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## **The embodied ethnographer: journeys in a healthcare sub-culture**

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### **Abstract**

This paper is a reflective account of conducting ethnographic research in training institutions for alternative and complementary medical (CAM) practitioners. The discussion will focus particularly on issues of embodiment (mine and my participants) and ethics.

In this paper, I argue that the ethnographic process is a way of constructing knowledge about the world that we live in, but knowing is not separated from the knower and so, as Jaggar argues, 'The reconstruction of knowledge is inseparable from the reconstruction of the self' (1989: 164), and moreover that that reconstruction is fundamentally embodied. Through exploring motivation for the study, access and immersion in the field, and the writing process, I will elucidate where my research touched and negotiated issues related to the ethics and politics of doing research.

Ethnography, with its origins in the anthropological studies of 'others', such as tribal communities, the community studies approach and the Chicago School, seemed an obvious choice for a study of groups of marginalized healthcare professionals. The research was able to explore the often subtle means of resistance to the colonisation of these healthcare practices by biomedical concepts and values (including organizational practices, such as ethics approval). Additionally, simple observation without participation may not have been sufficient for researching a situation where there are alternative and 'hidden' forms of knowledge, especially 'embodied' (non-cognitive) skills and knowledge (cf. Lawler, 1991). By 'spending time' and becoming immersed in the setting, I developed a practical sense of the training institution, which supports the validity of my findings. Being committed to reflective research practice, I avoided the potential pitfalls of 'going native' despite an interest and commitment to the field.

During the research, I began to appreciate the extent to which embodiment is fundamentally implicated in any learning process, including mine and that of my participants. This research, therefore, was able to provide insights, with a focus on embodiment, on the social meaning of healing practices, and the values attached to different ways of knowing the body and embodying knowledge.

### **Key words**

Embodiment, healthcare practice, complementary and alternative medicine, reflexivity, ethics, body, ethnography, writing, reflexivity.

## Introduction

This paper is a reflective account of conducting ethnographic research in training institutions for complementary and complementary medicine (CAM) practitioners as part of my PhD. By concentrating on three cross-cutting and non-linear aspects of the research process – motivation, immersion and expression – I draw out the conceptual links between theory, praxis and ethics in ethnographic research, as well as shedding light on the direction that my doctoral study of the two colleges took and the claims to validity of the findings. I argue that the ethnographic process is a way of constructing knowledge about the world that we live in, but that knowing is not separated from the knower and so, as Jaggar argues, ‘The reconstruction of knowledge is inseparable from the reconstruction of the self’ (1989: 164) and, moreover, that that reconstruction is fundamentally embodied.

From the viewpoint of *after* being in the research field, it is easy to forget where I started and the ups and downs along the way, but my body, in the widest sense of that word, was a witness to the whole process and bears the gifts and scars of the experience. In a lot of fundamental ways, I am still much the same. For instance, I am still (in no particular order and by no means exhaustively) a woman, a feminist, English, white, bisexual, middle-class, a sociologist, and as one of my very good friends put it, ‘in to all that hippy shit’. My ‘habitus’, to borrow the term, has a certain intractability to it. Nevertheless, I can still reflect on changes, shifts and developments in relation to the ways I inhabit my body to which I assign these identity descriptors. As Ellingson argues, ‘Categories of culture such as gender, age, race, class, sexuality and disability might be limited by essentialist definitions, but they provide a useful starting point for exploring the impact of people’s bodies on their research opportunities and challenges’ (2006: 305). It is also worth noting the contingency of these identity descriptors. In different circumstances and stages of access and immersion in the setting they were relevant to a smaller or greater extent. More pronounced changes also came from spending hours, weeks and months alongside my participants, working with them, experiencing and learning with them, talking and interacting with them in various ways, in and out of the institutional setting of the colleges. Many of the ways that I see and interpret the world and interact with the people around me have changed, and the way I write accounts of my research today is *from* the embodied person that I am on this day. Writing is at once the most challenging and the most rewarding part of the research process for me, as it forces me to crystallize my experience into words – to make it clear and tangible, something that in the context of the settings for this research was often a challenge. And for my participants, with all their complexity, I seek to honour their experience in an ethical way, while retaining my critical sociological edge.

The paper shows one attempt to integrate the perspectives of other academics with my own academic voice. This is a balance that it is often difficult to strike, especially for the student researcher; Kamler and Thomson refer to it as the ‘Goldilocks dilemma’: ‘How much persona is appropriate? Not too cold: passive, tentative, over-cautious or evasive. Not too hot: overly confident, too brash and assertive. But just right: confident, in charge, leading the reader through the dissertation’ (Kamler & Thomson, 2006: 59).

## Motivation

One of the most common questions I was asked by non-academics when I said I was doing a PhD is ‘How on earth do you find the motivation?’ and the answer, I believe, can be best explained by drawing out the embodied aspects of being a researcher. Motivation, clearly, is required for all parts of the research process, from submitting funding applications, to picking myself up after knock-backs about access to the field, to resisting the allure of sunbathing in the garden when chapters have to be written. My desire for doing research can in some ways be attributed to the fact that I have grown up and developed as a student and sociologist in a culture and institutional environment that values academic achievement. My successes in this area (I’d have never made a good concert pianist, as my childhood piano teacher – and now, in a twist of fate, my Reiki Teacher – would have told you!) have brought great personal and professional satisfaction and rewards. The training I have undertaken, and the academic culture I have participated in have validated my goals as worthwhile. Indeed, the validity of academic achievement as a goal, as a middle-class schoolgirl in the UK, was drummed into me from a very young age.

However, my integration into academia was a hindrance as well as a help for this research. As one tutor at the College of Homeopathy told me, ‘Academics make the worst patients because they are so *in their heads*.’ This way-of-being (‘in their heads’) may offer one insight into why the body and embodiment has emerged as a key sociological problematic in sociology only in recent years. The idea of *thinking from* our bodies, or *through* our bodies is a difficult thing to achieve when what our bodies seem to do best is to *do* or *feel*. We perhaps find it easier to use our heads to explore some emotive situations that cross the apparent boundaries of body and society (‘I think I am happy’ or ‘I think I am in love’) but some aspects of the physical body demand a gritty temporality that makes ‘I think my leg hurts’ a nonsense. Perhaps this is why diagrams of nerve pathways or debates on the social causations of disease come more easily to the scientist or academic. My motivation then is to facilitate the process of sociology coming to find a way to meaningfully express and, of course, critique the role of embodiment in the social organisation of health care. My challenge is to explain in an academic context, and through academic writing, professional practices that draw variously on wordless bodily sensations, emotional empathy, intuition, spiritual experience and ‘subtle energetic fields’.

Developing the focus of my PhD research started with a frustration with the lack of consideration given to alternative forms of healing in a sub-discipline (sociology of health and illness) that has torn itself from, in name at least, its origins in the service of medicine (medical sociology) (see Williams et al., 1998 for a good summary of the history of the sociology of health and illness), and an interest in practitioners’ experiences. The majority of the academic literature that there was on CAM focused on efficacy debates, historical and social patterns or patient usage and perspectives. The finding from my Masters research for which I interviewed experienced teacher-practitioners from a variety of therapeutic disciplines showed that, at least to some extent, these practitioners become an ‘embodiment of their therapy’. This insight begged the question of how and why this happened, and the dearth of research on training in any form of CAM made this an exciting potential subject and one that the Economic and Social Research Council were prepared to fund. In addition, the growth of popularity of CAM, the increasing numbers of people training (often as a complete career change) to be practitioners, and the increasing political demands for regulation of CAM made questions of ‘what’ was being learnt

and how competence could be validated, extremely topical ones. As Fox argues, from a perspective of the ethics of postmodern research where knowledge is 'local and contingent', 'research questions should be developed in such a way that the theoretical consequences will be of direct practical relevance' (1999: 190).

Motivation, and the desire for knowledge, is then at once individual (produced day by day through my habitus and my personal and political commitments) and social (through the requirements of the PhD process, the 'gaps' in sociological knowledge, the social values assigned to academic status, and interaction with participants). My desire may be reflective of those social values but it also contributes and (albeit minutely) modifies them as it is enacted. My body, for instance in terms of the capital it is accruing, plays an important role in the motivation, but being a researcher requires training and the embodied student researcher must learn new dispositions and practices. In a way, the learning of any new discipline has the potential to be an interesting subject of study for embodied sociology. The unique value of this particular study was the extent to which embodiment is explicitly recognized in the training process, and the distinctiveness of the ways in which the body is implicated in the training process for the student homeopaths and osteopaths.

I had both practical and theoretical motivations for the choice of the two case studies – the anonymized Colleges of Homeopathy and Osteopathy, including length and location of the course and the comparative potential, both with each other, and with the existing literature on orthodox medicine and allied professions. Osteopathy was chosen for its fascinating position as a newly state-regulated health care profession, which has brought with it changes to the core curriculum and increased opportunity for referral within the National Health Service (NHS). The changes required to move towards integration and to achieve government regulation have not been universally welcomed within the profession; many argue that the 'core' principles of osteopathy have been lost in pursuit of 'scientific medical' approval. Homeopathy occupies a much more ambivalent position in the social organisation of health care. Historically, it has had a long, complex and often antagonistic relationship with orthodox medicine. However, even since the inception of the NHS homeopathy has retained a foothold, albeit a small one, in the orthodox profession, in the form of NHS homeopathic hospitals and the Faculty of Homeopathy, which offers a postgraduate qualification for medical doctors. Alongside this the independent, 'lay' profession, that comprises non-medically qualified homeopaths, have managed to sustain their self-identity as 'professional homeopaths' (Cant & Sharma, 1996), despite being readily rubbished and ridiculed by orthodox medics. In turn, the professional homeopaths have often tended to reject outright much of orthodox medical knowledge because its ontological, epistemological and practical basis is at odds with homeopathic philosophy (Gale, n.d.). My decision to choose a lay homeopathic college was based on recognising the opportunity to explore this more marginalised group in terms of the experience of its student practitioners. Additionally, the nature of the therapeutic practices of osteopathy and homeopathy also provides an interesting contrast to each other. The focus of osteopathy is, to a large degree, on the physical body, for instance, the principles of structural alignment, flow and movement and treatment through structural adjustment. The focus of homeopathic practice, by contrast, is on the homeopathic interview rather than any form of physical touch.

The curriculum is not standardized across different colleges in either homeopathy or osteopathy, and because of this, and the individual histories of each

college, they differ widely in structure, content and emphasis. Therefore, it is necessary to be extremely cautious in making any generalisations about professional training on the basis of these case studies, especially on details of the curricula. On the other hand, the value of this study is derived exactly from that uniqueness. The body in training is implicated in uniquely explicit ways at these colleges, and my findings highlighted the need in sociology to consider the body in all forms of training and education.

Describing this study as an ethnography does not simply refer to the methods used for the research, which indeed were virtually exclusively participant-observation at the two case-study settings, but also the epistemological choices and ethical commitments of the research. As many alternative medicines work with paradigms of health and illness that contrast more or less starkly to the dominant biomedical model, it was important to consider how to explore and do justice to forms of knowledge that may be strongly internally validated. Ethnography, with its origins in the anthropological studies of 'others', such as tribal communities (Geertz, 1973; Malinowski, 1922), the community studies approach (Banton, 1966; Bell & Newby, 1971; Frankenberg, 1982; Stacey, 1960), and the Chicago School (Anderson, 1978; Bulmer, 1984; Suttles, 1968), seemed an obvious choice for a study of groups of marginalized healthcare professionals. Additionally, simple observation without participation may not have been sufficient for researching a situation where there are alternative and 'hidden' forms of knowledge (cf. Lawler, 1991, particularly her discussion of methodological issues in the introduction).

Another factor in the decision to conduct ethnographic research was comparative potential, as previous research on the training and practice of healthcare professionals has often been ethnographic or anthropological in nature (Atkinson, 1981; Becker et al., 1961; Fox, 1992; Fox, 1957; Sinclair, 1997). As Geertz puts it:

if you want to understand what a science is, you should look in the first instance not at its theories or its findings, and certainly not at what its apologists say about it; you should look at what the practitioners of it do (Geertz, 1973: 9).

This, I feel, is just as important for the healthcare professions as it is for ethnography or any other 'science'. *Spending time* is perhaps the most essential part of ethnographic research. Over the academic year, I spent approximately 200 hours in each college, taking part in classes, clinics and social activities.

An advantage of 'spending time' in the setting is the opportunity to develop subtle understandings of the social environment. At the Osteopathy college it was only through informal snippets of conversation and barely noticeable actions, which I only understood the significance of after some time, that I came to realise the extent to which the faculty and, to a lesser extent, the students were divided on the question of integration with orthodox medicine. Even more crucially, I was able to observe that this had a profound effect on the way that the students embodied identities developed in the setting, particularly in relation to professional skills and choices, such as about whether to do postgraduate studies, and, if so, which ones. Again returning to Geertz, it is not enough to hear what people say or write about themselves or their knowledge: 'Behaviour must be attended to, and with some exactness, because it is through the flow of behaviour – or, more precisely, social action – that cultural forms find articulation' (Geertz, 1973: 17).

The analysis process used in ethnographic research also motivated my choice of research method. Research is, of course, in the final instance, presented from a single perspective – my perspective – but the strength of ethnography is the multiple

perspectives, attitudes, actions and bodies that the researcher is exposed to over time, and then tries to make some sense of. Cultural analysis, Geertz argues, is ‘guessing at meaning, assessing the guesses, and drawing explanatory conclusions from the better guesses, not discovering the Continent of Meaning and mapping out its *bodiless* landscape’ (Geertz, 1973: 20, *my emphasis*). Ethnography can start to unravel the discourses by which a social enterprise is constituted, sustained and reproduced by social actors (Fox, 1999). From fairly general research questions about the form and structure of knowledge and the development of professional identity, came an informed interest in the social meaning of the healing process through the changing embodiment of the student practitioners, the power relations in the educational environment, and the social values assigned to different ways of knowing and being.

Ethnographic research was valuable in the exploration of healthcare cultures that contrast with the dominant biomedical model, and are, to a large extent, internally validated. The motivation behind the project was to produce a sociological account of the ways that knowledge about practicing osteopathy and homeopathy is re/produced in the settings, and to establish to what extent different methods of healing produce different learning environments and processes. I use the term re/produced to help emphasize the ambiguity between the ideas of being ‘taught’ embodied skills and knowledge and the acknowledgement that in some ways each student must learn these skills from scratch. In addition, re/produced hints at the Bourdieusian interest in the reproduction of legitimated knowledge in the body of the practitioners, rather than just how students relate to knowledge. While I was in the setting much of the motivation came from the people I met, my continued reading on the subject, and my immersion in the setting.

### **From entering the field to immersion**

Reaching a place of immersion in a setting (‘becoming part of the scenery’ or ‘feeling comfortable’) is a long and never-quite-achievable process for the researcher. Events often conspire to remind the researcher of her outsider status. The researcher is part of her setting, and yet also not fully included. She is committed to and constrained by her physical and social environment in the setting, and yet she is also free from the constraints that operate on its ‘full-time’ participants, and has less invested in the setting. She can theoretically leave at any point. For me, the challenge of entering the field and becoming immersed in it without losing sight of the aims of the research and ‘going native’ was a significant challenge.

Participant-observation was my key research method in the field, and Gold’s (1958) classic typology of various roles that a sociologist can take on in the research setting – complete observer, observer-as-participant, participant-as-observer, or complete participant – has remained a useful framework to draw on to explore this. It helps to assess not only the degree of access to and acceptance in the setting, and the validity of the findings, but also to remind the researcher of the need for reflexively-monitored participant-observation of the field (cf. Davies, 1999: 72-3). In this research, the shift between these different ideal-type ‘roles’ took place from day-to-day as well as in a more linear fashion as I became immersed in the setting. Joining new classes, or year groups, even after months in the setting, forced upon me the sensation of ‘complete observer’ again as I struggled to get to grips with the expectations and requirements of the situation. Conversely, I experienced the need to consciously drag myself back from ‘complete participation’ at times in order to focus on the aims of the whole experience which was to produce a PhD thesis, rather than

gaining a professional qualification in either of the therapies. At times I felt like an observer-as-participant, for instance in osteopathy ‘technique’ classes, when the students used me (my body) to practise their techniques on, while I was rarely able to reciprocate because I did not have the necessary experience and skills. At the College of Homeopathy, I spend much of my time feeling like a participant-as-observer because I felt like I *belonged* to two of the year groups and was able to learn alongside them in a more ‘authentic’ way, while still having different reasons for being at the College.

From the moment of entering the field, my passion for the therapies and the people seemed present a risk of ‘going native’. I had previous involvement with the CAM sector as a user, a researcher, a practitioner<sup>i</sup> and as the daughter of a homeopath<sup>ii</sup>. Nevertheless, I also had in place the collection of ‘motivations’ described above and my commitment to the field also brought significant advantages as well as challenges. The risk of ‘going native’ is an age-old ‘problem’ in anthropological research but with the growth in practitioner-researchers and other methods of researching from ‘within’ a culture, the challenges it refers to are not so straightforward. Certainly I had a precarious insider/outsider identity in the research field. The extent of success in sociological practice depends on two aspects of social activity identified by Bourdieu: *practical sense*, or the logic of practice, which is the ability to comprehend and negotiate cultural fields, and *reflexivity* about one’s own practices and relation to the cultural field (Webb et al., 2002: 49), which I now discuss in turn. Crossley notes, drawing on Bourdieu’s logic of practice:

The sociologist must approach each social field as if approaching a new game for the first time, attempting to discern the point and the sense at work within the hurly burly of practice (Crossley, 2001: 101).

However, for me, this was not a ‘new game’ but one in which I was already personally and professionally involved. I was able to identify with the aims and commitments of the participants. My previous experience of CAM served me well in aligning myself with the participants as a friend rather than an enemy from the outset. From an ethical point of view my commitment to the field acted as a buffer to any serious ethical conflict of interest. It also reassured participants that I was unlikely to willingly misrepresent them. At both colleges my identity as a student was probably the most important, although for different reasons. At the College of Homeopathy, my student status brought respect (being from a good university) and ‘maternal’ support as the older women, some of whom had children at university, were keen to encourage me. At the College of Osteopathy, my student status aligned me directly with the students. Discussions of ‘too much work’ and ‘not enough money’ were most common.

At the College of Homeopathy, gatekeeper access was granted very easily by the principal of the college. She was very interested in the research, and felt that, with the ongoing debates about the development of a single register for non-medically qualified homeopaths and possible government regulation, it would be an ‘insurance policy’ to have someone studying the college. Her only stipulation was that I should spend all of my first day in the field with one first year group. She did not really explain this, but I understood that this was to enable the group to bond effectively on the first day of the academic year without the uncertainty or disruption of someone coming and going. This turned out to be incredibly productive for the research as I did feel part of that group, as well as a third year group that I also joined and regularly spent whole days with. The trust that I had from the principal and the members of the

group served to open up many areas that might otherwise have remained elusive, and my membership of the group as a participant as much (I felt) as an observer facilitated the process of gaining a practical sense of the dynamics and internal 'logic' of the setting. I was initially surprised by the ease with which access was possible because homeopathy has a chequered history with 'scientists' and 'researchers' who in the main have sought to discredit the profession, but the presentation of my project as being interested in 'how do people learn to practise?' (rather than 'does homeopathy work?') was clearly not seen as threatening. I never gained written consent from the students at the College of Homeopathy, but because of the organisational set-up it was extremely easy to be sure that I had contacted all the students to explain my research and get verbal consent.

At the College of Osteopathy, access was more of a challenge. The argument that access must be continually renegotiated at every stage of ethnographic research (e.g. Hammersley & Atkinson, 1995: 54) became a stressful lived reality. While initially I thought that I had gained access relatively unproblematically through senior administrators and academics, I soon found, after some early observations mostly with first years and at a few student clinics, that I had exposed some 'raw nerves' in the organisational set-up and power relations within the College, and I ended up having to clear my research with many departments individually. As one member of staff put it, 'The line stops with me in [this area of the college] so I thought I should talk to you... your research has been causing some angst in the faculty'. Much of the problem lay also in the pressure that certain members of the College felt to demonstrate equally rigorous procedures as orthodox medical ethics committees as the profession became more allied with the orthodox system. Eventually, I managed to achieve a precarious agreement to access after a 'chat' with the senior research staff. Although I did not use these experiences directly as data for my analysis they were significant for my general understanding of the functioning of power relations in the college (see Burgess, 1984 on the link between access and knowledge about the field).

However, even once I had access to the building, I realised that the students were the real gatekeepers as it was their experience I was trying to research. I approached a second year group and a clinic group, explained what I was doing and asked them for written consent to participate in their classes. Because the College of Osteopathy was a much larger organisation with many unfamiliar faces, it was only once I became familiar socially with many of the students, that I started to 'feel comfortable' and be recognized by students as 'part of the scenery'. My development of a 'practical sense' of the workings of the College began to mean I could form meaningful, contextualized opinions and conclusions about aspects of college life and embodied experience. In the end, much of the observation that I did happened outside of the situations of 'written consent', through being invited to join classes and clinics by staff and students, through informal chats over breakfast or lunch, at the pub after college or in the library between classes. The second year class particularly 'took me under their wing', inviting me to social events, allowing me a deeper picture into the kinds of resistance to the college system that I could not gain through observation and participation in classes alone.

Informants are understood to be vital to any ethnographic research, and both students and tutors often selected themselves (cf. Davies, 1999: 78), to show me aspects of their world, particularly things they felt it was important for me to know. For instance, one osteopathy student wanted to show me the old books (early 1900s) in the library that presented a much wider scope for osteopathic treatment than that



encouraged by the College<sup>iii</sup>. I was, indeed, interested in this despite already being aware of the narrower therapeutic remit officially encouraged by the College. The active realization within the student population of the silencing of aspects of osteopathic 'history' provided me with additional insights to the production and reproduction of osteopathic knowledge in the College, and the student's experience and negotiation of the course.

This self-conscious mode whereby staff and students were 'informing' me was facilitated at both settings by the 'reflexive turn' that has moved from academic research into the world of medicine and healthcare training in recent years. The 'reflexive turn' in contemporary sociology and anthropology has ensured that researchers are required to recognize and reflect on their impact on the research. Illusions of detachment and objectivity have been criticized, and sometimes redefined, from many quarters but perhaps most notably in feminist research, where the commitment to feminist ethics and research '*on* women, in research carried out *by* women who were feminist, *for* other women' (Stanley & Wise, 1990: 21) has been incredibly influential. As Stanley and Wise explain,

Our position is that *all* knowledge, necessarily, results from the conditions of its production, is contextually located, and irrevocably bears the marks of its origins in the minds and intellectual practices of those lay and professional theorists and researchers who give voice to it. The existing discipline of sociology is neither neutral nor impartial; it reflects the practices and knowledge of groups of highly particular white, middle-class, heterosexual men while seemingly reflecting universalisms. Its sexism is no 'intrusion' or 'mistake' (1990: 39).

Wacquant, following Bourdieu, argues that we are not 'free floating': 'reflexivity is precisely what enables us to escape such delusions by uncovering the social at the heart of the individual, the impersonal beneath the intimate, the universal buried deep within the most particular' (1992: 44). Drawing on recent ethnographic studies of the workplace and Bourdieu's social theory of practice, Adkins (2004) points out that the idea of gender as a taken-for-granted characteristic of workers is being replaced by a more routinely reflexive approach.

Yet reflexivity is by no means exclusively the domain of the sociologist, indeed, concerns with 'reflective practice' in many healthcare professions have brought about assessment through 'reflective portfolios' and other modes (e.g. Driessen et al., 2005; Droege, 2003; Eraut, 2004; Heath, 2004; Phillips et al., 2002). From a methodological point of view, it is clear that the informants in my study were keen, as Rabinow puts it, to 'spend [...] more time in this liminal, self-conscious world between cultures' (1977: 39) which was facilitated by my role in the setting *qua* researcher. The informal nature of much of my interaction with participants meant I often had the opportunity to describe in some detail what my research was about. The ways in which my explanations were picked up, commented on and interpreted by the participants offered additional insights. In the main, participants seemed really interested in the research questions, asking probing questions, and offering their opinions. I got a lot of positive feedback at both colleges from people I discussed my research with in detail, both about the subject and about my participation. I was commonly told that I asked good questions, which illustrated that what I was doing in the setting was not only observation of 'authentic' action, but often an informal form of interviewing trying to elicit specific reflections from my participants. Tutors from the homeopathy college often asked about the research and commented on aspects of my participation in classes. Discussing my research was

also an ethically informed decision to ensure that participants did not completely 'forget' my researcher role as I became more immersed in the setting.

The development of practical sense was mediated most fundamentally by reflexivity. It was only through reflection that I realized 'how far I had come'. The practice of reflexivity was important academically, practically, and ethically throughout the research. Reflexivity gives some control over the day-to-day ethical dilemmas that the researcher faces, particularly when the unexpected happens. However, reflexivity is an ongoing process that affects all aspects of my life (maybe especially because I come *from* a place of sociological embodiment?), but during the research writing became a way to process, formalize and record it both for future reference, and to ensure that it modified my actions as appropriate. For instance, one issue that came up was whether I should wear a white coat in the student clinic at the College of Osteopathy. On my first visit to the clinic, I was 'offered' a white coat in a way that I was not expected to refuse, and the tutor found me a vastly oversized one, which ironically highlighted my outsider status anyway. On later visits, however, I began to resist the assumption that I would wear a white coat as I felt that it caused more confusion and ambiguity than was necessary.

Language is a vital component in ethnography. In traditional anthropological studies, the ethnographer usually needed to learn another national language, and in studies of institutions, learning anatomical terms, technical language or organizational acronyms are vital. However, in the study of interaction, verbal communication is often privileged over other forms of communication which are at best alluded to. Gleason's (1989) study of residential homes for people with severe developmental disabilities is an example of how focusing on observation of embodied actions and behaviour rather than spoken language can bring about insightful analysis of a situation. Reflecting on my ability to 'fit in' bodily as well as with my language in the setting was vital. In terms of language, I realized sometimes that I was 'in on jokes' that I would not have appreciated when I first entered the field, or I would find myself *not* asking things that I might have sought an explanation for previously. I would find myself responding to situations in an 'appropriate' way that through writing and reflecting in my research diary I knew would not always have been my response in other settings. For instance, there was a fascinating example of a discussion of an ex-College of Osteopathy student who had managed to graduate but was later struck off. He apparently told patients that he was 'removing pixies' from them. This caused much laughter in the group and disbelief from me. On further discussion, I eventually managed to get the tutors to explain that they did not necessarily think that what he was doing was bad, or even 'ineffective', but that this language for describing his healing work was not 'appropriate' in the osteopathic environment. This also explained my disbelief, because I evidently had made the 'strange familiar' and understood how to 'play the game' at the osteopathy college. In doing this, in a natural and spontaneous way, I understood that this was not acceptable language to use and it was OK for me to laugh.

Similarly, especially towards the end of my time in the field, I began to be able to recognize the differences in the ways that the two professions would approach a case. These are now part of my way-of-being with others and I find myself today wanting to put my hands on parts of my friends that hurt (my osteopathic body) and wanting to point out connections between my friends' symptoms that they may not have thought about before (my homeopathic body). There were also situations where it was necessary to participate in an explicitly embodied way, such as in Chi Kung classes at the College of Homeopathy, and in technique classes at the College of

Osteopathy. I felt that this participation could not be ‘faked’; it was necessary to absorb at an embodied level the impacts of these aspects of the training, and my growing familiarity with these activities that initially I often experienced with difficulty or with a sense of clumsiness (cf. Leder, 1990) contributed to my understanding of the lived experience of the students.

In my research, I was constantly being exposed, in a learning environment, to new ways-of-seeing the body and new forms of knowledge, and I was having new experiences and learning new things about my own body, emotions, and spirituality. Once these new ways-of-being had been explored and experienced, my body registered it and there was no ‘going back’. The impact epistemologically of ‘incorporating the research’ is not superficial. As the examples above show, doing this research fundamentally changed the number of ways I was able to interpret the dis-eased body. The production of the written product of research is from the embodied researcher that I am today, and not from where I started out. There are no objective tests or measures, but simply a sociological perspective, albeit an informed one, on the Colleges that I was part of for an academic year. Therefore, in terms of analysis, the research data and this thesis must be understood to be produced from a socially and temporally located embodied perspective. In this way, reflexivity comes into its own: the constant shifting of the researcher’s habitus means that it is problematic to make assumptions about the setting, to impose one’s own interpretations on the participants and their social world. However, interpretation and analysis is what the sociologist does, therefore, the requirement for careful and reflective observation, listening and embodied participation, as well as verbally confirming the nature of others’ experiences in similarity or contrast to one’s own when practicable is key.

Practically, reflection on my day-to-day experience, during the writing of my research diary, drew me back from the world of experience to the world of sociological analysis. In some ways, reflexivity (as conscious reflection on experience) is somewhat inimical to experience. You can not feel fully and think about feeling at the same time. The challenge I faced, alongside the students, of ‘getting out of the head and into the body’ (a phrase used by various homeopathy tutors) was limited by the need to constantly return to sociological thought and analysis. Reflexivity emerges as an embodied skill in the context of learned professional sociological skills. It is a way of being-in-the-world that affects all aspects of the sociologist’s life. The distinction between personal and professional skills is somewhat arbitrary. I certainly find it incredibly difficult to switch off my sociological gaze even when I am not working.

As noted above, my research was mostly non-covert; all the students and staff knew who I was and why I was there, but at times my participation was virtually full. I was witness to people’s emotional displays and personal issues on a large number of occasions, which raises ethical issues of confidentiality and exploitation. I could assure the confidentiality of my participants, but it was more problematic to argue that they were gaining anything particularly from my presence, which was so important for my PhD and future career. Indeed, the potential for exploitation is particularly high – and I knew that I had gained trust from the participants not to misrepresent them – in these settings where professional identities and qualifications do not hold the same cultural value as comparable professions such as medicine. What I can say is that in participating in these classes I was learning, alongside the students, the skills of listening and witnessing, which served only to deepen my empathy and research

skills. I quote here at length Arthur Frank on ethics, because he expresses so clearly some of the values I deepened while doing this research:

One of our most difficult duties as a human being is to listen to the voices of those who suffer. The voices of the ill are easy to ignore, because these voices are often faltering in tone and mixed in message, particularly in their spoken form before some editor has rendered them fit for reading by the healthy. These voices bespeak conditions of embodiment that most of us would rather forget our own vulnerability to. Listening is hard, but it is also a fundamental moral act; to realize the best potential in postmodern times requires an ethics of listening. I hope to show that in listening for the other, we listen for ourselves. The moment of witness in the story crystallizes a mutuality of need, when each is *for* the other' (Frank, 1995: 25).

I certainly developed a lasting friendship with many of my participants, an idea that is supported by feminist interviewing principles (Oakley, 1981), and offered them advice and support on a number of occasions, using skills I had both brought to the field and those I had learnt by participating in these colleges.

### **Inscription and expression of experience**

'What does the ethnographer do?' – he writes (Geertz, 1973: 19).

Notwithstanding Geertz (undoubtedly unconscious) assumption that all ethnographers are male, I agree that writing is fundamental to academic research as it is the prime mode of dissemination, discussion and debate of theories, methods and findings. For me, writing offers a way to process my experiences, my feelings and my ideas. It is a way of focusing and deepening my understanding. For this reason, the physical act of writing – of committing words to paper – is key to the analysis process, through activities such as writing field notes, analytic memos and keeping a research diary. To be part of a culture is not necessarily to be able to write of it, because to write of it is in itself an abstraction and signification of that culture and involves analysis and interpretation.

Geertz argues that ethnographers write about culture, more precisely he argues that they write 'thick description':

As interworked systems of construable signs (what, ignoring provincial usages, I would call symbols), culture is not a power, something to which social events, behaviours, institutions, or processes can be causally attributed; it is a context, something within which they can be intelligibly – that is thickly – described (Geertz, 1973: 14).

Patterns of behaviour in cultural settings can not be defined as 'laws' or 'rules' in the sense that the physical sciences use those terms. Bourdieu's use of the term 'strategy' encapsulates this idea well:

The notion of strategy is the instrument I use to break away from the objectivist point of view and the action without an agent that structuralism presupposes... One's feel for the game is not infallible; it is shared out unequally between players, in society as in a team... The habitus as a feel for the game is the social game embodied and turned into a second nature. Nothing is simultaneously freer and more constrained than the action of the good player (Bourdieu, 1990a: 62-3).

In this way, writing of ethnography is one form of interpretation and expression of the cultural settings and experiences that I (in an embodied sense) have been witness to and part of at the Colleges. The 'final' version, therefore, is an interpretation from

who I am today; it is inherently partial (Clifford, 1986), and may be re-interpreted in future years.

My research is, of course, in the final instance, presented from a single perspective – my perspective – but the strength of ethnography is the multiple perspectives, attitudes, actions and bodies that the researcher is exposed to over time, and then tries to make some sense of. Cultural analysis, Geertz argues, is ‘guessing at meaning, assessing the guesses, and drawing explanatory conclusions from the better guesses, not discovering the Continent of Meaning and mapping out its *bodiless* landscape’ (Geertz, 1973: 20, *my emphasis*). Ethnography can start to unravel the discourses by which a social enterprise is constituted, sustained and reproduced by social actors (Fox, 1999). From fairly general research questions about the form and structure of knowledge and the development of professional identity, came an informed interest in the social meaning of the healing process through the changing embodiment of the student practitioners, the power relations in the educational environment, and the social values assigned to different ways of knowing and being.

Understanding the role of writing in the development of ideas and thoughts can be illuminated in the context of an embodied understanding of sociology. Writing is, as noted above, a form of abstraction and signification of experience. However, the aim of this thesis is to be able to capture, to some useful degree, the embodied aspects of experience, which are by their very nature, difficult to capture in words, particularly for the ethnographer who has become a kind of insider to the setting. As Bourdieu notes, ‘what is “learned by the body” is not something that one has, like knowledge to be brandished, but something that one is’ (Bourdieu, 1990b: 43). Once the body becomes ‘absent’ (Leder, 1990) it is even more difficult to capture what is ‘known’ in words. Barrett (2000) notes that there is a risk that the sociology can become ‘boring’ because by retaining the cognitive style of the medium (academia), the sensuality and passion of the field could be lost. She argues that if sociology is to ‘wake up, it needs more humanity, it needs more imagination, it needs more perception, it needs to appeal to experience beyond cognition. It needs more respect for other ideas on their own terms, not translated into its own’ (Barrett, 2000: 20).

Creswell argues that a vital part of ‘good ethnography’ is that ‘the ethnographer makes explicit what is implicit and tacit to informants’ (1998: 212). With a concern for embodiment, this is even more important as the body tends to be devalued and silenced in our culture. To write of embodied experiences it is important to notice and make aware where there are absences of language (silences) to describe what is going on. By listening to the words and actions of those practitioners for whom the body is their daily focus, we have the opportunity to deepen our understanding of human experience, and for my specific research, the nature of human learning.

Writing as a woman is not easy to do in academic circles, as Devault explains:

Rhetorical processes – like all social interactions – are deeply gendered. Speakers and listeners produce and respond to statements on the basis of deep but usually unnoticed understandings of gender. In general, women’s right to speak (or write) authoritatively is attenuated and circumscribed. For a women to do scholarly work means speaking in the manner of the disciplinary tradition. They learn that, if they are to be heard, their text must enter a discourse whose contours reflect male perceptions and concerns. The readers whose judgements are influential – the teachers, the editors, reviewers and colleagues who will incorporate and perhaps extend their work – have, in the past at least, mostly been men (Devault (1990), cited in Hammersley & Atkinson, 1995: 254).

However, some women have challenged this by writing about the body and emotions in innovative ways. Wilshire suggests that women should let their bodies take the lead in new ways of learning (Wilshire, 1989: 109). Probyn's (2004) reflection on shame in the habitus explores the problems that sociology has with the body and how they can be tackled in academic work. Even Bourdieu's work, which is often cited as exemplary embodied sociology falls short, she argues. 'In terms of distancing the body's physiological and emotive unruliness, Bourdieu's habitus is repeatedly brought in to make sure that we know that it is the social that rules' (Probyn, 2004: 236). Social scientists often fear the trap of 'biological reductionism' or extreme methodological individualism when they begin to talk of the physical body. Sharma (1996) critiques Scheper-Hughes' (1988; 1990) work on the somatization of mental states by pointing out that although it goes a good way to bringing the body back into social action,

'she pulls back at the last moment... I am then disappointed by her use of the term "using" as though bodily distress is a ploy, a technique by some person who is located inside the body but for whom the body is a mere tool of communication rather than part of the she or he who communicates' (Sharma, 1996: 257-8).

The inability to express embodied experience through language may be part of the problem:

Maybe this is a matter of language, the poverty of English in providing ways to convey the relationship between body and social self as other than subject and object, the difficulty in expressing the possibility that bodily states might relate to the social states without being reduced to an instrumental 'expression' of them (Sharma, 1996: 258).

Certainly, it is a challenge to the social scientist to explain with words that which is taught without. One of the tutors at the College of Homeopathy suggested to the students, 'Sometimes I struggle to find the right words to explain what it is that flower essences do. We need to find a new vocabulary. Maybe that could be your gift.' Certainly ethnography remains one of the areas of academia where expressive language, metaphor and anecdote have survived as conventional form, and have not been 'killed by science' (Pratt, 1986: 32).

Another problem I had to consider is the validity of 'speaking for' the participants in the study. In writing the ethnography, I am drawing together multiple perspectives into a single text arranged in a way that I believe most accurately represents the setting and best provides a good structure for sociological analysis. Kreiger's (1983) monograph of a lesbian community tackles this problem by combining a stream-of-consciousness style speaking from different members of the community that melds together fiction styles and social science. Alternatively, trying to use direct quotations as much as possible, and providing detailed examples that aim to preserve the original context and tone of interaction may go some way to tackling this. Additionally, I tried to make clear my own contributions to social interaction in the settings by not editing out the personal voice in my writing.

As Atkinson points out, how one writes an ethnography does not and cannot totally determine how it is read. 'We read, and read into, the text, based on our own background knowledge and assumptions' (Atkinson, 1990: 2). Part of my skill then as a writer of an ethnography, is to make clear my own interpretations while organizing my textual representation of aspects of the students' experiences in such a way that it facilitates the reader's own internal process of analysis. Realistically, not

all potential strands of discussion, debate and comparisons can be tackled explicitly in the analysis, and if they were to be it would quickly become tedious and repetitive. However, the 'rich' description of the ethnography should permit the reader some access to the 'feel' of the settings, so that they can imaginatively transport their own bodies into the midst of the activity.

Certainly, it is now widely recognized that the process of writing is vital in the construction of ethnographic, indeed any sociological, research. However, Hammersley and Atkinson assert that,

There is no more damaging myth than the idea that there is a mysterious 'gift', or that writing is a matter of 'inspiration'... [such views] inhibit systematic reflection on writing (and reading) as necessary aspects of the disciplinary or craft skills of social scientists (1995: 239).

Clearly Hammersley and Atkinson are right that writing is a skill that should be developed and valued in the ethnographer. Perhaps they fear the relegation of writing to a 'natural' skill much like 'caring' with nurses, who had a huge struggle to get the value of their skills recognized. Nonetheless, something is missing here in their analysis, because the *experience* of trying to write 'feeds the myth'. Days can go by where the writer feels nothing of worth is being inscribed – writer's block sets in. It can be frustrating and depressing. Then, suddenly, everything is OK again and the words tumble onto the page (or computer screen). Writing then is an embodied experience, a social experience. The 'inspiration' may not be divine, but the product of a set of suitable circumstances, environmental, personal, emotional. For me, the most important thing is 'having my own head-space' so that I can concentrate, i.e. minimizing the negative influences of my body-in-the-environment – so I need to make sure I have had enough sleep, no hangover, a quiet environment, and am in the midst of no emotional crises.

To conclude briefly, then, this whirlwind account of my journey as a ethnographer has illuminated, challenged and extended some of the literature on research methodology and methods, in an unconventional ('feminine'?) way. The research process is a way of constructing knowledge about the world that we live in, in order to better understand it. However, knowing is not separated from the knower and during the research, I began to appreciate a deeper level the extent to which embodiment is fundamentally implicated in any learning process, including mine and that of my participants.

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<sup>i</sup> I have trained as a Reiki Master Practitioner, have taken courses in Thai Massage, Flower Essences, Transcendental Meditation, Kung Fu and Qi Gong, and have practised Yoga Asanas since I was twelve.

<sup>ii</sup> My mother started working as a regional tutor for the College of Homeopathy at the same time as I started the research. She also began working part-time at a private clinic around the same time alongside some osteopaths who taught at the College of Osteopathy (although I was unaware of this link when I first approached the College about the research).

<sup>iii</sup> The College encourages referral to GPs for a number of diseases that 'classical' osteopaths consider to be well within their therapeutic remit.