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## **The Multi-Sided Ethnographer**

Living the Field beyond Research

**[transcript]**

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## Fieldnotes

### From Intimate Impressions to Academic Discourse

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*Magnus Treiber*

#### **Thrown into fieldwork**

My PhD fieldwork certainly had a bumpy start – as is often the case. For me, the Eritrean capital, Asmara, had been a beloved place full of good memories from two tourist visits in the mid-1990s. In May 2001, after a few days of feeling excited to be back in the city, I realised that the Eritrean case was not the democratic post-revolutionary grassroots project I had somehow hoped for (Treiber/Redeker Hepner 2021). I had returned only a few months after the bloody border war of 1998–2000 between Eritrea and Ethiopia, which had resulted in the Algiers Agreement and a lasting ceasefire. Supportive members of Munich's Eritrean cultural association had provided me with a recommendation letter and contacted the Eritrean Embassy on my behalf. I was received at the airport by the family of a kind and well-respected diaspora activist, who allowed me to stay, for a time, in their house on the outskirts of the city. She had even put me in touch with her cousin, a university teacher in public health and a member of Asmara University's PhD committee. It was that committee that I would have to convince if I were to get research permission for my planned study on urban political culture and revolutionary emancipation at the grassroots level.

Soon my frustration grew, and I began to realise my initial naïveté. Certainly, I had to learn a lot – and in quick, drastic steps. I had to accept that Eritrea was a dictatorship; in fact, a few months later, the president

carried out his *coup d'état* from above. In that coup, he struck out at protesting students, long-time comrades from the days of the liberation struggle who were now part of Eritrea's government, journalists, all kinds of intellectuals and defectors and, above all, most of the people he claimed to represent. By this time, I had perceived that Eritrea's political culture was a deeply authoritarian one, founded fundamentally on fear. Following a protest letter authored by diaspora intellectuals – the so-called 'G13' – my diaspora support did not make much of an impression at all. The PhD committee's continuously repeated statement, "We are still discussing," was informally transmitted to me through my contact. It meant, essentially, "No-one here is willing to take responsibility for an apparently political topic, and we therefore prefer to wait silently until your three-month visa has expired." Furthermore, I tripped over academic intrigue of which I simply had no knowledge. For instance, when I tried to present my project idea to a local anthropologist, he started yelling at me in his university office. Calling my proposal unprofessional, he ordered me to leave and never return – an unforgettable encounter. Remarkably, though, this incident raised the interest of this person's long-time rival in the sociology department, who then declared his readiness to receive me. He awaited me in his office, along with two young Indian colleagues. While he silently chaired the evolving drama, I was once more yelled at and called ignorant and incompetent. A friend in the university administration laughed about both of my encounters and told me not to take it personally, but of course, I felt devastated. I had been introduced to local academic culture but not offered a seat. Apparently, the sociologist – we are still in loose contact – really tried to help me but could not gather sufficient internal support. Bewildered and insecure, I was about to give up. I felt particularly guilty because my parents had financed most of this first stay in Eritrea before I would secure a Hans-Böckler-scholarship the following year.

In addition, my host family's patriarch did his best to limit my movements in Asmara. He tried to make sure that I did not misbehave, until I could get my own room with other relatives of his in the Paradiso neighbourhood, closer to the inner city. From that point on, I would have my own key, at the cost of having no one to go to and no one with whom I

could talk. Unsurprisingly, I felt lonely and frustrated and often spent my evenings with only a stone-age laptop and the usual diet of dried dates, cookies, bananas and *raqi*, the local aniseed liqueur. However, I started to make interesting acquaintances and even rediscovered a friend whom I had come to know years before. When I got the chance to return to Munich for two weeks over summer, it felt like I was fleeing ‘the field’ and getting back to where I belonged. A colleague at the university welcomed me back the next day with a shout of “the fieldworker is back!” I answered many questions, but somehow I had to admit that my real successes were high scores in Minesweeper and Solitaire, a late-modern pastime that my more prominent forebearer Bronislaw Malinowski lacked:

Yesterday a week had passed since my arrival in Mailu. During that time, I was much too disorganized. I finished *Vanity Fair*, and read the whole of *Romance*. I couldn’t tear myself away; it was as though I had been drugged. Did some work, however, and the results are not bad for only a week, considering the terrible working conditions. (Malinowski 1967: 16)

While absent from what should have been my ethnographic field, however, I realised that I had indeed started a new social life elsewhere. Multiple relations had emerged and were evolving towards mutual curiosity, closeness, and responsibility. When I returned to Asmara, I was arriving in a place where I had friends who would welcome me back. My own pathway took an unexpectedly lucky turn, and just before my visa ran out, I received work and residence permits through the formally governmental – but relatively independent – Cultural Assets and Rehabilitation Project, to whom I offered to conduct an interview study on life in Asmara. Thus, my own problems grew to be less prominent, and I felt that I could pay deeper attention to the world around me. Eritrea had just come out of its border war with Ethiopia, and though the country was still in a state of shock, people felt that a political crisis was imminent. The fear of repression and renewed violence did not allow time for mourning.

25.09.2001.

The city is more beautiful than ever before. Almost empty, very sunny, warm, but not hot. Palm trees, whereas it has already started to rain at home. However, for one week, graveyard peace. Scary and dangerous. (Personal fieldnotes, own translation)

“I am a soldier”, a young man belonging to my host’s extended family told me laconically.

He did not try to explain much more, knowing that I had no idea of what he had been through, or of why he and his sick mother were hiding in a run-down shack. Slowly, I began to understand better. Strong impressions remain even after decades: a homeless mother’s baby crying at night in a dark backstreet just behind the Confederation of Eritrean Worker’s headquarters, the shocking news that Halima’s brother had died in unclear circumstances in national service or, that Mike was in prison, hands waving desperately from make-shift prison cells during a military raid for draft dodgers, the one bootleg CD with various Abba songs and Boney M’s “Daddy cool” (1976) that was played up and down in all of the city’s bars...

A close friend with a background in the Eritrean diaspora had come from Germany with her family for the summer holidays. On the evening of 10 August 2001, we were riding on one of Asmara’s red public buses together. Despite the darkness and the rain, we could see very well how the military police drove arrested students into Asmara Stadium, while desperate parents tried to throw blankets and food over the walls, not knowing what would happen to their loved ones. “*Dieses Land ist es nicht,*” my friend uttered. She was quoting a line from the song “Der Traum ist aus” (Ton Steine Scherben 1972), meaning, “This is not the country (we dreamt of).”

### **Diaries - grasping the immediate**

In his book “How Lifeworlds Work”, anthropologist Michael Jackson looks back on his fieldwork in the late 1960s in Sierra Leone (Jackson

2018, Treiber 2021). Although fifty years had elapsed since then – and fifty years is a long time – Jackson was able to show that his then-ethnography was still of use. The long timespan allowed him to mirror ethnography's situational moments in a wider life course. This freed the aging but intellectually mature anthropologist from emotional entanglements with the immediate. When I look back at my fieldwork and take a new look at my own initial fieldnotes, I find them highly emotional, in that they express my own initial disorientation, distress, and loneliness – which does not surprise me. This phenomenon, as such, is nothing new, of course, even if it was an intimate and existential one for me. Had Bronislaw Malinowski been in a position to speak openly about his own difficulties and emotions instead of setting up a distanced and analytical method (probably as a result of feeling forced to mimic the natural sciences), anthropology's history across the 20th century would have looked very different. While his offensive formulations – which helped to plunge anthropology into a decades-long, if extremely productive, crisis – cannot easily be excused, half a century after the posthumous publication of Malinowski's diaries, we can acknowledge that diaries are always intimate texts that are not necessarily meant to be read by others. After all, in diary entries, personal notes are inseparably mingled with what would one day become 'an ethnography'. Diaries turn thoughts, impressions, and encounters into words (or sketches, drawings etc., Taussig 2011). They are inevitably fragmentary, grasping the ephemeral: their typical chronological order is invariably nothing but a first and very basic structure, open to whatever comes up, including personal judgements of others that we might not want to communicate openly (Schönborn 2007). All of this is also true for ethnographic diaries. While Malinowski's diary is a prominent example of a private, intimate and in parts therapeutic text, which was not originally meant for publication, others were written to document and leave something behind (see Germaine Tillion, who wrote as an inmate and eyewitness of a Nazi concentration camp, [1946]). Some also became the basis of professional autobiographies, such as Hortense Powdermaker's "Stranger and Friend" (1966), or of real-life satire, like Johan Voskuil's

“Het Bureau” (volumes 1–7, 1996–2000), which documents Dutch and European anthropology’s professional culture across several decades.

I don’t care for life with the missionary, particularly because I know I’ll have to pay for everything. This man disgusts me with his [white] ‘superiority’, etc. But I must grant that English missionary work has certain favorable aspects. If this man were a German, he would doubtless be downright loathsome. Here the people are treated with a fair amount of decency and liberality. The missionary himself plays cricket with them, and you don’t feel that he pushes them around too much. How differently a man imagines his life from the way it turns out for him! (Malinowski 1967: 16)

Like Malinowski, I wouldn’t disclose all my diary entries to the public eye. After all, a good part of these were about missing my then-girlfriend. Incidentally, Malinowski had had similar experiences.

As for homesickness, I suffer little enough from it and very egotistically at that. I am still in love with [...] – but not consciously, not explicitly; I know her too little. But physically – my body longs for her. (Malinowski 1967: 15–16)

Maybe bashful and appalled by his own thoughts, he immediately adds, “I think of Mother [...] sometimes [...]” (1967: 16). Hortense Powdermaker (1966) was more open and daring in her diaristic writing, eventually making public her initiation into anthropological fieldwork. Looking back into her fieldwork among the Lesu on Latangai (then New Ireland) in the late 1920s, she writes in the chapter “First Night Alone”:

That evening as I ate my dinner, I felt very low. I took a quinine pill to ward off malaria. Suddenly I saw myself at the edge of the world, and *alone*. I was scared and close to panic. When I arrived, I had thought the place was lovely. Everything seemed in harmonious accord: the black natives, the vividness of the sea and of the wildflowers, the brightly plumed birds, the tall areca palm and coconut trees, the delicate bamboo, the low thatched-roofed huts, the beauty of the nights with the



moon shining on the palm trees. But now the same scene seemed ominous. I was not scared of the people, but I had a feeling of panic. Why was I here, I asked myself repeatedly.

There seemed to be no adequate reason: anthropology, curiosity, career – all seemed totally unimportant. *Why* had I come? (Powdermaker 1966: 53)

Justin Stagl mocks anthropology's fieldwork fetish and compares the anthropologist's existential crisis and personal learning process with a psychoanalytical voyage of discovery into one's own ego (2002 [1985]). The ethnographic novice will then have to successfully pass a phase of catharsis and endure uncontrollable and barely expected hardship before emerging as the triumphant hero (Stagl 2002 [1985], see also Baumann 2022, and Stodulka/Dinkelaker/Thajib 2019). This tactic, of course, would only work once the (male or female) anthropological Indiana Jones was back in an academic environment, claiming recognition and a successful career. To admit outright failure would have been unacceptable, of course, but the anthropological discipline has an academic culture that can render individual moments of crisis meaningful – albeit only in retrospect.

Anthropological fieldwork no longer has to be, or appear to be, a dire and mentally stressful experience. However, a certain challenging engagement can, admittedly, help open up new vistas. Fieldwork, as anthropologists do it, is inevitably intimate and personal – and crisis in the field is inevitably an immediate experience. We consider our being-in-the-world (Ingold 2011, Heidegger 2006: 2–15), our social and emotional entanglements (Kulick 1995, Dubisch 1995), and our resonances (Wikan 1992) to be instrumental to our research, to how we learn about and correspond with lifeworlds (Ingold 2017). We will always have to learn from scratch and *in situ*. After all, anthropology's starting point is not the top-down application of pre-defined models and theories, but the pairing of a fundamental lack of knowledge with curiosity and the desire to understand. In contrast to other disciplinary traditions in qualitative research, anthropologists do not have to hide themselves as acting and affective persons – less because of our own (negligible) importance than

simply as a result of dependence on the senses inherent in ethnographic learning (or what others might call “data collection”, Ingold 2017). Sensual impressions, however, are not truthful *per se*, as Adorno argues in his critique of classical phenomenology (1970), but they have to be subject to our interpretation and discussion. After all, the evolving fieldwork situation remains beyond “our firm control” (Amit 2000: 16), a fact that needs to be well reflected in our work. Peter Hervik has, therefore, tried to rehabilitate fieldwork’s decisive first impressions, taking them to a point beyond their anecdotal and self-legitimising character, understanding thoroughly that the immediate experience will soon be overgrown.

Today, I am no longer able to distinguish clearly between my general knowledge of Maya culture at the time of arrival and what I learned subsequently. My experiences of the first phase of fieldwork have to a certain extent been transcended by cultural models gained from shared social experiences. I can recollect feelings and understandings of the first-hand experiences, but I make sense of them in new ways, because the local knowledge that I bring into them evolved. (Hervik 1994: 86)

A field diary might be chaotic and disorganised, unfinished and unrefined, biased and full of emotions and contradictions. It can be a good friend in a time of crisis or (or as much as) a critical, omniscient and unforgiving self-other, which is what Canetti means when he speaks of a “cruel partner” (Canetti 1982, Wirz 2009). However, we need these notes and records of what we saw, heard, smelt, thought, and felt in quite an instrumental way. Beyond the immediate field situation, these notes will allow us to reflect – thereafter, when there is some distance – on the conditions under which we were learning and rationalise our attempts to understand. Thus, the field diary becomes a key document in our ethnographic archive, and it is subject to further study (Foucault 1973, Taussig 2011).

20.04.2004

[...] Yesterday evening, I saw military police hunting someone down. Seems to be a daily routine. [...] Due to Independence Day celebrations, lots of police in blue uniforms and MPs in camouflage can be seen. I am going to buy new trousers. (Personal fieldnotes, own translation)

In his “Vocabulary for Fieldnotes”, Roger Sanjek (2001) lists headnotes, scratchnotes, fieldnotes proper, fieldnote records, journals and diaries, texts and tape transcripts, letters reports and papers, etc. When I read his text, which is full of references, for the first time, I found it dry, far too detailed, even unnecessary. It took me some time to understand what it was ‘really’ about. Sanjek was attempting nothing less than the legitimation of anthropological knowledge as it is won during ethnographic fieldwork. Anthropologists are disciplined by the continuous work needed to elaborate more and more refined texts out of preliminary notes, and they are forced to summarise and sum up, to focus and structure. At the same time, emerging (and expanding) text production allows retrospective insights into past states of perception and knowledge as they occurred in our anthropological learning processes. Thus, diaries do not only invite self-dialogue and self-reflection in the specific moment of their writing, but they also do so over time – if we possess the right professional discipline and willingness to write. The diary becomes the cocoon for both product and process, the elaborated and refined ethnography, as well as the empirical and intellectual pathway towards it. Theoretical inspiration and evolving thoughts, methodological considerations and fieldwork experiences, early attempts to formulate findings and conclusions – all this can be traced back retrospectively. Yes, anthropological fieldwork is no longer objective and scientific in the narrow sense of the word. However, it is not just a personal story. It documents our epistemological development and hermeneutical learning process, it backs up our academic contributions, and it justifies the existence of anthropology as an academic discipline.

How, then, should I understand my own entry, the one where I had to buy new trousers? It would have been easier to omit this entry, so as to not disqualify my work by opening it up to ridicule. Two decades later, I can only loosely recall the situation. Looking back, I guess, I was out of my depth, unable to get along with what I was witnessing, but still a participant in the daily life that was going on despite everything, as people longed for some normality amid the exceptional. Furthermore, this statement can be read as a need to stay, to carry on and go out, to see people and interact with them despite the politically tight atmosphere that prevailed. This sounds strikingly laconic and unemotional, but maybe I was simply unable to say any more – an obvious sign of my own powerlessness. In later years, I would record people telling me about torture, loss, and fear. In contrast to my earlier field diaries, I would take much greater care to note and document what I was told, but I would also comment less and let the words speak for themselves. My later field diaries are thus far more systematic and structured than their predecessors, offering fewer immediate impressions and, consequently, having reduced room for self-reflection. Of course, by then, I was far more experienced and I also knew many more people, whom I had met during shorter visits to my respective field sites. However, this might also have protected me from what I was told.

### **From intimate impression to academic discourse**

Eventually, fieldnotes have to grow into academic contributions. They allow us to trace our finished work back to its beginning and show where our conclusions come from. However, two main problems remain, neither of which can be easily resolved: the accessibility of fieldnotes, and their interpretation.

If fieldnotes are the key to understanding ethnographic learning processes and academic writings, how can they be used? Should they be freely – or at least in limited ways – accessible? And, if so, how – in times of quick technological change? It is unrealistic to demand total transparency, since very few anthropologists would fully comply, fearing

harsh criticism, and sometimes consequences, for one's professional career. I do not see another way other than to appeal to anthropologists' common ethical responsibility, and to do so in the name of our common interest in the general transparency of academic debate and the construction of our arguments and conclusions. Fieldnotes do not have to be fully accessible, but anthropologists should allow partial access to them, in order to offer partial insights into their role. In this way, anthropologists can explain how they reached a certain conclusion, how they developed a certain argument further and even, perhaps, how they came to revise that argument thereafter. Fieldnotes are attempts to grasp surrounding lifeworlds from a necessarily personal perspective, and so they can never be objective and purely academic. Textual work is required to grow fieldnotes into academic (and academically interesting) publications. Notes have to be refined, organised, and properly edited in several steps (as Sanjek has shown). Looking back into this process, and providing, as they emerge, selected insights to others, will enrich our debate and render it more dialogical and less personal, less pseudo-heroic.

Who, then, has the right to judge? Those, who have been subject to the study, and their successors? Academic colleagues? The public? Just the author? Or all of us? I would not dare to give a general answer. In any case, our critique should accept that fieldnotes inevitably have a *preliminary* character. Of course, fieldnotes can be most interesting, particularly from a historical point of view. Fieldnotes mirror more than just personal encounters and immediate reflections. With some historical distance, we can expect to find in fieldnotes and diaries anthropological approaches and intellectual fundamentals, as well as wider political discourses, effects and the fragmentations of a certain era. It is probably easier to criticise what was written in a private, lost and lonely moment in a distant past than it is to capture ideological backgrounds and intellectual shortcomings in one's own work. However, it is an undeserved privilege that we, today, are able to contextualise earlier anthropologists' perspectives beyond what they could see and guess at the time.

Self-reflection and autobiography – Dilthey’s starting points – are not primary and are therefore not an adequate basis for the hermeneutical problem, because through them history is made private once more. In fact, history does not belong to us; we belong to it. Long before we understand ourselves through the process of self-examination, we understand ourselves in a self-evident way in the family, society, and state in which we live. The focus of subjectivity is a distorting mirror. The self-awareness of the individual is only a flickering in the closed circuits of historical life. *That is why the prejudices of the individual, far more than his judgments, constitute the historical reality of his being.* (Gadamer 2004: 278, emphasis in the original)

If it is true, then, that others might see more than oneself in one’s own fieldnotes – now, but even more so in future – what will they find? There may still be some sort of heroism, although its currency of success has changed. How do today’s academic capitalism (Münch 2014) and competition, the scramble for grant money, impact factors, careers and new buzzwords sediment into our very personal learning processes and our attempts to note these down? In the immediate crisis, we understand well that fieldwork is not an aim in itself, that we have also come for academic merits and a potential career: “*Lenfer c’est nous*”, ‘hell is us’ (Lévi-Strauss 1968: 422).

Still, it is our learning process, an inevitably personal one, that allows us to make a valuable contribution to the academic discourse and, perhaps, beyond. Fieldnotes documenting this process are fundamental to our academic contributions; they show how our arguments are built on an empirical base and they accompany our developing thoughts. They also show us as an unfinished learner and a “Child of Our Time” (Horváth 1939).

I think we had best soft-pedal the self-righteousness: We do what we do, create what we create. We should take pride in doing it as well as we can. But it’s not a bad idea now and then to take a look in that mirror we are so anxious to turn on others and to face some of the tensions in a role that we often need to explain and sometimes need to defend. (Wolcott 1995: 153)

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