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Pamela Nilan

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PAMELA NILAN
University of Newcastle, Australia

ABSTRACT This article takes two examples of trying to collect fieldwork data in dangerous or difficult circumstances in Bali and uses them to explore some issues central to qualitative research. These issues include shifting researcher subject positions in qualitative sociology approaches, and the coherence and usefulness of data collected in chaotic or risky circumstances. Methodological practices such as reflexivity are considered, as well as the task of writing research accounts up from messy and chaotic data sets. It is concluded that data collected at moments of fieldwork crisis may not be particularly useful, except as a cultural reminder of the insider/outsider status of the researcher, and to inform more productive factual data collected after the event.

KEYWORDS: *control, ethics, fieldwork, objectivity, reflexivity, risk, subject position*

Introduction

This article considers the problem of conducting ethnographic fieldwork in dangerous and difficult circumstances in the course of a sociological research project. Although 'dangerous fieldwork' is not a new topic – one must acknowledge the excellent work of Van Maanen (1988) and the valuable working paper by Lee (1995) among many others – this article takes a different slant. The specific difficulties this researcher found in conducting ethnographic fieldwork in risky and trying circumstances were further complicated by the constant sense that one ought to be more 'in control' of the situation than was really possible. Looking back at field journal entries which refer to this anxiety over control, brought the realization that this was probably not just an idiosyncratic or isolated fieldwork phenomenon, but was pertinent to contemporary debates in qualitative research more generally. So this article, despite its somewhat sensationalist title, actually grows out of some 13 years of conducting sociological research with young people on gender, education

and youth culture. Across a variety of projects in Australia and in the Southeast Asian region, this researcher has struggled to reconcile the combination of two different approaches in qualitative social science research: 'formal' methods (interviews, surveys, focus group work), and ethnographic fieldwork – participant observation. The major problem has been effectively moving between the two subject positions of researcher implied in the contrasting research paradigms here. The first is constituted within a discourse of control, objectivity, even emotional detachment. The second is constituted within a discourse of immersion, reflexivity and rapport. I find the former researcher position easier to assume, but the latter, although much more uncomfortable, always yields rich and interesting data, which the first sometimes does not. It is in the latter of these researcher subject positions that the possibility of danger and risk most commonly arises, yet this is the position of least control and enhanced emotional vulnerability.

The project from which the examples in this article are taken used both approaches – a common feature of contemporary qualitative research. For example, Eriksen advises that even anthropologists depend on a combination of formal and unstructured methods in their fieldwork (1995: 15). Elsewhere in the social sciences there exists a 'carnavalesque profusion' of methodological approaches (Atkinson et al., 2001: 4; see also Hammersley, 1998: 7). This is so, not only in 'foreign' fieldwork settings such as I describe here, but in institutional settings such as hospitals, welfare agencies and schools (Street, 2001). Using the more formal approach, the social science researcher obtains some 'hard' data through surveys, interviews, focus groups, then, using the second approach, seeks to support or interrogate it through ethnographic data gained through immersion in the local scene or culture. Taken together, these qualitative methods constitute a valuable tool for investigating the complex relationship between social structures and human agency (Goodman, 1998). At the level of the local scene, phenomena are both mapped using formal empirical methods, and deeply interrogated, using ethnographic methods. Inevitably the more formal approach seems much more 'scientific' than participant observation. The design and delivery of surveys, the design and execution of one-to-one interviews, the assembly and conducting of focus groups, all imply control of the research context by the researcher. He or she enters the research context and sets about deliberately constructing a 'controlled' set of research circumstances – controlled in the sense of deriving primarily from the intentions and interventions of the researcher, rather than primarily from local participants. The research subjects assemble themselves (physically) and their accounts of phenomena relative to the set of research circumstances constructed by the researcher.

Yet quasi-scientific 'control' is the very thing which must be set aside for effective participant observation to take place. The aim of the ethnographic researcher is to observe 'naturally' occurring phenomena relative to the research question. The term 'naturally' here refers to the fact that events and

accounts derive primarily from the intentions and actions of local participants, rather than from researcher interventions (Hammersley, 1998: 2). The researcher who is employing both survey/interview data collection methods and ethnographic methods in the same field context at the same time therefore has to move rapidly between different subject positions *qua* research. Sometimes she/he is in a very 'controlling' position (recruiting subjects, distributing surveys, conducting interviews at a specific time and place), and sometimes occupies the very opposite data collection position, with little or no control at all over the research context, attempting to blend in and be as unobtrusive as possible in order to better explore the taken-for-grantedness of local routines, rituals, procedures and assumptions. I did not find these rapid shifts in the subject position of researcher easy, especially in the foreign, exotic or risky fieldwork context, where one is beset by confusion and anxiety. In unfamiliar settings which carry an element of threat, the subject position of the controlling researcher seems far more secure and reassuring.

In qualitative research projects where ethnographic fieldwork is not used, the emotionally detached researcher invites subjects to present themselves in a formal research context, has minimal contact with them (especially in the case of surveys), then retreats back to the reassuring safety and familiarity of home, hotel or cultural enclave with neat stacks of satisfyingly ordered data. By contrast, observational and incidental data gathered while the researcher is living in, or spending extended time in the field, is inevitably far less neat and logically ordered. While Hammersley (1998) politely labels the process 'unstructured' (p. 2), Kumar (1992) describes it as a 'brash, awkward, hit-and-run encounter' (p. 1). Although potentially rich, fieldwork yields up messy notes, personal reflections, photographs, sketches, assemblages of documents and even locally collected items. Writing up fieldnotes relies on the frequently faulty device of memory. The researcher tends to collect as much data as possible, in part because it is often not clear what may turn out to be relevant in the end. The methodological mandate to create ethnographic 'thick description' (Geertz, 1973), and the fear of missing something which may later turn out to be important, usually results in the gathering of large amounts of data which cannot be easily categorized and analysed. The researcher may suffer from isolation, anxiety, stress and depression (Lee, 1995: 13), even in relatively straightforward fieldwork. The research accounts produced from ethnographic data can often seem like descriptive narratives, and inevitably, in the course of producing a research account which will be taken seriously by the academy, a lot of detail must be set aside. Finally, a range of ethical issues seem to emerge in the ethnographic component of qualitative research which were not considered at the time of planning and obtaining permission for the research. This is especially the case in difficult or dangerous fieldwork situations.

The issue I take up here is the positional shift of the researcher who moves between very different forms of data collection in these combined method

studies which take place in the same setting. My argument is that the two subject positions are not easily separated. Willson describes this as resulting in a qualitative research situation where the ethnographer is expected to be 'simultaneously *detached* and yet intensely *engaged*' (1995: 255, my emphasis). In my experience, the ethically enshrined precept of researcher 'control' tends to flow from the context of formal data collection over into the ethnographic context of data collection (see Geertz, 1983 – 'blurred genres'), both at the actual time of data collection and during the later phase of analysis and interpretation. This article looks at some extreme examples of ethnographic research in dangerous and unpredictable fieldwork situations. A combination of qualitative methods involved a move from the subject position of emotionally detached researcher, which implies control, to the subject position of emotionally immersed researcher, which implies vulnerability. The key themes of this article resonate with a question recently posed by Lerum, 'how do researchers' emotional engagements affect the kind of knowledge that is created?' (2001: 470). Since this is about my own recent research experiences, I acknowledge that the problem of moving from one research position to the other may be dismissed as a personal and idiosyncratic one, but accounts from doctoral students employing similar combinations of methods in field research indicate that this is a wider experience. The fact that I refer to extreme examples is not intended to glamorize the experiences. Rather the marked lack of researcher control at key moments in the situations described, points up the positional dichotomy issue I wish to address in a broader sense.

Evidence from ethics committee approval

We can initially examine this dichotomy through an examination of the research ethics application in which I requested permission to conduct the original case study. The reader should note that permission to conduct the case study was granted, and the project proceeded as planned.

The 1999 ethics approval application form at my university required the applicant to state:

9.SCIENTIFIC OR EDUCATIONAL AIMS AND SCIENTIFIC VALUE OF PROJECT

The applicant must immediately take up the available subject position of researcher within the discourse of scientific (controlled, emotionally-detached) research, just in responding to the question. Trying to frame an adequate response, I wondered about the ethnographic component. Embedded participant ethnography seeks to understand the meaning/making of members of a specific culture (for example, a remote African village, a workplace, an urban street gang and so on). Since the fieldwork context would not normally be considered as known in advance, or predictable, but something to be directly experienced by the researcher before any decoding or explanation is possible, the ultimate value of ethnographic enquiry cannot be

known in advance. Framing a response was therefore a source of tension. In the meantime, I stated the aims:

1. To identify to what extent 'culture', in the contemporary Balinese example, is being transformed through education, development, and political change, and to what extent new forms of identity for young people are being created through a synthesis of traditional and modernist discourses.
2. To investigate whether it is indeed the case that students in North Bali are coming to understand themselves primarily as individuals, or see themselves primarily as members of extended family, caste, clan or village.
3. To ascertain whether students, who were involved in recent political activism to support the cause of Megawati Soekarnoputri's PDI Perjuangan party, understand themselves as attempting to ensure a particular kind of imagined future, through their active support for her as a leader.
4. To chart student, community and employer dissatisfaction in North Bali with the Indonesian education system as it currently exists.

Suitable means of obtaining data which could fulfil these research aims were then stated:

11. RESEARCH METHODOLOGY

- (a) Observations of student life and activities recorded in a daily journal of my teaching and involvement with the teachers' college and the attached demonstration school – this will not involve any identification of students or teachers. It will be a record of events without names or identifying labels.
- (b) Fieldwork notes on student conversations, disputes and resolutions. Also field notes on any student demonstrations or political rallies. Once again, this will not involve any identification of the actual people involved.
- (c) Surveys and selected interviews with tertiary students, teachers, parents and local employers (165 subjects).
- (d) Focus group interviews with political activists (5 groups of 5 persons).

The application form demanded the submission of all documentation relating to data collection items (c) and (d) above, but (a) and (b), the ethnographic components, elicited no such demands. This differential emphasis indicates the dichotomy between researcher subject positions I wish to consider in this article. De Laine (2000: 21), with reference to Denzin (1997), argues that the researcher considers ethical issues of proposed ethnographic research within a dualistic configuration of moral concerns ranging from lurking positivism to overtly feminist concerns, and this is why fieldworkers are confused and anxious. However, this has not been my experience. Nevertheless, in supplying the necessary documentation for (c) and (d) above, the researcher is certainly constituted within the recognizable 'scientific' discourse of doing

research on humans. Yet (a) and (b) also refer explicitly to research on humans, some of whom the researcher will obviously get to know well in the course of prolonged participant observation (which was specified as four months elsewhere in the document). So what exactly constitutes the subject position of fieldworker researcher if he or she is not required to give much indication at all of what they will be doing? This silence is not just a fault in the form. There has been no publicly available 'rhetoric of method' (Marcus, 2001: 527) for fieldwork practice (see also Anderson, 1989: 263; Hammersley, 1998: 18). Perhaps this suits some researchers well. Nevertheless, the prior demand that the scientific value of the project be demonstrated is a powerful discourse, and one within which all proposed data collection practices obviously need to be considered. For this researcher it meant filling the empty semantic data collection space of (a) and (b) with sufficient ethics and rigour (or anxiety about these things) to ensure that the whole project would eventually turn out to have some 'scientific value' in the end, such as 'reflexive' techniques (Anderson, 1989: 254 – see discussion below), and focusing the ethnographic data collection very tightly on the aims of research. However, tight focus in participant observation carries the obvious danger of only noting down what you consider to be relevant at the time, which carries the risk of missing something which may later prove relevant.

Furthermore, set up to deal primarily with bio-medical experimental research, ethics committees require applications from researchers to sound as though all research subjects will be treated dispassionately and objectively while adhering to detailed protocols (Madjar and Higgins, 1996: 131). Implicitly this applies to the full range of methods used, including any ethnographic component. In this way, ethics committees manage to convey the expectation that, if approval to conduct research is granted, objectivity and impartiality will prevail in gathering data from human subjects. These concerns imply high levels of control over the research context and over the degree of researcher emotional involvement in the research setting (Willson, 1995: 255). However, while the researcher may have a great, if perhaps illusory, sense of control, certainty of practice and distance when using formal methods such as (c) and (d) above, the same cannot be said of ethnographic immersion to collect participant observation data. Popkewitz (1984) argues that immersed ethnographic research has always treated the social world as in 'flux', teeming with complexity, contradiction and human agency (p. 50). This research 'world' is not an orderly place and the humans in it are not orderly either. Far from being fixed, the ethnographic human research subject resembles the poststructuralist subject, imagined as constantly in process. Davies (1997) claims the human subject only exists 'as process' (p. 274). While Haraway claims that human subjectivity is 'partial in all its guises . . . always constructed and stitched together imperfectly' (1991: 193). If this is so for research subjects in ethnographic research, then the researcher as human subject is similarly in flux, dealing constantly with shifting realities

and contradictions.

With such slippery human subjects as its epistemological basis, there are relatively few 'scientific' rules and guidelines in ethnographic research, except for the researcher to find willing participants, record as much data as possible while trying not to affect the 'natural' setting, and to stay immersed for a reasonably long period of time (Hammersley, 1998: 8). Beyond this advice, it has been claimed that:

There is as yet no alternative modality of method or articulation of a set of regulative ideals governing fieldwork that gives professional legitimacy to what is in fact happening to fieldwork. (Marcus, 2001: 527)

Yet reflexivity in ethnographic research is often cited as a means to achieve a methodological rigour which at least parallels the 'scientific' rigour of quantitative social science research. However, there did not seem to be anywhere to insert reflexive considerations and practices in the ethics application form. Although a term subject to various interpretations, reflexivity refers on the one hand to the cultural attitude and approach of the ethnographic researcher, and on the other hand to certain practices, derived from the latter, which achieve rigour in the fieldwork process. The appropriate reflexive approach for an outsider entering a fieldwork situation is the constant acknowledgment of the social and cultural position of the researcher in relation to locals, for example, his or her relative privilege, ingrained cultural assumptions, language and communication problems, tendency to impose simplistic theoretical interpretations and so on. This intensive 'locating' of the researcher relative to the local scene and the data collected is usually realized in the form of a reflexive diary or journal in which all kinds of personal jottings are recorded. The purpose is to remind, and clarify for the researcher, exactly what kind of subject position he or she occupies at all stages of the fieldwork – to record the viewing of oneself (and the data collection) in a 'growing variety of local mirrors' (Herzfeld, 2001: 260). Yet despite the comforting reassurance of rigorous daily reflections and journal-keeping, reflexive practice did not soothe my preoccupation with considerations of 'scientific' control. In part this is because reflexivity belongs far more to the tradition of anthropological research than to modes of sociological enquiry.

Ethnography – participant observation – is the traditional method of anthropology (see for example, Eriksen, 1995; Malinowski, 1961), while surveys, interviews and focus groups as research methods prevail in sociology, education, human geography, economics, psychology and market research. If, as in my case, the researcher is not trained as an anthropologist, but was apprenticed within the latter set of social science disciplines, then the push for 'scientific' rigour is almost inescapable. The practice of fieldwork – while relatively conventional and assumed for anthropologists – may constitute an unusual or even a political act for those from other disciplines (Wolf, 1996: 8). As a sociologist, I undertake fieldwork because I have found it to yield excep-

tionally rich and politically significant data, yet the sociological researcher subject position remains contingent upon practices which embody 'objectivity, impartiality and disinterested observation' (Madjar and Higgins, 1996: 131), and I find this troubling for the practice of immersion. Hammersley (1998) maintains that within sociology the question still remains as to whether ethnography is scientific (p. 6).

The implication of 'scientific' control in fieldwork continues to be exemplified in published social science research accounts. Most research accounts in journals within the field of sociology analyse ethnographic data sets as if they were calmly gathered in a rational and orderly context. Indeed, I do it myself. The implicit concepts of experiencing the world are set up in such accounts so as to make certain things disappear – confusion, threats, danger, the unpredictable, the non-event. A universality of control, objectivity, things going according to plan is conveyed in neutral, academic prose – 'a measured intellectual style' (Van Maanen, 1988: 47). As Lerum points out in relationship to sociology, 'at the core of this objective ideology is the rule of *emotional detachment*' (2001: 472, emphasis in original).

In this article I want to explore what happens when things do go wrong and the idea of *emotional detachment* is a bad joke. The first example is a sudden civil emergency – a riot – which Lee (1995: 3) would describe as *situational* danger – unexpected crisis. The second describes an event when the researcher enters a local nightclub where drugs and sex are sold and violence threatens. Lee's term for this kind of danger is *ambient* (1995: 3) – goes with the setting. In both situations, gender increased the danger (see Warren, 1988). A middle-aged female researcher is trying to collect data in an environment dominated by potentially violent young men. Yet one does not seek to over-dramatize these events. Obviously I survived to tell the tale. The value of the examples lies in their utility to examine some pivotal qualitative research methodology questions which pertain to control and anxiety about the lack of it:

1. To what extent can ethnographic fieldwork data collected in dangerous and chaotic settings constitute orderly and coherent accounts of the phenomena?
2. What kinds of ethical and methodological issues emerge from the practice of a researcher collecting fieldwork data in dangerous and chaotic situations, which are, nonetheless, relevant to the major research question?

The research context

It might surprise the reader to learn that things got dangerous and unpredictable in Bali, one of the most popular tourist destinations in the world and generally counted as very safe. Yet beyond the tourist enclaves Bali seethes with many of the same kinds of political resentments as elsewhere in the archipelago, and there is a thriving underworld trade in sex and drugs which

does not exist just for the overseas tourist market. My research project was entitled 'Young People, Education, Politics and Radical Change in North Bali'. Bali seemed to offer an Indonesian context in which identity frames of tradition and modernity were more or less equally available as cultural resources to be taken up within a modern notion of the self by young people. The research was carried out over four months in Singaraja, North Bali, a large provincial town in economic decline. Two important sources of local revenue were the teachers' college (where I taught English and research methods) and the tourist enclave of Lovina, about ten kilometres away on the coast.

I arrived in August 1999 and spent two months conducting both the large scale survey and ethnographic research. I lived alone in a guest house on a main street near the college and close to the centre of Singaraja. The teaching was enjoyable, but everything else was extremely difficult, including getting out of town. This could only be achieved during daylight hours and involved three transfers between *bemos*, small, rusting, cramped vehicles which often broke down. Responses given on ethics applications tend to assume the target research setting can be relatively easily entered and exited by the researcher from his/her privileged position of professionally trained outsider/observer, even during the ethnographic component of research. We might summarize this as the unproblematized belief that privilege confers remoteness from consequences. However, any assumption of this kind is confounded by the methodological imperative to live simply in the local community in order to be 'in a far better position to be really in touch with the natives than any other white resident', as Malinowski (1961: 18) puts it. The ethnographic emphasis on investigating the subjective dimensions of human experience, and the contextual nature of those experiences, demand researcher immersion. And, logically, the immersed researcher is likely to suffer the same consequences as the local inhabitants or workers if anything goes wrong. Ethics committees are concerned about danger and risk, but not in reference to the researcher. Madjar and Higgins (1996) note that ethics committees tend to focus on the 'worst case scenarios' and 'what if' situations (p. 130), but this is only about participants, not researchers (see Lee, 1995). I had been in the field for about six weeks when East Timor went to the polls. Australia and Indonesia moved rapidly into a foreign relations crisis and local anti-Australian feeling was running high. I was verbally harassed in the street, which I duly recorded (keeping control). I felt extremely isolated, anxious and stressed (which I noted in my reflexive journal). None of this seemed very 'scientific', just personal and subjective.

By October 1999, when local political unrest began, I had three exercise books full of participant observation fieldnotes, interspersed with reflexive journal entries. I had become familiar with the student leaders of local political groups (whom I intended to interview in focus groups), and had gained some idea of regional Indonesian politics from the inside. At a deeper level I had become aware of the strong resentment of Balinese Hindus (a minority)

towards the Muslim majority in the country. Bitter feelings against the political hegemony of Java embodied by Suharto and his cronies (and the military), and fear of a rising tide of Islamic fundamentalism actively fuelled the almost fanatical support by Balinese for the political party PDI-Perjuangan (Indonesian Democratic Party of Struggle) (Surya, 1999), and for its leader, Megawati Sukarnoputri.

The riots

1999 was a landmark year in the growth of Indonesian democracy. Following the fall of Suharto in the previous year, early 1999 saw the first open democratic elections. PDI-Perjuangan, secured the most primary votes and a parliament was assembled in Jakarta. On 20th October 1999, election for President of the Republic was held among the members of parliament. It was widely expected that Megawati Sukarnoputri, as leader of the majority party, would take the presidency. However, this was not the case. Instead, Abdurrahman Wahid, known as Gus Dur, the moderate head of the Islamic party NU, was elected president by parliamentary vote. In reaction there were riots by PDI Perjuangan supporters in many parts of the country. Commentators afterwards agreed that the worst riots and property destruction took place in Bali (*Nusa Tenggara*, 1999: 1). In Singaraja some damage was done in small attacks during the night of the 20th, but the well-organized mass riots began early on the morning of the 21st, which was afterwards always referred to as *Kamis Kelabu* – Black Thursday (*Nusa Tenggara*, 1999: 3).

On the morning of the 21st October I was scheduled to give a lecture commencing at 7.00 am at the teachers' college. The campus was curiously deserted when I arrived, and only about 20 students came for the lecture. About 15 minutes after I began speaking I received a call on my cellular phone from one of my co-researchers, warning of violent riots up near the *kantor bupati* (mayoral office), about three kilometres away. He told me to go home as quickly as possible and stay inside. Having warned and dismissed the students I ran outside to the street but all public transport had vanished and I had to walk and run with several of my female students, up the long street to our houses. As we ran past the closed and shuttered, silent houses and government offices, a series of loud explosions shook the ground and two large plumes of smoke streamed up a hundred metres into the sky from the direction of the mayoral office. The roar of a thousand voices could be heard. All three of the students with me began to cry. I felt an intense sense of foreboding, but asked the students what they were feeling (researcher in control and getting field data). '*Takut!*' (frightened) they whispered, as they ran into their houses. Obvious really.

Over the next eight hours, nearly all the public buildings in Singaraja were burnt out, as well as many homes of government officials. Seven of them were in the street near my home or in the street behind. 'The mob just went by. They

have set huge burning pyres and hurled explosive missiles at the Department of Justice, and all the other government departments in my street' (Fieldnotes, 21/10/99). Around 20 cars of government officials were torched, as well as innumerable motorbikes of family members. The town centre where I lived was blockaded on all sides. No-one could get out. Power and telephone lines were cut, including mobile phone networks. The mob occupied the central business district, throwing fire-bombs, cutting down trees, throwing rocks, bottles, shouting slogans and beating on fences with sticks and iron bars. The smoke and noise were intense, especially during the two-hour period when my street was under attack. For most of this time I was lying on the floor of my house with a wet handkerchief over my mouth and nose, eyes streaming from the toxic smoke, in momentary expectation of a fire bomb coming through the front window, since mine was a typical government resident's house. There was nothing to do but endure. 'Emotional detachment' (Lerum, 2001: 472) was entirely lost.

The kind of fear you experience at such times is hard to describe. I thought about my children, partner and family back in Australia and wondered if I would ever see them again. However, I was also drawn, in the subject position of researcher, to peep through the curtains at the front window, despite the danger, whenever the mob raged by, to see whether there were faces I knew in the crowd. There were familiar faces, and I jotted their details down, while gasping for breath – an action which now seems ridiculous. I even considered, once or twice, running out to the front gate and trying to ask some questions of the young rioters. But instead I filled pages and pages of a notepad with commentary – sure that I should be both gathering data and practising reflexivity at this historically and politically significant juncture. Two of the entries made early in the riot period read: 'I sneaked to the fence and asked my neighbour where the police were. She replied *'pulang'* (gone home). Similarly it appears that the local army contingent has locked itself up in barracks', and 'Lots of the neighbours are out on the footpath watching. They express great solidarity with the demonstrators' (Fieldnotes, 21/10/99). However, after this the fire-bombers arrived and I did not dare venture out.

As mentioned, I wrote pages of commentary during the riot experience. The journal comments can be best understood like this:

ME
FAMILY
NEIGHBOURHOOD, COLLEAGUES, STUDENTS
POLITICAL SITUATION IN BALI
POLITICAL SITUATION IN INDONESIA
INDONESIA/AUSTRALIA RELATIONS (EAST TIMOR)
GLOBAL ORDER (RESENTMENT OF DEVELOPING WORLD CITIZENS
TOWARDS DEVELOPED WORLD CITIZENS)

The hierarchy in this notebook schema is of arenas of fear and concern, mov-

ing downward from the intensely personal level to the global level. So, first I was fearful for my own safety – an older woman, alone and unprotected in a flimsy house, with a mob of a thousand or so violently angry young men outside. Second, I was intensely aware of my family back in Australia and my responsibilities and feelings for them. Beyond that, I was concerned for my neighbours, my colleagues, the mayor (whom I had interviewed two days before) and above all my students, including those involved in the riots, those cowering at home, and the Muslim students who surely felt deeply threatened. Beyond that, I was concerned that the political situation in Bali had spiralled out of control and the local economy would suffer drastically in lost tourism revenue, which would impact adversely on people I knew. In the national context, I worried that the level of local civil unrest gave the clue to the long-term viability of Indonesia's fledgling democracy, and the country might lurch back into a 'balkanized' state characterized by small warring enclaves and ethno-religious primordialism. At the regional level, I was well aware that Australian troops were in East Timor, heading up the United Nations post-election peace-keeping force. Massive Indonesian resentment was simmering about this, fanned by media and political rhetoric in both countries. I had already been verbally harassed in the local neighbourhood as an Australian visitor and told to go home. It was not unthinkable that someone would choose to target the local Australian in the rage of riot fever. Finally, I was aware that some level of resentment is always present when a westerner from an affluent country is living in any developing country, no matter what the philanthropic orientations of specific individuals might be.

The schema above is an example of the self-reflexivity techniques advocated by many critical ethnographers, and it was comforting at the time to have to consider the crisis with some level of objectivity. Yet, like Hunt (1989), after the harrowing incident in the field, it first appeared I had written down a lot of strikingly irrelevant information. The urge to write it all down as it happened was driven by the recurrent fieldwork question, what kind of 'scientific value' for the project overall will this data prove to have? Every moment was supposed to count, so it had to be recorded. Yet there was nothing to record but explosions, smoke, shouting, and the thoughts of the researcher. In retrospect though, the reflexivity exercised in the act of writing was in itself valuable because it signified the researcher as a cultural presence outside, and yet within, the social and cultural context of a street riot in the developing world. So while it was not instantly utilizable data, it was a valuable reflexive moment because it shone a brilliant spotlight upon the epistemological problem of shifting between researcher subject positions. The writing made explicit the contrast between pitifully huddling alone on the floor, choking on smoke, (the ethnographic experience) and sitting down in comfortable surroundings to interview someone according to a carefully planned set of questions (formal qualitative method). If I had not chosen to live locally and conduct participant observation as a component of the project I would have

been safe in a heavily-guarded hotel in the tourist enclave.

The rioting and property destruction went on until late afternoon, when the power mysteriously came back on. The rioters all vanished to watch the vote for Vice-President, broadcast live from Jakarta. Megawati Sukarnoputri was elected Vice-President, and there was no more civil unrest. Of course it was after the immediate danger had passed that some excellent data collection prospects arose, because I was then theoretically able to move around the neighbourhood asking people how they felt. However, I asked no questions and wrote down no observations. I had rushed over to my neighbour's house and was glued to the television like everyone else, watching the voting count, holding the hand of my elderly neighbour who was sobbing and repeating 'Bu Mega' (Mother Megawati). Her husband, and dozens of relatives and neighbours, were all quite affected emotionally as we watched the outcome together. We were all in the same situation. If Megawati lost the vote for Vice-President, there was no telling what the rioting mobs might do.

This was a subject position far from the idealized position of neutral participant observer. However, it was a moment of deep rapport, problematic as that term is currently (see Marcus, 2001). Shared extreme experiences are psychologically bonding for those involved, as Geertz (1983) found so many years ago, and allow unique researcher insights. So, to return to my first question regarding this context of data collection; to what extent can fieldwork data collected in dangerous and chaotic settings be formed into orderly, coherent accounts of the phenomena? This article is the only attempt I have made to form the journal data into a navigable account of the phenomena as it actually happened. There is an article written around the theoretical notion of embodiment in which I include some of the empirical material (Nilan, 2001), but it deals with the manipulation of the young people in the rioting mob by political agitators, information which I did not piece together until later that week. So there are two answers to the question. The facts and explanations that allow a researcher to make sense of chaotic and dangerous research situations often are not available at the time, but have to be collected later. Second, the emotional experience of the researcher in chaotic and dangerous research situations is such a large part of the data that one cannot rely on it alone to create an orderly and coherent account of the phenomena.

If we turn to the second question of the ethical and methodological issues which emerge from the phenomenon of a researcher collecting data in dangerous and chaotic situations, then the duty of care of the researcher towards participants can be singled out as a most important ethical issue, although personal safety is also an issue. Clearly it is not appropriate, as I indicated above, to ask participants silly questions when there is a crisis going on. It does seem ethically appropriate to offer sympathy and consolation to informants and associates who have suffered in the crisis, such as my weeping neighbour, or her husband, whose consultancy firm had lost everything (few Indonesian businesses have any insurance), or my Muslim students, whom I telephoned

later to see if they were safe. The methodological issue is more complex. As I have shown by example, 'data' collected in chaotic and threatening circumstances not only lacks objectivity and neutrality, if these are indeed always desirable qualities, but often lacks factual content, not to mention coherence. The notes written by this researcher were not about the people I was supposed to be studying, but were mostly about me and how I was feeling. I concur with Probyn (1993) that reflexive writings are often self-centred and ethnographically sterile (p. 80). On their own, they serve the purpose of anchoring the researcher in a reflexive sense, while he/she is dealing with 'post-facto' explanatory data. Nevertheless 'crisis' data is packed with emotional immediacy, and this should dissuade any researcher from dismissing it out of hand (see Van Maanen, 1988). As Carspecken asks, 'how does an outsider gain an insider's view?' (1996: 17). In this instance, the shared experience allowed this researcher to sympathize more fully with the fears and anxieties of locals in the crisis, even though the two sets of responses could not be imagined as exactly congruent due to historical and cultural differences. Finally, it became clear to me after the riot that it was important I had witnessed these dangerous events myself from the same vulnerable position as my neighbours. Local people, and even some of the rioters themselves, were very prepared to talk to me openly about the riot. After all, I had been through it too, and they knew that. So, although I certainly experienced a sense of 'loss of control' at the time of greatest danger, this was an aspect of the situation that was more or less true of everyone who experienced the riot. The real research 'value' of the experience came afterwards, when I was able to achieve high levels of rapport with locals and obtain frank and detailed data about the events from all kinds of points of view.

The nightclub

The second example of trying to collect data in difficult and risky (rather than dangerous and chaotic) circumstances was the result of a decision on the part of the researcher, unlike the previous example. In the course of collecting background ethnographic data about groups of young people in Singaraja, I was told that some of them frequented a nightclub with a reputation for commercial sex and drugs (see *Bali Post*, 1999a; Fajans et al., 1995) in the tourist enclave about ten kilometres away. Since my research project (see above) implied the assembly of information about the available meaning/identity frames of the young people in the local area, I determined to go to the nightclub after midnight and observe. I was aware, generally speaking, of the potential dangers in fieldwork on illegal drug use (Lee, 1995: 39), but I did not anticipate any problems in Bali.

Although the neon sign outside was impressive, the California Club was in poor repair and did not improve during the time of my observations (I visited three times). There was a dance floor in the middle and a raised platform

where the band was warming up. To the left were two large pool rooms, dimly-lit and half-partitioned. To my surprise, drug dealing was going on quite openly in these spaces. Tables and chairs stood around the brightly-lit dance floor. A few tables were occupied by young (and some very young) prostitutes of both sexes, and 'guides' (see Lette, 1996) – who were not actually selling sex, but were looking for romance with a tourist. They were carefully dressed in western clothes, with styled hair and make-up. On both sides, at tables more dimly lit, were western tourists and backpackers, eyeing the colourful group near the dance floor. Everybody was waiting for the band to begin playing. I sat down at the bar. With little idea of how to get information, I started talking to one of the barmen about my purpose. Warmed by my use of the local language, he offered me some advice – pay one of the older 'guides' to act as bodyguard and informant. The place was dangerous and I might get robbed or worse. He knew more than I did in his acknowledgment of the dangers to personal security which arise for researchers in the drug subculture' (Lee, 1995: 40). While I was still thinking about his advice, a fight broke out among the drug dealers. At the same time I was approached by a woman offering in English to sell me *shabu-shabu* (crystal methylamphetamine). When I refused in Indonesian she glared and walked back to her friends, who all stared and muttered. Distinct qualms about my own personal safety started to register (Van Maanen, 1988: 86). I realized that it was going to be difficult, if not impossible in this potentially violent context to collect data if I was taken for a spy or a possible robbery target (Lee, 1995). It would be much more productive for data collection if I could 'pass', look like someone who had the usual reasons for being there. There was an older Australian man sitting at the bar. Earlier I had greeted him and he told me to 'f— off'. Even drunker than before, he was now calling out and trying to touch the very young *cowok jegeg* (pretty boys) who were flirting with a much older German woman. A glum-looking young Balinese man was sitting next to the older Australian. I leaned over and quietly asked him to explain what was going on (in Indonesian). He told me that he had an arrangement to go home eventually with the Australian but he didn't want to. He said that he much preferred romance with tourist women but that since he was no longer so young, and a heavy drug user, it was easier to find old men who wanted to pay him for sex. How old was he? Twenty. I explained my purpose in being there and offered him 20 dollars to walk around with me and explain things, a practice which is not uncommon in some kinds of fieldwork (see Lee, 1995: 41). He readily agreed. The older Australian was not pleased. He threatened to punch me but he was too drunk.

It was remarkable that as soon as I paired up with Wayan (not his real name), I felt much less conspicuous, and much more in control of the data collection possibilities. It was an uncomfortable thought that this sense of 'control' came primarily from my financial control over Wayan. However, the issue of passing was very relevant. There were quite a few older western

women paired up with young 'guides' (Jennaway, 1996, see also Bagus, 1997 and Lette, 1996), which seems to be an accepted pattern in Bali, so I did not stand out in the crowd. Wayan proved most helpful. As a local lad and long-time guide, he knew almost everyone and their local history. He helped me recognize the nightclub situation in a 'culturally typified' way (Carspecken, 1996: 19) which I could not do alone. For example, he confirmed that most of the young men and women offering sex were locals from poor backgrounds like himself, as were those selling drugs in the poolrooms. However, the group of very young *bencong* (transvestite men) (Oetomo, 1996) who had appeared earlier were from Denpasar, the capital of Bali. Wayan also assured me that the 'boss-boss' (pimps and drug distributors) who managed the local trade in sex and drugs were from Java or Madura. It may have been so, since most North Balinese are poor and would probably lack the resources to make the necessary capital investment. The California Club venue was reportedly run by a local man, but owned by a consortium of Denpasar businessmen.

Wayan was keen to show me as much as possible about the sex and drugs trade. He pointed out that the tourist 'action' was at the front near the band and the dance floor, while the local 'action' was going on out the back. Far in the right hand back corner was a flashing sign – DiscoHouse – above swinging doors. In the far left hand corner was a door which lead out to the toilets through an untidy courtyard garden. It had not been obvious to me before, but those areas were busy with the local 'action' as Wayan called it. It was at this point in the evening, when I moved from the tourist domain where I 'belonged', to the local domain backstage, as it were, that the data collection context became most difficult. While tourists are catered for, much of the sex trade in venues like the California Club involves local male clients. An article in the *Bali Post* in 1999 reported that most of the customers in a Buleleng (North Bali) *Seks Kafe* in the eastern part of Lovina were local 'civil servants or members of the military'. Young female sex workers were charging them 25,000 rupiah for a 'short time' (*Bali Post*, 1999b). Few tourists ever seemed to make their way through the gloom at the back of the California Club into DiscoHouse, which was the local scene. I noticed it was one-way traffic. Those who went in did not usually re-appear. Wayan explained; those who went into DiscoHouse continued out beyond the dance space into the courtyard outside, then went upstairs via a back staircase. We went through and stood at the bottom of the staircase. He pointed upstairs and said '*tempat tidur*' (beds), then started to ascend, urging me to follow. It was not a comfortable situation. I declined (keeping control?). As Lerum puts it, 'the study of sex work is a politically and emotionally tricky endeavour' (2001: 468). This was particularly so for me as a woman. I felt a strong sense of risk and distaste at the thought of being a sexual 'voyeur' (Kulick, 1995: 9). Yet afterwards I thought perhaps I should have gone up the staircase. The scientifically rigorous pursuit of ethnographic data probably demanded it.

We walked around to the area outside the toilets, talking about the local

drug trade. It appeared magic mushrooms, marijuana, ecstasy and shabu-shabu were readily obtainable. Heroin was also apparently available from the right source. Wayan discreetly pointed out some groups of young men, glimpsed only dimly in the bushes, who were injecting drugs, using shared needles. Did I want to meet them? No. I was embarrassed by both of Wayan's invitations. It did not seem respectful, sensitive to context, or indeed ethically sound, to go in and visually observe young people doing high-risk things they usually did in relative privacy. Besides, I did not want to draw too much attention to myself in such a high risk situation. Yet what important data might I be missing through such evasion? Moments like these convinced me that, not only did I find foreign culture fieldwork uncomfortable, but that I belonged less to the 'Indiana Jones' class of ethnographer, and more to the 'timid and retiring' class (Lee, 1995: vii). I fled to the women's toilets, only to find myself unwittingly listening in (see Humphreys, 1970) to two young local women arguing over money they had just obtained for a threesome with an Indonesian client. One wanted equal shares but the other maintained that she had done more in the encounter and deserved a higher proportion of the payment. The other flounced off with her lesser share. Just a little while later she left by the front door on the arm of a German backpacker. While this provided some data about the prevalence of multiple sexual partners, it was opportunistically obtained, in the sense that the girls probably assumed that, as a foreigner, I wouldn't be able to understand their conversation. It also worried me that these local girls were so young, and taking such high risks in sex work. I shied away, however, from getting involved. I was concerned that I would, as it were, blow my cover (Lee, 1995), and draw attention to myself, which would compromise my ability to freely observe.

Some time was then spent near the dance floor, observing the interaction between tourists and local young people. Once again, Wayan was able to confirm what I thought I was observing. Relations between young female 'guides' and tourist women are not mentioned much at all in accounts of tourist phenomena in the region, but it was happening in the California Club:

Two Indonesian girls came in together. Late. One was very pretty, with long curly hair and wore a dress. The other had very short hair and was frankly butch. The pretty one in the dress was dirty dancing centre-stage with a very butch Danish lesbian I had been talking to earlier. They were wrapped around each other. Her 'cowok' girlfriend seemed to be trying to chat up some older French women. (Fieldnotes, 23/10/99)

These girls were described to me by Wayan as 'cowok' (the boy) and 'cewek' (the girl) respectively. Blackwood (1995) concurs that these labels are commonly applied to butch-femme lesbian partners in Indonesia (see also Gayatri, 1993). Wayan confirmed that there was quite a lesbian 'scene' operating in the local tourist enclave, which survived on sex and romance with German and Dutch female tourists who 'knew' where to find it. Murray (1995) maintains that in Java open lesbianism is generally found only in

lower class butch-femme subculture. Furthermore, she notes that,

Lower class lesbian subcultures are . . . part of an urban milieu which includes elements such as crime and prostitution, and my observations are that a disproportionate number of lesbians are involved in the sex industry. (p. 23)

I include these references and quotes to indicate the relative accuracy of the data I collected in the California Club on this, and on other occasions, with the assistance of Wayan. There is no doubt that engaging him as a paid informant enabled the collection of significant data which I have written about elsewhere (Nilan, 2001), as well as endowing the data collection process with a satisfying sense of control. I mention this specifically because it is now appropriate to consider again the first of the questions which guide this article: to what extent can fieldwork data collected in difficult and risky settings be formed into orderly, coherent accounts of the phenomena?

First, in both the examples I have used in this article, the data I ultimately obtained was the most politically significant and volatile of that I gathered across the project as a whole. This is an important point as Lee (1995) points out. Often difficult and dangerous fieldwork is worth the experience because of the highly politically potent nature of the data, which is almost impossible to collect by any other means. Second, as I think I have demonstrated here, there are some difficult and risky fieldwork settings in which it is improbable that the researcher who is very much an outsider will be able to collect useful data. In the setting of the California Club, had I not passed as a client and engaged a paid informant, I would have been left conjecturing about almost everything. There are a lot of other data collected in the California Club, some of it relevant to the groups I was studying back in Singaraja, which I would never have obtained but for my paid informant. Because I did have access to an insider view during my observations in the venue, it has been relatively easy to form that data into orderly, coherent accounts. Had that not been the case, the data would have been far more disorderly, personally oriented and speculative in nature. However, it is notable that I did not mention the use of paid informants in my application to the ethics committee, and my practice in this regard was not pre-approved. Like many other qualitative researchers I was 'personally challenged' in trying to adhere to granted research protocols (Madjar and Higgins, 1996: 133).

This leads, obviously, to the second question in relation to this data: What ethical and methodological issues emerge from the phenomenon of a researcher collecting fieldwork data in difficult and risky situations? The first thing to note is that the collection of data in challenging participant observation situations demands considerable ingenuity on the part of the researcher (Lee, 1995). Ingenuity practised at the time can obviously lead to a range of ethical and methodological issues to be considered (and even suffered, see below) after the event. Another matter to settle here is whether the research questions justified the collection of data in such a venue using a paid inform-

ant. There is no doubt in my mind that they did, given the valuable data I was able to collect on the range of possible identity/meaning frames available to local young people, as well as the cultural constitution of health risks. However, the moral dilemma of having engaged a young male sex-worker as my paid informant remains, as does the question of whether it is ethical practice to merely observe young people engaged in criminal and high-risk behaviour without warning them in any way, or notifying anyone about it. Or, indeed, whether it is ethical to eavesdrop on other people's private conversations, without letting them know you can understand what they are saying. These are demanding ethical questions, but I argue that there would be few qualitative researchers who do not come up against similar dilemmas, since collecting ethnographic data never takes place in an orderly context in which all ethical issues can be foreseen, and guaranteed through specific protocols. Furthermore, as Lee advises, ethnographers studying drug scenes generally avoid drawing any official attention to local people or settings (1995: 47). Inciardi makes the point that a researcher's ability to intervene directly can be quite strictly limited (1993: 155). As described below, there were negative social consequences to my fieldwork at the California Club at a local level, and these would have been even more dire if I had chosen to draw any official attention to the high-risk behaviour of the young people involved.

I went back to the nightclub two weeks later and engaged Wayan again, then a week later as well. I do not consider that any damage was done by Wayan acting as a paid informant for me in the venue, and this is probably the major ethical consideration. Marcus (2001) acknowledges that the ethnographer routinely establishes these kinds of expedient fieldwork links as working partnerships for which no 'regulative ideal or modality' exists in documented fieldwork practice (p. 524). Unfortunately however, while Wayan's local reputation presumably remained intact, mine did not. Gossip went around Singaraja that I had a young 'boyfriend'. News travels very fast in Bali and a whole range of local people (both men and women) became suspicious of my motives in the research and were less than co-operative with me after that. I did explain myself most heartily, but I feel I was not believed by a lot of people. I keenly felt their disappointment at my apparent sexual misconduct (Kulick, 1995: 7), which was constituted within a complicated gendered discourse of the older academic western woman who, though foreign, had seemed to fulfil the ideal of the married 'modest woman' (Parameswaran, 2001: 82; see also Warren, 1988). Methodologically then, I solved one data collection problem, only to create another. Fortunately, this did not happen until just before I was due to leave for home. It is ironic that a distinctly non-rapport relationship compromised a range of carefully negotiated 'rapport' relationships. This example of rapport failure certainly bears out Coffey's (1999) claim that the 'self' of the ethnographer has an effect on every aspect of the research process (p. 6), especially the gendered aspect of the self (Kelsky, 2001; Sherif, 2001; Venkateswar 2001). It also demonstrates how the expediency of problem-

solving in the difficult and risky participant observation context can have unforeseen consequences. Reaching for greater control in one way may lead to an unexpected loss of it elsewhere. As Lerum (2001) observes, engaging fully 'in the moment' with one's subjects '*often ruins one's sense of control*' (p. 481, my emphasis).

Conclusion: the issue of writing accounts from data collected during difficult circumstances

In conclusion it must be stated again that, although composed of difficult and unsettling experiences, the ethnographic fieldwork described above provided immensely rich and valuable, as well as politically significant, sources of data. However, this potential could only be realized in the production of appropriate research accounts. As indicated earlier, there is very little difficulty in writing up accounts from data collected by formal methods within qualitative research. The data can easily be reduced to summary, and various analytical procedures, for example, descriptive statistics and content analysis can be carried out if needed. The subject position of writer, similarly to the subject position of the 'scientific' researcher, is constituted through the production of texts which speak with the voice of 'objectivity' (Lerum, 2001: 472), 'rationality' (Denzin, 1997: iv) and 'certainty' (Goodman, 1998: 63). However, as argued previously, data collected through ethnographic fieldwork is notoriously messy and chaotic. So, to what extent can data collected in dangerous and chaotic settings constitute orderly and coherent accounts of the phenomena which speak with a 'voice' which will be found acceptable within the academy?

The problem is that, if we put aside the accepted 'scientific' criteria for acceptable research accounts which conform to a quantitative research paradigm, then the criteria by which thesis examiners, journal editors and book publishers judge qualitative research accounts have to be taken into account. So, if it had not occurred before, then certainly at the stage of writing up data and publishing research accounts, the precept of researcher 'control' tends to blur across into the analysis and interpretation of ethnographic data.

Detachment and distancing from human informants secures a safe jurisdiction for academic work, and this protective boundary successfully facilitates the production of a specific brand of knowledge' (Lerum, 2001: 475; see also Willson, 1995: 255).

In a competitive publishing environment, fieldwork researchers who wish to see their work reviewed seriously need to put a satisfyingly neat presentation of apparently empirically valid 'facts' about local phenomena out into the public arena, no matter how poorly this may reflect the context of data collection (see Van Maanen, 1988). Any attempt to do otherwise implies the troubled power relations that underlie production of academic knowledge (Parameswaran, 2001: 93) and usually results in rejection. This dilemma is

routinely experienced by anthropologists:

The tidy, systematic and well-rounded texts written by anthropologists are more often than not the end-product of long periods in the field characterized by boredom, illness, personal privations, disappointments and frustration. (Eriksen, 1995: 15)

In my own successful publishing from the ethnographic component of the project so far, I have eliminated any reference to collecting data in difficult or chaotic circumstances, instead constituting the research account text primarily in terms of a particular theoretical 'take' on the data, which is expected within my discipline. Perhaps this can be reduced to a 'desire for mastery' over the data which is unlikely to be challenged (Alcoff, 1991: 22) by the academic review process. The texts certainly do not openly admit of any problems of representation (Lather, 1993: 674) relative to the data.

We might well then ask, what type of ethnographic subject is produced through data analysis, since it can be claimed that the very analytic texts we produce are themselves constitutive effects of discourse (see for example, Honan et al., 2000: 30). I would argue that the descriptive and analytic texts habitually written up from ethnographic fieldwork data constitute human research subjects which seem to be stable, orderly, rational, consistent characters in a coherent and compelling narrative. This is so because of the necessity to write within the acceptable discourses of academic prose and disciplinary genre. The human research subject 'constantly in process' (Davies, 1997: 274) – only existing in process, complexity and contradiction – is almost impossible to depict accurately (but see Honan et al., 2000). So what of the researcher/writer constituted as human subject in descriptive and analytic texts habitually written up from ethnographic fieldwork data? It might be just possible to depict research participants as constantly in process, complex and contradictory, but this is a luxury the writer/researcher cannot afford with regard to their own authorial voice in the text. Although as Goodman (1998) says, even though we know nothing 'for sure', we have to give the impression of 'certainty' (p. 63).

Turning to the directly practical matter of writing accounts, I suggest the strategy for the writer/researcher is not to identify the most emotionally intense moments in fieldwork research as the pre-eminent moments of data collection which must somehow find their way into the eventual textual accounts. It is obvious that such events stick in the memory long after more mundane experiences have faded, yet it is often impossible to collect really informative and useful data at such times. As I have tried to illustrate through the examples in this article, it is often data collected after the event or in quieter moments which turn out to be the most evocative, in the end. In the example of the riots, it was really everything I found out afterwards about the events which produced the most satisfying explanatory account of the phenomenon. In the example of the California Club, while the first visit was

emotionally challenging, subsequent visits with Wayan as informant yielded much more analytically rich data. This is the final argument for control as a significant component of the ethnographic researcher subject position. The researcher/writer needs to stay on top of the data, in the field and at the computer, in order to write respectful and well-informed accounts of people and their lives. In my discipline, like many others, there is a noted disdain for personal narratives in accounts (Kulick, 1995: 3). So while personally-oriented confessional pieces about difficult fieldwork (like this article) have their place, at their best ethnographic accounts permit the reader to experience some of the 'activity, creativity and human agency' (Willies, 1977: 3) of people and events in the field. Good ethnography creates meaning out of the fieldwork experience, in carefully crafted accounts which attempt to capture the richness and complexity of the original actors and events, while keeping within the accepted limits of reader comprehension and publishing criteria of the discipline.

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PAM NILAN is a Senior Lecturer in Sociology at the University of Newcastle in New South Wales, Australia. She is also currently Associate Director of the Centre for Asia-Pacific Social Transformation Studies at the University of Newcastle. Dr Nilan has been using qualitative research methods to study youth, gender and education since 1989. Her most recent projects involve ethnographic research with young people in the Asia-Pacific region on popular culture, identity and rapid social change.
Address: School of Social Sciences, University of Newcastle, NSW 2308, Australia.
[email: pamela.nilan@newcastle.edu.au]