**2 June 2022**

**Accepted Draft Book Chapter for Nerina Weiss, Linda Green and Erella Grassiani (eds.). *The Entanglements of Ethnographic Fieldwork in a Violent World.* Routledge, 2022**

**Chapter title:**

**Involved and Detached: Emotional Management in Fieldwork**

**Anna Hedlund, Lund University**

**and Steven Sampson, Lund University**

Even in the most benign situations, ethnographic fieldwork is always an emotional experience.[[1]](#endnote-1) How could it be otherwise, when we use our very selves as our research tool while interacting with other human beings? Ethnographic settings are hazardous in different ways. Some are overtly dangerous or spontaneously violent, while in other situations, there is insecurity, intimidation or implied threat. Under these cases, emotional pressures increase. The ethnographer must therefore learn to ‘read’ a situation which is inherently uncertain, insecure, perhaps secretive. We need to decipher signals, codes or messages which even our own informants may not be all too sure of. Situations of insecurity may thus vary, as do the consequences. This paper provides two examples of uncertainty, insecurity and how real or potential violence affected our emotional reactions to fieldwork. It draws on our own experiences from fieldwork in, respectively the eastern Congo (DRC) during 2012–2014 and in pre-1989 Romania. Hedlund’s fieldwork, among a group of Rwandan rebel military units in the Congo, was marked by the continuing threat and occurrence of violent confrontations. Sampson’s fieldwork in communist Romania took place with people who were surveilled and intimidated by the Romanian secret police and a network of informers. In both these situations the emotional challenges were marked by insecurity, frustration, fear for oneself and anxiety about one’s informants. In this sense, while these two fieldwork situations certainly differ, we believe that they can be instructive in revealing the dangers of being too close to one’s ‘people’, of being too involved.

 It is an axiom of social anthropology, in contrast to the other social sciences, that an emotional engagement with our interlocutors via participant-observation is the key to truly understanding the worlds of others. Typical accounts of fieldwork, while they describe the practical problems of access, building rapport and obtaining data, also describe the fieldworker’s position, emotions, frustrations and anxieties. The problem is often depicted as being an 'outsider', and the standard solution is to keep immersing oneself, and to sustain our empathy and solidarity with informants and their life situations. Our emotional engagement, the inserting of our participatory ‘self’ into our ethnography, is supposed to create insight. This preference for ever more proximity, friendships and engagement, however, has a down side. It may mislead our research process and distort our theorizing. Sometimes, we need to take a step back. We argue, therefore, for a dialectic between emotional proximity and the need for restraint. This restraint we call ‘emotional management’. We argue that ‘letting go’ of our emotions does not necessarily always help us gain anthropological insight. Rather, there are points and junctures in the fieldwork and post-fieldwork process where we should keep our emotions in check, try to regain control or simply ‘back off’ from the field, at least for a moment, when we analyze our data. It is this kind of control that we will call ‘emotional management’.

 The problem of emotional and existential dilemmas in tense, insecure or violent settings has been raised by other anthropologists (Maček 2016, Weiss 2016). These studies describe how settings of conflict or crisis affect the researchers and research topic. They remind us of the need for caution and sensitivity when working in volatile or violent settings and how 'empathic knowledge' can help researchers to deal with emotionally demanding material (Maček 2016). From these studies, it is clear that we cannot divide the emotional field into simply two poles, with solidarity and engagement on one side as the open, expressive and therefore good; versus a more repressed and closed detachment which is by definition bad or unproductive. Rather, 'empathic knowledge' can broaden our understanding of demanding field sites, and help us achieve deeper knowledge of violence, oppressive states, or other sensitive subjects (ibid).

 In this light, we view emotions on a continuum of expression, and our use of the word ‘management’ is meant to imply a more nuanced view of our own emotions as we conduct fieldwork and analyze data, even in situations as tense and unpredictable as Eastern Congo and communist Romania. The ‘management’ approach, we argue, can help us in two ways: first, we can gain a better understanding of the people and situations in which we find ourselves; second, we can formulate more productive theories about how people live in vulnerable or violent situations.

 How do we acquire and then apply ‘emotional management’ while we are ‘out there' doing fieldwork? One obvious strategy is for ethnographers to become familiar with messy stories of emotional ambivalence during fieldwork. Stories of such ambivalence help us acquire some kind of reference point for our own experience. Such a reference point might take the form of, ‘I am in the same kind of tense situation as was X when she did fieldwork in Place Y’, or ‘I don’t want to make the mistakes that A did when he did fieldwork among the Group B’. It is these kinds of messy fieldwork stories that we will present below. The stories are not guidelines or checklists for conducting the perfect fieldwork. They are illustrative cases of dilemmas where we really do not have the answers. These dilemmas gave us anxiety and concern, even after years of reflection. As human stories rather than purely scientific problems, they have an emotional component to them. They are our experiences. Which is why they are messy. Both of us carried out fieldwork in partially unpredictable settings (the unpredictability due partly to our own naivety), and we both experienced some emotional roller-coasters. Nevertheless, we believe that the emotions eventually helped enhance our understanding of our field situations.

Feelings as knowledge production?

Conducting fieldwork is an intersubjective activity. Our objects of study – other people – are themselves subjects. We use ourselves as research tools, which gives our research an emotional component. Ethnographic fieldwork is about positioning oneself while at the same time describing someone else’s lifeworld. It is proximity and distance at the same time. Anthropology has acknowledged that our research questions, analytical frameworks, methods and conclusions are themselves products of our own positions and ‘frames of reference’. These frames have an emotional aspect to them, in so far as we are not just engaged but at times profoundly committed to our scientific mission, including the mission of ‘successful fieldwork’. Nowadays, it is necessary for anthropologists to present their own self-reflections in order to show how their ‘positions’ (e.g., age, gender, ethnicity, class) may have influenced knowledge production in the field. One of the positions often missing, however, is our emotional position. Every fieldworker thus experiences moments of intense involvement and then detachment or distance. In our ethnography, we are in the position of being involved and detached at the same time. The ethnographic project is to enter a community (or organization or network or ‘assemblage’, be it close to home or far away). We are supposed to hang out where we can, get to know people, observe their activities and their life situations, and then at some point exit, perhaps returning, but always with the possibility of exit. Age, gender, race and class aside, being an outsider (even if we do fieldwork ‘at home’) is a very ambivalent position. The researcher can walk out of the village and back to the missionary station, or today, return to the NGO aid office; we can pack up our bags and notes and return home. Even when doing fieldwork close to home, we can leave the organization or group that we have chosen to study, we can reduce our fieldwork when grant funds run out, we can reduce our lines of communication, stop logging in and return to the university or just move away. While it is true that our informants can now easily contact us, we can decide not to respond, we can unfriend or unfollow when things become too intense or unpleasant. No matter how much solidarity or friendliness we have with those we study, our outsider status, for all its problems, is a position of privilege. And this kind of privileged position gives us a very different outlook on the risks and emotions involved in fieldwork, as we shall describe below. In particular, it demands, besides sensitivity, responsibility and reflexivity, a particular kind of emotional management. Emotional management takes place while we are in the field, when we exit the field, and for some of us, for a long time afterwards.

 Here we will describe two ethnographic fieldwork stories where emotional management was part of our fieldwork. For simplicity’s sake, we can call this a dialectic between involvement and detachment. Detachment, however, is not a lack of emotion. It is a tactic for balancing intimacy and distance. The one setting we describe is Hedlund’s fieldwork with an armed group in the Democratic Republic of Congo. This group had violent confrontations with other militias and with government troops, and they also perpetrated attacks on civilians. The other setting is Sampson’s fieldwork in Romania during the Ceausescu era, when he was tracked by the secret police as a potential spy, declared ‘persona non grata’ on entry, and where his informants were followed, interrogated and even threatened. Hedlund’s situation was one of insecurity and physical danger. Sampson’s was one of surveillance, suspicion and paranoia. Both led to strong emotional responses during and after our fieldwork, especially anxiety or guilt about interlocutors. And both fieldwork experiences were for us lessons in emotional management. After presenting the two cases, we offer some concluding remarks on how to navigate the dialectic between involvement and detachment in fieldwork, suggesting that the two concepts can help us 'manage' emotions in fieldwork.

*Example 1. Fieldwork with armed groups in the Congo*

It is not a new insight that fieldwork in so-called ‘conflict zones’ poses unique challenges to research ethics in terms of the obligations of the fieldworker, responsibility towards those with whom we do fieldwork, as well as the attendant difficulties in establishing trust and long-term relationships (see for example Wood 2009, Vlassenroot 2006). Many anthropologists have also highlighted the dangers of conducting fieldwork among hostile groups and perpetrators (Sluka 1995). Others have discussed the epistemological consequences of analysis and interpretation in settings where it is difficult to build rapport and trust. How does the researcher’s conceptualization of a ‘conflict zone’ and violence, including gratuitous violence, affect both the researcher and those ‘being researched’? Furthermore, anthropologists have discussed how researchers themselves cope with violence and trauma and how ‘existential shocks’ (Robben & Nordstrom 1995) affect analysis and theory building (see for example Cyanne and Simoni 2014, Kovats-Bernat 2002). A further identified concern are the analytical problems that might arise if researchers begin to empathize with perpetrators or fall into what Antonius Robben (1996) calls ‘ethnographic seduction’. We might misinterpret our own friendly feelings for our informants as ‘good data’, thereby compromising the kind of objectivity and critique that is essential to any analysis (Jauregui 2013: 144). Other scholars have raised the possibility of the political hazards associated with conducting research among perpetrators of violence. The perpetrators themselves may feel empowered by the recognition and attention given to them by researchers; furthermore, the researcher, by writing about groups and people who commit violence, may also become unintentionally responsible for reproducing ideologies of violence or even accused of culpability. These are all legitimate concerns. Research is never a neutral exercise (Cloeke et al. 2000), but where a peaceful setting may serve to mask the tensions between different interests and objectives, fieldwork in a conflict zone polarizes these tensions, throwing them into sharp relief. Even though fieldwork in insecure settings poses ethical and moral challenges, many anthropologists, including the authors cited above, argue that it is essential that we engage with those who carry out violence if we are to improve our understanding of conflict and of the root causes of violence. I will elaborate this issue by describing my Ph.D. fieldwork with a group of rebel combatants in eastern Congo.

 As a doctoral student at Lund University (2009-2914), I conducted long-term fieldwork in the eastern Congo (DRC). For decades, the eastern Congo has suffered from war, poverty, disease, refugee crises and intermittent conflicts between various rebel groups, militias, political factions and governmental troops. As part of my fieldwork, I lived in a rebel camp controlled by a group who had fled Rwanda in the 1990s and were now living in the Congo with their families. Since fleeing Rwanda, partly due to accusations of having participated in the 1994 genocide, this group has carried out vicious acts of violence against the Congolese population for more than 25 years. Violence and conflicts, and the need to flee from attacking troops to a safer camp deeper in the forest, were now a part of their everyday life. In the camp where I conducted my fieldwork, I spent time with both the armed combatants and their wives and children. The camp was run by its military leaders, but it was also vulnerable. It was home to many refugee families, and it could be attacked by Congolese military forces and other armed groups. The group had a hierarchical structure and a political and military agenda based on liberating Rwanda from what they viewed as an illegitimate regime. Propaganda against the current Rwanda government was part of daily life in the camp. In my research, I sought to understand the motives behind acts of violence from the perspective of the combatants, how the group was organized, and how their ideologies justifying practices of violence were transmitted from one generation to another. My research, therefore, focused on the daily life routines inside the rebel camp and the structures and behaviors behind these. Why did the group continue to fight what seemed to me to be an endless war? How did they justify acts of violence? I have addressed these questions in other publications (Hedlund 2015, 2017, 2019). I have also described the methodological and ethical difficulties in conducting fieldwork in a conflict zone with people accused of war crimes, and who are still actively conducting violent military operations in the Congo against both military and civilian targets. In these studies, I have tended to downgrade my own emotions throughout the process. I overlooked how I confronted, or ‘managed’ emotions such as fear, insecurity and confusion throughout the period of fieldwork, and long after the fieldwork ended. In this retrospective vein, let me look back on the emotions during my fieldwork.

*Managing emotions in contexts of insecurity*

Any anthropologist who spends time with the people they study inevitably becomes emotionally affected by their life stories. This involvement is even more pronounced when the situations include the high intensity emotions connected to conflict and violence, with accounts coming from victims, witnesses and combatants. Although I had prepared for how to ‘manage’ danger and risks by reading previous ethnographic research, ethics discussions in methods books, and various risk assessments, it was difficult to fully plan for the fieldwork and remain in a position of control. In the Congo as anywhere else, fieldworkers are at the mercy of fortuitous events and situations.

 As described elsewhere (Hedlund 2019), when I was invited to visit the military camp for the first time, I stepped into an unknown setting of combatant and rebel activity. I had to learn the functions of hierarchy and military discipline, I had to address the soldiers by their rank rather than by name, and I soon learnt not to question the authority of the leaders. While the early weeks of the fieldwork were characterized by mutual distance, I never experienced any real hostility from either the rebel soldiers or residents of the camp. I was treated with respect and courtesy, and I felt that people in the camp were genuinely positive about having a visitor in the camp (even if it was also obvious that they used my foreign position as a vehicle to transmit propaganda). My fieldwork consisted of regular participation in daily life and interviews and conversations with combatants and their family members. In the camp, I simply walked around carrying a tape-recorder, pen and notebook. In the daytime, I took part in the ordinary life of the camp, following the soldiers to the bamboo churches to pray, to the fields looking for food, and conversing with them and their families. During the evenings, I would sit by a fireplace speaking to the leaders and other high-ranking soldiers living close by. By participating in daily activities, I sought to gain insights into how my informants experienced their lives and to understand what it is like to live in the midst of insecurity (I did not participate in any military operations or violent attacks that were conducted outside the camp). Although camp activities were peaceful, violence was still present in narratives and in everyday life; the camp was highly militarized, combatants carried guns with them, and people regularly spoke about violence, threats, traumas and danger related to their personal history and present situation in a war zone. Following the unpredictability of war and the fact that conflicts can quickly escalate there was also a sense of danger surrounding the camp. People in the camp said they were living in fear of being attacked by enemy groups. The fear of violence was also close by, because many of my rebel informants were perpetrators of violence inside the camp as well. The atmosphere in the camp, therefore, was not one of simply a refuge from violence outside; there was also everyday fear inside the camp. After only a few days in the camp, I heard stories of how lower-ranking soldiers, many of whom were forcibly recruited (kidnapped) from villages that rebels had attacked, would be killed or taken to the ‘forest prison’ if they tried to escape from the camp (although I am not sure this is actually true, I heard similar stories several times). While the camp commanders spoke freely to me, the lower-ranking soldiers, women and children had less freedom of speech. They were the objects of manipulation, propaganda campaigns, compelled to make various ‘performances’, and generally deprived of their individual freedom to express their opinions, talk to me informally, or leave the camp.

 Fieldwork in this context posed methodological, and, of course ethical dilemmas. One dilemma was my dependence on the rebels for practical advice, and my need to trust them to ensure my personal security. During fieldwork, I was completely reliant the rebels’ knowledge of the local situation. I had to trust their advice on where to go and where not to go. My control over the situation was turned upside down – it was the rebels, not I, who had control of my fieldwork situation. As an outsider, I had to trust the rebels with whom I lived, and I had to acknowledge their stories 'as truths' in order to be included.

 Conducting ethnographic fieldwork in an insecure and unpredictable region is also emotionally tense. I often found myself being suspicious of what people said, and I sometimes had doubts about whether I could trust my interlocutors. I felt a lack of control over the situation, and I experienced difficulties getting close to my interlocutors. I sometimes had doubts about my own security, and I worried about all the things that could go wrong in a conflict area. I wondered to what degree I could trust my informants accounts and advices and what I would do in a situation of real danger. I also felt guilty about being close to a rebel group that had been causing so much harm to others. In the field, I knew that the safest way to conduct fieldwork was to be accepted by the group. Becoming included would enable me to get access to more dimensions of their lives. As a way to ‘manage’ the many feelings on a personal level, I kept notebooks and field diaries. The daily writing became a means of not only reviewing my data but also as a way to regain some kind of control and to manage my feelings of anxiety, fear and suspicion of others. However, just because I realized that these emotions were perfectly normal and predictable under the circumstances of fieldwork did not reduce their intensity. Although I sometimes had doubts about my fieldwork and the potential danger it could pose to myself and my interlocutors, I did not experience my fieldwork as naive or dangerous at the time. This was partly because I was working with co-workers whom I trusted but also because I believe that my interlocutors had a genuine interest in talking to me about their lives so that their situation could be known to outsiders.

 In anthropology, we know the importance of integrating our subjectivity into our research. We have to learn how to balance the situation, how to be involved in our informants’ daily lives (to get good data but also out of care for our interlocutors) while also learning how to detach ourselves. Interpretation, including interpretation of ethnographic data, requires some detachment from the situation. This is why analyzing fieldwork data takes months or years. The detachment is necessary so that we do not misinterpret the data. Achieving this detachment in violence-prone situations is difficult. One technique of achieving detachment is to be aware of signs of over-empathizing with informants, what Robben (1996) called ‘ethnographic seduction’.

 Robben (1996) notes that perpetrators of violence often begin by depicting themselves as ‘the good guy’, presenting themselves as victims and describing their violent acts as purely self-defense. As an example, Robben (1996: 73) gives an account of his interviews with high-ranking generals of the Argentinian junta (1976-1983). Despite knowing that they were widely accused of torturing civilians, kidnapping and causing the ‘disappearance’ of innocent people, Robben found that his informants met him with the utmost kindness, courtesy and gentlemanly good manners. Robben soon found himself in a situation where he was interviewing men whose politics he detested, but for whom he grew to feel a personal liking and attachment. In this sense, Robben’s experience is very similar to my own.

 My informants had a political project that included defense (and denial) of having participated in the genocide in Rwanda. They were views that I detested, but I did not endeavour to argue or provoke them, which only let me to feel more guilty. Even though my interlocutors always treated me with courtesy and politeness, I still knew that some of them had carried out brutal acts of violence and had views I deeply detested. People in the camp often spoke to me about their suffering in the forest, and much of the group's collective identity was based on a shared victim discourse. At first, I was swayed by the presentation of the victim discourse and people's stories of suffering. Indeed, who would not sympathize with refugee families, in fear of attack by government troops or other militias, seeking only to return to their home country and seek justice? My interlocuters repeatedly spoke about their misery, their fears of enemies and daily hardships, such as lack of shelter, food, and medicine. At the same time, it was obvious that some of the commanders were distorting the truth about their past history, denying and giving me false information (for example about the genocide in Rwanda). It was also obvious that they tried to promote their political agenda and convert me to their cause in the hope that as a foreigner, I could be useful to them. For them, I was a sympathetic listener and a useful political tool.

 Reflecting back on these experiences, I know there were times when I was blinded by the friendly relationships, I had with my interlocutors, and I often struggled against becoming biased or swayed by my own sympathy for their everyday predicament. This involvement took on many forms, such as feeling sorry for the women and children in the camp, being pleased that the high-ranking rebel leaders were nice and polite (denial of the real situation), feeling suspicious about the narratives they provided, but also having a genuine curiosity in trying to understand the root-causes of groups who were clearly committed to a violent way of life. It was sometimes difficult to manage emotions in the field and not analyze people’s stories from my personal moral or ethical standpoint. Years later, sitting in Sweden and having since acquired some emotional distance, I now find it easier to reflect upon individuals’ responsibility for their acts of oppression and violence against others. Some of my informants were war criminals, and I ‘represented’ them in my monograph. The issue here is not under what circumstances we legitimate the victim discourse and under what circumstances we dismiss, denounce or undermine it. Rather, the problem is to find a way to present the worldview of the people we study in a way that does not also promote their ideology. Our task is to represent the perpetrators without necessarily empowering them. This is a dilemma that I have not yet resolved.

 The solution, I think, is to take a step back, detach from some of the more intense feelings, to listen to what people say and put aside some of my personal judgements or moral standpoints. I spent much time thinking about this while writing my dissertation. Instead of only dismissing the victim ideology or describing the rebels’ propaganda as ‘false’, I let the ethnography speak for itself. This helped me to theorize about identity, meaning-making and shared ideology, which in the end provided a deeper understanding of how this particular group legitimized its acts of violence, both within the camp and towards the outsiders whom it attacked. Allowing the ethnography to speak provided an understanding of the way in which suffering is embedded in their own justification of violence. It can reveal how ideologies of violence are transmitted from one generation to the next, as well as showing how central these ideologies are to the group’s very existence as a group.

 Above, I have outlined a few dilemmas in the field. Emotions cannot fully be detached from the fieldworker, nor should they. However, we can learn how to manage and control them. As mentioned in the introduction, a privilege that many anthropologists have is that we can escape the field and go home, even when the field is in our own home towns or countries. It is often when we are back home that we can sit down and reflect upon our data, re-read and re-analyze our fieldnotes, and try to make sense of the messy reality we have observed and the experiences we had. We read our fieldnotes over and over again, recall how we felt in specific situations, and reflect upon what people have said. These processes create that detachment which is necessary for ethnographic analysis. We don’t want to repress or neglect our feelings. But we should try to manage them.

*Example 2. Paranoia in Romania*

Paranoia is an unreasonable fear that one is being watched, threatened or persecuted. To call someone ‘paranoid’ is to imply that their judgement of reality is distorted or flawed, that they are irrational. If you feel like there is a ‘they’ and that they are ‘out there’ watching you, that you are under threat from an unseen enemy, well maybe there is something wrong with you. You are suffering from paranoid anxiety.

The feeling of being watched generates not just anxiety, however. It can also generate inflated feelings of importance. You feel important because you believe that others, especially others with some kind of special power, find you important, if not threatening. According to the cultural theorist Sianne Ngai, paranoia is one of those ‘ugly feelings’ (2009). Unlike the grand emotions of anger, fear, passion and jealousy that can inspire toward great literature, ‘ugly feelings’ like cynicism, boredom, irony and paranoia are feelings that cannot be fully resolved or expressed. Paranoia, Ngai argues, is a way in which the subject constructs a world together with the object, a world of power in which the subject is now a player. Paranoia, conspiracy theories, and similar delusional emotions are in this sense empowering. You are an important person for the powers that be. So you look out for them. Even if you’re not sure they are there.

It was this combination of watchfulness and self-importance that I developed when I first went to Romania to conduct fieldwork. The year was 1974. I was a Ph.D. student at the University of Massachusetts-Amherst. Romania was a communist authoritarian state under the brutal Ceausescu regime. After six months fieldwork in 1974, I returned in 1975-1976 as part of an official academic exchange program between the United States and Romania, administered by the Fulbright Program (named after U.S. Senator Fulbright) and financed by the International Research and Exchanges Board (IREX), which manages exchanges to Eastern Europe. Like the other American exchange researchers in Romania, I was assigned a local academic advisor, and this advisor, a sociologist, was supposed to facilitate our research. However, the advisor also reported about us to the authorities, including the Romanian secret police, the *Securitatea*.

 I have written about this fieldwork elsewhere (Sampson, 1984, 2019, 2020; Kideckel and Sampson 1984; Sampson and Kideckel 1989), so I will not go into details on the larger context (see www.stevensampsontexts.com). Suffice it to say that I was doing research in a Cold War setting. For the Romanian regime paranoid about spies, an American researcher, even if he said he was an ethnographer, was someone to be watched. For diplomatic reasons, Romania allowed a small number of American researchers into the country, most of them ethnographers, folklorists, linguists, historians, etc.). As American grantees, we were oriented about the political environment in which we worked. We were instructed by IREX and by the U.S. Embassy cultural attache to be careful about what we said and did. We should not initiate political discussions, distribute anti-regime or religious literature, nor should we carry out any illegal activities such as smuggling icons or dissident manifestos or do illegal money changing. We should assume that our hotel rooms were bugged with microphones, that our activities in the villages were being monitored, and that anyone who talked to us, no matter how friendly, might be an informant for the secret police or simply called in for interrogation. In short, we should assume that the Romanians thought that we had some kind of ‘mission’, that we were spies of some kind (this atmosphere is described in detail in Verdery 2018).

Americans and West European scholars who went to Romania, perhaps 10 a year, tended to live in the capital of Bucharest or in the university towns of Cluj or Iasi. Here they could be easily followed, literally from door to door. We anthropologists had a bit more freedom: we were out in the villages, living amongst the villagers (after receiving permission), and we had learned to speak Romanian. Precisely because of our unique situation, this also made us especially interesting targets for observation.

The Romanian security organs assumed that any person from the West (and even from Hungary) was an agent or instrument of some espionage service. Spies undercover secrets. Did we anthropologists, doing fieldwork in villages, uncover secrets? Yes we did. Because a secret is not necessarily something of high strategic or military value. A secret is any kind of knowledge that a political regime does not want others to know. As ethnographers, the secrets we discovered were about how Romanians really lived and how Romanian society worked. The secrets that I discovered were what most Romanians already knew: that things did not work according to the plans, that many policies were propaganda and many actions pure theatre. This was ethnography for me, and in anthropology we learn that the ideology and the reality never match. But these same contradictions, that things did not go according to plan, were strategic knowledge for the Romanian authorities. It made me and my work a threat. It made anyone who talked to me suspect.

*Was I a spy?*

As an academic, I disseminated the knowledge that I had acquired in Romania in the form of conference papers, academic articles and articles and presentations in mass media. This kind of knowledge could be harmful to Romania, a country that had just signed the Helsinki Declaration of Human Rights and was under Western pressures of various kinds. This pressure came from governments and from various Romanian and Hungarian anti-communist exile groups in the West. The assumption that we could be connected to this agenda, that we were spies, was therefore not unreasonable when seen from the regimes point of view. After all, we researchers had U.S. government scholarships, we were there on a government exchange program, and we had relations with people inside the U.S. embassy, where we could collect mail from the diplomatic pouch and have a pizza. Some diplomats even visited us in our villages. One day I was even visited in the village by the United States Ambassador in his giant Cadillac limousine, with the US flag. In short, the Romanian authorities were paranoid about us, and it turned out that we were naive about them.

 In the village where I did my fieldwork, I lived with my wife in the home of a Romanian peasant family. Just outside the village was a secret uranium factory, called ‘Factory R’ (the factory is not secret anymore and has since been closed). Many young local men worked there. I was not interested in the factory as such, but of course I met some of these young men when I went to the village bar or visited their homes. I was not a spy, but everyone in the village knew that since I was approved to live there, that I must somehow be connected to higher powers. How else could I be there? They were correct. I was connected to higher powers. The authorities had allowed me to live there, the local mayor had an official letter from the regional administration. So that made me special. I was American in a Cold War context, with the mystique of American strategic power and American popular culture (TV shows such as ‘Columbo’ and ‘Dallas’ on Saturday nights). I wore blue jeans, had a beard and drove a foreign car. As a foreigner in Romania, I also had special privileges. I could shop for Western goods in special ‘dollar shops’ using my U.S. dollars (which were illegal for Romanians). Every anthropologist has similar experiences of being a stranger, having special resources that attract some people and create suspicion among others. Being special in Cold War Romania had its unique combination of privilege and suspicion.

 Like any ethnographer, I went around the village, visiting people in their homes, at workplaces and in the fields on the collective farm. My research focused on village planning and infrastructure. No one could figure out why I would be interested in their village, except for the presence of the secret uranium factory. Once in a while, our conversations got around to topics of espionage and spying; sometimes in a humorous or nervous way, since Romania also had its own secret police which for decades had been spying on its own population. I tried to explain in a rational way: if we Americans wanted to find out what was going on here in the village, we did not need to send an American student with a beard and blue jeans, driving a foreign car, with a blond wife, into the village to be a spy. We had satellites for that, I explained. I also tried to explain what an ethnographer was. Most Romanians knew about *etnografi* and *folclor*, which was normally associated with folk music and folk traditions. This village had nothing of this kind. What kind of ethnographer was I, wanting to study urban planning and infrastructure? Why would an ethnographer want to see household census documents or go to local communist party meetings? So again, I was a suspicious character. It was frustrating. There was no central authority I could talk to who could clear up this confusion about me. And I also know that far up into the Romanian security apparatus, they believed that I had some kind of secret mission. I know this because I have been able to review my secret police file, all 600 pages, containing interrogations, reports, observations, and photographs. The files spend a lot of space trying to figure out my true mission.

During my fieldwork in the village and at the planning office in the town of Brasov, I spoke with peasants, young workers, pensioners, planners and party officials. I drank beer with schoolteachers, played chess with the dentist, attended weddings and parties, and made friends with several village families. I finished my village fieldwork in 1976, after 18 months, but returned for a few weeks each year. From 1980, I undertook another research project at the Romanian Communist Party Training School in Bucharest, where I studied village leaders and visited other party training schools in the provinces. I had thus developed a network of friends, colleagues and informants in Bucharest and in other towns, and in some villages. This network included party officials, urban intellectuals and ordinary peasants and workers. It also included many Romanians seeking to emigrate from the country, which was a brutal Stalinist dictatorship. Through the mid-1980s, I returned to Romania usually twice a year, sometimes with my wife and child, doing research, updating my data and just visiting friends and colleagues. I felt at home, and people felt at home seeing me when I returned after many months away. It is a familiar emotion for most anthropologists when people say, ‘Oh now, you have returned to us again.’

 During the 1980s, however, Romania’s political and economic situation deteriorated. There were shortages of food, electricity, heat, everything. There were also worker protests and small human rights movements among intellectuals and the Hungarian minority in Transylvania, where I worked. The government’s suspicion of foreigners also increased. A law was now enforced making it illegal for any foreigner to stay overnight in a Romanian home. In addition, any Romanian who even spoke to a foreigner had to submit a written report to the police. Academic contacts were restricted, and all communications between foreigners and Romanians – letters, phone calls, visits, etc. -- were monitored, censored, controlled or simply forbidden. My own secret police file contains reports by police observers, statements written by ordinary Romanians explaining how they met me and what we talked about, copies of papers that I had presented at conferences abroad and letters I sent into and out of the country. In Denmark, where I have been living since 1978, Romanian diplomats in Copenhagen also made their own reports about me, having met me at meetings of the Danish-Romanian Friendship Association or when I applied for a entry visa. I was a ‘person of interest’. I was approached by the embassy and asked o gather information about exile Romanians living in Denmark; and by the Danish security police regarding a suspected Romanian spy living in a Danish asylum center. One night a Romanian diplomat, the embassy’s political officer, knocked on the door of my apartment with a gift of football tickets to the Denmark-Romania match. His report is in my file. Yes, life during the Cold War had this curious combination of paranoia and excitement. Why were the Romanian authorities and embassy so interested in an American anthropologist researching village planning working at the time as a teaching assistant at the university? One uses up emotional energy simply speculating on this

 The Romanians’ interest in me derived from the fact that many of my articles depicted socialist Romania in a negative light (see www.stevensampsontexts.com). I had written about planning and improvisation, bureaucracy and corruption, the underground economy, political rumors, emigration, Radio Free Europe and the secret police itself. My publications were also floating around the country among my Romanian friends, and they were found among the possessions of other foreign researchers in Romania. I had given interviews on the BBC and written for Danish and U.S. media. A few privileged Romanians who visited me in Denmark or who met me at conferences also made reports about me and our conversations. Some told me never to visit them in Romania, that they would get into trouble. Others invited me into their homes whenever I came to Romania, but they also filed reports with the secret police afterwards. Everyone had their reasons, as Katherine Verdery (2018) has described in detail. By the mid-1980s, I had become dangerous enough to ‘qualify’ as a threat to the state. The final pages of my secret police file detail my arrest at the Bucharest airport in 1985, when I tried to enter Romania on holiday with my wife and two young children. I was now persona non grata, my file stating that I had ‘conducted activities that were contrary to the interests of the Romanian state’, and that I should be denied entry into the country for a period of five years.

‘*Don’t visit us again. You only make trouble.’*

Let me now return to the years just before I was denied entry into the country. After ten years **visiting Romania, I had made some close friends. Two of my closest friends were ‘Andrei’ and** ‘Maria’ (pseudonyms). My wife and I had originally lived in the village with Andrei’s uncle, so we got to know each other at these family gatherings. Andrei and Maria lived in the city of Brasov, 20 km away, in an eighth floor apartment, with their young daughter. Andrei was an engineer in a tractor factory, and Maria worked in an architectural planning office. They were my age and very personable, and whenever my wife and I went to Brasov, we invited them out for dinner or just sat around and talked in their home. When I visited them, we could relax, gossiping about people in the village or talking about conditions in Romania (in a low voice). I would bring along some whiskey, cigarettes or Toblerone chocolate from the ‘dollar shop’, where Romanians could not enter. When visiting from Denmark, I would bring Danish baby clothes for their daughter or cosmetics for Maria. Over the years, we exchanged letters and phone calls, and I visited them whenever I came to Romania. They even invited me to stay overnight in their home illegally. Of course, we talked about them visiting me in Denmark, a procedure that required me to send them a formal invitation, after which they would apply for a passport to visit the West.

 By the mid-1980s, my visits to Romania had become more stressful. I could not obtain a research permission, so I visited on a tourist visa, sometimes with a Danish charter group going to a nearby ski resort in Brasov or to the Black Sea beaches, and then went off on my own. I tried to remain inconspicuous, traveling light and speaking as a Romanian. Visiting Andrei and Maria in their apartment at night, we talked softly. We did not go out anymore. I used the same procedure with other Romanian friends. I was paranoid, but as it turned out, naïve.

 In 1983 I sent a formal invitation letter to Andrei and Maria to visit me in Denmark. They would now have to apply for a passport, which involved extensive investigation and interrogation by the passport bureau and the security police. For Andrei and Maria, my invitation thus led them to have several interviews with the Romanian police and the security organs regarding their relations with me. Since I was not a relative, and since my name was already in their files, my invitation to them was suspicious. How did they know me? What had I asked them about the factory in the village? What about the factory in Brasov where Andrei worked? Did they know that I was a spy? Did not Andrei, an engineer in a strategic factory, understand that he was being used by me? Didn’t he understand that I had invited him to Denmark in order get secrets out of him? After some weeks, their application was rejected, and they were told not to correspond with me or meet with me if I came to Romania.

In the summer of 1984, I returned to Romania for a brief visit. I had written to Andrei and Maria that I was coming during these days. I landed in Bucharest and took a train to Brasov and made my way to their apartment. But they were never at home. For several days, every time I went to their apartment, there was no answer. Their phone did not answer either. I figured that they must be on vacation on the Black Sea coast. Some years later, they explained to me that they were terrified of meeting me for fear of the police. Every day for that week I was in Brasov, they had gone out into the forest for a picnic in order to avoid seeing me.

 Waiting to contact Andrei and Maria, I went out to the village, where people were more accommodating and happy to see me. I stayed with my former host family. I had brought a gift package for Andrei and Maria, some chocolates and children’s clothes. Andrei’s mother lived in the village, so to be safe, I asked the young boy where I was living to walk over to Andrei’s mother and give her the package with my greetings. She should tell Andrei and Maria that I was here, and that if they were around, they could come around the corner to where I was staying. He never came.

The next day, walking through the village, I ran into Andrei on the street. He was here visiting his mother. He spoke to me briefly and coldly: ‘Thank you for the package. No, we cannot meet. Please do not visit us. Don’t visit anybody, you’ll only make trouble for them. Good-bye’. Then he walked away. I knew things were bad, but it was not until now that I knew how bad. Here was a guy whom I had known for 10 years, having spent hours and days in their apartment, and he just walks away like I have some kind of disease. This was a period when any Romanian who spoke to a foreigner had to make a report to the police, and when others could inform on them. On the other hand, I had met dozens of people who did not care about the law, including the family with whom I was staying. So I could never be sure who felt intimidated by these laws and who did not.

I later found out that Andrei and Maria, even though their request to visit Denmark had been rejected, continued to be called in for interrogation. Their situation was so serious, they explained later, that they contemplated suicide. Their relationship with me was making their life horrible. Andrei had been threatened that he would not be promoted to chief engineer in the factory. Their daughter would not be allowed to enter gymnasium. Because of me, their life was falling apart.

Following my brief encounter with Andrei, I returned to Denmark, depressed and paranoid. Perhaps I could do something to alleviate the situation. In Copenhagen, ever the American optimist, I visited the Romanian embassy to try to renew my invitation to Andrei and Maria, and to ask the embassy attaché if they could intervene. Perhaps there was a mistake. One of the diplomats happened to come from the same village where Andrei had been born. I asked him if he could help with the invitation so that they could visit me in Copenhagen. We talked cordially, and he was impressed that I knew so much about his home district. The attaché said he would look into the case. Of course, nothing came of it. Instead, he also wrote a report about me in my secret police file, saying that I was suspicious. How naïve was I. After he retired, he wrote several books about his life as a secret police operative.

 The next summer, I again returned to Romania with my wife and two kids. Perhaps I could find a way to see Andrei and Maria again and find out what had gone wrong. Arriving at the Bucharest airport passport control with my tourist visa, the red light on the border police’s computer began to blinking. The young policeman in the passport control looked at me, looked at my passport, looked at me again, and then told me to wait over in the corner of the transit hall. I later discovered from my secret police file that some months earlier, I had been declared ‘forbidden to enter’. My family and I remained in the hot, stuffy Bucharest airport transit hall that night, and the next day we were put on the flight back to Copenhagen.

 Back in Denmark, I continued to write about Romania as the situation deteriorated. But I did not send any letters or Christmas cards to Romanian friends. On December 25, 1989, following mass demonstrations, Nicolae Ceausescu and his wife were captured, quickly put on trial for crimes against the Romanian people, and executed by firing squad. That was the end of the communist regime in Romania. Some days later, I received a Christmas card from Andrei and Maria wishing me and my family all the best and hoping that we could now meet.

 Three months later, I was in their apartment in Brasov. They explained what had happened to them: the forest ‘picnics’ to avoid meeting me, the continuing interrogations, the thoughts of suicide. All because of their relations with me. Because of me. Some months later, they finally visited me in Denmark, and I have met them several times since then. We discuss the secret police each time. They just bring it up, it’s a traumatic experience for them, and I feel like shit. I can also see the records of their interrogation in my secret police files, where not just them but many other Romanians whom I felt close to had to make statements about their relations with me, after which the police would then draw conclusions as to my activities (One of the officers writes, ‘Well, is he a spy or isn’t he?’).

 Of course, I can say to Andrei and Maria, ‘Well, neither you nor I did anything wrong, it was an oppressive system.’ But then of course, I realize that perhaps I was naive, thinking that relations between individuals could somehow dominate or win out over an oppressive system. I know other Romanians didn’t give a damn about the police; especially those in the village, with little to lose. But do I have the right to accuse Andrei and Maria of being too paranoid? Was I just too naive? Was I not paranoid enough? Should I have been less engaged and more detached?

 As anyone who has lived in or studied an oppressive system knows, people adjust to such systems differently according to circumstance and context. Some people endure their oppression silently, but will then suddenly become openly resistant. Others use their energies trying to beat the system, playing it as if it were a game, some as criminals, others trying to get around the censorship. Still others are simply passive, trying to live a normal life, shielding themselves from the system. Finally, it is the nature of oppressive systems that they can recruit, intimidate and reward people who collaborate. Some collaborate out of a sense of survival, while others collaborate for crudely opportunistic reasons which they can rationalize later as ‘I had no other choice’ or ‘Everyone was doing it’. In Romania, I came to know all these types of people. Some Romanian functionaries were visibly nervous if I walked into their office. They knew they would have to explain what they were doing talking to an American. They knew that they would now have to file a report. Other people did not care at all about the police, as when I observed an old peasant woman openly insulting the policeman when he came to visit her house during our visit.

Andrei and Maria’s reactions lie on one end of this fear/paranoia spectrum of how people cope with oppressive political systems. In the early days, both they and I were rather naïve. Later on, they began to realize that their relations with me carried some risk. They became paranoid, I remained naïve. Finally, especially when the education of their child was threatened, they decided that maintaining relations with me was a genuine threat: ‘Don’t visit anybody, you’ll only make trouble for them’. I’ll never forget it.

I still grapple with whether I should have been more paranoid. Should I have been more detached? Did I not let my optimism as an American, or my feeling of importance as a foreigner in Romania, overshadow my judgement? Do I have some kind of ‘right’ to engagement with my informants-cum-friends in Romania when I knew that it might get them in trouble? Or should I insist to Andrei and Maria that they were wrong to blame me when it was the system they should blame? Max Gluckman once edited a book entitled *Closed Systems and Open Minds: The Limits of Naivete in Social Anthropology*. I think of this book when I look back on my relations with Romanians during this time. We learn as ethnographers that we should be open and involved with the people we study. We are told to express solidarity with them, to be friendly, to expose ourselves, to put our ‘self’ into our fieldwork. We learn engagement. We don’t learn detachment. We don’t learn paranoia. Perhaps we should have.

*Conclusions: ‘distance learning’*

We have provided two examples of fieldwork in difficult settings. Despite the different settings–conflict and violence in the Congo and surveillance and intimidation in communist Romania–both cases involved working in contexts of unpredictability, anxiety, and fear. We have shown that while anthropologists are often taught that access and proximity will ultimately generate good data about people’s lived experience, we anthropologists also need to learn how to distance ourselves. We need to learn how to back off sometimes. How do we find the balance between the desired proximity of fieldwork and the scientific distance needed to make competent analysis? How do we avoid falling into the traps of ‘ethnographic seduction? How do we prepare for ‘messy’ fieldwork?

One way to deal with methodological or ethical dilemmas that might arise in the field is to read about other ethnographers’ dilemmas in the field. While we often teach students that anthropological methods are about getting close to the people we study, we should also teach them about distance, and how our own emotions may impact our research. This is the ‘emotional management’ we are advocating here.

In Hedlund's case, the problem was knowing when ‘ethnographic seduction’ was taking place and how it impacted the research. She needed to be aware of this when dealing with people who have carried out acts of violence. From an emotional or moral standpoint, she had to take a step back and try to understand how informants incorporated their violent practices into their life-world. This task is difficult and emotionally burdensome. Observing and hearing accounts of violence is not just gathering of ethnographic data. It generates its own emotional effects. We thus need strategies to distance ourselves from these experiences and learn how to better take care of and manage our feelings after fieldwork so that we do not produce analyses that are overly based on feelings. We need more ‘distance learning’ as we prepare for fieldwork.

In Sampson's Romanian case, there was a combination of naivety in understanding the power of an oppressive state to affect personal relationships, and the guilt caused by the privileges that an ethnographer has in being able to enter and leave the field setting at will. Here the task is to gain a better understanding of the setting in which one enters, and the possible emotional pitfalls that can arise when one becomes close to certain informants. Emotional management means knowing that someone is always keeping a file on you. Sometimes the file is written down; other times the ‘file’ is embedded in the attitudes and assumptions of others who may want to get close to you for the resources you offer, or who may distance themselves from you because you pose a risk. The proximity-distance dilemma is a kind of double bind. Calling this ‘messy’ is a euphemism. Messes can be cleaned up. Ethnographic double binds of this kind cannot. It requires some kind of emotional management. Management does not solve the problem; it only ‘manages’ it.

 We are beginning to read more about the messy realities or the ethical or emotional dilemmas that other anthropologists have experienced. There is more ‘confessional ethnography’ out there. Nevertheless, it is still those texts that are polished and finished, those with more intellectual analyses and theoretical insights that have priority. The ‘feelings’ of the ethnographer are used to provide the context. The emotional part of the ethnography remains in the introductory section, rarely forming the object of the analysis. As social scientists, we are afraid of appearing narcissistic; after all, our ethnography is supposed to be about them, not about us. Hence, we tend not to share ‘behind the scenes’ stories or talk about our own emotions and how that may have affected the analysis. We may talk about our fieldwork with other researchers or colleagues when we have a drink, but we refrain from talking about hardships and emotions and how they influence the research process. Such talk makes us look vulnerable or (heaven forbid!) unscientific. The task, therefore, is to replace this fear of vulnerability by combining the proximity so essential to ethnography with distance. We need both involvement and detachment. We need a new kind of ‘distance learning’, not in front of a computer screen, but learning how to distance ourselves from our ethnographic object. This promotion of ‘distance learning’ may seem paradoxical. We spend so much time and energy trying to get close; we spend weeks, months, even years. With a distance learning agenda, we also need to learn how to be there physically, but to detach emotionally, how to take a step back. It is the dialectic between involvement and detachment which is the core of ethnography, and which can make for better anthropology.

References

Cloke, Paul, Cooke, Phil; Jerry Cursons, Paul Milbourne & Rebekah Widdowfield (2000) Ethics, Reflexivity and Research: Encounters with Homeless People. *Ethics, Place & Environment*, 3 (2): 133–154 .

Cyanne, Loyle., Simoni, Alicia (2014). “Research Under Fire: Researcher Trauma and Conflict Studies". Blogpost, October 30, 2014: available at: http://politicalviolenceataglance.org/2014/10/30/research-under-fire-researcher-trauma-and-conflict-studies/ (Accessed Januari, 2021).

Hedlund, Anna (2019). *Hutu Rebels: Exile Warriors in the Eastern Congo.* Philadelphia: University of Pennsylvania Press

Hedlund, Anna. (2017) ”Simple Soldiers? Blurring the Distinction between Compulsion and Commitment among Rwandan Rebels in the Eastern Congo. *Africa: Journal of the International African Institute:* 87 (4): 1–19.

Hedlund, Anna. (2015) ’There was no Genocide in Rwanda’ History, Politics, and Exile Identity among Rwandan Rebels in the Eastern Congo Conflict. *Conflict & Society: Advances in Research*, 1(1): 23-40.

Kovats-Bernat, J. (2002). Negotiating Dangerous Fields: Pragmatic Strategies for Fieldwork amid Violence and Terror. *American Anthropologist,* *104* (1): 208–222.

Jauregui, Beatrice (2013). *Dirty Anthropology: Epistemologies of Violence and ethical Entanglements in Police Ethnography.* New York: Palgrave Macmillan.

Kideckel, David and Sampson, Steven (1984). Fieldwork in Romania: Political, Practical and Ethical Aspects. Amherst: UMASS Research Report nr 24, 1984 (www.stevensampsontexts.com).

Maček, Ivana (2014). Engaging Violence: Trauma, Memory and Representation. Abingdon: Routledge.

Ngai, Sianne (2009). *Ugly Feelings.* Cambridge, MA: Harvard University Press.

Robben, Antonius (1996). “Ethnographic Seduction, Transference, and Resistance in Dialogues about Terror and Violence in Argentina.” *Ethos*, 24: 71–106.

Robben, Antonius and Nordstrom, Carolyn. “The ethnography of Violence and Sociopolitical Conflict”. In, *Fieldwork under Fire: Contemporary Studies of Violence and Survival*, edited by Robben., Antonius and Nordstrom, Carolyn,. Berkeley: University of California Press, 1995.

Sampson, Steven (1984). *National Integration through Socialist Planning: An Anthropological Study of a Romanian New Town*. Boulder: East European Monographs (www.stevensampsontexts.com).

Sampson, Steven (2020) Tattoos and Ankle Bracelets: Recalling Fieldwork in Romania. In Raluca Mateoc and F. Ruegg (eds.) *Recalling Fieldwork: People, Places and Encounters*. Zurich: Lit. Forlag (www.stevensampsontexts.com).

Sampson, Steven (2019) How I became a ‘Romania expert’. Studiu Sociologica Univ Babes Bolyai, Cluj 2/2018 ed. by Iuliu Ratiu (www.stevensampsontexts.com).

Sampson, Steven and Kideckel, David (1989) Anthropologists Going Into the Cold: Research in the Age of Mutually Assured Destruction. In P. Turner and D. Pitt (eds). The Anthropology of War and Peace. Granby, MA: Bergin and Garvey ([www.stevensampsonstexts.com](http://www.stevensampsonstexts.com)).

Sluka, Jeffery (1999). *Death Squad: The Anthropology of State Terror*. Philadelphia: University of Pennsylvania Press.

Sluka, Jeffery (1995). “Reflections on managing Danger in Fieldwork: Dangerous Fieldwork in Belfast.” In, *Fieldwork under Fire: Contemporary Studies of Violence and Survival*, edited by Nordstrom, Carolyn and Robben, Antonius,. Berkeley: University of California Press.

Verdery, Katherine (1918) *My Life as a Spy: Investigations in Secret Police File.* Durham: Duke University Press.

Vlassenroot, Koen (2006). “War and Social Research. The Limits of Empirical Methodologies in War-torn Environments,” *Civilisations*, 54:191–198.

Weiss, Nerina (2016). Research under Duress: Resonance and Distance in Ethnograhic Fieldwork. In; Engaging Violence: Trauma, Memory and Representation (edited by Maček, Ivana). Abingdon: Routledge

Wood, Jean, Elisabeth (2006). “The Ethical Challenges of Field Research in Conflict Zones,” *Qualitative Sociology*, 29:373-386.

1. In writing this chapter, we collaborated equally in writing the introduction and conclusion sections. The case studies are written by each of us separately. [↑](#endnote-ref-1)