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## **The Embodied Ethnographer: Journeys in a Health Care Subculture**

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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. In it the author (a) seeks to extend the methodological literature in embodied sociology through reflection on conducting research in this context and discussion of particular events and processes and (b) offers an example of her attempt to strike the fine balance between respect and integration of previous scholarly work and her own emerging academic voice, while trying to avoid a sterility in the delivery that would belie her attempts to undertake an embodied sociology. By concentrating on three cross-cutting and nonlinear aspects of the research process—motivation, immersion, and expression—she draws out the conceptual links between theory, praxis, and ethics in ethnographic research as well as shedding light on the direction that the study took and the claims to validity of her findings.

**Keywords:** embodiment, health care practice, complementary and alternative medicine, reflexivity, ethics, feminism, writing, reflexivity, “going native”

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## **Prologue**

This paper is a reflective account of conducting ethnographic research in training institutions for complementary and complementary medicine (CAM) practitioners as part of my doctoral research. There are two main purposes of the paper: First, it is a methodological piece in which I seek to extend debates on embodied sociology and specifically to emphasize the centrality of embodiment and reflexivity in the practice of sociology; second, it is an example of my attempt to integrate the perspectives of other academics with my (fledgling) academic voice. This is a balance that it is often difficult to strike, especially for the student researcher; Kamler and Thomson (2006) have referred 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” (p. 59).

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 two case studies (the anonymized Colleges of Homeopathy and Osteopathy) 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, so, as Jaggar (1989) argued, “The reconstruction of knowledge is inseparable from the reconstruction of the self” (p. 164), and, moreover, 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, “into all that hippy shit.” My habitus, to borrow the term (e.g., Bourdieu, 1990a), 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 (2006) has argued, “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” (p. 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 small 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 now. 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. For my participants, with all their complexity, I seek to honor their experience in an ethical way while retaining my critical sociological edge.

## Motivation

One of the most common questions I was asked by nonacademics 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 would 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 United Kingdom, 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 organization 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 doctoral research started with a frustration with the lack of consideration given to alternative forms of healing in a subdiscipline (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, Annandale, & Tritter [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 existing academic literature on CAM focused on efficacy debates, historical and social patterns, or patient usage and perspectives. The findings from my master’s 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 was 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 learned and how competence could be validated extremely topical ones. As N. Fox (1999) has argued, 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” (p. 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 doctoral process, the gaps in sociological knowledge, the social values assigned to academic status, and interaction with participants). My desire might 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 UK National Health Service (NHS). The changes required to move toward 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 organization 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, which comprises non-medically qualified homeopaths, has managed to sustain its self-identity as “professional homeopaths” despite being readily rubbished and ridiculed by orthodox medical practitioners. 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, 2001). My decision to choose a lay homeopathic college was based on recognizing the opportunity to explore this more marginalized group in terms of the experience of its student practitioners. Additionally, the natures of the therapeutic practices of osteopathy and homeopathy provide 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 generalizations 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 might 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 health care professionals. Additionally, simple observation without participation might 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 health care professionals has often been ethnographic or anthropological in nature (Atkinson, 1981; Becker, Geer, Hughes, & Strauss, 1961; N. Fox, 1992; R. Fox, 1957; Sinclair, 1997). As Geertz (1973) has put 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. (p. 9)

This, I feel, is just as important for the health care 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 College of Osteopathy it was only through informal snippets of conversation and barely noticeable actions, which I understood the significance of only after some time, that I came to realize 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 (1973), 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” (p. 17).

Ethnographic research was valuable in the exploration of health care 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, 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 not only to assess 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 in the field (Davies, 1999). 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 on 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 to focus on the aims of the whole experience, which was to produce a doctoral 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 practice their techniques on, whereas 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 as if 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 to present a risk of going native. I had previous involvement with the CAM sector as a user, a researcher, and a practitioner<sup>1</sup> and as the daughter of a homeopath.<sup>2</sup> Nevertheless, I also had in place the collection of motivations described above, and my identity and commitment to the field 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), which I now discuss in turn. Crossley (2001) has noted, 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” (p. 101).

However, for me, this was not a new game but one in which I was already personally and professionally involved. I was able, therefore, 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 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 with which I regularly spent whole days. 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 in 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 checkered history with mainstream scientists and researchers, who in the main have sought to discredit the profession, but the presentation of my project as being interested in how people learn to practice (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 organizational set-up it was possible 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) became a stressful lived reality. Although 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-year students and at a few student clinics, that I had exposed some “raw nerves”, as one member of staff put it, in the organizational set-up and power relations within the college, and I ended up having to clear my research with many departments individually. The head of one department said, “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, what was termed, “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 realized 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 organization 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 formal 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. A 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) 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>3</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 students’ 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 health care training in recent years. In contemporary sociology and anthropology it 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, p. 21, emphasis in original) has been incredibly influential. As Stanley and Wise explained,

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. (p. 39, emphasis in original)

Wacquant (1992) has argued 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” (p. 44). Drawing on recent ethnographic studies of the workplace and Bourdieu’s social theory of practice, Adkins (2004) has pointed 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 health care professions have brought about assessment through reflective portfolios and other modes (e.g., Driessen, Van Tartwijk, Overeem, Vermunt, & Van Der Vleuten, 2005; Droege, 2003; Eraut, 2004; Heath, 2004; Phillips, Fawn, & Hayes, 2002). From a methodological point of view, it is clear that the informants in my study were keen, as Rabinow (1977) put it, to “spend . . . more time in this liminal, self-conscious world between cultures” (1977, p. 39), which was facilitated by my role in the setting qua researcher. The informal nature of much of my interaction with participants meant that 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 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 a example of how focusing on observation of embodied actions and behavior 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 a former 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 considered acceptable language to use and it was fine for me to laugh.

Similarly, especially toward 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 and of the centrality of embodiment to the learning process.

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 diseased 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 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 cannot 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 (Heidegger, 1962) 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 doctorate 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 (1995) 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. (p. 25, emphasis in original)

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 learned by participating in these colleges.

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

“What does the ethnographer do?”—he writes. (Geertz, 1973, p. 19)

Notwithstanding Geertz's (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. Yet there are significant challenges facing the novice ethnographer, which are compounded when the subject, embodied knowledge, is by its very nature difficult to grasp in words. For me, writing offers a way to process my experiences, my feelings, and my ideas in both my personal and my professional life. 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 and analytic memos, and keeping a research diary. However, there is always the need for a “final” version (or multiple); a version left to posterity, a version that goes out into the world, taking with it a great deal of myself, a version that opens me and my participants to new dialogues in knowledge but also new sources of judgment. Writing an 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 two colleges. The final version, therefore, is an interpretation from who I am today; it is inherently partial (Clifford, 1986) and might be reinterpreted in future years.

The written ethnography (for as well as describing the methodology, it is also a noun to describe

the written work) is a representation of culture. 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 (1973) has argued that “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” (p. 14). Writing in this way is a challenge in the world of science, funding bodies, and peer-reviewed journal articles where certainty and clarity are valued. Patterns of behavior in cultural settings cannot be defined as laws or rules in the sense that the physical and psychological sciences use those terms. Drawing on the work of Bourdieu (1990a) provided an important theoretical haven within which to explore some of these tensions, because it enabled me to strike a balance between overly determinist view of structure (“action without an agent”) with the acknowledgement that individual choices, behaviors, and ability to negotiate the social world (“feel for the game”) are deeply constrained by social norms and inequalities (pp. 62-63).

My research is, of course, in the final instance, presented from a single perspective, my own, 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 (1973) has argued, 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” (p. 20, my emphasis). Ethnography can start to unravel the discourses by which a social enterprise is constituted, sustained, and reproduced by social actors (N. 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. Writing about the learning experience of osteopaths and homeopaths requires that I am able to inscribe, 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 (1990b) noted, “What is ‘learned by the body’ is not something that one has, like knowledge to be brandished, but something that one is” (p. 43). Once the body becomes “absent” (Leder, 1990), it is even more difficult to capture what is “known” in words. Creswell (1998) has argued that a vital part of “good ethnography” is that “the ethnographer makes explicit what is implicit and tacit to informants” (p. 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.

Barrett (2000) noted 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 argued 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” (p. 20). In a world where students develop intense sensitivity to “subtle energetic fields” or the “quality” of muscle tissues, this becomes all the more pertinent. Yet there is also pressure to have final stage

coherence and boundaries around the story that is being told as not all the data collected can be included (Smart, 2010). This tension brings with it an obvious feminist critique: Structure and certainty are valued over fluidity and sensuality in writing, just as cognitive knowledge and certainty are valued over embodied knowledge and interpretative work in medicine and healing.

Rhetorical process—like all social interactions—are deeply gendered . . . 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. (Devault, 1990, cited in Hammersley & Atkinson, 1995, p. 254).

It is interesting to reflect on one of Bourdieu's (1990a) observations in the context of the novice (woman) ethnographer learning to write: 'Nothing is simultaneously freer and more constrained than the action of the good player' (p. 63). Being able to challenge the boundaries of sociological and ethnographic writing requires that one know where they lie in the first place. Although some rules can be broken, others, I discovered, are unbreakable, and it is not always easy to know which are which except through a "feel" for appropriate writing styles. I learned through trial and error when I made mistakes through comments and instructions by supervisors to rephrase something. Certainly, for a doctoral thesis and *viva voce*, the metaphorical boundary between student and peer in the academic world, demonstrating your grasp of the unwritten rules of academic writing (and speaking) is important.

Nevertheless, some women have challenged assumptions about appropriate academic writing style by writing about the body and emotions in innovative ways. Wilshire (1989) suggested that women should let their bodies take the lead in new ways of learning. In a reflection on shame in the habitus Probyn (2004) explored the problems that sociology has with the body and how they can be tackled in academic work. She argued that even Bourdieu's work, which is often cited as exemplary embodied sociology, falls short: "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" (p. 236). Social scientists often fear the trap of biological reductionism or extreme methodological individualism when they begin to talk of the physical body, and this can lead to some problematic analysis of the body and embodiment. In the context of complementary and alternative medicine, Sharma (1996) has critiqued Scheper-Hughes's (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. (pp. 257-258)

Sharma suggested that 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. (p. 258)

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, p. 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. In a monograph of a lesbian community Kreiger (1983) tackled this problem by combining a stream-of-consciousness style of speaking from different members of the community that melds 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 might 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 (1990) has pointed 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” (p. 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 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 (1995) asserted

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. (p. 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 fine again, and the words tumble onto the page (or computer screen). Writing, then, is an embodied experience, a social experience. The inspiration might 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, which means minimizing the negative influences of my body-in-the-environment, so I need to make sure that I have had enough sleep, no hangover, a quiet environment (a “room of one’s own”; Woolf, 1989), and that am in the midst of no emotional crises. Easy!

To conclude briefly, then, this whirlwind account of my journey as an 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, to better understand it. However, knowing is not separated from the knower, and during the research I began to appreciate at a deeper level the extent to which embodiment is fundamentally implicated in any learning process, including mine and that of my participants.

### Notes

1. 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 practiced Yoga Asanas since I was twelve.
2. 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).
3. The College encourages referral to GPs for a number of diseases that “classical” osteopaths consider to be well within their therapeutic remit.

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