, p.8.
[229] Page, *Down*, pp.78-79.

On one occasion, in the toilets of a café, one of Page's fellow tramps, Eddie, took a 'tab' of LSD (lysergic acid diethylamide) and another tramp, Keith, split a second 'tab' between himself and Page. Page found himself in a dilemma and considered taking the drug so as not to lose the confidence of Eddie and Keith, and also out of curiosity. However, he decided against it and later sold his half tab to Eddie for 15p.[230] Arguably, it would have been less ethically dubious for Page to have taken the drug than to have profited from its sale to a vulnerable person.

Involvement with illegal drugs was just one of the many ethical issues which surfaced during Patrick's research into Glasgow's gang culture. It has already been established that Patrick's research was on the boundary between academic and non-academic research and that he displayed an awareness of the academic literature not only on gangs but also on research methodology. It would be safe to assume, therefore, that Patrick would be aware of the debate surrounding the use of covertness in academic research. Patrick wrote that 'legal reasons' prevented him from providing a full and frank account of his relationship with the gang or the police in his role as a gang member and that as a result of this, discussion of 'ethical and methodological problems associated with participant observation' had to be postponed.[231] In the case of Patrick's research, it is important to remember that, whilst Tim was his key informant for research purposes, Patrick was simultaneously Tim's teacher at an approved school. Patrick was in a position of authority over Tim and had a duty of care to him and this makes Patrick's apparent acceptance of and participation in certain behaviours, in this case drug taking, particularly problematic. Patrick wrote:

> Only after I had been involved with the gang for three months was Tim prepared to talk to me about drugs; and it took him another month to admit to having taken them in the past. In fact, he was bringing them into the approved school when he returned from leave and taking them in small doses – to counteract boredom, he told me. Appearing to condone this practice was part of the price I had to pay for Tim's silence about my own activities; for, although making clear my reasons for condemning such a habit, my hands were tied as far as informing the authorities was concerned. I also believed that Tim's confession should be accorded the confidentiality normally extended to privileged information.[232]

At a house party attended by Tim and Patrick, the issue of drugs arose again. Tim was given some pills and, having taken some himself, passed them on to Patrick who put two in his mouth and 'drank deeply from a can of lager'. Patrick attempted to spit the pills out as

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[230] Ibid., pp.29 & 37.
[231] Patrick, *A Glasgow Gang*, p.9. Unfortunately, no such discussion appears to have been published.
[232] Ibid., p.124.

soon as possible but by then he had swallowed one and the other had dissolved in his mouth.[233]

Of course, covertly joining a gang meant that Patrick witnessed various criminal offences, including violent ones, being committed and failed to inform the police. Most surprising, however, were the ethically dubious aspects of Patrick's research in which he engaged through choice. On more than one occasion, Patrick made use of his position as an approved school teacher to access confidential information relating to members of the gang scene in Glasgow.[234] In his televised criticism of Patrick's work, the Assistant Chief Constable of Glasgow pointed out that Tim would be badly affected by his relationship with Patrick if his identity should be found out, but this does not appear to have happened.[235] Patrick noted that once his undercover research had come to an end, he was still in contact with Tim in his capacity as a teacher at the approved school. Tim continued to behave badly, was in trouble with the police, and ended up in borstal. Patrick's account of his field work ended rather sadly:

> My connection with him, perhaps to my discredit, is now non-existent. Though horrified by his brutalities, I became quite fond of him, and I presumed the feeling was reciprocated. Nevertheless, since he left the school he has never telephoned, visited, or written to me, although I not only invited but encouraged him to do so – whether he was in trouble or not.[236]

One reviewer of Patrick's ethnographic research did make fleeting reference to the existence of an ethical debate relating to the use of subterfuge by researchers but did not explore the issue in any depth.[237]

There is one ethical issue which two of our 1970s non-academic ethnographers were challenged on which was not raised in relation to the work of their academic counterparts, and that was the question of financial gain through covert research. Sandford was saddened by the suggestion made by some commentators that he had undertaken his project on homelessness for gain, noting that the 'amount of research necessary' meant that his fee was more than exceeded as he had conducted 'first hand research...in fields where there are few written sources'.[238] When Patrick appeared on the BBC's Current Account show in 1973 alongside William Gray, the Lord Provost of Glasgow, it was suggested that his attempts at anonymity were 'merely a gimmick to boost the proceeds of the book'.[239]

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[233] Ibid., p.125.
[234] Ibid., pp.38-40 & 50.
[235] 'Profits', *Glasgow Herald*, p.5.
[236] Patrick, *A Glasgow Gang*, pp.141-142.
[237] D.A.M., Review, p.92.
[238] Sandford, *Down and Out*, p.165.
[239] 'Profits', *Glasgow Herald*, p.5.

Patrick offered profits from the sale of *A Glasgow Gang Observed* to the Glasgow Corporation to be used for a charitable purpose to counter such claims.[240] As non-academic social researchers, Sandford, Page, Toynbee and Patrick were not under the same pressure as academic covert researchers to justify their research on ethical grounds in an attempt to defend the credibility of their profession. Nevertheless, their research experience was by no means free from ethical considerations.

Conclusion

This analysis of the work of Toynbee, Sandford, Patrick and Page has reflected the complex nature of the use of covert ethnography in Britain since the 1880s. On the one hand, significant connections can be made between the non-academic covert ethnography of the 1970s and the tradition of covert social exploration. Similarities were found between some late Victorian and Edwardian examples of covert social exploration and the work of some 1970s covert ethnographers, especially Sandford and Page, in terms of reformism and their use of covertness as a dramatic device. Furthermore, particularly in Page's *Down among the Dossers*, there was a consciousness that such covert research into homelessness was part of an established tradition. On the other hand, there is some evidence to suggest that the barriers between the tradition of social exploration and the academic model of covert ethnography were being breached, although perhaps not broken down, in the 1970s. This can be seen in the complex structures of Patrick's and, to a lesser extent, Toynbee's ethnography, which deviated significantly from the narrative structure associated with the tradition of social exploration. Other aspects of Patrick's *A Glasgow Gang Observed* also suggested that, although Patrick was not an academic social scientist, he was aware of and made use of the academic discourse on his research subject, gangs, and his research method, covert ethnography. However, although it was demonstrated that 1970s non-academic covert ethnographers had a limited awareness of the ethical issues related to their research, this did not mirror the extent to which the ethics of covertness had come to dominate academic covert ethnography.

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[240] Ibid.

Conclusion

Working with the existing fragmented historiography, and a broad range of undercover research projects, it has been the aim of this thesis to develop a more comprehensive understanding of the history of covert ethnography between the 1880s and 1980. All practitioners of covert ethnography have been considered as participants in the same broad enterprise, and the essential changes and continuities in the use of ethnography over the period have been mapped. It has been demonstrated in other contexts that methods of social research develop over time in syncopation, dialogue and contestation with each other, and this certainly seems to have been the case with covert ethnography.[1] Between the 1880s and 1980, covert ethnography can be usefully considered to have been simultaneously enmeshed in two practices, social exploration and academic ethnography. During this period, these traditions overlapped and can be considered as different expressions of the same practice, which developed in contestation with other methods of research, particularly survey methodology.

In chapter 1, the complex relationship between covert social exploration and the dominant survey method was explored with reference to the contrasting experiences of Beatrice Webb and Mary Higgs around the turn of the twentieth century.[2] At a time when the survey method was in the ascendancy, Webb chose to marginalise her use of covert ethnography, and in so doing, ensured the recognition of her prominent role in the nascent professionalisation of social science in Britain. Despite distancing herself from the practice of covert ethnography, Webb has received more recognition as a pioneer of the method than has Higgs. Higgs used the method of covert ethnography with commitment, and conviction in its methodological superiority, despite her willingness to engage with other forms of social research and their results.

Hugh Massingham's incognito account of working-class community was testament to the survival of the tradition of social exploration into the inter-war period, a time of transition for covert ethnography.[3] A strong academic model of ethnography emerged from the professionalising discipline of anthropology, and the influence of this model was shown to have shaped the practice of covert ethnography in Britain by academics and non-

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[1] Brian Harrison, *Peaceable Kingdom: Stability and Change in Modern Britain*, (Oxford: Oxford University Press, 1982), p.261; Eileen Janes Yeo, *The Contest for Social Science: Relations and Representations of Gender and Class*, (London: Rivers Oram Press, 1996), pp.xiv-xv.
[2] Beatrice Potter, 'Pages from a Work-Girl's Diary', *Nineteenth Century*, Volume 24, Number 139, (1888), pp.301-314; Mary Higgs, *Glimpses into the Abyss*, (London: P.S. King and Son, 1906).
[3] Hugh Massingham, *I Took Off My Tie*, (London: William Heinemann Ltd., 1936).

academics alike in chapter 2. The emergence of Mass-Observation and 'functional penetration' represented efforts to adopt and mould Malinowski's anthropological ethnographic model to the study of Britain, with, in the case of Edward Wight Bakke, limited success.[4] Despite Marie Jahoda's confident use of covert ethnography, the survey method continued to dominate British social research, and it was not until the post-war period that qualitative empirical research was embraced by sociology.[5]

In chapter 3, it was shown how, between 1946 and 1969, the academic discipline of sociology underwent a paradigmatic shift, and that the anthropological model of ethnography was established as a legitimate and academically authoritative way of studying and understanding British society. The problematic shift from the survey paradigm to the anthropological paradigm, combined with a post-war emphasis on empiricism and problem-centred research, shaped the practice of covert ethnography significantly. John Spencer's covert research into the relationship between crime and service in the armed forces embodied the difficulty of embracing a new methodological paradigm.[6] Just as the establishment of the anthropological paradigm in sociology legitimised the use of ethnographic method, an emergent concern over the ethics of undercover research in a professionalising discipline inhibited the use of covert participant observation by academic social scientists. Spencer's reluctance to dwell on the covert nature of his research, and the marginalisation of Enid Mumford's covert ethnography from the University of Liverpool's record of industrial sociology, both demonstrate the extent to which ethical concerns shaped the use and history of covert ethnography.[7]

In chapter 4, it was shown that in the 1970s a considerable number of academics made use of the anthropological paradigm of ethnography to produce confident accounts of their covert research, demonstrating the complete integration of the ethnographic model into the British social sciences. However, the debate regarding the ethics of covertness gathered steam, first in America and then in Britain, and the British Sociological Association's decision to publish a 'Statement of Ethical Principles and their Application to Sociological Practice' in 1970 demonstrated the problematic nature of such research in a professional context.[8] The criticism levelled at Roger Homan in the wake of his covert

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[4] Mass-Observation, *War Factory*, (London: The Cresset Library, 1987); Edward Wight Bakke, *The Unemployed Man: A Social Study*, (London: Nisbet, 1933).
[5] Marie Jahoda, 'Some Socio-Psychological Problems of Factory Life', *British Journal of Psychology*, Volume 31, (1941), pp.161-206.
[6] John Spencer, *Crime and the Services*, (London: Routledge and Kegan Paul, 1954).
[7] Enid Mumford, 'Social Behaviour in Small Work Groups', *Sociological Review*, Volume 7, Issue 2, (1959), pp.137-157.
[8] V.L. Allen, Correspondence on 'Statement of Ethical Principles and their Application to Sociological Practice', *Sociology*, (1970), Volume 4, pp.114-117.

study of Pentecostal believers, and Peter Archard's discomfort with the covert aspects of his research show how academic covert ethnographers operated on the periphery of their professional discipline.[9]

As well as the considerable number of academic covert ethnographies to emerge in the 1970s, this decade witnessed the re-emergence of the tradition of social exploration. It was established in chapter 5 that significant connections can be made between the late Victorian and Edwardian practice of covert social exploration and 1970s examples of non-academic covert ethnography. However, the work of James Patrick on Glasgow's gang culture suggests that models of covert social exploration and academic covert ethnography were not incompatible.[10] The deconstruction of covert ethnography - particularly the analysis of structure, language and imagery - demonstrates the complexity of the covert ethnographic project and suggests that social exploration can be considered a legitimate forerunner to later, more modern, forms of covert ethnography. That is not to say that, by 1980, there was no distinction between the academic and non-academic traditions of covert ethnography. In particular, the pressure to conform to professional standards of ethics provided an important framework to the academic use of covert methods. However, it is possible, and useful, to consider all examples of covert ethnography as part of the same broad enterprise.

Beyond 1980, academic and non-academic social researchers have continued to use the covert ethnographic method. Writing in 1976, Peter Keating suggested that in the final quarter of the twentieth century, 'far from dying out', the practice of social exploration had continued, perhaps with 'increased power'. Developments in technology, according to Keating, meant that 'voyages of social exploration' were broadcast 'into the homes of millions of viewers' on a weekly basis.[11] This has continued into the twenty-first century with television programmes such as the BBC's 'Famous, Rich and Homeless' aired in June 2009, in which famous volunteers, including the journalist Rosie Boycott, spent ten days without money, dressed as tramps on the streets and in homeless shelters.[12] There are also examples of post-1980 non-academic covert ethnography in print, notably journalist Tony

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[9] Roger Homan, 'Interpersonal Communication in Pentecostal Meetings', *Sociological Review*, Volume 26, Issue 3, (1978), pp.499-518; Peter Archard, *Vagrancy, Alcoholism and Social Control*, (London: Macmillan, 1979).
[10] James Patrick, *A Glasgow Gang Observed*, (London: Eyre Methuen, 1973).
[11] Peter Keating (ed.), *Into Unknown England, 1866-1913: Selections from the Social Explorers*, (Manchester: Manchester University Press, 1976), p.31.
[12] 'Famous, Rich and Homeless', BBC One Television, June 2009, http://www.bbc.co.uk/programmes/b00lfhhx , [accessed 29th March 2010].

Wilkinson's *Down and Out* published in 1981, a contribution to the well-established genre of incognito research into homelessness.[13]

More recently, two journalists have published undercover accounts of poorly paid work: Fran Abrams' *Below the Breadline* and Polly Toynbee's *Hard Work*.[14] Tom Hall, social sciences lecturer at Cardiff University and overt ethnographer, published a review article in the journal *Sociology* in which he described the work of Abrams and Toynbee, as well as an American undercover journalist, Barbara Ehrenreich, as 'social exploration'. He compared their undercover accounts to those produced in the Victorian and Edwardian era. However, he also recognised the parallels between their work and sociological participant observation.[15] Furthermore, he suggested that the twenty-first century social explorers wrote 'with a clarity and focus that sociologists would do well to match'. As an academic discipline, he noted that '[w]e publish less of this sort of work – good, realist ethnography addressed to pressing social issues – than we might'.[16] Given the important role that ethics played in shaping the use of academic covert ethnography between 1945 and 1980, it is significant that Hall was critical of the covert aspect of Abrams' and Toynbee's research. He suggested that one of the strengths of ethnographic research is 'openly acknowledged difference and the (then mutual) attempt to work across this'.[17] Although the question of ethics was considered by Abrams and Toynbee, Hall 'would have welcomed rather more unease' about the use of covert methods.[18]

Regardless of the continued concern over covertness in the post-1980 period, academic sociologists have continued to use covert ethnography. In 2000, David Calvey, a senior sociology lecturer at Manchester Metropolitan University, studied the professionalisation and criminalisation of doormen and, although he considered the ethical problems, he nonetheless used the method of covert participant observation.[19] There is also evidence to suggest that, post-1980, academic ethnographers have begun to modify the anthropological paradigm of ethnography. Thomas Schwandt has suggested that towards the end of the twentieth century, ethnographers began to experiment with alternative

[13] Tony Wilkinson, *Down and Out*, (London: Quartet Books, 1981).

[14] Fran Abrams, *Below the Breadline: Living on the Minimum Wage*, (London: Profile, 2002); Polly Toynbee, *Hard Work: Life in Low-Pay Britain*, (London: Bloomsbury, 2003).

[15] Tom Hall, 'Through a Glass Darkly: Undercover in Low-Pay Britain and America', *Sociology*, (2004), Volume 38, Number 3, p.624.

[16] Hall, 'Through a Glass', p.629.

[17] Ibid., p.626.

[18] Ibid., p.627.

[19] David Calvey, 'Getting on the Door and Staying There: A Covert Participant Observational Study of Bouncers', in Geraldine Lee-Treweek and Stephanie Linkogle (eds.), *Danger in the Field: Risk and Ethics in Social Research*, (London: Routledge, 2000), pp.46-49. Another example of post-1980 academic covert ethnography: Hester Parr, 'Mental Health, Ethnography and the Body', *Area*, (1998), Volume 30, Number 1, pp.28-37.

structures and communication strategies.[20] The academically trained anthropologist Kate Fox, for example, has produced a number of ethnographic accounts aimed at the general reader, such as *The Racing Tribe: Watching the Horsewatchers* in 1999 and, even more recently, *Watching the English: The Hidden Rules of English Behaviour*, in 2005.[21] The history of covert ethnography between the 1880s and 1980 is complex and rich. Given recent developments and continued popularity of the method, it would appear that covert ethnography will continue to evolve throughout the twenty-first century, with academic and non-academic overlapping, and the ethics of covert research shaping its practice.

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[20] Thomas A. Schwandt, *Dictionary of Qualitative Inquiry*, (London: Sage, 2001), p.12. See for example, the blending of formal ethnographic accounts of India with literary genres in a feminist context in: Kamala Visweswaran, *Fictions of feminist ethnography*, (Minneapolis: University of Minnesota Press, 1994).
[21] Kate Fox, *The Racing Tribe: Watching the Horsewatchers*, (London: Metro, 1999); Kate Fox, *Watching the English: The Hidden Rules of English Behaviour*, (London: Hodder and Stoughton, 2005).

Bibliography

Brian Abel-Smith, *The Poor and the Poorest: A New Analysis of the Ministry of Labour's 'Family Expenditure Surveys' of 1953-54 and 1960*, (London: Bell, 1965).

Nicholas Abercrombie, Stephen Hill & Bryan Turner, *The Penguin Dictionary of Sociology*, 4th Edition, (Harmondsworth: Penguin, 2000).

Fran Abrams, *Below the Breadline: Living on the Minimum Wage*, (London: Profile, 2002).

V.L. Allen, Correspondence on 'Statement of Ethical Principles and their Application to Sociological Practice', *Sociology*, (1970), Volume 4, pp.114-117.

Nels Anderson, *The Hobo: The Sociology of the Homeless Man*, (London: University of Chicago Press, 1923).

Peter Archard, *Vagrancy, Alcoholism and Social Control*, (London: Macmillan, 1979).

Paul Atkinson, *Understanding Ethnographic Texts*, (London: Sage, 1992).

Edward Wight Bakke, *Citizens Without Work: A Study of the Effects of Unemployment upon the Worker's Social Relations and Practices* (New Haven: Yale University Press, 1940).

Edward Wight Bakke, *The Unemployed Man: A Social Study*, (London: Nisbet, 1933).

Edward Wight Bakke, *The Unemployed Worker: A Study of the Task of Making a Living Without a Job*, (New Haven: Yale University Press, 1940).

J.A. Banks, 'The British Sociological Association – The First Fifteen Years', *Sociology*, Volume 1, Number 1, (1967), pp.1-9.

Michael Banton, 'Anthropological Perspectives in Sociology', *British Journal of Sociology*, Volume 15, Number 2, (1964), pp.95-112.

John Barnes, 'Some Ethical Problems in Modern Fieldwork', *British Journal of Sociology*, (1963), Volume 14, Number 2, pp.118-134.

John Barnes, *Who Should Know What? Social Science, Privacy and Ethics*, (Cambridge: Cambridge University Press, 1979).

Lady Florence Bell, *At the Works: A Study of a Manufacturing Town*, (London: E. Arnold, 1907).

Ruth Benedict, *Patterns of Culture*, (London: Routledge and Kegan Paul, 1935).

Walter Bernstein, 'The Cherubs are Rumbling', James Short (ed.), *Gang Delinquency and Delinquent Subcultures*, (Chicago: Harper and Row, 1968), pp.22-52.

Gerald Berreman, *Behind Many Masks: Ethnography and Impression Management in a Himalayan Village*, (Indianapolis: Bobbs-Merrill, 1962).

Michael Bevan and Jeremy MacClancy, 'Rivers, William Halse Rivers (1864–1922)', *Oxford Dictionary of National Biography*, Oxford University Press, (2004), http://www.oxforddnb.com/view/article/37898 , [accessed 25th March 2010].

W.H. Beveridge, 'Review: *Glimpses into the Abyss* by Mary Higgs', *The Economic Journal*, Volume 16, Number 64, (1906), pp.581-583.

Jeremy Boissevain, 'Ethnographic Fieldwork', in Jessica Kuper (ed.), *Methods, Ethics and Models*, (London: Routledge, 1987), pp.22-25.

Charles Booth, *Life and Labour of the People in London*, 10 volumes, (London: Macmillan, 1892-1897).

Helen Bosanquet, 'Marriage in East London', in Bernard Bosanquet (ed.), *Aspects of the Social Problem*, (London: Macmillan, 1895), pp.75-81.

Walter Brierley, *Means Test Man*, (London: Methuen, 1935).

Richard Brown, 'Participation, Conflict and Change in Industry: A Review of Research in Industrial Sociology at the Department of Social Science, University of Liverpool', *Sociological Review*, Volume 13, Issue 3, (1965), pp.273-295.

Lodewijk Brunt, 'The Ethnography of "Babylon": The Rhetoric of Fear and the Study of London, 1850-1914', *City and Society: Journal of the Society for Urban Anthropology*, Volume 41, (1990), pp.77-87.

R.W. Buckingham, S.A. Lack, B.M. Mount, L.D. MacLean and J.T. Collins, 'Living with the Dying: Use of the Technique of Participant Observation', *Canadian Medical Association Journal*, (1976), Volume 115, Number 12, pp.1211-1215.

Martin Bulmer, 'Comment on "The Ethics of Covert Methods"', *British Journal of Sociology*, (1980), Volume 31, Number 1, pp.60-61.

Angus Calder, 'Mass-Observation, 1937-1949', in Martin Bulmer (ed.), *Essays on the History of British Sociological Research*, (Cambridge: Cambridge University Press, 1985), pp.121-136.

David Calvey, 'Getting on the Door and Staying There: A Covert Participant Observational Study of Bouncers', in Geraldine Lee-Treweek and Stephanie Linkogle (eds.), *Danger in the Field: Risk and Ethics in Social Research*, (London: Routledge, 2000), pp.43-60.

Ann Cartwright and Peter Willmott, Correspondence on 'Research Interviewing and Ethics', *Sociology*, Volume 2, Number 1, (1968), pp.91-93.

Gwen Cash, *Off the Record: The Personal Reminiscences of Canada's First Woman Reporter*, (British Columbia: Stagecoach, 1977).

William Caudill et al, 'Social Structure and Interaction Processes on a Psychiatric Ward', *American Journal of Orthopsychiatry*, (1952), Volume 22, pp.314-334.

Rosemary Chadwick, 'Higgs, Mary Ann (1854–1937)', *Oxford Dictionary of National Biography*, Oxford University Press, (2004), http://www.oxforddnb.com/view/article/38523 , [accessed 25th March 2010].

Malcolm Chase, 'Massingham, (Harold) John (1888–1952)', *Oxford Dictionary of National Biography*, Oxford University Press, (2008), http://www.oxforddnb.com/view/article/34922 , [accessed 25th March 2010].

James Clifford, 'On Ethnographic Authority', *Representations*, Number 2, (1983), pp.118-146.

Richard Cloward, *Delinquency and Opportunity: a Theory of Delinquent Gangs*, (London: Routledge, 1961).

Stanley Cohen (ed.), *Images of Deviance*, (Harmondsworth: Penguin, 1971).

Susan Cohen and Clive Fleay, 'Fighters for the Poor', *History Today*, Volume 50, Issue 1, (2000), pp.36-37.

Lewis Coser, 'A Question of Professional Ethics?', *American Sociological Review*, (1959), Volume 24, Issue 3, pp.397-400.

[Walter Thomas Cranfield] Denis Crane, *A Vicarious Vagabond*, (1910).

C.W. Craven, 'A Night in the Workhouse', in Mark Freeman and Gillian Nelson (eds.), *Vicarious Vagrants: Incognito Social Explorers and the Homeless in England, 1860-1910*, (Lambertville, New Jersey: The True Bill Press, 2008), pp.181-188.

Sheila Cunnison, *Wages and Work Allocation: A Study of Social Relations in a Garment Workshop*, (London: Tavistock, 1966).

Andrew Davies, 'Street gangs, crime and policing in Glasgow during the 1930s: the case of the Beehive Boys', *Social History*, Volume 23, Number 3, (1998), pp.251-267.

Fred Davis, 'Comment on "Initial Interaction of Newcomers in Alcoholics Anonymous"', *Social Problems*, (1961), Volume 8, Number 4, pp.364-365.

John Davis, 'Webb, (Martha) Beatrice (1858–1943)', *Oxford Dictionary of National Biography*, Oxford University Press, (2008), http://www.oxforddnb.com/view/article/36799 , [accessed 25th March 2010].

Norman Dennis, 'Changes in Function and Leadership Renewal: A Study of the Community Association Movement and Problems of Voluntary Small Groups in the Urban Locality', *Sociological Review*, Volume 9, Issue 1, (1961), pp.55-84.

Norman Dennis, Fernando Henriques and Clifford Slaughter, *Coal is our Life: An Analysis of a Yorkshire Mining Community*, (London: Eyre & Spottiswoode, 1956).

Norman Denzin, 'On the Ethics of Disguised Observation', *Social Problems*, (1968), Volume 15, Number 4, pp.502-504.

B.I. Diamond, J.O. Baylen and J.P. Baylen, 'James Greenwood's London: A Precursor of Charles Booth', *Victorian Periodicals Review*, Volume 17, (1984), pp.34-43.

Jason Ditton, 'Alibis and Aliases: Some Notes on the "Motives" of Fiddling Bread Salesmen', *Sociology*, Volume 11, Number 1, (1977), pp.233-255.

Jason Ditton, 'Baking Time', *Sociological Review*, Volume 27, Issue 1, (1979), pp.157-167.

Jason Ditton, 'The Fiddling Salesman: Connivance and Corruption', *New Society*, November 1974, pp.535-537.

Jason Ditton, *Part-Time Crime: An Ethnography of Fiddling and Pilferage*, (London: Macmillan, 1977).

Jack Douglas, 'Observing Deviance', in Jack Douglas (ed.), *Research on Deviance*, (Random House: New York, 1972), pp.3-34.

François Dubet, 'Participant Observation' in William Outhwaite and Tom Bottomore (eds.), *The Blackwell Dictionary of Twentieth-Century Social Thought*, (Oxford: Blackwell, 1993), pp.447-448.

George Zachary Edwards, *A Vicar as Vagrant*, (London: P.S. King & Son, 1910).

Gary Easthope, *A History of Social Research Methods*, (London: Longman, 1974).

Isabel Emmet, *A North Wales Village: A Social Anthropological Study*, (London: Routledge and Kegan Paul, 1964).

David Englander and Rosemary O'Day, *Retrieved Riches: Social Investigation in Britain, 1840-1914*, (Aldershot, Hants: Scholar Press; Brookfield: Ashgate, 1995).

Kai Erikson, 'A Comment on Disguised Observation in Sociology', *Social Problems*, (1967), Volume 14, Number 4, pp.366-373.

Kai Erikson, 'A Reply to Denzin', *Social Problems*, (1968), Volume 15, Number 4, pp.505-506.

Edward Evans-Pritchard, *Witchcraft, Oracles and Magic among the Azande*, (London: Clarendon Press, 1937).

Leon Festinger, Henry Riecken and Stanley Schacter, *When Prophecy Fails*, (Minneapolis: University of Minneapolis Press, 1956).

Joseph Henry Fichter and W.L. Kolb, 'Ethical Limitation on Sociological Reporting', in J.H. Fichter, *Social Relations in the Urban Parish*, (Chicago: Chicago University Press, 1954), pp.218-232.

Raymond Firth, 'An Anthropologist's View of Mass-Observation', *Sociological Review*, Volume 31, Issue 2, (1939), pp.166-193.

Kate Fox, *The Racing Tribe: Watching the Horsewatchers*, (London: Metro, 1999).

Kate Fox, *Watching the English: The Hidden Rules of English Behaviour*, (London: Hodder and Stoughton, 2005).

Mark Freeman, 'Folklore Collection and Social Investigation in Late-Nineteenth and Early-Twentieth Century England', *Folklore*, Volume 116, (2005), pp.51-65.

Mark Freeman, ''Journeys into Poverty Kingdom': Complete Participation and the British Vagrant, 1866-1914', *History Workshop Journal*, Number 52, (2001), pp.99-121.

Mark Freeman, 'The Provincial Social Survey in Edwardian Britain', *Historical Research*, Volume 75, Number 187, (2002), pp.73-89.

Mark Freeman, *Social Investigation and Rural England, 1870-1914*, (Woodbridge, Suffolk; Rochester; New York: Royal Historical Society: Boydell Press, 2003).

Mark Freeman and Gillian Nelson (eds.), *Vicarious Vagrants: Incognito Social Explorers and the Homeless in England, 1860 – 1910*, (Lambertville, New Jersey: The True Bill Press, 2008).

Barbara Freire-Marreco and John Linton Myers (eds.), *Notes and Queries on Anthropology*, 4th Edition, Edited for the British Association for the Advancement of Science, (London: Harrison and Sons, 1912).

Celia Fremlin, *The Hours before Dawn*, (London: Victor Gollancz, 1958).

Celia Fremlin, *King of the World*, (Sutton: Severn House, 1994).

Celia Fremlin, *The Seven Chars of Chelsea*, (London: Methuen, 1940).

Jürgen Friedrichs and Harmut Lüdtke, *Participant Observation: Theory and Practice*, (Hants: Saxon House, 1975).

James Fulcher and John Scott, *Sociology*, (Oxford: Oxford University Press, 1999).

Judy Giles, ''A Little Strain with Servants'': Gender, Modernity and Domesticity in Daphne Du Maurier's *Rebecca* and Celia Fremlin's *The Seven Chars of Chelsea*', *Literature and History*, Volume 12, Number 2, (2003), pp.36-50.

Erving Goffman, *The Presentation of Self in Everyday Life*, (London: Penguin, 1990).

Lawrence Goldman, 'A Peculiarity of the English? The Social Science Association and the Absence of Sociology in Nineteenth-Century Britain', *Past and Present*, Number 114, (1987), pp.133-171.

Howard J. Goldsmid, *Dottings of a Dosser, Being Revelations of the Inner Life of Low London Lodging-Houses*, (London: T. Fisher Unwin, 1886).

Sophie Goodchild, 'The Social Vocalist', Interview with Polly Toynbee, *Society Now,* Issue 4, 2009, p.23.

James Greenwood, 'A Night in a Workhouse' in Peter Keating (ed.), *Into Unknown England, 1866-1913: Selections from the Social Explorers*, (Glasgow: Fontana, 1976), pp.33-54.

James Greenwood, *On Tramp*, in Mark Freeman and Gillian Nelson (eds.), *Vicarious Vagrants: Incognito Social Explorers and the Homeless in England, 1860-1910*, (Lambertville, New Jersey: The True Bill Press, 2008), pp.141-180.

Tom Hall, 'Through a Glass Darkly: Undercover in Low-Pay Britain and America', *Sociology*, (2004), Volume 38, Number 3, pp.623-630.

A.H. Halsey, *A History of Sociology in Britain: Science, Literature and Society*, (Oxford: Oxford University Press, 2004).

Martyn Hammersley, *Reading Ethnographic Research: A Critical Guide*, (London: Longman, 1998).

Brian Harrison, 'Miss Butler's Oxford Survey', in A.H. Halsey (ed.), *Traditions of Social Policy: Essays in Honour of Violet Butler*, (Oxford: Blackwell, 1976), pp.27-72.

Brian Harrison, *Peaceable Kingdom: Stability and Change in Modern Britain*, (Oxford: Oxford University Press, 1982).

[Ronald George Hatton] Christopher Holdenby, *Folk of the Furrow*, (London: Smith, Elder and Co., 1913).

Sydney Frank Hatton, *London's bad boys*, (London: Chapman and Hall, 1931).

Joan Hedrick, *Solitary Comrade: Jack London and his Work*, (Chapel Hill: University of North Carolina Press, 1982).

Frances Heidensohn and Richard Wright, 'The *British Journal of Sociology* at Sixty', *British Journal of Sociology*, Volume 61, Issue s1, (2010), Special Issue: The BJS: Shaping Sociology over Sixty Years, http://www.bjsshapingsociology.com/view/0/index.html , [accessed 28th July 2010], pp.2-6.

Stuart Henry, 'Review of *Part-Time Crime: An Ethnography of Fiddling and Pilferage* by Jason Ditton', *Sociological Review*, Volume 26, Issue 1, (1978), pp.162-164.

Mary Higgs, *Glimpses into the Abyss*, (London: P.S. King and Son, 1906).

Mary Higgs, *How to Deal with the Unemployed*, (London: C.S. Brown, Langham, 1904).

Mary Higgs, *A Night in a Salvation Army Shelter*, (Oldham: 1905).

[Mary Higgs] Social Investigation by a Lady, *Five Days and Five Nights as a Tramp among Tramps*, (Manchester: John Heywood, 1904).

Mary Higgs, *Three Nights in Women's Lodging Houses*, (Oldham: 1905).

[Mary Higgs] Viatrix, 'The Tramp Ward', *Contemporary Review*, Volume 85, (1904), pp.649-667.

Gertrude Himmelfarb, 'Mayhew's Poor: A Problem of Identity', *Victorian Studies*, Volume 14, 1971, pp.307-320.
Roger Homan, 'The Ethics of Covert Methods', *British Journal of Sociology*, (1980), Volume 31, Number 1, pp.46-59.

John Hollingshead, *Ragged London in 1861*, (1861).

Roger Homan, 'The Ethics of Covert Methods', *British Journal of Sociology*, (1980), Volume 31, Number 1, pp.46-59.

Roger Homan, 'Interpersonal Communication in Pentecostal Meetings', *Sociological Review*, Volume 26, Issue 3, (1978), pp.499-518.

Irving Horowitz, *The Rise and Fall of Project Camelot: Studies in the Relationship between Social Science and Practical Politics*, (Cambridge, Massachusetts: Massachusetts Institute of Technology, 1974).

Anne Humpherys, *Travels into the Poor Man's Country: The Work of Henry Mayhew*, (Athens, Ga.: University of Georgia Press, 1977).

Laud Humphreys, *Tearoom Trade: A Study of Homosexual Encounters in Public Places*, (London: Duckworth, 1970)

Ken Hyland and Polly Tse, '"I Would Like to Thank My Supervisor": Acknowledgements in Graduate Dissertations', *International Journal of Applied Linguistics*, Volume 14, Number 2, (2004), pp.259-275.

Marie Jahoda, Paul Lazarsfeld and Hans Zeisel, *Marienthal: A Sociography of an Unemployed Community*, (London: Transaction Publishers, 2002).

Marie Jahoda, 'Socio-Psychological Methods of Fieldwork', *Man*, Volume 42, (1942), pp.115-117.

Marie Jahoda, 'Some Ideas on Social and Psychological Research', *Sociological Review*, Volume 30, Issue 1, (1938), pp.63-80.

Marie Jahoda, 'Some Socio-Psychological Problems of Factory Life', *British Journal of Psychology*, Volume 31, (1941), pp.161-206.

'John C. Spencer, 1915-1978', *British Journal of Criminology*, Volume 19, Number 1, (1979), pp.1-4.

D.C. Jones (ed.), *The Social Survey of Merseyside*, 3 Volumes, (Liverpool: Liverpool University Press, 1934).

Gareth Stedman Jones, *Outcast London: A Study in the Relationship between Classes in Victorian Society*, ((Oxford: Clarendon Press, 1971).

Danny L. Jorgensen, *Participant Observation: A Methodology for Human Studies*, (London: Sage, 1989).

Brett Kahr, 'Foreword', in Pearl King, *Time Past and Time Present: Selected Papers of Pearl King*, (London: Karnac, 2005), pp.xi-xv.

Peter Keating (ed.), *Into Unknown England, 1866-1913: Selections from the Social Explorers*, (Manchester: Manchester University Press, 1976).

D. S. Kenrick, 'Sandford, (Christopher) Jeremy (1930–2003)', *Oxford Dictionary of National Biography*, Oxford University Press, (2007), http://www.oxforddnb.com/view/article/90003 , [accessed 29 March 2010].

Raymond Kent, *A History of British Empirical Sociology*, (Aldershot: Gower, 1981).

Madelaine Kerr, *The People of Ship Street*, (London: Routledge, 1958).

Alex Kershaw, *Jack London: A Life*, (London: Flamingo, 1998).

Pearl King, 'Task Perception and Interpersonal Relations in Industrial Training: The Development of a Training Project in the Hosiery Trade. Part I', *Human Relations*, Volume 1, Number 1, (1947), pp.121-130.

Pearl King, 'Task Perception and Interpersonal Relations in Industrial Training: Part II', *Human Relations*, Volume 1, Number 3, (1948), pp.373-412.

Tom Kitwood, 'Cognition and emotion in the psychology of human values', *Oxford Review of Education*, Volume 10, Number 3, (1984), pp.293-301.

Hugh Klare, 'Review of *Crime and the Services* by John Spencer', *Sociological Review,* Volume 3, Issue 1, (1955), p.144.

Lisl Klein, 'Jahoda, Marie (1907-2001)', *Oxford Dictionary of National Biography*, Oxford University Press, (2005), http://www.oxforddnb.com/view/article/75873 , [accessed 16th June 2010].

Seth Koven, *Slumming: Sexual and Social Politics in Victorian London*, (Oxford: Princeton University Press, 2004).

Ernest Krausz, *Sociology in Britain: A Survey of Research*, (B.T. Batsford Limited: London, 1969).

Frank Land, 'Mumford , Enid Mary (1924–2006)', *Oxford Dictionary of National Biography*, Oxford University Press, http://www.oxforddnb.com/view/article/97841, [accessed 1 July 2010].

Carney Landis, 'National Differences in Conversations', *Journal of Abnormal and Social Psychology*, (1927), Volume 21, pp.354-357.

Margaret Lassell, *Wellington Road*, (London: Routledge, 1962).

Laurie Lee, *As I Walked Out one Midsummer's Morning*, (Harmondsworth: Penguin, 1971).

Elliot Liebow, *Tally's Corner: A Study of Negro Streetcorner Men*, (London: Routledge, 1967).

Kenneth Little, *Negroes in Britain: A Study of Racial Relations in English Society*, (London: Kegan Paul, 1948).

Martha Loane, *The Queen's Poor: Life as they find it in Town and Country*, (London: E. Arnold, 1905).

John Lofland and Robert Lejeune, 'Initial Interaction of Newcomers in Alcoholics Anonymous: A Field Experiment in Class Symbols and Socialization', *Social Problems*, (1960), Volume 8, Number 2, pp.102-111.

Jack London, *The People of the Abyss*, (London: Journeyman Press, 1977).

D.A.M., Review of *A Glasgow Gang Observed* by James Patrick, *Probation Journal*, Volume 20, Number 3, (1973), p.92.

Ross McKibbin, *The Ideologies of Class: Social Relations in Britain, 1880-1950*, (Oxford: Clarendon, 1994).

Ross McKibbin, 'Social Class and Social Observation in Edwardian England', *Transactions of the Royal Historical Society*, Volume 28, (1978), pp.175-199.

Angus McLaren, 'Smoke and Mirrors: Willy Clarkson and the Role of Disguises in Inter-War England', *Journal of Social History*, Volume 40, Number 3, (2007), pp.597-618.

Charles Madge and Tom Harrisson (eds.), *First Year's Work, 1937-38*, (London: Lindsay Drummond, 1938).

Bronislaw Malinowski, *Argonauts of the Western Pacific: An Account of Native Enterprise and Adventure in the Archipelagoes of Melanesian New Guinea*, (London: Routledge and Kegan Paul, 1922).

Bronislaw Malinowski, 'A Nation-Wide Intelligence Service', in Charles Madge and Tom Harrisson (eds.), *First Year's Work, 1937-38*, (London: Lindsay Drummond, 1938), pp.81-121.

Olive Malvery, *The Soul Market: with which is Included "The Heart of Things"'*, London: Hutchinson, 1906).

Hermann Mannheim, 'Foreword', in Spencer, *Crime and the Services*, (London: Routledge and Kegan Paul, 1954), pp.v-vii.

Gerald Mars, *Cheats at Work: An Anthropology of Workplace Crime*, (London: Allen and Unwin, 1982).

Gerald Mars, 'Dock Pilferage', in Paul Rock and Mary McIntosh (eds.), *Deviance and Social Control*, (London: Tavistock, 1974), pp.209-228.

Gerald Mars, 'Hotel Pilferage: A Case Study in Occupational Theft' in Malcolm Warner (ed.), *The Sociology of the Workplace: An Interdisciplinary Approach*, (London: Allen and Unwin, 1973), pp.200-210.

Gerald Mars, *Occupational Crime*, (Aldershot: Ashgate, 2001).

Gerald Mars, 'Review of *Part-Time Crime: An Ethnography of Fiddling and Pilferage* by Jason Ditton', *British Journal of Sociology*, (1980), Volume 31, Number 1, pp.136-137.

Gerald Mars, *Work Place Sabotage*, (Aldershot: Ashgate, 2001).

Catherine Marsh, 'Informants, Respondents and Citizens', in Martin Bulmer (ed.), *Essays on the History of British Sociological Research*, (Cambridge: Cambridge University Press, 1985), pp.206-227.

Thomas Marshall, *Sociology at the Crossroads and Other Essays*, (London: Heinemann, 1963).

Mass-Observation, *War Factory*, (London: The Cresset Library, 1987).

Hugh Massingham, *I Took Off My Tie*, (London: William Heinemann Ltd., 1936).

Henry Mayhew, *London Labour and the London poor*, Volumes 1-4, (London: Griffin; Bohn, 1851-1862).

John Barron Mays, *Growing Up in the City: A Study of Juvenile Delinquency in an Urban Neighbourhood*, (Liverpool: Liverpool University Press, 1954).

Margaret Mead, *Coming of Age in Samoa*, (Harmondsworth: Penguin Books, 1929).

Margaret Mead, 'The Human Study of Human Beings', *Science*, 20th January 1961, Volume 133, Number 3447.

Robert L. Miller and John D. Brewer, *The A-Z of Social Research: A Dictionary of Key Social Science Research Concepts*, (London: Sage, 2003).

G. Duncan Mitchell (ed.), *A New Dictionary of Sociology*, (London: Routledge & Kegan Paul, 1979).

G. Duncan Mitchell et al, *Neighbourhood and Community: An Enquiry into Social Relationships on Housing Estates in Liverpool and Sheffield*, (Liverpool: University Press of Liverpool, 1954).

George Sinclair Mitchell, 'Review of E. Wight Bakke, *The Unemployed Man*', *American Economic Review*, Volume 24, Number 3, (1934), pp.520-521.

D.H.J. Morgan, 'The British Association Scandal: The Effect of Publicity on a Sociological Investigation', *Sociological Review*, Volume 20, Issue 2, (1972), pp.185-206.

Claus Moser and Graham Kalton, *Survey Methods in Social Investigation*, 2nd edition, (Aldershot: Dartmouth Publishing Co., 1971).

Enid Mumford, 'Social Behaviour in Small Work Groups', *Sociological Review*, Volume 7, Issue 2, (1959), pp.137-157.

Enid Mumford, *Redesigning Human Systems*, (London: Information Science Publishing, 2003).

Bill Murray, *The Old Firm: Sectarianism, Sport and Society in Scotland*, (Edinburgh: J. Donald Publishers, 1984).

W.B. Murray and R.W. Buckingham, 'Implications of Participant Observation in Medical Studies', *Canadian Medical Association Journal*, (1976), Volume 115, Number 12, pp.1187-1190.

Eyo Ndem, 'The status of colored people in Britain', *The Phylon Quarterly*, Volume 18, No.1, (1957), pp.82-87.

H. W. Nevinson, 'Massingham, Henry William (1860–1924)', *Oxford Dictionary of National Biography*, Oxford University Press, (2008), http://www.oxforddnb.com/view/article/34923 , [accessed 25th March 2010].

Deborah Epstein Nord, *The Apprenticeship of Beatrice Webb*, (Amherst: University of Massachusetts Press, 1985).

Deborah Epstein Nord, 'The Social Explorer as Anthropologist: Victorian Travellers Among the Poor' in William Sharpe and Leonard Wallock (eds.), *Visions of the Modern City: Essays in History, Art and Literature*, (Columbia: Columbia University Press, 1987), pp.122-134.

Deborah Epstein Nord, *Walking the Victorian Streets: Women, Representation, and the City*, (London: Cornell University Press, 1995).

Philip O'Connor, *Britain in the Sixties: Vagrancy*, (Harmondsworth: Penguin, 1963).

Oscar Oeser, 'Methods and Assumptions of Fieldwork in Social Psychology', *British Journal of Psychology*, Volume 27, (1937), pp.343-363.

Oscar Oeser, 'The Value of Team Work and Functional Penetration as Methods in Social Investigation' in Frederic Bartlett, M. Ginsberg, E.J. Lindgren and R.H. Thouless (eds.), *The Study of Society: Methods and Problems*, (London: Routledge and Kegan Paul, 1939), pp.402-417.

David Oldman, 'Chance and Skill: A Study of Roulette', *Sociology*, Volume 8, Issue 3, (1974), pp.407-426.

David Oldman, 'Compulsive Gamblers', *Sociological Review*, Volume 26, Issue 2, (1978), pp.349-371.

Dan Ondrack and Stuart Timperley (eds.), *The Humanisation of Work: a European Perspective*, (London: Alan Armstrong and Associates, 1982).

George Orwell, *Down and Out in Paris and London*, (London: Victor Gollancz, 1933).

George Orwell, *The Road to Wigan pier*, (Harmondsworth: Penguin, 1969).

Robin Page, *The Benefits Racket*, (London: Tom Stacey, 1971).

Robin Page, *The Decline of an English Village*, (London: Davis-Poynter, 1974).

Robin Page, *Down among the Dossers*, (London: Davis-Poynter, 1973).

Robin Page, *How the Fox got its Pointed Nose*, (Bird's Farm Books, 1989).

Robin Page, *How the Heron got Long Legs*, (Bird's Farm Books, 1989).

Robin Page, *The Hunter and the Hunted: A Countryman's View of Blood Sports*, (London: Davis-Poynter, 1977).

Robin Page, *A Peasant's Diary*, (London: Robinson, 1993).

Howard Parker, 'Review of *Endless Pressure* by Ken Pryce', *British Journal of Sociology*, Volume 32, Number 1, (1981), p.145.

Hester Parr, 'Mental Health, Ethnography and the Body', *Area*, (1998), Volume 30, Number 1, pp.28-37.

Carol Ann Parssinen, 'Social Explorers and Social Scientists: The Dark Continent of Victorian Ethnography', in Jay Ruby (ed.), *A Crack in the Mirror: Reflexive Perspectives in Anthropology*, (Philadelphia: University of Pennsylvania Press, 1982), pp.205-219.

James Patrick, *A Glasgow Gang Observed*, (London: Eyre Methuen, 1973).

The Pilgrim Trust, *Men Without Work: A Report to the Pilgrim Trust*, (Cambridge: Cambridge University Press, 1938).

Ruth Pinder, 'Turning Points and Adaptations: One Man's Journey into Chronic Homelessness', *Ethos*, Volume 22, Number 2, (1994), pp.211-212.

Jennifer Platt, *The British Sociological Association: A Sociological History*, (Durham: sociologypress, 2003).

Jennifer Platt, 'Participant Observation', Michael S. Lewis-Beck, Alan Bryman and Tim Futing Liao (eds.), *The Sage Encyclopaedia of Social Science Research*, (London: Sage, 2004), pp.797-799.

Norman Polansky et al, 'Problems of Interpersonal Relations in Research on Groups', *Human Relations*, Volume 2, (1949), pp.281-291.

Ned Polsky, *Hustlers, Beats and Others*, (Harmondsworth: Penguin, 1967).

Beatrice Potter, 'East London Labour', *Nineteenth Century*, Volume 24, Number 137, (1888), pp.161-183.

Beatrice Potter, 'Pages from a Work-Girl's Diary', *Nineteenth Century*, Volume 24, Number 139, (1888), pp.301-314.

Hortense Powdermaker, *After Freedom: A Cultural Study in the Deep South*, (New York: Atheneum, 1969).

F.K. Prochaska, *Women and Philanthropy in Nineteenth-Century England*, (Oxford: Clarendon Press, 1980).

Ken Pryce, *Endless Pressure: A Study of West Indian Life-Styles in Bristol*, (Harmondsworth: Penguin, 1979).

Peter Ratcliffe, 'Sociology, the State and Social Change: Theoretical Considerations', Peter Ratcliffe (ed.), *The Politics of Social Science Research: 'Race', Ethnicity and Social Change*, (Basingstoke: Palgrave, 2001), pp.3-17.

Elizabeth Ray, 'Soyer, Alexis Benoît (1810–1858)', *Oxford Dictionary of National Biography*, Oxford University Press, (2004), http://www.oxforddnb.com/view/article/26076 , [accessed 25th March 2010].

Maud Pember Reeves, *Round About a Pound a Week*, (London: Virago Press, 1994).

Stephen Reynolds, *A Poor Man's House*, (London: Bodley Head, 1909).

Audrey Richards, 'The Development of Field Work Methods in Social Anthropology' in Frederic Bartlett, M. Ginsberg, E.J. Lindgren and R.H. Thouless (eds.), *The Study of Society: Methods and Problems*, (London: Routledge and Kegan Paul, 1939), pp.272-316.

W.H.R. Rivers, 'A General Account of Method' in Barbara Freire-Marreco and Linton Myres (eds.), *Notes and Queries on Anthropology*, 4th Edition, Edited for the British Association for the Advancement of Science, (London: Harrison and Sons, 1912), pp.108-127.

Ken Robinson, 'Introduction to Pearl King and her Work', in Pearl King, *Time Past and Time Present: Selected Papers of Pearl King*, (London: Karnac, 2005), pp.1-12.

Fritz Roethlisberger, *Management and the Worker: An Account of a Research Program Conducted by the Western Electric Company, Hawthorne Works, Chicago*, (London: Harvard University Press, 1939).

Julius Roth, 'Comments on "Secret Observation"', *Social Problems*, (1962), Volume 9, Number 3, pp.283-284.

Julius Roth, 'Dangerous and Difficult Enterprise?', *American Sociological Review*, (1959), Volume 24, Issue 3, p.398.

Donald Roy, 'Quota Restriction and Goldbricking in a Machine Shop', *The American Journal of Sociology*, Volume 57, Number 5, (1952), pp.427-442.

Donald Roy, 'Banana Time: Job Satisfaction and Informal Interaction', *Human Organization*, (1959), Volume 18, pp.158-168.

Paul Rock and Mary McIntosh (eds.), *Deviance and Social Control*, (London: Tavistock, 1974).

Ellen Ross (ed.), *Slum Travelers: Ladies and London Poverty, 1860-1920*, (London: University of California Press, 2007).

Julie Rugg, 'Poverty and social exclusion', in Francesca Carnevale and Julie-Marie Strange (eds.), *Twentieth Century Britain: Economic , Cultural and Social Change*, 2nd Edition, (Harlow: Pearson-Longman, 2007), pp.310-314.

Jeremy Sandford, *Down and Out in Britain*, (London: New English Library, 1972).

Thomas A. Schwandt, *Dictionary of Qualitative Inquiry*, (London: Sage, 2001).

Thomas A. Schwandt, *Qualitative Inquiry: A Dictionary of Terms*, (London: Sage, 1997).

Christopher Scoble, *Fisherman's Friend: A Life of Stephen Reynolds*, (Tiverton: Halsgrove, 2001).

W.H. Scott and J.B. Mays, 'Research Report No.1: Department of Social Science, the University of Liverpool', *Sociological Review*, Volume 8, Issue 1, (1960), pp.109-117.

Carole Seymour-Jones, *Beatrice Webb: Woman of Conflict*, (New York: HarperCollins, 1993).

Percy Bysshe Shelley, *A Defence of Poetry*, Edited by Albert Cook, (Boston, Massachussetts: 1891).

Jerry Shepperd, 'Review of *Vagrancy, Alcoholism and Social Control* by Peter Archard and *Alcohol, Reform and Society: The Liquor Issue in Social Context* by Jack Blocker (ed.)', *Contemporary Sociology*, Volume 10, Number 3, (1981), pp.380-381.

Edward Shils, 'Social Inquiry and the Autonomy of the Individual', in Daniel Lerner (ed.), *The Human Meaning of Social Science*, (New York: Meridian Books, 1959), pp.114-157.

Thomas Simey, 'The Analysis of Social Problems', *Sociological Review*, Volume 1, Issue 1, (1953), pp.71-86.

Michael Smith, 'Hockey violence: a test of the violent subculture hypothesis', *Social Problems,* Volume 27, Number 2, (1979), pp.235-247.

Richard Sparks, 'Review of *Images of Deviance*, Stanley Cohen (ed.)', *Sociology*, Volume 7, (1973), pp.148-149.

J.C. [John Carrington] Spencer, Book Review, *British Journal of Sociology*, Volume 1, Number 2, (1950), pp.180-182.

John [Carrington] Spencer, *Crime and the Services*, (London: Routledge and Kegan Paul, 1954).

James Spradley, *Participant Observation*, (London: Holt, Rinehart & Winston, 1980).

James Spradley, *You Owe Yourself a Drunk: An Ethnography of Urban Nomads*, (London: University Press of America, 1988).

Margaret Stacey, Correspondence on 'Professional Ethics', *Sociology*, Volume 2, (1968), p.353.

W.T. Stead, *The Maiden Tribute of Modern Babylon: The Report of the Secret Commission*, Edited by Anthony Simpson, (Lambertville, New Jersey: The True Bill Press, 2007).

George Stocking Jr., 'The Ethnographer's Magic: Fieldwork in British Anthropology from Tylor to Malinowski' in George Stocking Jr. (ed.), *Observers Observed: Essays on Ethnographic Fieldwork*, (Madison, Wisconsin: University of Wisconsin Press, 1983), pp.70-120.

Mortimer Sullivan, Jr., Stuart Queen and Ralph Patrick, Jr., 'Participant Observation as Employed in the Study of a Military Training Program', *American Sociological Review*, (1958), Volume 23, Number 6, pp.660-667.

Penny Summerfield, 'Mass-Observation: Social Research or Social Movement?', *Journal of Contemporary History*, Volume 20, Number 3, (1985), pp.439-452.

Penny Summerfield, 'Women, Work and Welfare: A Study of Child Care and Shopping in Britain in the Second World War', *Journal of Social History*, Volume 17, Number 2, (1983), pp.249-270.

Penny Summerfield, *Women Workers in the Second World War: Production and Patriarchy in Conflict*, (London: Routledge, 1989).

Gerald Suttles, *The Social Order of the Slum: Ethnicity and Territory in the Inner City*, (London: University of Chicago Press, 1968).

A.J.M. Sykes, Book Review, *Sociology*, Volume 2, Number 1, (1968), p.100.

A.J.M. Sykes, 'Review of *On the Shop Floor* by Tom Lupton and *Coal and Conflict* by W.H. Scott et al', *Sociological Review*, Volume 11, Issue 3, (1963), pp.371-374.

Tom Lupton, *On the Shop Floor: Two Studies of Workshop Organization and Output*, (Pergamon, 1963).

Bertrand Taithe (ed.), *The Essential Mayhew: Representing and Communicating the Poor*, (London: Rivers Oram Press, 1996).

Andrea Thomas, 'James V (1512–1542)', *Oxford Dictionary of National Biography*, Oxford University Press, (2004), http://www.oxforddnb.com/view/article/14591, [accessed 26th March 2010].

Stuart Timperley, *Personnel Planning and Occupational Choice*, (London: Allen and Unwin, 1974).

Stuart Timperley, 'A Study of a Self-Governing Work Group', *Sociological Review*, Volume 18, Issue 2, (1970), pp.259-281.

Peter Townsend, *Poverty in the United Kingdom: A Survey of Household Resources and Standards of Living*, (London: Allen Lane, 1979).

Peter Townsend, *Poverty, Socialism and Labour in* Power, (Fabian Society, 1967).

Polly Toynbee, *Hard Work: Life in Low-Pay Britain*, (London: Bloomsbury, 2003).

Polly Toynbee, *Leftovers*, (London: Weidenfeld & Nicolson, 1966).

Polly Toynbee, *A Working Life*, (Harmondsworth: Penguin, 1973).

Barry Turner, 'Review of *The Sociology of the Workplace: An Interdisciplinary Approach* by Malcolm Warner (ed.)', *Sociological Review*, (1974), Volume 22, Issue 2, pp.281-282.

University of Liverpool, Department of Social Science, *The Dock Worker: An Analysis of the Conditions of Employment in the Port of Manchester*, (Liverpool: University Press of Liverpool, 1954).

Martha Vincinus, *Independent Women: Work and Community for Single Women: 1850-1920*, (London: Virago, 1985).

Kamala Visweswaran, *Fictions of Feminist Ethnography,* (London: University of Minnesota Press).

Andrew Walker and James Atherton, 'An Easter Pentecostal Convention: The Successful Management of a "Time of Blessing"', *Sociological Review*, Volume 19, (1971), pp.367-387.

Judith Walkowitz, 'The Indian Woman, the Flower Seller and the Jew: Photojournalism in Edwardian London', *Victorian Studies,* Volume 42, (1998), pp.3-46.

F.G. Wallace-Goodbody, 'The Tramp's Haven', in Mark Freeman and Gillian Nelson (eds.), *Vicarious Vagrants: Incognito Social Explorers and the Homeless in England, 1860-1910*, (Lambertville, New Jersey: The True Bill Press, 2008), pp.119-140.

Roy Wallis, *The Road to Total Freedom: A Sociological Analysis of Scientology*, (London: Heinemann, 1976).

Murray L. Wax, 'Apprenticing a Career', *Anthropology and Education Quarterly*, Volume 14, Number 3, (1983), pp.184-187.

Murray L. Wax, 'Tenting with Malinowski', *American Sociological Review*, Volume 37, Number 1, (1972), pp.1-13.

Beatrice Webb, 'The Diary of an Investigator', in Beatrice Webb and Sidney Webb, *Problems of Modern Industry*, (London: 1898), pp.1-19.

Beatrice Webb, *My Apprenticeship*, (Harmondsworth: Penguin, 1971).

Beatrice (Potter) Webb, 'Pages from a Work-Girl's Diary', Ellen Ross (ed.), *Slum Travelers: Ladies and London Poverty, 1860-1920*, (London: University of California Press, 2007), pp.266-279.

A.F. Wells, *The Local Social Survey in Great Britain*, (London: Allen & Unwin, 1935).

George S. Wheeler, 'Review of E. Wight Bakke, *The Unemployed Man* and *Insurance or dole?*', *The Journal of Political Economy*, Volume 44, Number 5, (1936), pp.707-709.

William Foote Whyte, *Street Corner Society: The Social Structure of an Italian Slum*, (Chicago: University of Chicago Press, 1943).

J.R. Widdup, *The Casual Ward System: Its Horrors and Atrocities. Being an Account of a Night in the Burnley Casual Ward, Disguised as a Tramp*, (Manchester: The Labour Press Society, 1894).

J.R. Widdup, *The Casual Ward System: Its Horrors and Atrocities. Being an Account of a Night in the Burnley Casual Ward, Disguised as a Tramp,* in Mark Freeman and Gillian Nelson (eds.), *Vicarious Vagrants: Incognito Social Explorers and the Homeless in England, 1860-1910*, (Lambertville, New Jersey: The True Bill Press, 2008), pp.189-205.

Tony Wilkinson, *Down and Out*, (London: Quartet Books, 1981).

[Josiah Flynt Willard] Josiah Flynt, *Tramping with Tramps: Studies and Sketches of Vagabond Life*, (London: T. Fisher Unwin, 1900).

James M. Williams, 'Review of E. Wight Bakke, *The Unemployed Man*', *American Journal of Sociology*, Volume 40, Number 2, (1934), pp.271-272.

Alexander Wilson, 'A Note on the Social Sanctions of Social Research', *Sociological Review*, Volume 3, Issue 1, (1955), p.109-166.

Anthony S. Wohl, 'Social Explorations Among the London Poor: Theatre or Laboratory?', *Revue Francaise de Civilisation Britannique*, Volume 6, Part 2, (1991), pp.77-97.

Everard Wyrall, *The Spike*, in Mark Freeman and Gillian Nelson (eds.), *Vicarious Vagrants: Incognito Social Explorers and the Homeless in England, 1860-1910*, (Lambertville, New Jersey: The True Bill Press, 2008), pp.231-278.

Lewis Yablonsky, *The Violent Gang*, (Harmondsworth: Penguin, 1967).

Eileen Janes Yeo, *The Contest for Social Science: Relations and Representations of Gender and Class*, (London: Rivers Oram Press, 1996).

Michael Young and Peter Willmott, *Family and Kinship in East London*, (London: Routledge, 1957).

Newspapers and Periodicals

'Celia Fremlin: Author who wrote The Hours Before Dawn', Obituary, *Times*, September 9th 2009, http://www.timesonline.co.uk/tol/comment/obituaries/article6826388.ece , [accessed 7th July 2010].

Tim Devlin, 'Jeremy goes home to the gypsies', *Times*, 14th July 1973; Clifford Longley, 'No High Life for Author who Exposes Low Life', *Times*, 21st October 1971.

Bob Dingwall and Ronald Frankenberg, Correspondence, 'Covert Observation. A Question of Ethics', *Network*, Number 11, (1979), p.7.

Chris Dunkley, 'Edna, the Inebriate Woman', *Times*, 22nd October 1971, http://callisto10.ggimg.com/doc/LT/WrapPDF=contentSet=LT=recordID=0FFO-1971-OCT22-009-F.pdf [accessed 31st March 2010].

Jean Fisher, 'Letters to the editor', *Glasgow Herald*, Wednesday February 14th 1973, p.10.

Alex Fraser, 'Letters to the editor', *Glasgow Herald*, Wednesday February 14th 1973, p.10.

'Gang book profit to help research', *Glasgow Herald*, Saturday 10th February 1973, p.14.

'Going Native in the East End', *Times Literary Supplement*, (September 9th, 1936), p.740.

Anthony Hayward, Obituary of Jeremy Sandford, *Independent*, 15th May 2003, http://news.independent.co.uk/people/obituaries/article36532.ece , [accessed 8th January 2008].

Margaret Kettlewell, 'Celia Fremlin', Obituary, *Guardian*, Sunday 6th September 2009, http://www.guardian.co.uk/theguardian/2009/sep/06/celia-fremlin-obituary , [accessed 11th July 2010].

William Langley, 'Profile: Polly Toynbee', *The Telegraph*, 26th November 2006, http://www.telegraph.co.uk/comment/personal-view/3634626/Profile-Polly-Toynbee.html , [accessed 27th December 2009].

Margaret Lassell, 'A Stupid Boring Job Makes a Stupid Boring Mind' Review of *A Working Life* by Polly Toynbee, the *Times*, July 26th 1971, p.8.

Andy McSmith, 'Polly Toynbee: Re-born as a Lady of the Right', *The Independent*, 26th November 2006, http://www.independent.co.uk/news/people/profiles/polly-toynbee-reborn-as-a-lady-of-the-right-425833.html, [accessed 20th Dec 2009].

Terence Morris, 'Poverty culture', a review of *A Glasgow Gang Observed* by James Patrick, *Times Literary Supplement*, May 18th 1973, Issue 3715, p.547.

George Orwell, 'Desert and Islands', *Time and Tide*, Volume 17, Number 47, (November 21, 1936), pp.1643-1644.

George Orwell, 'Real Adventure', *Time and Tide*, Volume 17, Number 29, (July 18, 1936).

George Orwell, 'Travel Round and Down', *Time and Tide*, Volume 17, Number 42, (October 17, 1936), p.1453.

Robin Page, 'Come bye and see us – sheep are back on TV', *Daily Mail*, 8th December 2007, http://www.dailymail.co.uk/tvshowbiz/article-500578/Come-bye--sheep-TV.html , [accessed 13th January 2010].

Robin Page, 'Country Diary', http://www.telegraph.co.uk/earth/earthcomment/country-diary/ , [accessed 13th January 2010].

Robin Page, 'Robin Page: Trying to Derail the Gravy Train', *Telegraph,* 21st May 2009, http://www.telegraph.co.uk/earth/earthcomment/country-diary/5357413/Robin-Page-Trying-to-derail-the-gravy-train.html , [accessed 13th January 2010].

'Profits on gang book offered to corporation', *Glasgow Herald*, Friday February 9th 1973, p.5.

Review of 'London's bad boys', *Times Literary Supplement*, (Thursday, January 21st, 1932), p.47.

Review of 'The Seven Chars of Chelsea', *Times Literary Supplement*, (August 3rd, 1940), p.379.

Review of 'The Unemployed Man', *Times Literary Supplement*, (September 21st, 1933), p.619.

Norman Shrapnel, 'Just Visiting', A Review of *Down among the Dossers* by Robin Page, *Times Literary Supplement*, 6th April 1973, p.372.

Julia Stuart, 'One Man and his Dog: the Robin Page Story', *Independent*, 24th March 2001, http://www.independent.co.uk/news/media/one-man-and-his-dog-the-robin-page-story-688730.html , [accessed 13th January 2010].

Time and Tide, Volume 17, Number 28, (November 28, 1936).

Time and Tide, Volume 17, Number 31, (August 1, 1936).

Time and Tide, Volume 17, Number 41, (October 10, 1936).

Time and Tide, Volume 17, Number 43, (October 24, 1936).

Time and Tide, Volume 17, Number 48, (November 28, 1936).

Times, 'A Year Amongst Americans', Tuesday 25th August, (1908), p.6.

Times, 'Recent Novels', Tuesday 26th November, (1889), p.8.

Times, 'Oxford-House, Bethnal-Green', Wednesday 12th June, (1895), p.10.

Times, 'University Intelligence', Thursday 25th January, (1906), p.7.

Phyllis Willmott, 'Bottom people', Review of *Down and Out in Britain* by Jeremy Sandford, *Times*, 10th December 1971.

JoAnn Wypijewski, 'Winners and Losers', A Review of *Hard Work* by Polly Toynbee, *The Guardian*, 15th February 2003, http://www.guardian.co.uk/books/2003/feb/15/highereducation.news , [accessed 20th December 2009].

Unpublished Theses

Enid Mumford, 'An Evaluation of Participant Observation as a Research Method for the Study of Work Groups in Industry', (Liverpool University, 1951).

Websites

Alcoholics Recovery Project, www.arp-uk.org , [accessed 26th August 2008].

James Atherton, James Atherton's Home Page, www.doceo.co.uk/me.htm , [accessed 27th August 2008].

William Blake, 'Jerusalem' (from 'Milton'), Poetry Archive, http://www.poetry-archive.com/b/jerusalem.html , [accessed 31st March 2010].

The British Theatre Guide, 'ACE [Arts Council England] announced the new council', http://www.britishtheatreguide.info/news/ACE-newcouncil.htm, [accessed 4th August 2008].

Famous, Rich and Homeless', BBC One Television, June 2009, http://www.bbc.co.uk/programmes/b00lfhhx , [accessed 29th March 2010].

'"Girls of the Town" Give Avon Street a Bad Name', http://www.thisisbath.co.uk/news/Girls-Town-Avon-Street-bad/article-469483-detail/article.html , [accessed 24th March 2010].

Guide to the E. Wight Bakke Papers, 1932-1971, Kheel Center for Labor-Management Documentation and Archives, Cornell University Library, 'Biographical Note', http://rmc.library.cornell.edu/ead/htmldocs/KCL05522.html, [accessed 21st June 2010].

LSE History, http://www2.lse.ac.uk/aboutLSE/lseHistory.aspx , [accessed 20th March 2010].

Professor Andrew Walker, Kings College London, www.kcl.ac.uk/schools/sspp/education/staff/awalker.html, [accessed 27th August 2008].

United Kingdom First party, 'About Us', http://www.ukfp.org/about-us , [accessed 13th January 2010].

Women Psychoanalysts in Great Britain, Pearl King,
    http://www.psychoanalytikerinnen.de/index.html?greatbritain_biographies.html , [accessed 21st June 2010]