

**Anthropological Practice  
Fieldwork and the Ethnographic Method**

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Dedicated to  
EDMUND LEACH  
who first introduced me to  
Social Anthropology

## Theoretical and Historical Overview

Anthropological fieldwork is the subject in practice. It cannot be reduced to the implementation of techniques. No one can rote learn what to do and how to be when moving among people whose daily lives and total context, unfamiliar or seemingly familiar to the researcher, are to be studied over an extended passage of time.

When anthropology applicants are asked to outline their research proposals and methods, would they dare reveal the following? That they will learn to shin up tree trunks, as Morris (see chapter 6) kept attempting in Tropical forest India; or pound manioc hour upon hour like Christine Hugh-Jones (1979); ride horses on migration in Afghanistan, as Lindisfarne (see chapter 6); take peyote on a sacred journey, as Myerhoff (1974); hunt monkeys for dinner with poison darts, like Stephen Hugh-Jones (1977); dance as did Smith-Bowen (1954) and Powdermaker (1967); learn to gut fish day long in Iceland (Johnson 1984); or walk Greek mountain paths barefoot on a pilgrimage, then write about the smell of incense like Kenna (2005). Should the monitoring committee know that anthropologists also make friends rather than interrogate 'informants'?

Will research proposals suggest the anthropologist will clean lavatories in a hospice (Hockey 1990), weep with the bereaved, play children's games the day long (Hardman 1973), or drink the water of the Ganges, as Parry (see chapter 6), when it contains the remnants of a burning ghat? I did not know that I would have to drive a 1,500-weight van for scrap collection, hand-milk cows and join twelve-hour Normandy banquets. I was to appear as character witness at the central London criminal court for a Traveller charged with kidnap, possessing a firearm and attempted murder. Rewarded as intellectuals, anthropologists use their bodies. Long out of the arm-chair, they have moved down from the verandah. They are at the mercy of their hosts' acceptance and then set on unpredictable paths. They can hardly mimic bureaucratic research designs and pursue a preordained project, increasingly set by a top-down managerial culture. Grounded theory may have recognized the back and forth of knowledge through process (Glaser and Strauss 1967), but not grounded in the whole being and the researcher's body. Such theory is interview-privileged and rooted in text and word, divorced from hand, heart, movement and the senses. By contrast, as chapter 6 will explore with vivid examples, the anthropologist puts his or her body on the line, at the disposal of the subjects. Knowledge comes through the skin and all the senses (Stoller 1989; Howes 2003; Okely 2006c) There is a relationship with the people(s) through continuing, not one-off, shared experiences. We are forever

changed in mysterious, unpredictable ways (Young and Goulet 1994; Coffey 1999; Borneman and Hammoudi 2009).

This book is about the possibilities and creative potential in ethnographic fieldwork. Although primarily addressed to anthropologists, there are lessons for other social scientists and beyond. Social anthropological fieldwork provides unique insights into long-term cross-cultural encounters. Few anthropology academic textbooks explicitly analyze fieldwork as what is done in practice. Courses have privileged sociological definitions of ethnography with positivist remnants. I explore the contrasts between pre-fieldwork assumptions with what anthropologists actually did. I had initially hoped to find the lived examples in the introductions, or even footnotes of anthropological monographs. These were elusive. I was thus drawn to tape-record informal dialogues, many up to four hours. The anthropologists were willing to divulge hitherto unrecorded accounts as superb narrative.

The book concentrates on aspects of the largely unique field practice of anthropology. Clifford (1988) and others argue that while the method of long-term immersion via participant observation is the hallmark of the discipline, few have explored its intellectual implications. Of the ethnographic method, Sanjek suggests 'anthropologists have done a better job of using than articulating it' (1991: 617). In Okely and Callaway (1992), progress was made in the discussion of the individual encounter and the need to explore further the means by which fieldwork is accomplished.

505 Autobiographical accounts have served as alternative approaches and subversions, defying any suggestion of universalistic rules of method. Through the personal, they 507 undermine the notion of the neutral data gatherer. I argued for their integration into the mainstream rather than as marginalized narrative for entertainment (Okely 1992). Fortunately, numerous edited collections of personalized fieldwork accounts have emerged (Bell, Caplan and Karim 1993; Young and Goulet 1994; Kulick and Willson 1995; Amit 2000; Dresch, James and Parkin 2000; De Soto and Dudwick 2000; Hume and Mulcock 2004; James and Mills 2005). These necessarily, by their format of individualized articles, remain detached from linked monographs and indeed from each other.

By contrast, this book synthesizes through one author/analyst the commonalities and contrasts in multifaceted individual dialogues. I have therefore inserted extended extracts from the spoken (not written) voices of each anthropologist. Nevertheless, the selection, editing and commentaries are my responsibility alone. Ultimately, texts 'are written from a particular author's point of view' (Hastrup 1992: 125).

These exchanges moved beyond any rigid interviewing formulae which the sociologist Anne Oakley (1981) so convincingly challenged long ago. They were dialogues between anthropologists where they could exchange parallel or contrasting experiences. Burgess rightly suggests that interviews can be conversations, but his example is of an adult researcher with school children, where there is a power imbalance with little or no reciprocity in the process (1984: 101–22). Similarly, while Dwyer attempts to avoid potential imbalance in *Moroccan Dialogues* (1982) to give

the perspective of the Faqir, there is little reciprocity, thus restricting the full meaning of dialogue as exchange. At the time, it was considered innovative merely to record individual lives (Crapanzano 1980), as later creatively confirmed by Caplan (1997).

For my dialogues, I chose individuals I knew, ensuring a trusting exchange. The occasional recorded encounter with relative strangers failed. Unease inhibited free dialogue. The majority of my dialogues were recorded in either the anthropologists' or my home space, with notable exceptions. That with Michael Herzfeld was recorded in a Copenhagen airport lobby. Malcolm McLeod, then Curator of the Huntingdon Museum, Glasgow, welcomed me to his office, while Helena Wulff and I sat in a Stockholm café. Her tape recorder malfunctioned, so I hand-wrote the answers. When she did not want personal confidences noted, she said: 'Turn the tape recorder off!' Indeed, many anthropologists trusted me to turn off the real machine at important, sometimes dramatic junctures. Louise de la Gorgendière, in her Edinburgh flat, insisted on ironing throughout the interview. Roy Gigengack and Raquel Alonso López brought their toddler son to my home. He, like Hélène Neveu's crawling baby daughter, found plenty of objects to play with in an academic's paper and book-heaped spaces. The anthropologists had the confidence to reveal hitherto hidden, unrecorded aspects of fieldwork. The extended, vivid quotations eventually pushed earlier chapter drafts to the edges.<sup>1</sup>

In reproducing quotations in this text, some of my own interjections and comparisons have been largely deleted to avoid repetition across interviews or the recycling of published narratives (e.g. Okely 1994b, 1996b: chapter 1, 2005, 2008). With a very limited word length, I have been obliged to reserve some aspects for publication elsewhere. These included: acts of recording through field notes and memory, then analysis and writing up (cf. Okely 1994a).

Although the book is ultimately one author's interpretation, nevertheless the text is dominated by multiple voices. The anthropologists proved to be brilliant narrators.<sup>2</sup> I challenge any high theorists' triumphant put-down that ethnography is 'just descriptive'. They are immune to the detail of human possibility. The minutiae in the anthropologists' testimonies carry profound theoretical implications, if the reader will only surrender to the emergent flow of knowledge.

These anthropologists have lived fieldwork in Afghanistan, in India, whether the tropical forest, Banaras, an iron and steel complex or a stone quarry south of Delhi. Others have lived fieldwork in Iran, tropical forest Malaysia, Indonesia, the Amazon region of Venezuela or Mexico City. Many have researched in Africa, in Ghana, Senegal, Uganda, Malawi, Sudan, Kenya or Nigeria (Okely 2010a). Others have done fieldwork in Europe, both before and after the collapse of communism, in Poland, Bulgaria, Macedonia and Slovakia. There are fieldwork testimonies from Western and Northern Europe, namely Sweden, Germany, Norway, the Basque country, England, Ireland, several Greek islands and New York. The continents are Africa, Asia, South America, North America, and Europe, north, south or central. Fieldwork collectively spans the late 1960s to the present. The anthropologists have

done fieldwork both in so-called remote localities (Ardener 1987) and in or near the Western metropolises.

For the younger researchers included here, fieldwork only commenced from 2001 and is continuing. My own fieldwork has been in Europe, namely Ireland, the United Kingdom and France, mainly from the late 1970s and through the 1990s. The work of these ethnographers around the world thus extends across space and time. While anthropologists have experienced the wonders and sometimes dangers of participation in alternative cultures, they have also confronted aspects of their own cultures which were taken for granted or controversial, indeed dangerous.

The anthropologists were of sixteen nationalities, including individuals of Japanese, Indian, Senegalese and Mexican descent. While the majority were of European and North American descent, the anthropologists included Polish, Swedish, Norwegian, Dutch, Basque, French, Maltese, English, Scottish, Franco-Canadian and US citizens. Their religious and ethnic identities also varied.

The narratives refute the critique that anthropologists have done interesting things but produced boring texts (Pratt 1986). The analysis of the material reveals an extraordinary set of both commonalities and some pertinent contrasts; all open to systematic theorizing. The consistent findings are indeed of generalized scientific value in the broader meaning of science (Okely 1996a). What emerges, indeed cascades, from the accounts are the tumultuous and unexpected experiences across the multiplicity of cultures. Anthropologists have quietly challenged the straight-jacket of Euro-American prescribed scientized methods which are now finally being questioned beyond anthropology (Law 2004). While methods 'training' had been persistently institutionalized through the 1990s in the United Kingdom, little or no interest was shown in earlier textbooks towards those approaches which did *not* fit a positivist, ultimately ethnocentric agenda.

Informally, it has been taken for granted that anthropologists should be open to what confronts them in the field. Indeed these anthropologists responded to the people's own interests and the specific context, avoiding pre-formed questions dictated by the anthropologists' academic cultural contexts. Thus anthropologists have in practice experimented for decades with alternatives. Yet these ingenuities and differences have not been formally and creatively expounded to challenge dominant models in social science.

Anthropological methodological silence has not been restricted to the United Kingdom. In 1997, it was claimed that 'most leading departments of anthropology in the United States provide no formal (and very little informal) training in fieldwork methods' (Gupta and Ferguson 1997b: 2). When lecturing at conferences and at Smith College in the United States, exposing the anthropologists' flexible practices, I was surprised by the relieved responses from US postgraduates, for example at the annual conference of the Ethnological Society (Okely 2003c).

There was likewise an absence of detailed discussion of anthropological practice. Postgraduates were puzzled as to why they had not been told what *actually*

happens in fieldwork. They were reassured to learn that established anthropologists had encountered experiences similar to their own. They had believed that changing research perspectives and making mistakes were proof of personal failure. Many methods textbooks circulated for anthropologists in North America and on book display, for example at the 2003 American Ethnological Conference, reveal similar positivist, pre-meditated intent. Fortunately, some informative wider-ranging methods books are emerging, for example that by Aull Davies (1999), although without the range of direct examples offered here.<sup>3</sup>

Without knowing in advance the outcome of my dialogues, I discovered many commonalities in the anthropologists' experiences and responses. All the anthropologists found very different concerns and conditions than anticipated, either on first arrival or after the initial period of participant observation. Everyone changed focus to a large or lesser extent. They delved into their own resources. Any prior reading, cross-cultural knowledge and indeed a range of disciplines and earlier life experiences, became a rich resource for comparative comprehension. When the verb 'to conduct' is used in relation to fieldwork, this implies that fieldwork is managed and pre-directed. The more satisfactory verb is 'to experience'. This is consistent with Borneman's concern with what 'anthropology does or can do in and through experience-based fieldwork' (2009: 6). Regrettably, a managerial *modus operandi* has increasingly been imposed on university research (Okely 2006a).

Fieldwork, in the tradition explored in this volume, is embarked upon and completed by the anthropologist, often alone. This is not as part of a multi-disciplined research team, as implicitly critiqued by Shostak (1981). The anthropologist is the embodied participant observer, researcher, scribe, analyst then author. The anthropologist can be a vulnerable figure in the field often, but not always, the outsider with prior affinities. Additionally, some anthropologists I recorded for this study were accompanied by their partners and children, with creative consequences (see chapter 7).

I have aimed to explore the total context whereby the anthropologist acquires knowledge through experience. Autobiographical insertions and accounts can always give illumination (Okely 1992). Too often, however, we have to search the crevices of the text for those throwaway remarks and anecdotes about lived practice. My doctoral supervisor, Godfrey Lienhardt tried to discourage me from including a chapter on fieldwork methods in my thesis. He advised relegating anything of this to an appendix. I refused and this became chapter three of my monograph (Okely 1983). My interest in practice had also been influenced by the obsessive questioning I faced when 'ordinary' non-Gypsy people discovered my seemingly dramatic and 'dangerous' field subject just a few miles' drive from Oxford (Okely 2008).

Many classical monographs have proved thoroughly engaging to anthropological readers and beyond. Subsequent generations have brought innovations to the genre. Regardless of the excitement of the texts, there remain absences and deceptions in the practice. Anthropologists, caught in the nets of scientism, have claimed

to be, or pretended to mimic, the detached observer, turned voyeur, when it has been thought that mere co-residence was sufficient. A few it seems (though not among those recorded here) never learned the first elements of the language and, like other social scientists, delegated 'data-gathering' to local interviewers armed with questionnaires. Somehow, it passed as anthropology merely because the fieldworker was in a foreign place. Some pioneers did not engage with their own bodies, except as passive sufferers of disease or as conspicuous strollers. They were living elsewhere, emotionally and bodily detached, while the data gathering proceeded perfunctorily. By contrast, anthropologists in this volume revealed a wide range of potential participation, depending on the appropriateness. The numerous examples are explored in chapter 5.

### Methodological Silence

Preparation for anthropologists, in Britain at least, where the majority of the anthropologists in this book were educated as postgraduates, has too easily relied on a notion of 'instinct', ideally detached interaction but in practice an open-ended approach. The absence of formal methods courses has changed in recent years. But it is important to examine the historical context of recent decades. Johnny Parry, whom I recorded, stated:

Edmund [Leach] was the most incredibly good supervisor, both as an undergraduate and as a post/fieldworker. But in the period when you're actually preparing and doing the research, this was general in Cambridge in those days—you were just left to get on with it. And: 'Come back and talk to us when you've got the data and we'll see what we can make of it'.

Suzette Heald described a similar approach in the late 1960s:

I had no fieldwork training. It wasn't done in those days. We had fieldwork seminars where someone recommended a particular HB pencil and someone else told us that notebooks six inches by three were a good idea as they'd fit into your pockets. In the field it was largely as though one was treading in the footsteps of one's ancestors, trying to learn the techniques by remote control through their books, and then finding out that it was all much more complicated. It was a question of establishing relationships. So, as to what works and what doesn't, there can't be a uniform answer since everyone will form a different kind of relationship.

When I studied with Leach, who was running a year-long postgraduate course devoted entirely to the works of Malinowski, the reading list included everything except his diary (Malinowski 1967), but this, as subsequently argued, gave unique insights into fieldwork practice (Okely 1975, 1996b: chapter 2). Leach declared that this diary should never have been published.

Traditionally it has been a stiff-upper-lip model (cf. Asad 1986: 142) or the dated sink-or-swim British attitude (Kenna 1992: 160). Anthropologists risked absorbing such values regardless of whether they had been schooled this way. The British elite 'public' school ethos of maturity through severance from the mother, and emotional detachment, in accord with an ideology exported with British colonialism, fits with the ideal of a cerebral, detached observer. Emotions and creative imagination are treated as disruptive, if not dangerously 'feminine'. Just as neophytes may be sworn to secrecy in the liminal stage, so the returning anthropologist had been encouraged to keep quiet. Self-revelation might be perceived as a loss of face or cracking of the masculine mask of competence (Okely 1992).

A researcher's pose as detached and trained 'data gatherer' is undermined by the leakage of tales of incompetence revealing sensitivity and entanglement. The self-possessed social scientist does not want to appear as buffoon but as the all-confident hero. Sondheim aptly suggested, as a female outsider, that anthropology 'is one of the rare intellectual vocations that do not demand a sacrifice of one's manhood' (1970: 189). Silences preserve the mystique of the researcher in control. By contrast, tales of mistakes, tears and laughter allow the hero(ine) to be someone with feet of clay and fractured ego. It is also clear that mistakes are made, whatever the prior field experience of the anthropologist, precisely because the relevant and detailed contexts cannot be predicted because they are part of the emergent discoveries (cf. Hume and Mulcock 2004). In this book, unique and original dialogues confirm the inescapable relevance of mistakes for vital insights, as Needham (1967) found out when he removed a tick from his flesh and threw it into the fire, to the consternation of his companions, the nomadic Penan of Sarawak.

### The Demand for Methods

In British and North American universities, methods courses are now *de rigueur*, but initially in response to political scepticism. The social sciences have long been pressured to prove their credibility. Steven Rose (1997: 8) suggests the 'predictive tag' was added:

precisely to privilege simple sciences like physics and chemistry . . . physics is . . . a 'hard' science, whose principles can be expressed mathematically and so it is supposed to be the model to which all other sciences should aspire. By contrast the social and human sciences are seen as the 'softest' because they are the least capable of precise mathematical expression.

Ironically, science has its own hierarchies of value. Rose argues how even biology has 'physics envy' (Rose 1997: 9). Some anthropologists now have 'biology envy'. Yet some early anthropologists brought some of the practices of biological enquiry to social anthropology, though not socio-biological reductionism.

The background to this volume has a specific history. In the early 1980s sociology was conflated by the political Right with communism and dubbed a 'non-subject'. Thatcher, a trained chemist, despite the fact that her successful 1979 election campaign was organized by Maurice Saatchi, with a first class sociology degree from the London School of Economics, attempted to abolish the UK Social Science Research Council. Her minister, Keith Joseph, selected the chairman of an independent enquiry. Lord Rothschild conducted a fulsome in-depth investigation and unpredictably defended, indeed celebrated, social science disciplines. He declared it would be an act of extreme vandalism if the Council were to be disbanded. In response, the Conservative government decreed that the Council drop the word 'science' and privilege economics; re-naming it the Economic and Social Research Council (Kuczynski 2006).

The new title privileged economics in an era of monetarism, Reaganomics and deregulation. The lucid prose of Keynesian economics had been lost in de-contextualized metric paradigms which set the agenda for all social science strategies. Increasingly phantom quantification, detached from any grounded knowledge, was to lead decades later to the banking crisis; then studied by an anthropologist experienced in ethnographic fieldwork (Tett 2009). It seems no coincidence that Lord Browne who devalued the arts, humanities and social sciences in his 2010 report for higher education, has an undergraduate degree in physics, though no PhD.

In the 1980s the newly named ESRC was indeed vulnerable (Bell 1984) and pressurized to prove its 'utility' in training for 'transferable skills'. All social science disciplines in the United Kingdom were at first to be subjected to a one-size-fits-all methods training. Fortunately, each discipline successfully argued for its specificity. The pressure, however, for formulaic methods mounted. Anthropology, with its practice-based traditions, had no ready cookbooks. Instead, anthropology students were filtered into other methods courses. Quantitative techniques, more appropriate to surveys, were simplistically downsized for 'qualitative hangers-on'. In the 1990s I watched my Edinburgh postgraduates' creative confidence crushed by course conveners from other disciplines. Anthropology was mocked for its lack of hypotheses and 'advance management'.

Such research priorities could already be inventions. Long before, the sociologists Ditton and Williams (1981) declared that 'the doable is unfundable and the fundable is undoable'. Theoretical sociologists may have abandoned positivism, but their colleagues who privileged survey research continued to teach methods asserting the primacy of 'replicability', the alleged dangers of 'contamination' and the ideal of numerical majorities for generalization. The aims of such 'rigorous' training hint at the rigor mortis of mind and body. Such positivist priorities hold their sway, like physics envy, in public discourse. In 2009 the influential journalist Polly Toynbee (2009) cited, as gospel, a psychologist's assertion:

It is as good a science as physics, says Rutter. A hypothesis is tested, each result raises further questions, and progress is incremental. Proof of accuracy is in replication.

The irony is that social science positivism has only vaguely replicated aspects of science practice. Scientists have also, as in anthropological fieldwork, exploited the role of chance and accident. Likewise, anthropologists in this study embraced serendipity (see chapter 3).

Some readers of earlier drafts of this chapter have questioned my ensuing discussion of the theoretical and historical context of anthropological research, paradoxically wanting me to launch immediately into 'descriptive' examples. There will be plenty in subsequent chapters. However, it is crucial to disentangle the many pre-suppositions and diversions in research proposals, their application and conclusions. While the abstracted theoreticians would dismiss ethnography as 'mere description', anthropologists need to expose the pseudo science or positivism long embedded in social science research. There is always theoretical potential in anthropological research and science in the broadest sense, as knowledge (Okely 1996a).

## The Hypothesis

In the 1980s the sociologist David Silverman (1985) argued that hypothesis testing was usually, although not exclusively, associated with quantitative research. He repeats his reservations about the potential of hypotheses in qualitative research (Silverman 2000: 7–8). Nevertheless, such approaches were largely overlooked in government funding. Steven Rose noted, 'so enthusiastically were Popper's ideas taken up that during the 1970s and 1980s grant applications in Research Councils in Britain tended to be turned down if they failed to state that the purpose of the proposed research was to "test the hypothesis that. . ."' (1997: 46). This continued well into the 1990s in ESRC application forms including those for doctoral grants. Eventually the request was changed to more open-ended 'research questions'. Nonetheless, Rose (1997) aptly commented that the 'testing mode' was replaced by 'relevance' and 'wealth creation'.

The State has obliged academic grant-giving bodies to prove their 'usefulness', something made more explicit over a decade later in the dying days of the 2010 UK Labour government and to be reaffirmed in the Browne Report by Lord Browne, formerly head of the maligned oil company BP. He advised no funding for the teaching of the Arts, Humanities and Social Sciences. Only 'science' was to be ring-fenced. At the same time the 2011 Coalition government attempted to impose its own political agenda on the seemingly independent Arts and Humanities, causing academic fury ('Academic Fury over Government Order' 2011).

Already, utility for universities had been extended in the mid 1990s to a demand for 'income generation' via links with businessmen as users. Benefits to minorities or the powerless were ignored, or dismissed. In my annual self-assessment for my head of department at Hull University, in answer to proof of 'outreach', I recorded having lectured (without payment) in the local prison and even recruiting two undergraduates

after their release. But this initiative was rejected as 'not income generating'. No matter that I later celebrated witnessing the ex-prisoners graduating.

The view of research, as producing short-term monetary utility as well as the predictable, destroys 'blue skies' openness which paradoxically brings *both* the unpredictable and unforeseen utility. Fortunately, many anthropology departments are increasingly open to innovation and creative scrutiny in methods courses. Elsewhere, positivist research practices are being challenged. A major sociologist has unpicked predetermined designs which ignore or deny the creative 'chaos' of the research process (Law 2004). Such debates continue. Felicia Hughes-Freeland, in our dialogue, declared:

Sometimes all the verbiage is meaningless. It's what the ESRC want with their datasets instead of our versions of events. It's as if you can go and collect 'stuff'. The words are the data, but there's something more. You get big chunks of quotation. I fall into that dodgy area. That criticism of interpretive analysis where your voice merges with that of the people you represent.

Research planning, based largely on quantitative criteria, is misleading not simply inappropriate. The anthropologist can never fully plan. Indeed, as this book demonstrates, she or he should *not* plan with precision. The unplanned incident or 'anecdote' can be the most informative. The anthropologist may indeed have to jettison preconceived notions. Chapter 3 explores the experiences of anthropologists where they had to reject prior plans once confronted with the unpredictable complexity of others' reality in the field.

## Science

Anthropology should also reject a dated view of science as applied to the study of human beings. While Comte asserted that human societies could be studied for universal laws, comparable to gravity, Evans-Pritchard (1962) argued no one had come up with a single universal law about human behaviour.

Another view of science privileges notions of falsification and repeatability, but again, the philosophy of science provides precedents for a healthy scepticism for ethnographers. Contrary to Popper (1961), scientists do not follow the ideal of setting up theories put through rigorous procedures of falsification. Scientists also form interest groups with unifying paradigms (Kuhn 1962). The community of researchers works within these until a new paradigm emerges, again without falsification procedures. So, even in science, the idealized model of falsification is inappropriate. Scientists also have flexible practices. With massive ideological power in the public imagination, however, science holds its mystique regardless of informal accounts by its practitioners who challenge the imagined projection.

Byatt, the novelist, claimed that science 'brings the truth, while the arts is just a story'; however, the physiologist with whom Byatt was consulting disagreed: 'Science is also a story. It is the best story we have at the moment. Science is about change and about curiosity' (1996).

It is therefore absurd to demand of social scientists positivist procedures which others may have abandoned. Anthropologists do not waste time with falsification experiments. They are open to better explanations if they emerge as part of the intellectual enterprise. Anthropologists take others' field material on trust; however, this does not preclude detailed scrutiny. The material has to be presented in sufficient depth and with coherence. Anthropologists use acquired ethnographic sensibilities from their own fieldwork to make sense of fellow anthropologists' new material, from possibly the other side of the globe. Their suggestions may be provisional, but they have the power of insightful comparison and contrast.

Just as fieldwork involves an openness to anything that may shake preconceptions, so theoretical conclusions are open to refinement. It may be that the entire paradigm is overturned. The subjects of interest may have shifted. The historical context affects the intellectual questions. There may also be circumstances when the intellectual's enquiry is silenced. The State or the market may support only deceptive short-term aims proving counter-productive in the long run. Before 9/11 many Arabic and Islamic studies and university departments in the United Kingdom and the United States had been run down. They had been deemed merely 'esoteric' pursuits for gentlemanly scholars.

Given the provisionality of knowledge at *every* stage, the scrutiny of one's own or other cultures should be recognized as an intellectual adventure for its own sake. There are risks and surprises. Anthropological fieldwork has not been pedestrian data collection. But there can be fear of admitting it in print, lest joyful discovery be thought to undermine scientific enterprise (Okely 1996a).

## Universalisms and Specifics

There is a tension between examining the specific and seeking the universal. Universal aspects of humanity are not the same as universal laws. Social anthropology has had phases of examining universalisms, for example the incest taboo or the claim that everywhere societies distinguish between nature and culture (Lévi-Strauss 1973/1977: 321), subsequently contested (Bloch and Bloch 1980). Nonetheless, social anthropologists are cautious about world generalizations because their knowledge is steeped in the minutiae of differences and exceptions. Western psychologists, economists and some sociologists may assert universalisms which are in effect ethnocentric. The same applies to socio-biologists and popularist evolutionists. Symbolic-interactionists have also sought universalisms, so generalized as to be banal, while subtle differences are overlooked.



The privileging of the universal in knowledge quests has consequences for the recognition of cross-cultural examples with the potential for enlarging understanding. If differences are pushed to the margins, ethnocentrism retains its hegemony. Differentiations within the 'West' are also bulldozed in the rush for generalizations. Regrettably, anthropologists who insist that the discipline should be focused primarily on non-Western localities have themselves generalized about some imagined homogeneous 'West' (Okely 1996b: 5; Houtman 1988).

### Generalizations: Reliability versus Validity

Generalizations are a subcategory of universalisms. The standard quantitative critique of detailed field studies is that the material from participant observation is 'valid' but not 'reliable' because it is not generalizable. Reliability is associated with a 'measurement procedure', repeatability and numerical criteria (Marshall 1994: 446).

Social anthropological studies based on intensive fieldwork in a limited locality are then judged to be 'not reliable'. Seemingly, generalizations cannot include even neighbouring localities. This critique is fashioned for mass surveys and presumes that the research is confined to a single geo-political domain. Paradoxically anthropology confronts questions *beyond* any mapped locality or bounded domains to far reaches of the globe by comparison and contrast. The emergent knowledge raises questions which may be asked across space and time. Debates are triggered by the challenge of differences or similarities across continents. In contrast, quantitative reliability is culturally and geographically confined and its advocates necessarily parochial.

### Earlier Ethnography: Own Can Be Other

Understanding others beyond the familiar has centuries of history. Rosalie Wax (1971) brilliantly outlines this, moving from Herodotus in the fifth century BC through to the late twentieth century and the evolution of ethnographic fieldwork. Charles Booth's 1880s studies of English life mix statistics and participant observation with detailed descriptions. The Webbs studied the London urban working class. Ethnography has long been associated with the study of others; what is unknown or outside the regional context. The so-called exotic can be within the same politics, but across class, culture, the urban or rural. It would have been counter-productive to confront suspicious, often non-literate people with questionnaires.

Whyte's study of Chicago street gangs (1943/1955) with his 1955 appendix on methods was a landmark. Many of the Chicago school of sociologists confronted their white, male, middle-class identities and the unknown quarters of their own city. It

should not, however, be concluded that such research should be confined to deviants and the underprivileged. It can and should be used for studying up (Nader 1974; Nash 1979a; Tett 2009). Nevertheless, such methods are easier with the powerless. The powerful by definition can block access as well as publication (Punch 1986; Okely 1987: 67–8; 1996b: 25–6).

### Numbers Unnecessary

On a micro level there are strengths from analysis which reveal a *system* where numbers become irrelevant for explanation and certainty. Edwin Ardener (personal communication, 1987) suggested looking at a room with multiple chairs. Someone could do wonderful charts recording the chairs in a certain position. The layout could be measured when in a circle, then in a square. The investigator could plot how the chairs were shifted during the week. But the quantitative data would ultimately be unnecessary. All could be explained by saying: 'This room is a dining room, sometimes used for assembly, sometimes for a dinner dance.' A qualitative study thus throws light on quantitative material when the *system* is revealed. Such arguments were made at a previous stage of early anthropological fieldwork practice (Kuklick 2011).

Similarly, Leach (1967), in a devastating critique of a survey-based study, argues that one in-depth micro study can best explain a mass of quantitative data. Long-term participant experience helps to make sense of even the most detached survey. Confronted with an extensive survey of landownership in fifty-seven villages in Ceylon (Sarkar and Tambiah 1957) Leach (1968) drew on his fieldwork in just *one* village to counter the interpretations of the statistics. The survey concluded that 335 households were landless peasants. From his detailed observation of inheritance practices, however, Leach revealed that over time a considerable number of the young would inherit land. Many sharecroppers were in fact heirs.

Leach (1967) argues that there is 'a wide range of sociological phenomena which are intrinsically inaccessible to statistical investigation of any kind'. Whereas the survey sociologist focuses on 'units of population' and 'individuals', by contrast, the anthropologist envisages 'systems of relationships'. Just as feminists pointed to the inadequacies of privileging 'the (male) head of household', Leach questioned the tradition of singling out one individual as representative of a household: 'The anthropologist . . . purposely chooses a small field within which all the observable phenomena are closely interrelated and interdependent' (Leach 1967: 87).

Leach argued that some of the interpretation in the study was convincing only because the main researchers, already familiar with the region, arrived inadvertently 'at their conclusions by intuitive methods' (1967: 76). I suggest that 'intuition' is acquired experientially, whether in one's own or other culture, after intensive fieldwork.

## Hypothesis-led Research

The emphasis on understanding a system contrasts with the unidirectional hypothesis. Leach's mentor, Malinowski, declared;

Good training in theory, . . . is not identical with being burdened with 'preconceived ideas' . . . Preconceived ideas are pernicious in any scientific work, but foreshadowed problems are the main endowment of a scientific thinker. (1922: 9)

Another student of Malinowski, Powdermaker, insisted:

A lack of theory, or of imagination, an over commitment to a particular hypothesis, or a rigidity in personality may prevent a field worker from learning as he stumbles. (1967: 11)

The privileging of the quantifiable comes from hypothesis-led research. Agar outlines the essentials:

A hypothesis . . . has some predicted truth value . . . [and] states a relationship among a group of variables . . . To test the hypothesis, some kind of measurement is necessary. . . Defining how values are assigned is called an operational definition. (1980: 63-4)<sup>4</sup>

Operationalization is defined as:

the transformation of an abstract, theoretical concept into something concrete, observable, and measurable in an empirical project. Operational definitions are . . . crucial to the process of measurement. (Marshall 1994: 368)

For anthropologists both hypotheses and their operationalization may be inappropriate and counter-productive.

## Neutrality

In much of the social sciences there have been presumptions that the researcher should be so neutral as to have no influence on the encounter. This is clearly impossible in anthropological fieldwork, where the participant observer must either be involved or perish. Survey research, in the quest for reliability, works with the metaphor of 'contamination' of interviewees or research subjects. Thus 'having once interviewed someone, a repeat interview may be contaminated by the earlier experience' (Marshall 1994: 447). The anthropologist, by contrast, needs to interact on multiple occasions with the same individuals in the field. 'Contamination' through daily contact is a sign of integration not failure. The subjects are implicated in the fieldworker's presence. Granted, some anthropologists have ignored

or denied the implications of their presence. In practice there were always tell tale signs.

Scientists are themselves questioning the value placed on detachment and invisibility. It is therefore even less plausible that social scientists should cling to this in the name of what they imagine hard science to be. Social scientists are listening through keyholes and behind doors, the other side of which they imagine what scientists are up to.

Controversies within science challenge notions to which social scientists have outmodishly clung, including the contrast between quantum physics and classical physics. Only the latter is premised on the existence of a reality separate from the observer. The former confronts the role of the questioner. The study of humanity poses even greater complexity in that human beings can respond with infinite diversity and with relative autonomy.

## 'Reality', Othering and Autobiography

Anthropology has engaged with postmodernism and earlier misgivings about the fixity and objectivity of 'reality out there'. Labelled a 'crisis of representation' (Halstead 2008), this is not only an epistemological crisis but one based on political context. Post-colonial critiques confront the political history of anthropology, traditionally a study of 'the other' by Europeans and by non-indigenous North Americans.

The provisional character of ethnographies, scepticism about the existence of solid and external 'facts' as things, the constructed production of the published text (Clifford and Marcus 1986) and the specificity of the anthropologist as category (Okely and Callaway 1992) in relation to the people as research subjects: all these dilemmas have been widely debated. Nonetheless, fieldwork guidance in many textbooks continues only partially touched by these debates. Alternatively, they are used to reject the tradition of fieldwork 'in favor of mimicking textual analysts' (Borneman 2009: 9).

For a while, the standard monograph sidelined the innovative debates in articles or autobiographical accounts. Given the emergence of experimental texts, there often remains a curious split between the reflexive examination of field practice and other less personalized theoretical developments.

## Definition of Ethnography

Qualitative research's association with the word 'ethnography' has had potentially misleading consequences. In sociology there has been a strong presumption that ethnography is aligned to only one theoretical perspective, namely symbolic-interactionism, which tends to focus on the immediate minutiae of one-to-one encounters at the expense of any wider structure (Hammersley and Atkinson

1983; Silverman 1985). This is a betrayal of the pioneering Chicago sociologists who first created the term participant observation.<sup>5</sup> This does not mean of course that those who first devised the label were the first to practise the method (cf. Kuklick 2011).

The term 'ethnography' in anthropology has different meanings and history. Social anthropologists will have accumulated a mass of material from long-term fieldwork. It is this body of knowledge through which anthropologists may work for some years and continue to draw ideas. Ensuing monographs are also referred to as ethnographies. Ideally, monographs are both theoretical and rooted in fieldwork. The theoretical stance can be the full range, from Durkheim to Radcliffe Brown, from Marx to Weber, Gramsci, Derrida, Althusser, indeed as many of the theoretical perspectives as exist in the social sciences. Some lauded theoreticians, such as Foucault, have been most brilliant when combining historical ethnography with embedded theory, as in *Discipline and Punish* (1977).

Ethnography can also refer to specific field material. Anthropologists may ask of each other 'But have you got the ethnography right on that question?' or 'You don't seem to have enough ethnography to convince us'. It is assumed that the specific detail emerges from a larger corpus. Occasionally, it may be said that someone has some wonderful ethnography, but doesn't seem to know what to do with it. Accumulated facts are not good enough on their own. Theoretical issues are emergent as I outlined in the American Association of Anthropologists (AAA) Distinguished Lecture for the Society of the Anthropology of Europe (Okely 1998).

Anthropology's well-grounded ethnography has risked being downgraded by high theorists as 'just description'. For anthropologists however, anything 'descriptive' is already highly loaded and selective. Continuous choices are made: be it topic, locality, group, event or specific statements. Description may or may not render explicit the emergent theories. There is a major contrast with other social scientists' practice of separating so-called 'substantive' from 'theoretical' issues; something I was to discover among my then colleagues in a prestigious sociology department.

This is another legacy of quantitative empirical traditions where hypothesis as theory was separated, as organizing principle for the ensuing survey-induced facts. There are indeed vast divisions in sociology between theorists who are rarely involved in direct empirical research and those steeped in it. The former, I learned from Ted Benton and Ian Craib, my then colleagues at Essex University, may have greater intellectual empathy towards ethnographic approaches than quantitative empiricists. Although there may be chronological changes in emphases among anthropologists, where the monographs may be followed by more general overviews of the discipline, there is little place for the revival of the hegemony of the nineteenth-century armchair anthropologist.

## Holism

Malinowski's approach is close to an holistic tradition:

the field Ethnographer has seriously and soberly to cover the full extent of the phenomena in each aspect of tribal culture studied, making no difference between what is commonplace, or drab, or ordinary, and what strikes him as astonishing and out-of-the-way. (1922: 11)

Ethnography, as understood and practised by anthropologists, often commenced as a detailed description of every aspect of a particular topic.

The significant contrasts between anthropological and sociological empirical research have implications for analysis (Okely 1994a: 18). The two disciplines came from different historical contexts. Sociology's empirical work was concerned mainly with Western societies of which the sociologist was a member.

Unlike the sociologist, the anthropologist could not take much as given. She or he could not isolate one theme extracted from a wider context since the society as a whole was largely unknown to the researcher and undocumented. Rigidly formulated questionnaires were inappropriate. The very interview mode is culture bound. The sociologist could afford to be more presumptuous in knowledge of the wider social context. Whereas Durkheim (1897/1952) could claim to identify and subclassify suicide in France, Malinowski (1926) had first to discover and then redefine such a practice among the Trobrianders. He had no official statistics, let alone context.

The historically divisive association of sociology with Western societies and anthropology with non-Western societies is no longer appropriate. Each discipline has strayed into the other's territory. While retaining its traditional methods, social anthropology can be used in the study of *any* group or society (Okely 1996b).

## Armchair to Verandah, Tent to Tarmac

In the nineteenth century so-called armchair anthropologists such as Frazer (1890) lived off the material brought back by Westerners who had travelled to distant places. Travellers, traders and missionaries followed the traditions of the explorers and conquerors. In the many myths of first encounters 'the others' were often accredited the inverse of the incomers' norms (Arens 1979).

The perception and selection of material were governed by the search for peoples allegedly at an earlier stage of mental and social evolution. Little thought was given to the notion that (a) the peoples had their own histories and centuries of transformations and (b) the peoples' own voices, in their own words, should be heard and listened to (cf. Tonkin 1995).

With the professionalization of ethnology, sets of questions were sent to local Western officials about a range of topics. This was possibly the initiative for *Notes and Queries* (1874/1951) devised by the Royal Anthropological Institute. Eventually it was recognized that secondhand information from non-scholarly amateurs, was untrustworthy. While this method had some similarities with questionnaires and pre-decided questions of relevance, these were not administered to the indigenous peoples but addressed to the Western outsiders, taken to be experts.

It was gradually recognized that ethnologists themselves had to enter the field for first hand research. Scholars, such as Haddon and Seligman, journeyed to New Guinea (Stocking 1983). Many of the early anthropologists were in fact trained as biologists, with a tradition of amassing observational material without too many pre-filtering hypotheses. The practice obviously excluded questioning the non-human subjects. Instead, biologists depended on grounded observation of the *total* field site for their study of the vegetation and wildlife environment, to anthropology's profit.

Thus the early ethnographers inherited the recognition of the total context, as opposed to extracting the individualized subject in 'uncontaminated' isolation. This prefigured what was to become the classical holistic approach in anthropological fieldwork. Sensitivity to the immediate wider context was thus fortuitously significant for the study of human cultures and actions. It does not follow that this methodological strategy in subsequent anthropological fieldwork is selected for the study of an imagined 'primitive humanity in its natural state' (Gupta and Ferguson 1997b: 8).

The pioneering scholars, driven by enquiry, snatched at any immediately available information, without prior language training. Thus what I label the 'verandah' anthropologists, as in the photo of Seligman (Stocking 1983: 82), depended largely on interpreters in extended and semi-formal interviews with indigenous peoples. Such bilingual intermediaries, even if they had a grasp of the indigenous language, might not make satisfactory translations.

Brian Morris, the first anthropologist I tape-recorded in this study, provides an excellent example of how, even today, an indigenous interpreter may mistranslate not just technical words but deliberately transform the cultural concepts in the interpretation of local medicine. In Malawi, Morris witnessed a Scottish botanist on a brief visit interviewing, through an interpreter, the local medicine man about his use of specific herbs. Morris, fluent in the local language, noted that when the herbalist explained that one herb was for dealing with 'spirit possession', it was translated for the academic as a 'headache'. When the medicine man was asked about the use of a very phallic-looking plant, he said it was for 'impotence' but the local interpreter translated this as 'stomach ache'. It seems the interpreter was *not* lost for words, but seemed determined to shield his people from being branded 'primitive'. Likewise cross-cultural knowledge and deliberate mistranslation by interpreters may also have affected the answers for the verandah anthropologists.

Those early researchers began to communicate in intermediary pidgin languages. Others came to recognize the importance of communicating directly. Malinowski (1922) presented himself as the originator of intensive fieldwork, whereby the anthropologist moved from verandah to tent, which he pitched in the centre of the village. Stocking (1983) argues that others before him pointed to this. In any case, recent anthropologists, guided by the celebrated Malinowski and his students, have taken as given the need to live alongside their subjects. In my case, motorway road-side Gypsy encampments with tarmac surfaces in the British Isles. This continuity through change should be celebrated rather than destabilized by a new return to the privileging of armchair theorizing over fieldwork, downgraded as seemingly quaint (Borneman 2009: 8–9).

### Holism Not Hypotheses

A significant methodological outcome of the earliest anthropological fieldwork was an approach whereby the topics and focus could not be simplistically formulated in advance. In the study of an unfamiliar, non-literate culture, with few if any written records, nothing could be taken for granted. Specific issues could not be privileged to the neglect of all else. Everything and anything could be relevant for recording and interpretation. Thus social anthropology was ironically saved by biologists from a pastiche of laboratory techniques (cf. Kuklick 1991).

Anthropological practice generates unique material. Despite months of literature reviews, possibly years of theoretical and comparative reading, hypotheses will be ejected like so much ballast. The people may not live as recorded. There could be famine, strife or abundance. Rituals may be missionized, nomads dispersed, documentation distorted or concealed from the outsider. The original focus may be an irrelevance, as chapter 3 documents.

As fieldwork traditions have developed, the anthropologist learns about the group or culture by immersion over an extended period of time. It has been generally accepted, at least in British anthropology, that fieldwork should be for a minimum of one year. Other traditions and new approaches have been embraced. Repeat visits are what Wulff has called 'yo-yo fieldwork' (2002). Fieldwork has not necessarily entailed fixture in one tiny location. Malinowski travelled from island to island on the celebrated Kula expeditions. 'Multi-sited' fieldwork has emerged as acceptable practice (Marcus 1998). Regrettably, some have interpreted this as being ever on the move. 'Do not linger seems to be the motto' (Hammoudi 2009: 25). Many have consolidated their initial fieldwork with follow-up years (Kenna 1992, 2001a,b). In addition, anthropologists have explored return fieldwork and reanalysis through decades (Hirsch, James and Parkin 2000; Howell and Talle 2011).

Historical ignorance of classically a non-Western 'exotica' encouraged a recognition that ritual, kinship, the economy, politics, religion and many other aspects were

to be comprehensively included. In this holistic approach a single custom or practice could not be torn from context. Holism coincided also with the rise of the more controversial functionalism as developed by Malinowski and Radcliffe-Brown. This had inbuilt problems, in that every custom and practice was deemed to be contributing to a balanced harmony from which conflict and change were banished. Moreover, Malinowski reduced social structures to the satisfaction of a few biological needs. Thus, although his ultimate theories may be challenged, Malinowski's holistic approaches have a fruitful legacy. His covert biological reductionism does not exclude others' potential privileging of the politico-economic base.

Given the flaws of functionalism, it does not follow that holism need also be jettisoned. There continues to be a vital case for studying groups or practices in the wider context and as much in 'the West' and the anthropologist's own culture(s). In my research among the Gypsies I found it especially important, thanks to Leach's influence, to be open to classical holism, thus studying the inter-relations of the Gypsies' economy, politics, kinship, ritual and travelling aspects of the community (Okely 1983). The holistic study, as examination of the total context, necessarily extended to relations with non-Gypsies, the wider politico-economy, as well as non-Gypsy ideological representations (Okely 2008).

Asad's critique of holism (1973) is misleading by presuming that it is necessarily limited to the micro. Holism can be extended to the global. It need not be confined to the imagined isolate of a bounded village, but should extend to wider terrains, be they national or global. Thus anthropology has moved beyond any earlier tradition which sometimes tended to construct isolates, regardless of colonialism and world systems. Anthropological research progressed from Frazer's armchair (1890) to verandah to tent and now tarmac, namely 'where my caravan was resting', or any location, urban or rural, and anywhere in the globe.

Regrettably, some social scientists have presumed that the overall anthropological concept of culture, which should include political economy, is as restricted as that defined by Spradley:

The concept of culture as acquired knowledge has much in common with symbolic interactionism, a theory that seeks to explain human behaviour in terms of meanings. (1980: 8)

Perhaps that restricted definition explains some 1980s and later neo-Marxist hostility to anthropology (Okely 2007a: 240).

The rippling outwards of multifaceted holism is very different from the generalizability with variables of which anthropologists have been cautious. Agar argues that holism helps understand:

why ethnographers are cautious with the idea of a variable. For what is a variable but something that can be measured in a standardized way across situations, across people, across groups, and even across cultures. (1980: 76)

For the anthropologist such variables, if they exist, can only be banalities if they extend beyond all contexts. Nevertheless, Agar, in 1980, regretted that anthropologists were increasingly moving towards 'hypothesis-testing methodologies'. He footnoted that 'many of the recent text books in anthropology emphasize quantification, standardization, and hypothesis-testing'. Thus the positivist pressure elaborated in the United Kingdom, after the Thatcherite threats to the Social Science Research Council, had already emerged in the United States.

Intensive research methods may have been adopted initially in classical anthropology's study of exotica for instrumental reasons. But these methods have force *anywhere*; in the anthropologist's own or another, less familiar, culture. Intensive fieldwork by one person, wherever the locale, throws up a special or different type of material. The theoretical and methodological approaches from anthropological participant observation can be pursued in literate and industrialized contexts, in the metropolis and all continents, as this book will demonstrate.

Any group, culture, area or subject can be approached as if all is strange. Anthropology's need for constant awareness of cross-cultural comparisons and its 'technique of estrangement' (Lévi-Strauss 1973/1977: 272) can be fully exploited, wherever the place. Inevitably, the very selection of a research area is a limiting act of definition, influenced by intellectual, political and theoretical concerns; even apparent whims. Within those bounds an holistic approach does not proceed by pre-selection.

An open-ended approach allows and encourages questions to *emerge* throughout the endeavour (Okely 1998). The material speaks for itself and to the researcher. It presents its own problems, which are unforeseeable. The people speak out of turn and as they are wont. Chapter 3 reveals the continuous and necessary responsiveness by anthropologists to the people's voices and concerns.

## Advance Knowledge/Theory

To reject hypotheses is not to abandon advance theoretical knowledge and a sense of enquiry. The anthropologist embarks on field research with all her past reading and intellectual instruction. The research is free to generate new theoretical problems rather than be constrained within old ones.

Seemingly, positivism lives on among some economists. As recently as 2006, I watched an Oxford development economics professor browbeat an anthropology postgraduate, insisting that she have a hypothesis before embarking on intensive fieldwork among nomads in the Middle East. Otherwise, he asserted, she would 'only drift'. No matter that she spoke the language and had spent some time with the people, yet wanted, and indeed needed, to find her way among them and on their terms.

An advance hypothesis is not the same as advance knowledge. Acquaintance with a wider range of knowledge will necessarily be advantageous. Agar suggests:

When you attempt to describe some aspects of a group's life, you may be drawing from conversations, casual observations, twenty formal interviews, a previous ethnography, two novels, your general idea of the human condition, childhood experiences with your parents and who knows what else. (1980: 6)

The influences start long before the process of description, interpretation and writing up. The experience of fieldwork is also directed by the anthropologist's knowledge of the discipline. This includes admixtures of theory and cross-cultural comparisons. Agar, in Kentucky, found there were informative similarities with his previous work in south India (1980:16).

Similarly, the prior reading of details of the Trobrianders, the Azande or the Balinese offer resonances wherever in the globe the anthropologist may be standing. The quest is not for comparisons through quantifiable reliability, but critical questioning arising from the anthropologists' other cross-cultural possibilities. Both contrasts and similarities from elsewhere stir the anthropologist's thinking. Where the anthropologist is grounded, fieldwork has the potential for *lateral*, not just linear, knowledge.

### The Funnel

A potential fieldworker may be asked by non-anthropologists, what 'theory' she or he has adopted and is 'testing'. If the anthropologist admits to no such thing she or he risks being labelled a vulgar empiricist or someone concerned only with a-theoretical description. Agar, finding himself 'the lone ethnographer among sociologists and psychologists', was frequently asked, during his study of narcotics: 'Where is your instrument? What is the sample design? What is your plan of analysis?' (1980: 16). He suspected these were the wrong questions, yet was unable to explain why.

Agar offers a superb alternative metaphor for fieldwork practice. Somewhat mechanistic, it doubtless allays the worries of those wanting proof of research 'tools'; he calls it a

'funnel approach', with breadth and humanity at the beginning of the funnel, and then, within the context of that beginning, depth, problem-focus, and science at the narrow end. (1980: 13)

From the outset, the anthropologist adopts an open-ended approach to the full range of information and to all manner of people. The material and ethnographic concerns are not cut to size at the start. The people, as subjects, are themselves freer

to volunteer their concerns in their own voice and context. All this has implications for the kind of material and field notes which the anthropologist is faced with when writing up.

Both during fieldwork and after, themes emerge. Patterns and priorities impose themselves upon the ethnographer. Voices and ideas are neither muffled nor dismissed. To the professional positivist, this seems like chaos but is creatively inevitable (Law 2004). The voices and material lead the researcher in uncontrollable directions. This indeed is *not* a controlled experiment. The fieldworker cannot separate the act of gathering material from its continuing interpretation. Ideas and hunches emerge during the encounter and are explored or eventually discarded. Writing up involves a similar experience. The ensuing analysis is imaginative, demanding and all-consuming. It cannot be fully comprehended at the writing-up stages by someone other than the fieldworker.

### Serendipity

Given this open-ended approach at every stage, the anthropologist is or should be 'disponible', a term I have borrowed from Breton (1937) and surrealism. The anthropologist is available for and open to what may come *par hasard*, however irrational and absurd at first encounter. Susceptibility to that which is above and beyond the 'real' is integral to anthropological experience. The ethnographer has, like the surrealist, to be open to *objets trouvés* (found objects) after arriving in the field. The anthropologist learns to abandon ethnocentrism and looks for the strange in the familiar and sees sense in the strange. She or he flies above the restrictions of realist banality and pedestrian common sense.

Knowledge is, at crucial stages, acquired through accident. We accept from childhood, stories about Archimedes' discovery when he was lying in a bath that a body immersed in a fluid shows a loss of weight equal to the weight of fluid it displaces. His famous cry 'Eureka!' has become a noun, meaning a brilliant discovery. We also learned the fable of Newton sitting under the apple tree, and comprehending the law of gravity thanks to a falling apple.

The great scientists were open to chance and non-directive thought. Moreover, many of their discoveries were made in moments of relaxation or dreamy contemplation; in the bath or under the apple bough. Likewise, anthropological practice includes moments of nondirective discovery. Lévi-Strauss (1955/1973) described how the anthropologist might spend days waiting seemingly doing nothing. As with Archimedes and Newton, anthropologists combine such 'drifting' days with months and maybe years of concentrated and diligent background work. Yet in social science adulthood, we are expected to put away childish things to see through a glass darkly. Knowledge, it seems in the training manuals, can only be acquired through purposive, cerebral intent and tunnel, not funnel, vision.



### Majority/Minority: Overt or Covert

Ironically, with the rise of neo-liberalism and the ideology of the free market reducing state welfare obligations in Britain and elsewhere, the need for majority-based policy research has diminished. Instead, statistical information may be laundered to reduce official unemployment figures and poverty levels. Qualitative research has taken on new significance in mass media contexts with the use of focus groups and the emphasis on 'target' minority groups; both rich and poor, powerful or peripheral. From 1997, UK New Labour spindoctors, following the practice of advertising, saw the relevance of qualitative focus groups. But again, the potential of detailed and in-depth ethnography was ignored. Decision makers with power have rarely pursued the findings from participant observation.

There has been one ancient exception; namely, its covert use in the long-established tradition of industrial and political espionage (see chapter 2). Here (Okely 2006b), positivist and number crunching criteria, too frequently demanded by government and other funders for social science, are recognized as utterly irrelevant. Cumulative anecdotes, which ultimately expose systems, are more informative than numbers, as Leach (1967) long ago confirmed.

Post 9/11, anthropology has witnessed new attention by the Central Intelligence Agency, the MI5 Security Service and the military. While the UK Research Councils continued to privilege quantifiable data and quantitative research methods, the centralized state apparatus is looking to qualitative methods and ethnography for political intervention, if not conquest. Numbers are good for public rhetoric and voter majorities. While democracy is seemingly displayed through statistical arguments, real power lies in systems. Ethnography finds these. And the powerful must know and command them. As I discovered through decades of research among Gypsies and government policy (Adams et al. 1975), and explored in the Third Eric Wolf Lecture (Okely 2006b), when it suits the state, ethnographic knowledge can inform and change policy. Other times, it will be abandoned or ignored if politically inappropriate or merely embarrassing.

### Writing Up

It is the practice for the anthropologist to be both fieldworker and analyst/author. Division of research labour into discreet tasks or between individuals is at a minimum. The anthropologist/fieldworker records, interprets and writes up her or his own material. For the anthropologist, the stages of knowledge as the research progresses are not sectioned between persons. There is no need for mechanical procedures and managerial instructions to ensure uniformity of perspective along chains of command. The anthropologist does not have to check and double check whether numerous assistants and interviewers have understood or even faked the collection

of data, as happens in delegated questionnaires. She or he has instead to look to her- or himself and her or his specific relationship with the people who are the subject of study. The anthropologist becomes the collector and a walking archive, with ever-unfolding resources for interpretation (Jackson 1990).

By contrast, I recall a social scientist in my research centre in the 1970s asserting that in order to follow the correct scientific procedure to ensure 'objectivity', ideally someone other than the fieldworker should write up the final report using my field notes. The fact that I completed the task myself was seen uneasily as a form of intellectual cheating rather than a scientific necessity and standard anthropological practice.

Such a division between collection and analysis might be possible in a research tradition where the researcher delegates the 'data collection' to a reserve army of interviewers with pre-ordained questionnaire and clone like application. Thus the pre-selected choice of answers gives material which can be mechanically classified for the analysis (Okely 1987: 59–60). Anthropology does not work that way. Similarly, the multi-faceted anthropological approach permits variety and the full range of writing styles, now celebrated without apology (Clifford and Marcus 1986; Van Maanen 1988, 1995; Geertz 1988: 140; Sanjek 1990; Bradburd 1998; Beatty 2009). Malinowski (1967) spent hours reading novels in the field, and this showed in his publications. Literary, indeed poetic, traditions are just as appropriate for non-fiction. Anthropologists spend years with the full range of humanity whose many nuanced languages and experiences transcend banal reductionism and scientific pastiche.

Although the anthropologists in this study were asked about their recording, note taking, analysis and writing up, limitations of space preclude exploring this in detail (cf. Okely 1994a), but to be developed elsewhere, for example in Wulff (forthcoming). I also argue that issues of ethics rest mainly with what the anthropologist chooses to publish rather than pre-selection of questions and topics. The anthropologist cannot always extract her- or himself from witnessing controversy but she or he can hold back from individual identification (Okely 1999a) and full publication (cf. Okely 2005). Nevertheless, the broader political issue of the subject of research has been transformed and narrowed in recent decades to a bureaucratic controlling gaze, now labelled ethics (see chapter 3).