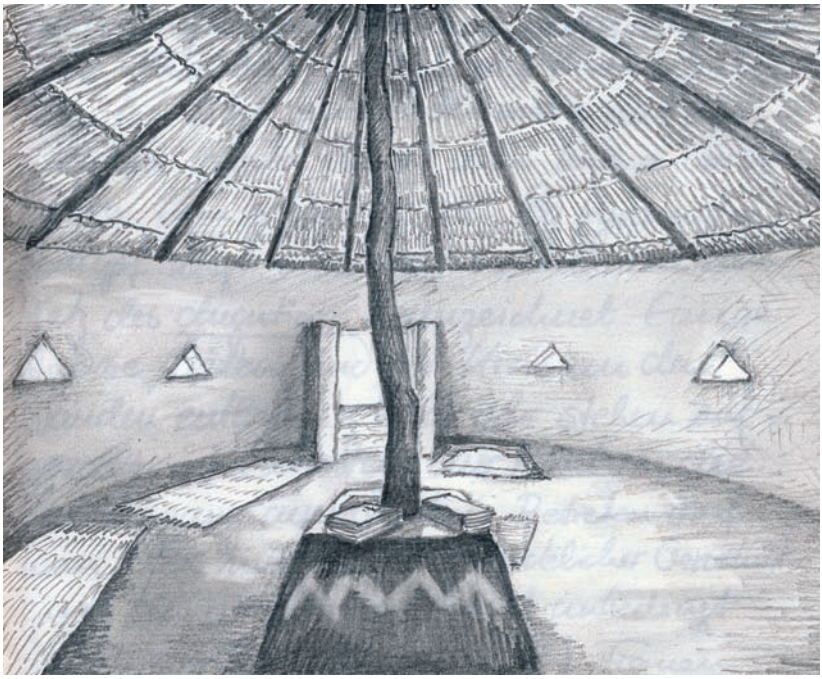
JJ Bachofen Lecture

Basic Questions of Anthropology

No 3

# Anthropologists Writing down and Writing up: through Fieldwork to Publication

Professor Judith Okely



# Anthropologists Writing down and Writing up: through field-work to publication

## By Professor Judith Okely

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# Anthropology as Social Science

There are continuing controversies as to what is understood as ethnographic fieldwork. Despite more than a century of anthropologists doing what Malinowski called pitching one's tent in the village, others have revised and reinterpreted research in what is regrettably scientistic ways. The physicist Professor Stephen Rose regretted how many social scientists had ‹Physics Envy› (1997: 8). This ideal of imagined abstraction defies the power of disciplines such as social anthropology. Only recently, a commentator reverentially described the physicist Wolfgang Pauli as ‹not getting his hands dirty› (Trefil 2003: 313) formulating the quantum mechanics principle. By supreme contrast, anthropologists have to get their hands dirty. That is part of the fun and what is also a scientific route to knowledge about the full range of humankind. We get our hands, indeed our bodies, dirty through participant observation. Then our fingers are smudged when writing down that experience.

Sociology, whose Chicago specialists invented the term ‹Participant Observation›, has sometimes drifted into quantitative priorities, where questionnaires are privileged over total engagement. Note-taking is replaced by ticking boxes for prearranged questions. The celebrated JJ Bachofen may not have engaged in ethnographic fieldwork in the 19th century, but he travelled far from any laboratory to varied countries, driven by curiosity for both differences and continuities in humanity's past. Clues and signs were to be disentangled from ancient objects, as well as literature: the published word conserves centuries' old texts. This classicist lived with the published voices of the dead and their artefacts, especially tombs as revered traces of what are called ancient, indeed awesome civilisations, where only the dominant elite were literate (1967).



## Fieldwork

By the early 20th century, social anthropology had moved from Armchair to Verandah and Tent. Frazer's *The Golden Bough* (1890) was constructed from others' written examples around the globe. His sources were missionaries, travellers, traders and colonial officials who had face-to-face encounters with different peoples. Frazer built on the notes of others as witnesses; rarely researchers.

Social anthropology has long since moved to direct contact, shared living and participant observation. The fieldworker becomes the multi-skilled researcher, recorder and final author:

very different from desk-bound ‹Principal Investigators› analysing others' data, without encountering the research subjects, made objects.

> In Ethnography, the writer is his own chronicler and the historian at the same time, while his sources are no doubt easily accessible, but also supremely elusive and complex; they are not embodied in fixed, material documents, but in the behaviour and in the memory of living men. (Malinowski 1922: 33)

Here I celebrate those who have embraced the multi-faceted role from fieldworker to author. I interviewed 20 social anthropologists with research around the globe in the late 20th century and after (Okely 2012). A number of cultures then studied, were still primarily non-literate. The anthropologist is necessarily literate.

In the 1980s, ensuing anthropology publications were explored for textual strategies (Clifford and Marcus 1986). Only later were fieldnotes problematised by Sanjek and others (1990). The Emerson, Fretz and Shaw (1995) publication was explicitly for student guidance, almost instruction. Overhomogenisation of the research process risks burying individual details in footnotes. Instead, this lecture offers specific examples linked to the grounded contexts of some of the fieldworkers I have analysed in *Anthropological Practice* (Okely 2012). Their hitherto unpublished testimonies on fieldnotes are integral to their published explorations of fieldwork practice. Additional examples highlight recent fieldwork by younger generations, including a Geneva university doctoral candidate I recently examined. Nevertheless, the majority of the cited anthropologists did their major fieldwork before any institutionalised methods courses in most anthropology departments. Their direct, detailed testimony offers insights into innovative strategies devised in unique, often unpredicted circumstances.

The context and process of writing fieldnotes are intriguingly varied. Details of the anthropologists' descriptions are elaborated in this publication of the delivered lecture. As with the research process, already explored (Okely 2012), there are both contrasts and commonalities. The anthropologists with whom I had dialogues were of some 16 nationalities, including persons of Asian, African, European, South and North American descent, with research in Africa, Asia, the Americas, and Europe (Okley 2015). The majority of the interviewees completed postgraduate studies in the UK. Others had studied at universities in Poland, Sweden, the Netherlands, Switzerland, Mexico, the USA and India. Predictably, many had done fieldwork far from previous experience. Others chose the seemingly familiar which always includes strangers.

Since the majority of those interviewed had first embarked on doctoral fieldwork before the 1990s, they had never received ‹training› on writing down or up. Johnny Parry, before fieldwork in India in the late

1960s, was merely shown some of Malinowski's fieldnotes by his supervisor, Edmund Leach, and advised to separate the personal in diary form.[1] Malinowski's dense handwritten style, with 'jumbled topics' all on one page, using several languages, including Kiriwinan, is reproduced by Young (1998: 143–4).

Margaret Kenna, with fieldwork on Anafi Island in Greece (2001), recalled:

> Doing the postgrad year at the LSE, we were shown a page from Raymond Firth's notebooks: very neat handwriting, native terms underlined, etc. But no ‹training› about how to write field notes that I remember … I typed up all the field notes, sent monthly reports to Paul Stirling. Then sorted them through into themed chapters or chronological accounts.

The *only* advice about fieldwork methods given in the 1960s to Suzette Heald, embarking on fieldwork in Uganda, by Darryl Forde, at University College London, was: «Take a pocket size notebook and ensure a sharpened pencil». Similarly, before fieldwork among English Gypsies, I received no guidance during my postgraduate Cambridge anthropology course.

## Writing Notes as Risk Taking

Here I expand on the preliminary context of my field research among Gypsies in England. Paradoxically, just 40 miles from Oxford, in the early 1970s, the act of writing fieldnotes proved more problematic than for the majority of my contemporaries and later anthropologists, with fieldwork around the globe. After my appointment at the independent Centre for Environmental Studies (CES), I had to convince my manager, Barbara Adams, trained in social administration, of the value of qualitative research, and ethnographic fieldwork. She, a civil servant, was seconded from the Ministry of Housing and Local Government where the priority was quantitative data from questionnaires. Having directed a Census of Gypsies in England and Wales (MHLG 1967), primarily conducted by the police, Adams envisioned my administering, throughout England and Wales, a multi-page questionnaire for this mainly non-literate people.[2] Seemingly, my anthropology qualification was merely interpreted as relevant for an imagined exotica; anthropology's unique methods not addressed.

News of this outsider female's presence on a Gypsy site rapidly cir-

1 In fact, Leach, a student of Malinowski, expressed anger at the publication of Malinowski's diary. As postgraduate, I celebrated its insights (Okely 1975).

2 Access to the archives of the census in my field locality revealed how the police and officials deliberately undercounted the Gypsy population. The pre-set questions showed the limitations of such procedures.

culated among non-Gypsy (*gorgio*) academic ‹experts› or ‹activists›: a scattered network, mainly in London. Several were linguists tracing early Sanskrit Indian links in varied forms of the Romany language, decontextualised from contemporary culture. ‹Pamela›, a local social worker, warned me that a ‹Dr K›, ill acquainted with social science, let alone fieldwork, kept asking what this ‹Oxford woman› looked like and her whereabouts. He intended to visit my Gypsy site and inform the coresidents that Okely was an ‹Enemy Government Spy›. Fearing the consequences, I avoided all activist gatherings. Dr K's misogynist ignorance could have been life threatening.

Such malicious intent was sufficient for the anthropologist never to risk any misunderstanding through public note-taking. Moreover, I had already experienced fieldwork in Western Ireland where public note-taking, among a literate community, was seen as intrusive (Okely 2009). Choosing to adopt a visibly non-interrogatory role, this anthropologist simultaneously embraced the ethical obligation to ensure the Gypsies' anonymity and never knowingly betray this persecuted minority.[3]

The hazards were especially melodramatic as, within days of my arrival, the Gypsies spontaneously offloaded details of a dramatic internal feud to this outsider, treated as empathetic ‹therapist›. The feud was triggered by a deserted Gypsy husband reporting his wife's new partner to the police for traffic offences, bringing unexpected imprisonment to his Gypsy rival.[4] This intra-ethnic treachery ended in murder. Nothing of this was published for decades (Okely 2005).[5] Similar dangers might have awaited the anthropologist had Dr K's strategy succeeded in defaming this female intruder on ‹his› research territory.

Ironically, in *direct* contrast to the linguist's life-threatening designation of the anthropologist as ‹enemy spy›, the government had tried to block the Gypsy research at the CES. Adams, worried that the legislation, triggered by her Gypsy Census report (MHLG 1967), had sidelined the Gypsies' perspectives, despite media representations of the 1968 legislation as entirely pro Gypsy. Knowing the government had little sympathy for nomads, Adams feigned plans to conduct uncontroversial housing research, and thus obtained ministeri-



3 After I mentioned this in a 2014 lecture, a retired judge expressed astonishment at the accused's imprisonment for minor offences. I explained this was unsurprising if the accused was a Gypsy.

4 In my subsequent monograph (1983), not only are names changed, but individuals are composites, using direct quotes. The location of sites is never revealed. Shockingly, Dr K. later named the exact location on a policy website.

5 Given the continuing rival academic threats from ‹fellow› specialists on Gypsies, linguists or one sociologist, I managed to ban my doctorate from access for thirty years.

al secondment. The sympathetic CES Director then obtained charitable trust funding for the Gypsy project. On hearing this, the indignant Ministry wrote to the Director insisting that *all* research on Gypsies should be done *only* under government supervision. They threatened to block all future ‹independent› funding. The Director boldly defied them. Dr K was thus maliciously deluded in branding the project as ‹Government espionage›. The research was launched despite state intimidation.

In contrast to such threats from this non-Gypsy linguist, the anthropologist has always been safe with the Gypsies. The first night I, a single woman in her twenties, moved into a caravan, a concerned Gypsy man, whose shed is visible in the photograph (see Figure 1), advised:

> If anyone knocks on your door, don't open it, just holler (shout). You'll be alright with *us* to look after you.

The Gypsy saw the threat coming from *outside* the site, not from his people. The real dangers were, according to him, beyond the camp: non-Gypsy rapists, thieves or even murderers, as confirmed in this warning left by a non-Gypsy resident near another encampment:

> Gypsy Tinker camps to be cleared completely or suffer petrol bombs and shotgun attacks. Loss of life will be your responsibility. Final Warning.



Figure 1: Gypsy site

Through subsequent decades, I was indeed protected, but had immediately to prove trustworthiness. I avoided anything risking interpretation as threatening. ‹Pamela›, the social worker, warned that, when being introduced to Gypsies, I must never identify myself as researcher, only her ‹friend›. Sure enough, individual Gypsies spontaneously declared: «As soon as ever someone opens his jacket and pulls out a pen, we know that's a *gavver*» (policeman). The very sight of a pen was proof of enemy intent.

Later, I discovered how literacy, as danger, was further projected. Gypsy children were warned: «The Devil is a man with a Briefcase». Literacy was associated with enemy officials ready to take children into custody, something which happened regularly, in contrast to the myth that Gypsies steal children (cf. Okely 2014).[6] Today, laptops have replaced briefcases. Many, now literate Gypsies use them. But 1970s fieldwork for this anthropologist gave no such facilities for note-taking.

**Self-Description**

Explaining my presence, I told the Gypsies the nearest to the truth: as future teacher, I needed to know about their way of life in the event of having Gypsy pupils. Wonderfully, my postgraduate students have included a Bulgarian Roma and a Scottish Traveller. English Gypsies seek my advice about postgraduate research. I had no idea I would write a book, let alone a doctorate. My employer's plan was a report, displaying statistics on every page.

In the 1970s, there were no such impediments as written ‹informed consent› which, as Macdonald (2010) and others (Okely 2010) have argued, would have prevented most classical anthropology. For non-literate Gypsies, signing procedures would be interpreted as government appropriation. Never knowingly betraying ‹the other›, I embraced anthropology as celebrating the full range of alternative ways of living, including that of Gypsies.



6 When I gave the keynote at a recent conference of occupational therapists, I mentioned this. A member of the audience revealed that as trainee, she visited a Gypsy site with her professional mentor, routinely examining a mother and baby, checked out from the hospital. The official seized the baby for adop-tion, because there was no running water. Yet on my sites, we faced the same deprivation, even though the residents paid rent for caravan parking. Requests for one tap were rejected. The tenants were given only elsan toilets and rubbish skip, emptied weekly. Official files revealed that, since the site had ‹only temporary status›, the Gypsies might get too settled if given water. Instead, we had to visit local garages and beg for water. Ironically, while Gypsies were punished for being nomads, the policy makers feared they might settle.

## Note-Taking in Private

Nonetheless, I knew I *had* to write notes. This I did privately at night in my trailer/caravan or the local library. Hearing key names and concepts, I disappeared into the elsan toilet to scribble them down. On my second site (Okely 1983: 35), my council trailer, according to mortuary rites, should have been burned. The previous Gypsy resident had died there. Given it was haunted after dark, no-one disturbed this nightly note-taker. Initially, I recorded according to preconceived categories such as Kinship, Perceptions of Outsiders, Occupations. But when themes are selected in advance, much is overlooked. Malcolm McLeod, with fieldwork in Ghana, under the tutelage of Evans-Pritchard, informally produced the best advice: «Write down everything you see, hear or smell», then mischievously: «Ideally, you should complete an exercise book each day». I stored multiple pages in a friend's nearby house, then posted them to the London Centre.

Everything was so different from the 1938 photograph of Margaret Mead and Gregory Bateson in New Guinea, each at their typewriters (Sanjek 1990). I could never have appeared with such a machine; as threatening as a briefcase to my co-residents. Another celebrated photograph shows Malinowski at his writing desk in his tent, observed by Trobrianders (1913). Such images of the anthropologist, as writer, were impossible during my fieldwork. In this Western country, the majority's literacy was experienced by the minority as a weapon.

## Analysis and Organising

Decades later, re-examining the preliminary narratives, I realised that hidden, later in plain sight, were key themes subsequently explored in my doctorate and monograph (Okely 2011). Thus, the y cannot predict all, if any, in-depth cross-cultural understanding during or immediately after participation. The material has to be ruminated, then considered in relation to any literature on this or other peoples, and theories from across the globe. Thus, cross-cultural research e.g. by Lévi-Strauss (1966) and Tambiah (1973) assisted in unmasking the latent code in Gypsies' animal classification (Okely 1983: 89–104).

Eventually, after reading my extensive fieldnotes, Adams acknowledged the value of fieldwork, and arranged for the ever-accumulating pages to be typed by the Centre's secretaries. They were enthralled by the unfolding narratives, in contrast to statistics on traffic flows or housing policies. The typists, when heard excitedly discussing my ‹adventures›, were ordered to respect confidentiality.

While clerical assistance was a privilege, I inevitably excluded all inner revelations and experiences appropriate for a personal diary. Van Maanen's ‹Confessional› category (1988) uses ‹I›, without revealing dark secrets. Similarly, although my notes used 'I' as

narrator, any inner psyche was unrecorded. Malinowski was freer to write a personal diary (1967) separate from fieldnotes. In my case, the overbearing pressure to prove the 'objectivity' of fieldnotes precluded time and space for noting privatised experiences.

Fieldwork over, I read the newly typ-ed text, then manually bracketed paragraphs according to emergent themes. Some overlapped. Long before the intricacies of ‹cut and paste› computers, twelve photocopies were made of each typed, bracketed page. Superbly, Cathy, our research assistant, scissor-cut the sections, sometimes overlapping, then hand stapled them into classified files. Details about and quotes from members of a caravan unit were placed in family folders. Fieldwork on four different sites, through some 18 months, including follow up visits (Okely 1983: 39), produced reliable ethnographic data for some 73 family units. Elsewhere, I explore the complex process of analysing fieldnotes (Okely 1994). The material had emerged from multiple contexts and one fieldworker's participant observation, rather than multiple, depersonalized questionnaires. The potential for major errors in the latter was superbly exposed by Leach (1967), demonstrating that one anthropologist living in one locality, through extended time, reveals the total ‹system›. Eventually, my ethnographic chapters were submerged in a joint-authored book (Adams, Okely et al. 1975). I registered for a doctorate at Oxford, eventually publishing *The Traveller-Gypsies* (Okely 1983).

**Other Examples: Varied Contexts**

Here I explore comparisons and contrasts among other anthropologists (identified by key publications). Some also faced the accusation of espionage, although not in such threatening circumstances as mine.



Figure 2: Paul Clough, left

Paul Clough in Nigeria (2014) revealed:

> The villagers in Hausaland were suspicious or confused by my taking notes in front of them … From early on, there was the rumour I was a government spy. They grew to like me. There was the assumption that I couldn't be spending so much time with them, asking many questions, unless I was getting something out of it. They were sure I was writing down everything in order to take it to England and ‹make profit›.

Carol Silverman could not pursue plans for a doctorate on musicmaking among Roma in Bulgaria because their music was banned under communism. Others' fieldnotes and recordings had been confiscated at the Customs. Here note-taking might be acceptable to the people themselves, but external politics risked their destruction. Instead, Silverman completed her doctorate on diasporic Roma in New York, revisiting Bulgaria after the collapse of communism (2011).

The Czech anthropologists Marek Jakoubek and Lenka Budilova (2006) revealed the constraints on public or private note-taking:

> When we were doing fieldwork in Roma settlements in Eastern Slovakia, the biggest problem was absence of privacy. We did not have space to change our clothes, be alone or speak with each other without the presence of other people. It was not possible to write any fieldnotes. When we tried to write in the home of our landlords, we were quickly interrupted by someone from the family: «To whom do you write?» The only purpose of writing they could imagine was a letter to somebody. So, our activity was seen as suspicious. We soon quit these attempts, because writing was obviously so unnatural that it always triggered surprise, concern and questions. Although our companions knew we were doing fieldwork (as far as we were able to make clear what it was), they did not understand why and what we should write. As for «taking notes» – we were not able to take notes in the lavatory (latrines or just the woods). There was NO private space, even at night. We shared the house with our informants. We ate, we slept alongside them. We solved this problem, using our car. We could visit relatives of «our family» living elsewhere, but it was a kind of burden, since the presence of a car – with petrol and driver more or less willing to drive – somehow invoked the constant need to use it. Sometimes we felt like taxi-drivers. From time to time, we took our car, using an excuse to do shopping, left the settlement for the afternoon, or overnight. (We had Slovak friends in neighbouring villages and town). During this time, we did all the necessary things: taking a shower and writing notes being of prominent importance. But the most important work started when we came back home – only there

we could systemize all the scattered notes and put them together. But – nothing comes out of the notes – no matter how complex and exhaustive they are – without an idea. But where the ideas are born (and how articles are written), that is a different story.

Marek Kaminski, of Polish descent, with research in Sweden (1980), discussed writing fieldnotes among Gypsies:

In my early research stages in Poland & Czechoslovakia in 1966–72, I frequently did. However, in Sweden, I seldom did, as it seemed very suspicious to the Gypsies (and fellow refugees).

The Japanese anthropologist, Akira Okazaki, after living with the Maasai, years later, returned to Africa with his family for anthropological fieldwork among the Gamk in the Sudan (1997).

Note-taking interrupted his involvement. He wrote after key events:

Usually the best and most interesting answer or discussion is when we are doing a similar thing like walking or working. There's a problem. When moving I cannot write. My memory is limited anyway. The exact word is important. There is another moment, like rituals, because, among the Gamk, they start talking almost two/three hours before making a final sacrifice.

Tape recording was welcomed:

I attended so many times. I came to be known as a harmless person: «Turn on your tape recorder». I never asked any questions. I was attending in the same style, in front of the ancestors. It's hard to take notes in the field. One situation in which I couldn't make notes, but urgently needed to put (something) on paper, I just went to the toilet. Otherwise I could forget, not because they rejected (me writing).



Figure 3: Akira Okazaki with Family

But I felt very awkward. People were talking about dreams. It is a different experience.

They associated writing, with documents and used the Arabic word wara for paper and IDs – something official, government-imposed. Someone who can write is a very precious person and (is) asked: «Just please write down this» or «I can't read this». That kind of power relation might occur if he or she uses it up front.

Malcolm McLeod, with fieldwork from the late 1960s:

You can't sit there writing at someone's funeral, or circumcision, or whatever. You learn to memorise things at least for a few hours, so that you can jot them down in sequence with quite a lot of detail. Edwin Ardener taught me you should keep a carbon copy! History is littered with people who claim, «I lost my fieldwork notes, and that's why I haven't got anything worth reporting!».

Decades later, computer typed fieldnotes can be copied and emailed. Felicia Hughes-Freeland in Indonesia:

People always tell you the best things when you can't make notes. Usually when you're on the back of a motorcycle or in a storm up a mountain, when you're not in a position to do anything except pay incredible attention as you hear it. You're already processing it into something you can understand. Immediately the words hit your ear, you are doing something to that information.

Concentrating on Dance (2008), Felicia made diagrams:

I was sketching, always trying to find a way of drawing the dance movements: fruitless ultimately. I've got a lot of very strange little squiggles. By the end I was so tired. As the year progressed, I got more tired. I got better at not doing anything, learning to not do things. That got me enormous approval.

Anthony Howarth, my current Cambridge doctoral student, found no problems writing notes in front of Nepalese nomads. But his recent fieldwork among Irish Travellers in Central London was:

Completely different … I didn't write anything down in front of them unless something couldn't wait. I would write fieldnotes at the end of the day alone in my trailer. On a few occasions, Travellers would visit me while I was doing this and I'd put the journal to one side. The head man, on seeing me do this, remarked; «I want to read this book when you're finished» (he was literate), to which I replied «Of course. I can't wait to hear what you think».

If I had constantly written things down, it would've been very difficult to establish trust. During my fieldwork, I kept one journal and everything went in there: – personal feelings, interpretations of the day's events – everything. I

also worked with men (doing building etc.). This meant note-taking was impossible. Here I did as they did and learned with the body to experience the city as near as possible to the way they did. This was a kind of knowing that was profound, due to it being beyond linguistic interpretation (cf. Okely 2012: Ch. 6).

Again, the anthropologist, as fieldworker, reserves note-taking for mainly private, secluded times. However, in contrast to my 1970s fieldwork, many decades later, for Howarth, being seen to write among the Travellers was acceptable. Indeed, once full trust was gained, writing about the peoples was seen as offering potential, experiential exchange. Thus, in the examples above, ideally participant observation entails not writing during participation. Observation entails memorisation until there is time alone for writing down.

**Notebooks and Jottings**

In contrast to those of us who had to sneak off to scribble down names and phrases, others were tolerated when they jotted down key words on the spot.

The Swedish anthropologist, Helena Wulff, doing fieldwork among South London teenagers (1988), used a typewriter back in her lodgings:

Some days I just wrote one sentence. It might take a few days before the next entry: – half a page or five. I didn't hide my notes. I had no place to hide them. Not that I



Figure 4: Helena Wulff, left, with teenagers

wrote about anything that would have upset them. But to make sure, I sometimes wrote in Swedish. Also, I might have misunderstood something I would be able to correct. There were some sensitive issues with the teenage girls that I preferred to write in Swedish. One thing I learnt from Ulf Hannerz before I went to the field, was to jot down keywords even late at night after a long busy field day. You forget, when you sleep.

When Wulff researched ballet in Stockholm, she kept a tiny note pad in her jeans and, like Okazaki and myself, a short break had added advantages for jotting key words. Mohammad Talib studied stone-breakers outside Delhi (2010), commuting some 20 miles:

Initially I would scribble, just points, and then I had to elaborate them. It was always difficult to do that in the field. I would do it at home. But it had to be done the same day. That was very important. One day I went to the field, jotted down notes. I thought they were intelligible. I came home. For some reason, I just couldn't come to writing – not for three days. When I came back to that jotting, I couldn't make head nor tail. I had to go back to the people to ask: «What exactly was the point you raised there?» It had to be done all over again. Yes, shorthand in the field, then elaborating at home was very important. It had to be done the same day. Otherwise it was not possible. I classified them into topics much later. Initially it was shorthand, a longhand, then the translation. I also made some notes in Hindi then translated them into English.

Here, as in other examples, the choice of language, when writing down, is contextualised.

Louise De La Gorgendière, Franco-Canadian, among the Asante in Ghana:

I took my fieldnotes on a stenographer's notepad … not shorthand, but «short form». And in the evening, diligently wrote up my notes every day. I stayed up until two o'clock in the morning most days doing it. Then I would also set out questions or things I had to examine the next day.



Figure 5: Daniela Peluso, right, with Amazonians

**Shared Process**

Daniela Peluso described fieldwork in Bolivian Amazonia:

Figure 6: Nancy Lindisfarne with Pashtun man

I carried a jot book – where I jotted down a word that would jar my memory later – when I had a moment alone to write. I'd try to break away every few hours. But there were days I had to wait until everyone else was asleep … not that I didn't want them to see me write but I did not want to miss a moment of reality!

Peluso consulted her son, a small boy:

Dimitri (most of the time he was with his multi-generational gang of friends), also became a source of fieldnotes or repository of information. I often asked him for his version of particular events – his comments were always insightful.

Nancy Lindisfarne (1991), with husband Richard, lived with the Pashtun in Afghanistan in the late 1960s:

Richard travelled a lot. I stayed put in this much more domestic setting. Then eventually worked through genealogies and had a comprehensive record of people's histories of marriage: enormously telling about tribal politics, and wealth. I did it all the time. I got some wrapping-paper from the local bazaar and eventually did my genealogies. I've only just found them again. They look like circuit diagrams – huge. I did them in the tent. We used small notebooks. Camels carry trunks. So, you buy trunks, and they have to be balanced on either side. We had trunks that had rice and notebooks.

Literacy was:

Virtually non-existent. Our host was literate. Because there was no privacy, we would stuff ourselves in a corner and write notes at various times. Otherwise one tended not to use notebooks. When talking to people, it's terribly intrusive. But using the excuse of language learning, there would be a word, enough to allow one to write down. Then you could write down two and a half facts, sure to go out of your head before the evening writing up. Most of the day was just spent with people, *occasionally* pulling a notebook out, saying, «That's a new word.» That was not seen as terribly intrusive.

Like Nancy Lindisfarne, Roy Gigengack, of Dutch descent, researched as a couple. He and his Mexican partner, Raquel Alonso Lopez, did fieldwork among street children in Mexico City:

> First of all, if you are trained, it is possible that afterwards you write down your notes and there are things you can remember. But we had another advantage that we worked together. We could also correct each other and, apart from correcting, also elicit memories: «Don't you remember that he said this and this?» «Oh yes, and then he said that and that.» So, we could reconstruct it. In that sense, to do it together was really surplus value.



**Fieldnotes as Core to Writing up**

Ekaterina Cupelin, who completed her Geneva doctorate on Bulgarian Roma (2017), provided ‹Some notes on notes›. Her techniques reveal crucial changes, namely access to a computer:

> My fieldnotes saved me. With a distance of three years between completing fieldwork and writing up my dissertation, they were crucial for the writing up … I always carried around a notebook where I quickly jotted down key words or phrases from conversations, names of people and places, dates of important events, and new words in Romanés. This became a sort of a shorthand diary. My fieldnotes I typed on my computer. I tried to do that every morning early, before my friends reclaimed my time and presence. I sat down and wrote away, trying to recollect every detail, sight, smell, emotion, from the previous day. I did not follow my research question when I settled to write (up). Instead, I wrote everything. Similar to the way I conducted fieldwork, letting myself follow the daily lives, needs and struggles of my Kalaydji friends without filtering out those that seemed irrelevant to my major «question». I wrote all of these experiences without judgement, hoping the dots would connect one day. (Truth be told, my commitment to write every day was challenged a number of times. There have been mornings when I had to write in several days, even weeks at a time.)

**The Visual**

Cupelin:

> If my fieldnotes saved me, it was my ‹videonotes› which breathed life into my project. At the later stages, together with my partner, we recorded with camera a number of interviews. Sitting down to watch those was like an electroshock – I was immediately transported back to the field and its complexity; only with a new way of seeing. I could be simultaneously 'in' and ‹out›, noticing themes that had completely escaped my attention until that time.

Cupelin and partner's visual recording of Roma in Bulgaria contrasts with

the negativity of Anthony Howarth among Irish Travellers in London in the same era:

> The Travellers I lived with did not want to be photographed, interviewed or filmed, since, (they) would feel shame to end up on a programme like the ‹Big Fat Gypsy Wedding› series.

This TV series constructed and exploited every stereotype. No mainstream UK channel would dare depict the same with other ethnic minorities. Earlier anthropologists, in other contexts, were also able to use visual methods providing insights not perceived when writing notes. Tony Simpson in Zambia: «I found the video material even more revealing – I noticed aspects that I had not remembered noticing at the time of recording.»

Marek Kaminski discovered that it all depended who was filming. The Gypsies in Sweden performed for him when he filmed. They did not like him note-taking.

> Instead, after completing the first video course in Stockholm in 1975, I left this ancient, heavy video equipment with my Gypsy friends, and asked them to take video records during my absence. Subsequently, I described how their video notes on purity and pollution greatly differed from my earlier shooting. When I was video record-ing their laundering habits, they carefully segregated the female and male laundry. But when they filmed by themselves and coincidentally recorded in the background the laundering, there was no segregation. Male and female clothes were laundered together. I have been studying the same Gypsy refugee clan for the past 45 years. The last time I stayed, I filmed with my iPad.

**Writing Everyday**

Just as Wulff, Talib, Peluso, Lindisfarne, Cupelin and de la Gorgendière described, most anthropologists tried to write notes, often in diary form, every day or as soon as possible. Nancy Lindisfarne and Richard kept:

> a diary of the day. We didn't have typewriters. You can only write so much, before your hand falls off. But the diary notebooks had this important advantage, which was that it anchored one's own feelings with what one was hearing. So that at all times the information – the ‹hard data› – collected on that day, was also being fed through the gamut of experiences that one had.

Lindisfarne suggests that the act of writing helps implant the experience in the memory in mysterious ways. She confirms the importance of lived, multi-faceted experience. Thus, only selective aspects of the totality can be written down. The specificity of experience in the field has finally been accepted as dependent on the positionality of the anthropologist (Okely and Callaway 1992). Similarly, there can be specificities in the act

of writing. Just as the experience of participant observation differs according to each individual, so styles, jottings and flow of writing will vary. This repeatedly challenges the positivist notion that researchers as observers can or should be recorders with ‹scientific detachment›.

Signe Howell, of Norwegian descent, living with hunter-gatherer nomads in Malaysia (1984), would write fieldnotes:

> Every day. That was my one discipline, very often at night. Sometimes the hours just stretch and stretch. Nothing happens at all, there's nothing to do! I would sit down and write some things. I would go through some of them, and look for little bits and pieces. I wanted to check. I would have some separate books, for example for myths.

She was living with relatively isolated non-literate peoples but where the act of writing was neither perceived nor understood as threatening.


Figure 7: Hélène Neveu Kringelbach, left, with daughters and associates.

Hélène Neveu Kringelbach, of Franco-Senegalese identity, when studying dance in Senegal:

> I mostly wrote up my notes after the events on my own in the evening, or sometimes, a few days later, I'd sit down and recall the events, write up as accurately as I could remember. There were a few occasions, outside Dakar, I'd brought my notebook just in case, not intending to use it because I usually didn't write notes in front of people. In the dance world and in front of my friends, I didn't feel it was appropriate, because it would have been like reminding them all the time that I was doing research.

Here again the anthropologist has not hidden her identity as researcher. But she recognises that, in a world of non-verbal bodily dance, total immersion, bringing reciprocal research knowledge, will be enhanced by avoiding intellectualized distraction and interruption.

Joanna Overing, studying the Piraoa in Venezuala (1975) revealed:

> Every night, I copied down all my notes from the day. In copying them, you analyse or raise questions for the next – what to follow through, what you don't understand …what you want it to relate to. Also, keeping things in various categories, and cross-indexing. That was the most useful. Every single night I'd copy. Even if we had tons of data on chant language … Or working with the language. That's the only time you

see gaps. If you wait, you forget what your reasoning was.

Going around with a notepad depends. I took extensive notes during the day. The Piraoa understood that. They liked it. They were trying to figure out ways that we could. The Piraoa understand complicated symbolism. It's very graphic, for instance, their basketry. If they saw patterns graphically on basket work then they could see writing as another form.

Here writing, perceived as pattern, was appreciated in terms which might resonate with the hosts' culture.

**Rereading for Writing up**

Overing:

I have all these (note) books on myths, language … Even if they're categorised, it's still like looking for a needle in a haystack because I'm looking for different things now. I studied those books. That's how I really learned Piraoa cosmology and language. Because when I went back – I could understand so much better! A strange phenomenon. Those notebooks were absolutely essential.

Johnny Parry, with years of fieldwork in India, commented:

You have these thousands of pages of notes, but – as you're writing, you find, «Oh! Why didn't I ask that?» Very often you find that you've got a great deal of data that you had no idea you'd got … It is a very peculiar thing. I'd write the most perhaps absurdly, detailed daily diaries … I've always found that one learns things from rereading, that you didn't know that you knew. There are some kinds of information I record separately if doing a census. I would record conversations in rough form. It would depend on the circumstances, unless it was anything terribly sensitive, I'd take rough notes on a small notepad during the conversation.

If I look back at my early Kangra notes, they were short and skimpy partly because, you were so bewildered you didn't know what to write down. You couldn't understand half of what was going on. Whereas, on a good day – the problem is to find time to do the writing. A day's notes from my Bhilai fieldwork might be 15 typed A4 (pages). Both the Kangra and the Benares fieldwork, I wrote out by hand. In India you can buy leather bound notebooks.

In his study, Parry handed me one: «This is from Benares.» I asked about a figure on one page: «What does that mean – 5, 9, 2, 5?» Parry coolly explained: «Five thousand nine hundred and twenty-two!» This was the page number, giving a glimpse into the systematized mass of material accumulated by a dedicated scholar. The rereading, selection and thinking through would produce key articles and monographs (Parry 1979, 1994). Fieldwork was in a continent with

peoples where literacy was interlaced in the long-established practices of caste hierarchy.

Cupelin gives clues into the challenge of writing up, namely her doctorate. Fieldnotes trigger themes. But the final submitted text, whether doctorate or publication, is a different process from more recording:

> It took time for the dots to connect. I needed distance – physical, temporal, mental – to zoom out and see the larger picture that started emerging from my fieldnotes. The dots connected in unexpected ways. I abandoned my initial research question. New directions and major themes emerged out of the fieldnotes. The topics of identifications and transformations, and womanhood and personhood were directly born from my notes. In other chapters, like the details of Elena's legal fight, were luckily captured and had to be uncovered among my free-flowing text. I reread and reordered themes and narrative multiple times, copied chunks that would eventually make a single story from 20–30 different entries into separate documents. When the skeleton of a text started emerging, I went back to my interviews and integrated important quotes.



## Outer/Inner Perspectives through Time

As the images through time of Kenna in Greece (1992: 148–9) demonstrate, the anthropologist, visiting the same location through decades, will not only change themes but also approach note-taking differently. Tony Simpson has repeatedly returned to Zambia where he did initial research on a boarding school where he had once taught (2003). Here is a difference among a younger generation of anthropologists who have been given methods courses. They are experimenting in different ways.

Simpson:

> The whole process of producing an ethnographic text is intriguing. Fieldwork is how we learn, but when we write up, it's almost as if we screen out the learning process. Perhaps this is because of the demands of journal editors or publishers? I see a real shift in my note-taking. When writing my PhD, I felt I had – was probably told! – to write down 'everything'! An impossible task, of course, how to record as much as possible the imponderabilia of everyday life (cf. Malinowski 1922).
>
> So, in my ‹early› days, I produced copious notes. I never wrote while in conversation or during interviews. I did a lot of recording – both audio and visual – during PhD fieldwork and for the first time in my life I faithfully kept a diary. I spent hours almost every evening writing up. While at times I felt I had this tremendous resource to draw upon – transcripts of interviews, video material, and a diary stretching to hundreds of pages – at other times, the task of delving into this wealth

of material seemed too daunting. I found it exhausting to plunge back into the diary.

Here Simpson reveals the significance of the diary, although seemingly this was more a chronological narrative, not necessarily a separate repository for private, personal secrets.

The diary did prove extraordinarily useful as I discovered how much I had forgotten – or, perhaps more interesting – how my memory of what had happened sometimes differed significantly from my contemporary recorded account.

Louise de la Gorgendière chose to record the personal elsewhere:

I wrote the most personal thoughts in letters, back to friends and family. That's where all the personal stuff came out. I got most of them back. Because they were very important to me when I started looking back at my fieldwork, and those early impressions in particular, of what it was like to be in the field, and the early feelings of loneliness, and «Oh my God I don't know where this is going!» … It had a cathartic effect, writing it off to somebody else.

Tony Simpson, when subsequently writing up, acknowledged:

The challenge becomes how to write faithful to this mountain of material, only a minute fraction of which will ever appear in an ethnographic text. (How to distil what seems to us, at a particular moment, the essence of the matter?) Now, I write far fewer notes. Is it because I have become lazy? More familiar? More confident I can capture the essence? The notes, long or short, should always assist in our attempts to capture the lives of others – and bear witness to these lives – in as ‹honest› a fashion as possible.

Suzette Heald, after decades of fieldwork in Uganda (1999) and Kenya, revealed:

I don't write biographical details about the people I'm interviewing because I know it. I write the standard things, like dates, but I don't write my impressions of their personality, because they're in my head. Either it becomes irrelevant, or it's so much part of the embedded context by which you interpret your fieldnotes, which is why it's really difficult for me to think about archiving them. I would have to write this stuff out, with some more description than would be in my fieldnotes which would say «Bumped into M…on the road. He was doing X, Y, Z». That doesn't mean anything to anyone who doesn't know M and the relationship between us, or indeed where he was going. That kind of archiving becomes a real problem. It was head notes (cf. Jackson 1990).

Several anthropologists revealed they took few notes when studying their own cultures, for example, Zulaika. Or, like Simpson, when returning to the same localities through the years. Clearly, there is a build-up of experi-

ence from years of participation especially as a member. Some statements and events, anthropologists, indeed most humans, always remember, without explicit recording. Contexts and values change through time but some, especially in childhood, stain the memory and emotions. Never written down, they are forever registered. Similarly, anthropologists in the field may be affected, consciously or not.

**Interviewing and Tape Recording**

Given this method can be used, in addition to participant observation, interviewing may appear more acceptable for visible note-taking or tape recording. The anthropologist and interviewee are likely to be stationary, even seated, not walking. Okazaki vividly mentioned the restriction of movement on note-taking. Ann Maria Viljanen, in Finland, discussed her experience of first interviewing Roma women and naively asking about childbirth. The women laughed. Gradually, Viljanen realised such topics were taboo. Eventually, she found that once she spent informal time with them, they gradually accepted discussions and her tape recorder.

This was the same in my case. The Gypsies said they did not mind journalists tape recording them. Perhaps, because to non-literates it was a controllable record of their voice, in contrast to alien pen and unreadable paper. Towards the end of fieldwork, I produced a tape recorder. The children were excited recognising their replayed voices.

On one occasion, wanting exact words and accents, I walked around with the tape recorder concealed in a shoulder bag. Later, I discovered the machine had stalled. Simultaneously, I had 'switched off my internal mental recorder', and could recall nothing. Thus, the necessity to memorise internally for subsequent note-taking demands extra mental dimensions in participant observation.

Gigengack, in Mexico City, developed an ingenious use of the tape recorder:

> If there was a strategy, it was «Don't do interviews». What I did that was very good, was to have street children interview each other with a tape recorder.

Joseba Zulaika, a Basque studying ETA terrorism (1982), rarely wrote notes:

> I did interviews. I was talking with these guys all the time. At some moments with the peer group of the village, we met and I asked concrete questions. That I taped. I did interviews with ex-ETA members, usually informally when having dinner or driving. I interviewed some ETA members *not* from the village. I taped. They didn't mind.

Here, consistent with Simpson above, note-taking may be less frequent if the anthropologist is thoroughly familiar with the people, as member or long-term participant observer.

## The Context of Note-Taking Affects the Topic

Fieldwork among the rural aged in Normandy, France, I found utterly different from the Gypsies. The French welcomed this researcher intellectual: pen and paper taken for granted. The residents were honoured that this Professeur Anglaise was focusing on *them*. I visited Jacqueline Grégoire being head of the local Club for the elderly. To prove authenticity, I felt compelled to write down her answers as I interviewed her. After a while, she said «J'ai du travail à faire». I accompanied her to a barn sheltering cows which she proceeded to handmilk. After several cows, I asked to have a go. She was astonished. I was introduced to a special cow which didn't kick. After I was shown the basics, Mme Grégoire said: «Attendez, je reviens». Returning with a flash camera, she photographed me in action.

It was apparently bizarre that this professeur should stoop to manual labour: something a celebrated Parisian ethnologist echoed with astonishment. Research through manual labour proved a wonderful entry throughout the community – just like collecting scrap metal with the Gypsies. But in Normandy, writing about the residents was perceived as respect, never invasion.

Figure 8: Judith Okely handmilking photo-raphed by Mme Grégoire

There are other contrasts, which unexpectedly in Normandy, linked note-taking with the very research focus. I had recently finished my book on de Beauvoir (Okely 1986), dictating paragraphs by phone from the French town. Rewriting drafts had brought repetitive strain injury, making it painful to continue handwriting. Fortunately, my funding included typing assistance, so I dictated my Normandy fieldnotes onto cassettes then posted to England. An unexpected change in research focus emerged from living in idyllic rural surroundings. Dictating, during daylight, I gazed for hours at the rural landscape, finding resonances with Millet, Monet and Pissarro. Thanks to this *visualised* context of recording notes, I became increasingly alert to the local residents' differing engagements with agriculture and contrasting class views of the Normandy landscape (Okely 2001).

## Contrasts and Comparisons between Poetry, Art, Fiction

The poet Simon Armitage recently declared «Writing is static, unsocial». An ethnographer has to participate and observe, before writing about a highly social process. The challenge for the

anthropologist I argue is to transform action and experience into *exact* records, not lyrical imaginaries. There are other contrasts. The actor has to voice other's ready texts, learned by heart. The ethnographer listens repeatedly to others' spoken words, not yet written. The ethnographer must remember, *then* write them down.

The ethnographer uses all the senses, including vision, but not usually the same as the artist painter. Art may be recognised as figurative, realist, impressionist or abstract. But in all styles, for the artist, vision is primary. Listening may not be crucial. Some artists make preliminary sketches, with subsequent transformation. A novelist is also observer and memoriser. But, as with poet and painter, there are transformations alongside realism and plausible authenticity.

An artist/painter may gaze, sketch, then finalise on canvas. Transformations depend on style and preferences in shape and colour. A poet seizes themes, images and events: trivial or melodramatic. Some are selected and condensed, then made musical through rhyme and rhythm. The ethnographer works in the *other* direction. S/he may either enter the unknown or seemingly familiar, but the aim is not to transform with creative licence for originality. The anthropologist tries to record every aspect, to reproduce and ultimately interpret what is seen, heard and felt. The ethnographer is witness. Note-taking is not preparation for imagined transformation but as reflection of exactitude, leading to ground-breaking analysis and underlying explanatory theories.

The ethnographer, occasionally with family, and eventually with key local intermediary associate, aims or attempts to capture all, whether exotically different or commonplace and banal. Inevitably, there are specific angles, depending on the positionality of the fieldworker. The notes are later expanded, but rarely changed. They are triggers.

**Political and Ethical Loyalty**

Fieldnotes have unexpected parallels with a private diary, except the anthropologist is necessarily recording *others'* intimacies, within wider contexts. Reciprocal trust is vital. Ultimately, the anthropologist has profound ethical/political loyalty and obligations: something to which this anthropologist was committed, especially towards the persecuted.

Some time after I had left the CES, the 1979 Conservative government sought its closure; consistent with Margaret Thatcher's contempt for the Social Science Research Council which she also attempted to abolish. My former manager, re-immersed in her Ministry, decreed that our Gypsy research archive, including my typed fieldnotes, be transferred to Ministerial possession. Thousands of pages: all unedited narratives, and 73 family files, naming individuals and exact locations, could be misappropriated and weaponised. Dr K's labelling me as traitor spy would be unexpectedly vindicated. I hastily recruited friends, a van and drove to

the Regents Park building. In the chaos of closure, we freely entered and located my multiple documents. These we uploaded and departed. I informed my ex-manager, insisting this repossession of fieldnotes was justified, given my/our ethical commitment towards this minority's future safety.

**Conclusion**

The anthropologist, as fieldworker, travels from holistic, mobile, bodily and mental engagement through daily, nightly scribbles into the static, eventually transformed, published text. Fieldnotes are the core component of anthropological fieldwork, whether recorded with pen, typewriter, tape or laptop. In every case explored here, the anthropologist had to participate, usually limiting simultaneous recording. The memory is trained in new ways to ensure the experience and observations can be re-captured. The longer the wait, the greater the loss.

Tolerance of or even requests by different peoples for visible note-taking vary across place and time. Non-literate peoples might be either fascinated and supportive, or suspicious and fearful. In a caste-divided culture, the literate anthropologist may be revered as superior scholar. By contrast, elsewhere, literacy can be perceived and experienced as the state's weapon against the non-literate, especially subordinate minorities. Whatever the differing contexts, as in these anthropologists' examples from the 1960s to recent times, the anthropologist, as fieldworker, participant and would-be author, is fully involved at every stage. Crucial is the detailed noting of daily experience within a short time span. Fieldnotes, whether scrappy or convoluted, remain central to ethnographic research. Eventually, the fieldworker organises and classifies the material for recall, rethinking, and writing up. The emergent publications have been transformed from immediate notes through analysis and interpretation, alongside decades of cross-cultural theories from the study of cultures and peoples around the globe.

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University of Basel

# JJ Bachofen Lecture

Basic Questions of Anthropology 2017

Die Basler Ethnologie hatte schon lange vor dem 100-jährigen Bestehen der akademischen Ethnologie in Basel (seit 1914) einen gewaltigen intellektuellen Einfluss auf die globale Anthropologie. Zu den **wichtigsten anthropologischen Vordenkern** in Basel gehörte **Johann Jakob Bachofen-Burckhardt**, studierter Jurist und Professor für römisches Recht an der Universität Basel. In seinem 1861 erschienenen Hauptwerk «Das Mutterrecht» stellte er grundlegende Fragen nach der Geschichte und dem Verhältnis der Geschlechter. Er wertete das Matriarchat positiv – damals ein Bruch mit dem dominierenden Patriarchat und entschieden gegen den damaligen anthropologischen Mainstream gedacht. Bachofen wurde mehrfach wiederentdeckt (Ludwig Klages, Rainer Maria Rilke und Walter Benjamin). Seine Thesen sicherten ihm noch in den 1970er Jahren eine intensive Rezeption seitens der Frauenbewegung. Heute werden die Fragen, die Bachofen stellte, anders beantwortet. Relevant sind sie jedoch geblieben. In Anlehnung an diese Tradition stellt die jährlich stattfindende Bachofen Lecture Grundfragen der Ethnologie neu.

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