

## TES Special Issue

# Bias, Intimacy and Power in Qualitative Fieldwork Strategies

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**Abstract:** *Efforts to understand the inter-relationship between environmental trends and local livelihoods has led to the adoption of qualitative fieldwork approaches in many interdisciplinary environmental studies in developing countries. This paper discusses how bias, presuppositions and power relations affect and form part of such approaches, with a particular emphasis on data generation through qualitative household interviews. Specifically, the paper critically reviews the notion that we may shed our roles as pre-disposed outsiders by entering the field with an “open mind”, and by developing close relationships with community members. It is argued that these approaches, while effective in many ways, contain and are constrained by a number of biases, and that they therefore need to be complemented by explicit reflexivity. Such reflexivity includes greater attention to the power relations and epistemological frames of reference within which the researcher is embedded, as well as dedicated attempts to engage other disciplines and actors in highlighting unreflected biases and worldviews.*

**Keywords:** Fieldwork, transdisciplinary studies, epistemology, qualitative methods, bias, power, developing countries

### 1. Two Assertions

Recent years have seen an increasing emphasis on the role of local communities and their livelihood patterns in relation to environmental planning and natural resource management in developing countries. Worldwide, this has prompted a substantial number of interdisciplinary studies that seek to understand the linkages between environmental trends and the social, economic and cultural conditions and processes in local communities.

At the same time, the spread of collaborative and participatory approaches to environmental management has prompted some academics to pursue increased interaction with local communities in the knowledge production process. The objective of such approaches range from simply wishing to draw on local insights and knowledge systems as a

means of validating and qualifying a given research effort, to more change-oriented approaches such as action research and the various approaches under the “Participatory Learning and Action” label.

In all of these approaches, however, one of the main challenges is the difficult question of how we as field researchers or academic change agents cope with the role as outsiders in the local (community) context. How do we access the “foreign” empirical world that we are trying to study and make explicit, how do we secure the best possible insight despite our status as outsiders?

Addressing such questions is highly relevant within the field of environmental studies. As has been widely pointed out in e.g. Environmental Sociology and Political Ecology, the environment may well

have a material quality determined by (more or less) objectively identifiable physical laws, but it is also a highly social field, subject to differing perceptions, discourses and power relations (see e.g. Buttel 1987, Goldman & Schurman 2000, Peet & Watts 1996, Stott & Sullivan 2000).

This is, of course, not a problem particular to environmentally related studies. Ethnography, for instance, has long attempted to address the issue (e.g. Fettermann 1998), while in broader terms there is an increasing focus on the issue as part of the increasing popularity of qualitative approaches within the social sciences (e.g. Denzin & Lincoln 1998). However, let me be a bit more specific and refer to a recent course I attended. The course addressed methodological aspects of fieldwork in Asia, and I took special note of two assertions made during the lectures and subsequent discussions:

The *first* assertion was that, in order to avoid ethnocentric biases in our fieldwork we need to retain an “open mind” when entering the field, thereby allowing the local context to influence our thinking. The *second* assertion was that, by developing a close and personal relationship with the agents being studied, we are able to gain a clear insight into the sensitive, unspoken issues of community life (the “best guarded secrets” as the instructor called them).

These are plausible and sympathetic notions that form core elements in ethnographic research, and would probably be widely accepted by many field researchers in general. Indeed, they are ideals to which I have striven myself during current fieldwork in Southern Thailand, and during past fieldwork elsewhere.

The question, however, is whether and to what extent these two principles really do allow for such easy access to the “closely guarded secrets” as it is sometimes claimed? In the following, I shall seek to problematize and seek out the limits of these assertions, arguing that it is highly problematic to take them for granted without making explicit their underlying conditions and inherent biases.

## 2. The Field as a Construction

The notion of the “open mind” takes a variety of forms, but one of the purest can be found in empiri-

cist approaches such as Grounded Theory. Here, the idea of *tabula rasa* suggests that we can and should commence fieldwork with a “clean slate”, free of prior experience and theoretically informed constructions of the world (Glaser & Strauss 1968).

Such a notion, however, ignores the degree to which we are socialized and institutionalised into given ways of structuring and labelling the empirical context we explore. Consider, for instance, the field of environment and development, within which my own research takes place: We talk of *sustainable* resource management practices and *unsustainable* ones, thereby implicitly establishing categories of *people* who live sustainably or unsustainably. And we inscribe ourselves into storylines such as that of *biodiversity conservation*, which come to form the basis of entire research agendas and development interventions, and thereby shape our basic ontological frames of reference (Escobar 1998, Leach & Mearns 1996).

This *framing* of the world through our pre-conceived ontologies often takes place through dichotomies: When addressing environment and development problems, we frequently approach the world as divided into the poor and the wealthy, the rural and the urban, the community and the state, the traditional and modern, the natural and the degraded. Although we may attempt to overcome some such dualisms, they are powerful notions that to a large extent provide our only means of negotiating the world.

We may think, of course, that this implicit construction of the empirical world can be avoided simply by taking one step back and making this process of labelling – and the power behind it – a research object in itself. In my own current fieldwork for instance, I have taken a largely sociological perspective on a participatory coastal zone management project, seeing it as a social process loaded with power and interest. However, while such a power perspective may sharpen attention to some of the processes of labelling taking place, it immediately establishes others:

In seeking to identify community members to interview, I divided them first into “participants” and “non-participants”, subsequently developing and applying additional categories such as “fishermen” and “non-fishermen” and “Buddhist” and “Muslim” households. Not to mention the application

of the terms “community” and “household” in the first place. This structuring of my fieldwork area rested on a more deeply embedded perception of communities as essentially heterogeneous, stratified entities, steeped in struggles over access to and control over natural resources. This underlying conflict perspective constitutes an implicit yet distinct way of thinking, into which I have been socialized through many years of interaction with teachers and peers at University.

We cannot, then, escape the pre-disposed construction of the field that we undertake prior to and during fieldwork. This is also recognized in (newer) Ethnography, another approach which has long advocated the “Open Mind”. As Fetterman (1998) puts it, the ethnographer “enters the field with an open mind, not an empty head” (see also Massey 1998). What ethnography and most other approaches rarely acknowledge, however, are the underlying ethnocentric conditions of the fieldwork process *as such*. I turn now to this more fundamental aspect of fieldwork, focusing particularly on the Asian context.

### 3. The Western Bias

Shamsul (2003a, 2001) describes how the present patterns of knowledge production in most of Asia today reflect a mode of thought originating in the West, introduced during colonial times as a means of rationalizing, naturalizing and thereby legitimising the science-state-capital complex that provided the basis for colonial resource extraction and domination (see also Sangwan 1998).

In this way, “Western science” came to structure and to some extent even re-create Asian societies, through processes such as classification (e.g. naming ethnic groups) and spatial categorization (e.g. through mapping). Even *time* was laid out and defined through a Western epistemology, establishing for instance “traditional” resource management as opposed to “modern” resource management, thus contributing to the “Invention of Tradition” discussed elsewhere (Hobsbawm & Ranger 1992). As Shamsul has pointed out, such a notion is applicable also in Thailand, which despite its lack of a distinct colonial history has *de facto* appropriated the same Western modes of knowledge production as the rest of Southeast Asia (Shamsul 2003b).

It is, of course, possible to dismiss Shamsul’s point as part of a wide-spread “blame-colonialism” discourse that seeks the roots of all evil in this time, but such an argument faces problems in the light of Shamsul’s further argument that the reproduction of Western knowledge production systems *continues* in Asia today through the national Universities themselves, and through, for instance, the Banks and the Aid complex (Escobar 1995 makes the same point on a more general scale).

This has important implications for the argument made here: Firstly, it suggests that it is not only I, as a foreign researcher, who may have problems encountering the field with an “open mind”: My Asian colleagues may well be part of the same presuppositions and frames of reference as myself. Secondly, and more fundamentally, it poses considerable constraints on just how “open-minded” we can hope to get in the first place: If the field is *already* structured by Western rationality and labelling when we arrive, how are we to even *see* the implicit categorizations we make, and how are we to avoid reproducing them?

Consider, for instance, anthropology. There is no doubt that anthropology (using, of course, ethnographic methods) has helped to greatly enhance our understanding of the way significant features and concepts are defined locally (including, for instance, crucial concepts such as “nature” and “the environment”). And yet anthropology itself is, if anything, a primarily Western science: Here is a science that, perhaps more than any other, imbues the “local” and the “traditional” (concepts with distinctly Western overtones) with *meanings* and *categories*. Is such a process not in fact just a more subtle version of the traditional Western labelling and categorization that Shamsul refers to?

I hasten to add that I am myself a believer in ethnographic methods. I am by no means out to deride any particular approach, but to point out the substantial barriers that confront the ideal of the “open mind”. In the following I will address one last such constraint, which may well be the most fundamental of all.

#### 4. The Academic Bias

Like much other research on the relationship between the environment and a given group of actors, part of my own fieldwork is focused on seeking to understand people's motives and strategies. Here, of course, the "open mind" is more important than ever. Most people will probably agree that understanding other humans fully is impossible, and that different cultural backgrounds imposes further constraints on this. However, the French sociologist Pierre Bourdieu adds a further dimension to this by drawing attention to the particular constraints inherent in the academic point of view.

Space does not allow me to elaborate here on Bourdieu's conceptual framework, but for the purposes of this discussion, his basic claim is that we as academics apply a distinct view of the world that is always different from that of the agents under study (Bourdieu & Wacquant 1989, 1992). Like any other social space, says Bourdieu, the Academic Field has its own particular schemes of perception, its own associated values and norms. Through training and systems of reward we are socialized into these schemes of perception, which become a deeply ingrained part of us, whatever our specific position in the scientific debates (Bourdieu 1984). We come, in other words, to possess a particular *academic bias*, based, for example, on features such as abstract thinking, theorizing, conceptualisation, analytical categorization, and scientific "rigour".

These features influence both our construction of the empirical field, and our understanding of what takes place within it. Through interpretation and analysis we become cognitively *remote* from the practical world we are studying. This inevitably distances us from the practical everyday worlds (with their own associated values and schemes of perception) of the agents that we seek to understand. Hence, the very same values and systems of perception that define "good science" also come to provide a major constraint in undertaking it. This suggests that as *academics* we can never simply "learn another culture like we learn a language" as one participant claimed at the course referred to above. Only if we were to shed our academic agendas fully and become involved in the field as practical individuals might we hope to do so – but then we are by definition no longer researchers.

It is important here to point out that Bourdieu's argument does not in any way imply that the academic field is somehow superior to other social fields – it is just *different*. Nor is there an assertion here that the "open mind" should not be striven for – only that our academic point of view constrains it. Indeed, Bourdieu suggests that when seeking to understand the social practices of others, we may often gain important insights by referring more to our *own* non-academic experiences (the *other* fields that we inhabit) than the academic ones (Bourdieu & Wacquant 1992).

In extension of this, let me emphasize that the point of the above discussion has not been to suggest that we could or should do away with the notion of the "open mind" altogether. Rather, the argument is that we need to be explicitly aware of its limitations, including the larger processes and structures into which we are inscribed as field researchers.

#### 5. Intimacy and Power

I turn now to the second assertion made at the PhD course referred to above: That the best overall strategy for securing in-depth, qualitative knowledge about local societies is to develop good relationships with the agents under study. In order to elaborate on the logic behind this notion, let me quote at some length from the reading material provided for the course (Lindberg 2003, p.1):

*"The main tactic a fieldworker has to develop is to play the game as an outsider making friendly visits, learning about the local ways and thoughts without criticising these. [...] Showing respect, socializing, developing friendship and keeping it, is not just a tactic, but it is also necessary in order to get accurate information, which most often comes out of relaxed casual conversations, off the record, so to say. The hospitality, friendliness and natural socialising ability of South Asians means that the fieldworker needs to play on these strings to create rapport."*

Although rarely stated in such (jarringly) explicit terms, this quote does capture much of the inherent logic in qualitative field research within the social sciences today: In this respect, the Norwegian psychologist Steinar Kvale has begun to question what it actually is that we *do* when we pursue qualitative information from individuals (Kvale 2003). He arrives at the conclusion that much qualitative inter-

viewing works through a postulated *intimacy*, which legitimises our quest for data in the field.

Within ethnography, this strategy of intimacy (although never referred to as such) has long formed a core means of accessing information, crystallized in the anthropological principle that fieldwork should be initiated with an extended period of time allocated solely to becoming accepted and known within the community under study. In recent decades, the strategy of intimacy has expanded to become widely embedded in qualitative fieldwork more broadly, and furthermore forms a key element in many participatory data collection techniques related to community-based natural resource management in developing countries (e.g. WRI 1990, AFN 2002, Rambaldi & Manila 2003. See Kothari 2001 for a discussion of hidden domination in participatory field approaches).

The strategy of intimacy has also been evident in my own past and current fieldwork: Much effort is spent on developing a good relationship with the communities where I work, and on creating a relaxed context before and during the interviews themselves. I try also to disassociate myself from any threatening perceptions that the respondents have of me, seeking a friendly and good-humoured dialogue. Indeed, *dialogue* is a key word: The implicit intention is always to establish a sense of open dialogue between two good-natured beings.

What actually takes place, however, is essentially an extraction of intimate details about the person or persons under study, designed first and foremost to generate “good” information for my research purposes. We may, of course, seek to apply two-way information-flows or apply participatory methodologies. Yet even within such approaches, the researcher ultimately remains in charge, and ultimately pursues a given (usually hidden) research agenda, however veiled this process may be (Kothari 2001, Christoplos 1995). Hence we are reminded again of Shamsul’s linking of Western knowledge production to colonial exploitation and labelling, although here of course the labelling is more sophisticated, and the exploitation is not about material resources: it is about knowledge – the knowledge of people’s lives, no less.

Behind the strategy of intimacy, says Kvale, lies a subtle and unspoken relationship of power: It is only

through power that we are able to pursue intimacy as a data collection strategy. This was particularly explicit in my own recent fieldwork in Southern Thailand: The project I studied is funded by the Danish Government, through the very same agency that funds my PhD research, and for whom I have worked as a consultant on several occasions. To this was added my status as an Academic, a position that holds not inconsiderable respect in Thai culture. Although I actively attempted to disassociate myself with these roles during fieldwork in the communities, they *de facto* constituted a major element in legitimising my right to be there in the first place - to access certain information, to ask questions about people’s lives, to pursue intimacy.

## 6. The Pitfalls of Intimacy

The role of intimacy and power in qualitative fieldwork raises significant moral questions, of course, but also – more cynically - illuminates the analytical pitfalls involved.

Firstly, we risk becoming unknowingly trapped within our limited world of intimate relations: Our view becomes limited to those agents with which we are able (or willing) to become intimate. Secondly, and more seriously, the strategy of intimacy rests on core assumptions that are dangerous to take for granted: As Atkinson & Silverman (1997) have pointed out, qualitative interviews have come to form a keystone in what they term the “interview society” – a society resting on the assumption that the agent’s *themselves* are the best sources of data. This is often taken to imply that such data is somehow “deeper” and more “detailed” than other types of data. Depending on the situation, this *may* be so, but it may also include substantial distortions, and is certainly never more “real” than other types of data.

A third potential pitfall in the strategy of intimacy is the ease with which we may come to believe that we *control* the data generation process: The combination of intimacy and power is alluring, and may lead to a false sense of having gained the complete confidence of people, and of being able to “see through” their attempts at manipulating data.

A related aspect of this is the tendency among some to suggest that techniques such as triangulation (the checking of data or methods against other data or

methods) can serve as some sort of technical fix to data distortion (Fetterman 1998). Such methods may certainly help us along the way, but unreflected faith in them is dangerous – partly because they are not always as compatible with qualitative methods as we like to think (Massey 1999, Richardson 1994), and partly because the field researcher rarely finds himself in the objective position that a fully unbiased triangulation would require.

To elaborate on this, we need to appreciate another side of the power-intimacy relationship, not mentioned by Kvale: Through intimacy, we automatically become enrolled in local power relations. There are two aspects of this: For one thing, we need to appreciate that we as field researchers inevitably form part of the power plays related to the issues we study. We do so through our overall role as knowledge producers, and – in some instances at least - as social agents ourselves. For instance, by studying the activities of a participatory environmental project, I was developing data that could potentially be used by others (e.g. the donor agency) to pass judgement on the project and the actors within it. Likewise, it is quite possible – even likely - that my presence as an observer at community meetings affected the meetings to some extent (i.e. we are *always* “participant observers”).

However, exactly because we constitute players within the field under study, we may also become the subject of strategic manipulation by the local agents themselves. Although we may possess the power to pursue intimacy, this does not mean that the agents we study have no means of “manipulating back”. In my own case, I was clearly considered a strategic asset by some of the agents I interacted with, who saw me either as a potential instrument for securing further funding to the project, or as an asset in the power plays surrounding the community project activities.

For instance, when interviewing community members about an ongoing conflict over fishing regulations in the case area, respondents would sometimes appeal to one of two rationales: Those who were in favour of the fishing regulations would argue – to me or others present – that I, *as a Western academic*, would no doubt understand the importance of environmental conservation and the regulations. By contrast, those who were against the fishing regulations

sometimes argued that surely I, *as someone interested in community-based approaches*, would understand the primacy of poverty alleviation and the hardship imposed by the fishing regulations.

We are, then, always enrolled in the field in one way or another, and this in itself makes it highly problematic to assume that the strategy of intimacy (or techniques such as triangulation) allows us to somehow pass directly by manipulation and get at the “best guarded secrets”, as was claimed by one lecturer at the methodological course referred to earlier.

## 7. The Need for Reflexivity

I have in the above outlined a range of potential biases and problems associated with the notions of the “open mind” and intimacy as strategies for exploring the local as an outsider. I have done so from a mainly qualitative, social science perspective, with a certain emphasis on ethnographically inspired methods. Clearly, there are elements herein that are specific to such approaches, and yet I would claim that some of the issues touched upon are relevant beyond the typical, relatively long-term ethnographic study.

Hence, from a more overall perspective we may see the spreading of participatory approaches to project planning and Social and Environmental Impact Assessments as expressions of similar notions. Here, the attempted intimacy may take place at a larger scale (in focus group interviews, public hearings etc.), but much of the logic is the same: The allusions to an unbiased, open-minded planning approach and the pursuit of intimacy between the external and “the local” (Nicro 2000). In this process, we as academics often play a significant role as knowledge producers and masters of the instruments (the methods) of intimacy - as seen, for example, in the widespread use of academics as consultants and facilitators within such processes in Thailand.

Again I should emphasize that my aim here is not to suggest that open-mindedness and intimacy is in any way a “bad” approach as such. What I do object to is the assumption that these approaches will somehow do away with bias, power and manipulation in the data generation process. These features are always there in some form or other, irrespective of the approach.

What is needed, then, is to couple these approaches with an ongoing *reflection* over their limitations. The actual means of ensuring such reflection has been approached differently by different schools of thought. Attempts within anthropology have included careful registration of the field researcher's daily interactions and emotions during fieldwork. Such efforts at self-understanding may be important enough for the researcher herself, but risks a situation where narcissistic self-revelation comes to prevail over the actual research process (what some have dubbed the "diary disease"). Instead, I feel the reflexive process would benefit from two key efforts, namely (1) attention to the role and implications of the power relations and associated world views and epistemologies of which the researcher forms part at home and in the field, and (2) dedicated efforts to engage other actor's perceptions of the researchers' biases and interpretations. In the following I will elaborate briefly on these, drawing mainly on my own experience.

## 8. Making Power and Epistemology Explicit

Dealing first with the issue of power, Bourdieu (2000) has emphasized the need to reflect on the implications of the underlying power structures into which we are inscribed as academics. By practising such "Participant Objectivation" (as Bourdieu calls it), we come to see better our own predispositions and biases, and their impact on our work. Above, I have discussed some of the results of my own reflexive process, including consideration of the way power relations have played out in my own fieldwork through the "strategy of intimacy". Apart from this, additional reflections on the hierarchies and codes of conduct that make up my research environment at home has led me to see several limitations and biases in the way I have framed the empirical reality in my fieldwork.

For instance, as a relatively young academic at a traditionally leftist University I am subject to a range of subtle pressures and unwritten rules with which I am expected to comply if I am to succeed - including a deeply ingrained emphasis on critical inquiry. Reflecting on this, I realized how I had subconsciously read a distinct *conflict perspective* into the way I asked questions and interpreted responses during my fieldwork - without ever really considering alternative perspectives. This led me to question

whether the community members I interviewed actually thought and responded from a similar angle (i.e. did they see "conflict" where I saw it?). These reflections added a valuable new dimension to my next round of fieldwork, and to the data interpretation process in general.

The power relations in which we are embedded thus have a distinct impact on our worldviews and epistemological frames of reference. While this has been preached in constructivist approaches to social science for some time, it is surprisingly rarely done in practice - and is still more or less unheard of outside of the social sciences and in practically oriented approaches such as action research and Participatory Learning and Action. In extension of this, Alvesson & Sköldbberg (1999) argue for a critical reflection over the results that *other* epistemologies and approaches might have brought us in terms of findings and conclusions. For instance, by reflecting on the possible approach and conclusions that a traditional anthropological study would have produced if it had it been applied to my case, I was reminded of several limitations in my own findings vis-à-vis the lifeworlds of the fishing communities under study.

## 9. Capitalizing on "Otherness"

While reflections on the power-relations and epistemologies into which we are inscribed can take us a long way, they also have their limitations. We need, therefore, to engage with others who can help provide alternative perspectives. Exactly who this is will obviously depend on the setting and context, but for my own part I have found it beneficial to engage two main groups of actors:

Firstly, I have found it rewarding to engage in cross-disciplinary dialogue with other academics as a means of highlighting my own un-acknowledged worldviews. For a sociologist such as myself, interacting with engineers and ecologists can be quite an eye-opener in this respect. Likewise, close contact with practitioners can, I find, highlight important unreflected aspects of one's academic bias. Indeed, whenever possible I find it highly inspiring to act as a practitioner myself, providing as it does an opportunity to approach the empirical matter with a different mindset, and thereby put my academic worldview into perspective. The fact that such prac-

tical work is often ignored and sometimes even discredited in the academic field (and vice versa) illustrates just how ingrained the biases of professional fields actually are.

A further group of actors who can help us in our reflexive process are the actors under study themselves. In seeking alternatives to the “strategy of intimacy”, Kvale (2003) suggests a more agonistic approach to data collection that openly acknowledges *dissensus* (as opposed to the underlying aura of “consensus-seeking” in more conventional approaches). In its more extreme form, such an agonistic approach would actively challenge and provoke respondents by explicitly highlighting and drawing out power relations, taboos and differences of opinion. While this may be too radical for some settings and some parts of fieldwork (e.g. the first phases), there is no reason why the basic notion cannot be modified to fit a given situation.

Hence during the final stages of my own recent fieldwork, I decided to break with the strategy of intimacy and began instead to present to interviewees my own analysis of the unspoken power relations and conflicts within natural resource management in the case area. The response was highly rewarding: Exactly because I had presented this as *my* views, and exactly because I had made explicit the fact that I *was* an outsider, interviewees saw me more clearly and were able to respond more directly and comment on issues otherwise considered off-limits. This not only provided me with valuable new data, but also gave inspiration for my own reflexive process. I began, for instance, to realize that I had placed undue emphasis on certain aspects of the case I was studying, while underestimating others.

There is, of course, a fine balance that needs to be struck in such an approach, and due attention must be paid to the moral and ethical obligations of field research. However, it is exactly here that true intimacy and open-mindedness comes into play. By acknowledging clearly that we as field researchers come loaded with pre-suppositions and a distinct agenda of collecting data for our research project, we also provide the basis for a more open and morally legitimate fieldwork process. By making clear who we are, we open for the inclusion of inputs from others on the biases and misconceptions they see in our work.

In such a process there is no hiding of the fact that we are outsiders: Indeed, it is exactly this “otherness” that can help us see our biases: Only by drawing in those who stand at some distance to us, we can begin to see ourselves fully.

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