

Fieldwork (though not agricultural)

At the end of the previous chapter I explained my reason for avoiding the use of the term 'narrative' in the title: it does not seem to me to have any specificity as a technical, methodological term. I would have liked to title the present chapter, 'ethnography', but this term is even more highly contested. I recall two students (not mine) who had been instructed by PhD examiners to remove every incidence of the word 'ethnography' and its derivatives from their thesis before resubmission. I also remember a reviewer's comments on a paper that I submitted to a sociology journal that I should not refer to this work as ethnography as this method involves total immersion in a culture for an extended period of time of a year or more and the article that I had submitted did not report research that conformed to this criterion. The journal editor wanted me to adjust the language of the article. I declined, being piqued by this attempt to police inappropriately a methodological term and instead published a developed version of the article as chapter 7 in *Sociology as Method* (Dowling, 2009); I discuss this chapter briefly below. Since these traumas I have consistently advised my students to avoid describing their work as 'ethnographies' even where their work did involve extended fieldwork in the relevant setting: as I mentioned in a previous chapter, 'there's no accounting for ignorance', but one might at least anticipate its assaults.

Ethnography generally (though there are exceptions) involves researching a cultural group—which may be as small as a classroom or as big as a society—in its natural setting. The approach originated in and has evolved from cultural anthropology, though some of the earliest examples of anthropology involved no fieldwork at all. James Frazer relied on ancient history and surveys of officials in his 'study in comparative religion' (Frazer, 1894) and Émile Durkheim carried out no fieldwork of his own for his seminal *The Elementary Forms of Religious Life* (1912, 1995). Rather later, Ruth Benedict was commissioned by the US government during the Second World War to produce an anthropological study of Japan apparently to facilitate control by the invading army following the 'inevitable' US victory. Benedict was, of course, unable to visit Japan at the time and relied on available written texts and the evidence of Japanese Americans many of whom were scandalously incarcerated at the time. Her study, *The Chrysanthemum and the Sword: Patterns of Japanese culture* (1946, 2006) still resonates today, though presents a rather stereotypical 日本人論 (*nihonjinron*—study of the Japanese people) a form that has received strong criticism recently (Goodman, 2005) for its treatment of Japanese culture (eg Benedict) or Psychology (eg Doi, 1973) as singular.

Benedict was, of course, in no sense at all responsible for the Pacific War, but the field of anthropology, itself, does seem to have had a rather belligerent history some of the battles of which are recounted in Eriksen & Nielsen's *A History of Anthropology* (2013). As well as what can legitimately be referred to as 'ethnography', the disputes have concerned epistemology, ontology, psychology, evolutionary biology, politics—particularly the politics of race and colonialism—and bitter arguments have raged over the relative merits of structuralism (but which kind?) and poststructuralism, functionalism, sociobiology, interpretativism,

postmodernism, feminism, marxism and so on. Now my own view is that many of these positions are capable of presenting interesting and original interpretations of the human condition and so are worthy of attention. As to which one is the truth, well all (or at least most) and none. This is a view that is anathema to Martin Hammersley, who suggests that:

[This] point of view is that the choice of context by ethnographers is necessarily arbitrary, in the sense that a host of different stories could be told about any situation, each one placing it in a different temporal and spatial context. From this perspective, ethnography is simply one means among others for telling stories about the social world, stories that need not be seen as competitive in epistemic terms. Of course, given this orientation, there would be a puzzle as to why anyone would go to the trouble of engaging in ethnographic fieldwork. Why not just write fiction in the manner of novelists and short story writers?

(Hammersley, 2006; pp. 7-8)

No, No, No! That which constitutes anthropology and, indeed, the reporting of research in any other 'academic' discipline as distinct from the writing of novels is, first and foremost, that it foregrounds its methodology. This is an explicit requirement in respect of any thesis submitted for the award of PhD at my institution (and I would imagine most others in the UK at least) and is an empirical feature of writing generally in social research, the humanities, as well as the natural sciences. Novelists may explain their methods, but this is rarely incorporated into their novels. Secondly, novels are read differently from anthropology: they may be regarded as artistic expression or entertainment (maybe both), however the author intends them. Of course anthropology may incorporate these characters as well (see anything by Clifford Geertz in respect of the former), but these will be understood as secondary functions, whereas one or the other is expected (by readers, I'm guessing) to be a primary functions of most novels. Another key function of academic writing is education and this may or may not be in the mind of the novelist¹. None of this is to diminish the value of novels or of fiction in general. I have, I hope, learned an enormous amount from novels and, indeed, from ficiton in other media: it provides me with explorations of the ways in which people (and other entities) might be imagined, of the creative ways in which language might be deployed and developed, and with metaphorical structures that excite my own imagination. For the most part I will have no truck with the question of 'truth' or, for that matter, with Coleridge's 'suspension of disbelief' (unnecessary if one is seriously engaged with the text), though there are, naturally, certain activities for which the question of truth becomes paramount, though perhaps these are not as widespread as is commonly thought.

Ethnography in educational studies has a rather shorter history than anthropology more generally. It became popular in the UK from the late nineteen sixties with a series of studies of secondary schools (Hargreaves, 1967; Lacey, 1970; Willis, 1977; Woods, 1979; and Ball, 1981) and a study carried out in a primary school (Sharp & Green, 1975). These studies and other ethnographic work generally entails, as I have said above, studying the relations and practices of a

¹ I recall (I hope correctly) a TV interview with Martin Amis in which he said that his principal aim in writing was to educate.

group of whatever size in its natural setting—*fieldwork*—as far as this is possible. Data collection often involves participant or and/or non-participant observation, formal and/or informal interviews, still and/or moving photography, drawings, maps, collecting artefacts and documents and so on. Researchers will aim to collect rich data or, what Clifford Geertz (1977) has described as ‘thick description’. Introducing this expression, Geertz borrows from the philosopher Gilbert Ryle, who put the problem like this:

Two boys fairly swiftly contract the eyelids of their right eyes. In the first boy this is only an involuntary twitch; but the other is winking conspiratorially to an accomplice. At the lowest or the thinnest level of description the two contractions of the eyelids may be exactly alike. From a cinematograph-film of the two faces there might be no telling which contraction, if either, was a wink, or which, if either, were a mere twitch. Yet there remains the immense but unphotographable difference between a twitch and a wink. For to wink is to try to signal to someone in particular, without the cognisance of others, a definite message according to an already understood code. It has very complex success-*versus*-failure conditions. The wink is a failure if its intended recipient does not see it; or sees it but does not know or forgets the code; or misconstrues it; or disobeys or disbelieves it; or if any one else spots it. A mere twitch, on the other hand, is neither a failure nor a success; it has no intended recipient; it is not meant to be unwitnessed by anybody; it carries no message. It may be a symptom but it is not a signal. The winker could not *not* know that he was winking; but the victim of the twitch might be quite unaware of his twitch. The winker can tell what he was trying to do; the twitcher will deny that he was trying to do anything.

(Ryle, 1968, no age numbers. Retrieved from <http://www.compilerpress.ca/Competitiveness/Anno/Anno%20Ryle%20Thinking%20of%20Thoughts%20USASK%201968.htm>)

... and so on.

Is Geertz aiming to achieve a description that is thick enough to get him to the truth? Well, no: the answer to this question lies in the title of his 1977 book, *The Interpretation of Cultures*. He wants data that is sufficiently rich to enable him to generate a narrative that will be meaningful to his audience. In a work published a decade later, he argues that, whilst anthropologists have often gone to great lengths to demonstrate their sometime presence in the field,

In itself, Being There is a postcard experience (‘I’ve been to Katmandu—have you?’). It is Being Here, a scholar among scholars, that gets your anthropology read ... published, reviews, cited, taught.

(Geertz 1988; 130; ellipsis in original)

The subtitle of this book says it all: ‘The anthropologist as author’; the anthropologist, not the culture that s/he has been studying.

My experience of being here, a scholar among scholars, has persuaded me, belatedly, perhaps, that ‘thick description’ should generate an extensive knowledge of the context of the culture that one has studied. This being the case, my supervision meetings with doctoral students who have been undergoing fieldwork includes detailed interrogation about the culture that they are studying. If they are unable to answer my questions, then the proper course of action should

be for them to return to the field and thicken their description until they can. Naturally, there is always going to be a limit on how far I can push this!

I want to introduce, briefly, four studies that all involve fieldwork, three of which are referred to by their respective authors as 'ethnographic', but they are very different in scope and in the ways in which they recruit and deploy theory. The first is a study by Julie Park whose:

... ethnographic study examine[d] Korean American collegians who were involved in KORE, a Korean American bible study within the InterVarsity Christian Fellowship (IVCF) at California University (CU).

(Park, 2011; pp. 193-4)

Park reports that:

[she] spent nine months with KORE as a participant observer from October 2007 to June 2008. I conducted ethnographic fieldwork during KORE's weekly bible studies, IVCF events, and informal times of hanging out, oftentimes over meals. I would jot down notes during events and then write a fuller, detailed field note as immediately as possible after each observation. I conducted semi-structured and informal interviews, and received permission to audio-record bible study during discussions that focused on the topic of race.

(Park, 2011; p. 198)

Park's reading had led her to two theoretical positions that explained opposing relations between religion and race and ethnicity:

Allport's [...] work shows the possibility of how religion can provide a powerful motivation to bridge racial divides, moving one from seeing ethnicity as a boundary that separates oneself from others to viewing ethnicity as still salient, yet not exclusive; while Kim's work [...] points to how practicing religion in an ethnic-specific community can reinforce ethnic distinctions, tightening ethno-religious identity. My analysis examines how students negotiated competing desires between a faith that bridges racial-ethnic divides and their strong desire to be in community with peers of the same ethnicity.

(Park, 2011; p.197)

These theoretical positions led Park to focus her attention on four individual members of KORE, who had joined the group because they wanted to interact more with non-Koreans and KORE was seen as potentially facilitating this move. Park's ethnography enabled her to write the narratives of her four subjects. The two men followed the trajectory described by Allport's theory, becoming more involved in multiethnic contexts, whereas the two women's stories were consistent with Kim's account. Each narrative reflects an individual experience in terms of both dispositions and social interaction as well as background. Park also discusses the ways in which her own particular Korean and Christian identities and her role as an educator imposed on her fieldwork and on her personal response to her findings. The insertion of information relating to the researcher herself and her own relation to the research setting is consistent with Hortense Powdermaker's observation:

The continuing relation between personal feelings (sensory, aesthetic, emotional) and intellectual perception is stressed [in her methodological book]—how the anthropologist feels as well as what he [sic] does, since he is part of the situation studied. In recounting my field experiences, I look inward as well as outward, with the benefit of hindsight. An anthropological voyage may tack and turn in several directions, and the effective field worker learns about himself as well as about the people he studies.

(Powdermaker, 1966; p. 14)

... and also with Clandinin and Connelly's emphasis that:

When narrative inquirers are in the field, they are never there as disembodied recorders of someone else's experience. They too are having an experience, the experience of the inquiry that entails the experience they set out to explore. [...] The narrative researcher's experience is always a dual one, always the inquirer experiencing the experience and also being a part of the experience itself.

(Clandinin & Connelly, 2000; p. 81)

Park is reporting ethnography in the context of educational studies, Powdermaker is also talking about ethnography, but in the discipline of anthropology, and Clandinin and Connelly are referring to what they describe as narrative research. Different methodological approaches, different disciplines are not to be hermetically sealed from one another; they, too, can dialogue productively.

Herbert Kalthoff's 'ethnographic study of educational assessment' is very different from Park's research. Kalthoff describes his fieldwork thus:

The empirical material reported in this paper was gathered through ethnographic fieldwork focusing on teaching and learning practices in German High Schools (Gymnasium) in the course of nine months; major parts of the fieldwork were carried out during 1992-1993 in two Jesuit Colleges located in the south-west and west of Germany [...]. I was able to locate five teachers from various disciplines (biology, geography, German language and history) willing to give me access to their grading procedures and allowing me to participate in eight oral final exams (A-Levels; Abitur). I followed the students through their written examinations and their final oral examinations. I visited the teachers at their working desk, at home, or in school, where they performed the grading. After the oral examinations, I listened to them discussing the grades. These direct observations were supplemented by interviews with teachers and students. During the interviews, I asked how they (the teachers) organised the exam, how they prepared the questions and what kind of difficulties they faced when correcting the written exams or assessing oral examinations. I asked the students how they prepared for the exam, how they processed the examination, and about the importance of grades within their peer groups. The empirical data were analysed using the open coding procedure of Grounded Theory [...].

(Kalthoff, 2013; p. 91)

Kalthoff was interested in how educational assessments are achieved and for this purpose he needed to see this achievement in action and discuss with participants about the processes involved. It is perhaps interesting that, whilst he describes educational assessment as involving self-assessment by the assessor—i.e. the teacher—he does not, unlike Park, reflect on his own identity or his personal responses to his setting. Also, his research concerns a specific procedure and he

does not follow his subjects beyond their involvement in this procedure either in terms of their backgrounds or their other experiences in schooling. So this is a very limited ethnography that contrasts starkly with the immersion in a society that characterised the early anthropological work such as, that engagingly described by Hortense Powdermaker (1966) or her teacher, Bronislaw Malinowski (1922), both of whom provide invaluable methodological information and advice. The limitation of scope is not, however, a limitation on the value of this kind of use of fieldwork in researching a targeted aspect of cultural practice.

The third 'ethnography' to which I shall refer, briefly, is on an even smaller scale than Kalthoff's study. Graham Hall describes his work as an 'ethnographic diary study'. The research took place over four weeks with adult learners of English as a foreign language. The twelve students—'from a mainly German or Swiss-German L1 background' (Hall, 2008; p. 114) were initially given the following instruction:

Write about anything you think was interesting in class today—maybe what events stood out most during the lesson, and why you remember them. You could talk about what you did, what other students did, and what [the teacher] did. Why not write about a part of the lesson that you really enjoyed, or if you like, something that you wish had happened a little bit differently. Write about anything as long as it interests you.

(Hall, 2008; pp 115-6; parentheses in original)

There's certainly no attempt at 'thick description' here, but nor does Hall claim this. Rather, he is exploring the possibilities of using a diary study in this way. He describes the role of the students as:

... participant observers, examining their own experiences of language teaching and learning and recording their feelings as openly and honestly as possible ...

(Hall, 2008; p. 114)

Hall identifies a number of problems with this mode of data collection, not least that, for his own convenience, he required the students to record their experiences in English, rather than their native German. Some of the students seemed to be a little uncertain to begin with over just what to write and the number of entries varied between them, which would certainly problematise quantification (as if there weren't enough problems with units of analysis already). Hall was also concerned about how to interpret the entries and whether the activity itself may have altered the students' perceptions (what we might refer to as a social 'observer effect') and about the possible significance of self-deception and self-editing, not to mention deliberate researcher deception! He did include interviews with the students on the basis of their diary entries and also a feedback questionnaire, though the study, being confined to a period of four weeks clearly limited the scope of the findings.

Ultimately, Hall concludes that 'we should operate with a systematic "distrust" of diary data and what we think it might show' (Hall, 2008; p.120), but his dissatisfaction is perhaps an artefact of an inadequate research design. The purpose of the study seems to have been limited to an exploration of possibilities, but it was a very limited exploration, rather less than an ethnographer might hope

to gain on a short vacation. Very little interview data is reported—a question and answer from one student and one brief comment from the teacher in interview—and there is no evidence of probing nor, indeed of open questions. Without a clear understanding of what one is trying to get at in terms of a research question *and/or* the use of penetrating probes to access the subjects' meanings and priorities, interviews are unlikely to reveal very much and they didn't here!

In the mid-nineteen-nineties Andrew Brown and I were funded by the Overseas Development Agency to work with the University of Cape Town (UCT) and the University of the Western Cape (UWC) on the development of research capacity in the area. We stayed in Cape Town for three weeks in each of the three years of the funding. We were involved in teaching and supervision and holding seminars in the universities. It occurred to us that, whilst many of our students during this time were school teachers and had told us about their experiences in schools in the Cape Town area, we had not actually seen inside any of these schools. This was a critical time for South Africa, a time of great optimism following the election of an ANC² government to replace the National Party that had held power since 1948. Nelson Mandela was President and Apartheid was officially over. Officially over, but schooling continued to be structured along racial lines. The 'White' population were still economically dominant and the children of this group attended the generally well-resourced 'Model-C' schools. Many African South Africans still lived in so-called 'informal settlements'—shanty towns on the outskirts of the city. Schools in the informal settlements had been run by the Department of Education and Training (DET). These schools were generally very poorly resourced, with classrooms containing fewer chairs and desks than the number of students who were supposed to occupy them—they had to share. The majority 60% population of the Western Cape, however, had been designated by the Apartheid government as 'Coloured'. This was complex group, but could loosely be divided into Moslems, whose first language was English, and Christians, who spoke Afrikaans, though most of the population of the area were were functionally fluent in both of these languages. The Coloured population had been restricted to live in small suburbs, each of which housed all social class groups, their residency having been defined by race and not income. This was a condition similar to the 'Negro' [sic] population of 1930s Indianola in Mississippi that had been studied by Hortense Powdermaker (1967, 1993). The 'White' population were similarly divided in linguistic terms and as diverse ethnically and socially, though they certainly occupied the dominant economic positions and were geographically more widely distributed. Many of the African South African population were immigrants to the area and so were linguistically diverse, though Xhosa was dominant and Afrikaans virtually absent, being widely regarded as the language of the oppressor.

Being hosted by Schools of Education, we were able to obtain introductions to a secondary school³ in each of these three categories that was regarded as a

² African National Congress

³ Elementary schools would have presented language difficulties in the informal settlement area as young children were unlikely to have any substantial command of English and we did not speak any Xhosa.

'good' school. As we were ignorant of the conditions and criteria for standard of schooling in South Africa at the time, we felt that this was as good a principle as any for selecting three schools, which was as many as we were going to be able to manage in the time available. We called the Model C school, Mont Clair, the 'Coloured' school, Protea, and the informal settlement school, Siyafunda; these are fictitious names. We drove ourselves to Mont Clair and Protea, but our hosts at UCT insisted that we were accompanied by a guide who would be aware of the conditions in the township and who would let us know when it would be safe to go. There was, understandably, a good deal of anxiety amongst the White population of Cape Town at that time, with sporadic violence directed against Whites⁴. It seemed to be the case that there was also violence in the African townships, but whether or not our hosts were over-reacting we had no way of telling. Certainly we identified at least one myth⁵ that was widespread amongst our academic colleagues and gangsterism was mentioned in interviews with students at Protea.

Our fieldwork during three week periods in each of two years in these schools included interviews with teachers, including the Principals of the schools, attending school assemblies and a staff meeting at Protea, class shadowing with Standard 7 and Standard 10 classes in each school, and interviews with students from these classes. The student interviews were conducted as single-gendered, group interviews with four volunteer students in each group. The interviews were audio recorded and one of us took handwritten notes in addition. When class shadowing, we did not participate in the lessons, but sat quietly taking handwritten notes; we did not record the lessons. Two of us working together on data collection proved to be productive and data analysis began with discussion in the car on the way home after each day's fieldwork. Transcription of the group interviews involved lots of stopping and re-winding of the tape because, distinct as the voices sounded during the interviews, they tended to blend into one another in transcription; a video recording would have reduced this difficulty.

The focus of our analysis was on the possible relation between the pedagogic practices in the schools and the structure of the communities that they served. This entailed, of course, that we had to gather data on the local communities. We had a stroke of luck in this regard in Siyafunda. We had been interviewing four Standard 10 girls and, when they left, we asked them to send in the boys from their class who had offered to be interviewed. We were expecting four sixteen-year-old boys, when in walked four men, one of whom was 34 years old, was married with three children and was chairman of his street committee; he and the other men were wearing immaculate school uniform. The focus of the interview speedily changed to take advantage of this unexpected gift and we were able to

⁴ One restaurant that we visited was bombed a day later, not, we think, because of our visit!

⁵ During our visits to Siyafunda we encountered two teenage boys who were dressed in a flamboyant style, one sporting a green, felt bolero hat which he took great care of, sitting it on the chair next to him in class. Both boys kept themselves apart from the other students. We were told by all but one of our academic colleagues that these boys were gangsters. They were not. They had recently returned from their period of initiation into manhood and were required to set themselves apart—including by dress—for a period, during which they would certainly feel highly embarrassed, individuation being generally avoided by the students (Dowling & Brown, (2009).

access a good deal of local knowledge about the community that might otherwise have been unavailable to us: we certainly would not have felt able to wander around the township asking questions.

This was certainly a very limited study in terms of the amount of time that we were able to put into it, but on revisiting the original conference paper (Dowling & Brown, 1996) I felt able to develop a theoretical description of our conclusion about the pedagogy/community relationship and this was included in the version of the chapter in Dowling & Brown (2007) and (2009). There is no suggestion that this theory is final, but it stands as a kind of ‘cadence’ in our analysis that is available to interrogate and be interrogated by subsequent data collection. Of course, 1990s Cape Town has long since gone, so the research setting would likely be very different. I will introduce here just one aspect of the theory.

The first dimension of the relational space that I produced refers to the organisation of the students by student and/or teacher strategies as either ‘individualised’ or ‘collective’. The second dimension concerns the position of the teacher as ‘recruited’ by the community or as a ‘leader’ of the community. Where the student is individualised, then if the teacher is a recruit, this constitutes the teacher as a service provider and the student as client; where the teacher is a leader, then we can constitute the relation as guardian/ward. Where the student is a member of a collective, then if the teacher is a recruit, this places the relation as that between community servant and community member. Finally, the student as a member of a collective and the teacher as leader constitutes the relation as general/footsoldier. These relations are summarised in Figure 5.1

	Teacher	
Student	Recruit	Leader
Individualised	<i>service provider/client</i>	<i>guardian/ward</i>
Collective	<i>community servant/community member</i>	<i>general/footsoldier</i>

Figure 5.1. *Teacher/Student Identity* (from Dowling & Brown, 2009; p. 181)⁶

This scheme enabled us to make sense of our observations in the schools and, indeed, to give some sense to the meaning of what might be considered a ‘good’ school, that is, where there is a consistency between the dominant form of relations that obtained within the community and the teacher and student actions. I will give just two examples. The relations in Siyafunda were generally consistent with the teacher as a recruit of the student collective and we observed this in all but two cases. We were told by the Principal that Afrikaans was despised by most of the students, who saw it as the language of the oppressor and so refused to cooperate with teachers of the subject. One teacher, however, whose first language was Afrikaans—unusually in this area—deployed a highly charismatic pedagogic style. She was very well prepared, there were posters on all

⁶ The ‘teacher’ categories are named differently in the online version of this paper (Dowling & Brown, 2007).

of the walls of her classroom, she had prepared worksheets for the Standar 7 class and there was an exercise already on the board at the beginning of the lesson. Evidence also that she was equally well prepared for her other classes. She moved around the class, moving very close to students, calling them 'darling' and allowing them to whisper to her:

... in an early interaction with an individual, she moved very close to him, putting her face very close to his and at the same level. He apparently answered her question incorrectly, because she reached behind her, took a ruler from another student's desk and struck the first student on the hand with it. A little later, she slapped another student on the back several times with her hands. Neither of these actions were sufficiently forceful to inflict pain. The effect appeared to be, however, to disorientate the students.

(Dowling & Brown, 2009; p. 167)

Extremely disorienting—to us as well! Unsurprisingly, this teacher's switching between 'guardian' and 'general' strategy worked very well: her students all regularly did well in Afrikaans and in biology, which she also taught.

The Principal in the first year of our visit to Siyafuna wisely took this teacher with him when he was appointed Principal to another school. His own replacement also attempted 'leader' strategies, both in her own (English) classroom and in the school assembly (the former Principal had acted almost as a visitor in his assembly, which was effectively run by the student body). The new Principal, however, lacked the charismatic energy shown by the Afrikaans teacher and in both of the settings in which we observed her, she was ignored by the students.

The dominant community relations, then, were not determinant in respect of pedagogy, but a good deal of effort and charisma was needed to overcome them.