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Tessa Diphorn^{1,2}

Abstract

This article explores methodological issues as a prominent subject in ethnographic fieldwork conducted on a specific group of private security officers, namely, armed response officers, in Durban, South Africa. Through analyzing several experiences from the field, this article highlights the dialectic between emotions and participation in the field and its imperative role in analysis of the research setting. This article explores three different modes of participation, namely, active participation, reluctant participation, and passive participation. As a heuristic device, such a typology allows us as researchers to analyze our position, as participants, in relation to other research participants. This exemplifies the importance of the emotionality of field experiences in researching violence (and the perpetrators of violence).

Keywords

ethnography of violence, emotion, participant observation, private policing, South Africa

¹Utrecht University, Utrecht, Netherlands

²Sabancı University, Istanbul, Turkey

Corresponding Author:

Tessa Diphorn, Department of Cultural Anthropology, Utrecht University, P. O. Box 80140, 3508 TC Utrecht, Netherlands.
Email: t.g.diphorn@uu.nl

During a brief return to my research location (Durban, South Africa) in March 2011 while on vacation, I visited one of the private security companies from my research. Within ten minutes upon arrival, news came in about an armed robbery. Without hesitation, I jumped into the company's security vehicle to rush off to the site in question. But as we drove off, I started to sweat profusely and feel extremely anxious, uncomfortable, and agitated. Everything inside me screamed, "No, no, no! Get out of this car!" If word had not come in that it was not an armed robbery, but a theft that occurred a few hours before, I am certain that I would have fled.

I was astonished by my reaction. Between 2007 and 2010, I conducted approximately twenty months of ethnographic fieldwork to analyze the niche of a specific group of private security officers, namely, armed response officers, in policing the streets of Durban, South Africa. During my first visit to the field in 2007, I was intrigued by the enormity and diversity of the private security industry in South Africa that accounted for 7,496 companies in 2010.¹ I set out a year later to uncover the world of armed response, a specific sector of the industry that provides armed assistance to clients (both business and residential) to protect them from potential crime and violence. Participant observation among the armed response officers was the core of my data collection, and it entailed wearing a bulletproof vest and accompanying them in their vehicles during their twelve-hour shifts.

Although violence was certainly not a daily encounter, I experienced several fearful, emotionally charged, and ethically ambiguous incidents in the field. Yet I was troubled by this new sense of aversion and increasing depth of repugnance in 2011. Several methodological questions that had previously surfaced became more urgent. How had I mentally and physically coped in the field, and more importantly, how had I *enjoyed* it at times? What role did my emotions have in the research process, particularly in my participation in and analysis of the research setting?

Substantial literature in anthropology, criminology, and sociology analyzes the troublesome nature of researching violence in conflict, postconflict, and "peaceful" settings (Cramer, Hammond, and Pottier 2011; Ghassem-Fachandi 2009; Lee 1995; Punch 1986; Robben and Nordstrom 1995; Rodgers 2001, 2007; Sluka 1990). These scholars extensively depict the emotional, ethical, and moral dilemmas inherent to ethnographies on violence. Although I concur with these accounts, I aim to contribute to the literature by showing that these are not necessarily predicaments but essential building blocks of analysis of the research setting. I argue that the emotions evoked, experienced, and shaped in the field largely influence the way we as researchers act in the field and are therefore imperative in understanding the violence that we research. Rather

than viewing our experienced emotions as obstructive, we must recognize that they are crucial empirical data and are interrelated to other data that we regard as knowledge. I make this claim by focusing on how I acted in the field and analyzing how these actions were steered by my emotions and how they provided me with a deeper understanding of the research setting. Thus, by focusing on method, I aim to show the importance of the emotions of the researcher in understanding the social reality of one's research participants.

I employ the concept "emotionality of participation" to show the dialectic between emotions and participation by exploring several experiences from the field in a typology of modes of participation: active participation, reluctant participation, and passive participation. This is not a fixed classification but a heuristic device that allows us to understand how our emotions are interconnected to the various actions we as researchers perform in the field. Although such a categorization emerged in retrospect and is not necessarily clear-cut and self-evident, it provides a framework to analyze our position, as participants, in relation to other research participants that the larger concept of participant observation does not permit.

The Emotionality of Participation

In the past, social sciences generally ignored the emotional self of the researcher in analysis for objectivity purposes. Due to the crisis of representation, influence of feminist theory, and reflexive revolution, the importance of reflexivity is currently undisputed. It is an "unavoidable pre-condition" (Madden 2010, 23) to any ethnographic fieldwork to "achieve a methodological rigour" (Nilan 2002, 369). Although what is meant by *reflexivity* remains to be debated, there is a general consensus that ethnographers must recognize that they as persons, with their own personality traits, background, and perceptions, shape the entire research process (Beatty 2010; Coffey 1999; Denzin 1997; Madden 2010). I regard reflexivity as the "reciprocal interplay of one's relationship with oneself and with others" (Jackson 2010, 36).

However, the emotions of the researcher remain marginalized or disregarded, due to a reigning assumption that the "emotional" and the "analytical" are dichotomies and two different sets of data (Campbell 2002; Coffey 1999; Davies 2010; Leibing and McLean 2007). I claim the opposite. In concurrence with the main arguments presented in the edited volume of Davies and Spencer (2010), I argue that emotional experiences are not obstructive and detached experiences from the more objective analytical data, but when reflected upon, they are illuminating and interconnected to other data that we regard as knowledge. Emotions are thus not merely research tools, but they

frame data, are a part of other data, and function as data on their own (Beatty 2010; Campbell 2002; Davies 2010; Hage 2010; Lee-Treweek and Linkogle 2000; Lumsden 2009).

Campbell (2002) stresses the importance of the “emotional dialogue” between emotions and knowledge. This not only refers to the emotions of the researched or the research setting but also includes “researching the researcher” (Campbell 2002, 35) and “the emotional practice of doing research” (Pickering 2001, 491). I examine the emotions I embodied during the research process to analyze the interrelatedness between emotion and participation; I am concerned with the emotionality of participation. This refers to the observable behavioral reactive of an emotion experienced while participating in the field.

This is particularly important when researching emotionally charged topics, such as violence, where our emotions are more salient and weigh heavier in our analysis (Campbell 2002). After all, to research violence is emotional—for both the informants and the researcher—and thus carries additional responsibilities and dilemmas that outweigh those associated with traditional ethnography (Campbell 2002; Cramer, Hammond, and Pottier 2011; Ghassem-Fachandi 2009; Robben and Nordstrom 1995). The existing ethnographies on violence clearly portray the emotional turmoil of such research, and my research follows suit. Feelings of estrangement, frustration, guilt, and disgust and the recurrence of nightmares and insomnia were all part of my research experience. Such emotions often made me dread going on duty with my informants, and I frequently had to persuade myself to “get over it.” The quick and recurrent shift from one strong emotion to another was exceptionally exhausting, and I regularly felt unable to share my experiences with others—both friends and colleagues—because “they didn’t understand” and “they hadn’t been there.”

In the field, I experienced this as impeding, and I felt an incessant need to neutralize or subdue certain emotions. I felt I was failing to understand my informants when I was disgusted by their behavior or felt aversion toward them. When I overtly defended my informants and sympathized with their views, I feared falling victim to what Robben (1996) calls “ethnographic seduction.” This refers to “a complex dynamic of conscious moves and unconscious defences” (Robben 1996, 72) that can surface during interactions with perpetrators and victims of violence that prevents researchers from losing “their critical stance toward the manifest discourse” (Robben 1996, 72) of their interviewees. However, as Hage (2010, 150) argues, undergoing certain emotions, whether similar or different to our informants, does not mean we are unable to employ an analytical lens; it merely requires “much more effort—an effort that is in itself emotional.” Furthermore, the perceived need

to ignore or transform certain emotions is in itself an emotional process of empirical value (Davies 2010).

The empirical narratives in the following sections will show how experiencing feelings, such as anxiety, anger, fear, and many others, steered my actions in the field and allowed me to understand my informants' experiences and the prominent role such feelings have in their occupation. For example, when I shared my experiences of fear, many informants strayed from the "tough men" image that they had initially conveyed. Originally being distant and treating certain situations as commonplace, many of them eventually expressed the opposite and shared stories about traumatic incidents, nightmares, and domestic issues stemming from "taking the work home." This transformation and willingness to discuss their emotions and experiences were primarily possible because I had also experienced them.

Beatty (2010, 431) argues that analogous experiences and centralizing the anthropologist's personal experiences are not the answers to "making sense of others' worlds." I concur and argue that my emotions and those of my research participants were never fully shared. Such a perspective robs "emotions of the personal significance that is—as most authors would agree—their essence" (Beatty 2010, 440). The researcher's feelings provide insight and a certain degree of commonality, but our experiences are founded upon individual histories and discourses that set us apart. However, I argue that the *unshared* emotions are also empirical data. I do not come from an environment with high levels of crime where armed robberies and rapes are daily occurrences, where violence is "normal," tolerated, or encouraged. Being a white, foreign woman made me experience violent encounters differently than the South African men from my research. The empirical examples that follow are thus my narratives of violence, where phrases such as "crying excessively" and "going at it hard" are my subjective interpretations. My informants would probably narrate them quite differently.

However, as Hage (2010) argues, we need to problematize both what we do and do not share with our informants. In employing an "interlocutory approach" to emotions as Crapanzano (2010, 59) does, all that is experienced and felt with any encounter is data. One must therefore focus on the relational aspects of the fieldwork, referring to "the product of dialogue and intersubjective encounters" (Leibing and McLean 2007, 7) between the researcher and the researched. In recognizing the impossibility and undesirability of emotional detachment and equivalent experiences, I focus on the locus of attachment and the interface and intersubjectivity of emotions, perceptions, and experiences. I employ the emotionality of participation as a tool in understanding the shared and unshared world of the researcher and the researched.

The Various “Selves”

In being reflexive and centralizing my emotions, I grapple with the required depth of reflexivity and the positioning within my narrative texts. Much critique has been placed on ethnographers since the mid-1980s for being “emotional exhibitionist” (Pickering 2001, 485) and “narcissistic, overly reflexive, and not scientific” (Denzin 1997, xv). I argue that writing about the self should not be the heart of the ethnography: it is a crucial part of the research setting, but it is *one part* among others. It is about “bringing the author/observer into the analysis—as a source of light but not as the light itself . . . as directly related to the object under study, but not the object itself” (Leibing and McLean 2007, 6). Being reflexive and presenting personal data should elucidate the research process (Leibing and McLean 2007, 13) rather than overpower the process of knowing “the other” (Hage 2010, 133). I therefore do not intend to create an autoethnographic account that solely describes my position in the field. Rather, I focus on my own emotions and actions to help illuminate the research process and outline my core argument, namely, that analyzing the dialectic between emotions and method provides deeper insight into the experiences and actions of our informants.

In an attempt to dissect and understand my impact in the field, I discovered various “selves” (Coffey 1999; Denzin 1997) that emerged in different field settings; sometimes certain selves were prominent; at other times, absent. Occasionally, I tactically implemented a degree of “impression management” (Goffman 1959), and at other times, this was reactive, spontaneous, and identifiable in retrospect. Furthermore, the numerous selves were continuously reshaped throughout the research process (Coffey 1999; Davies 2010; Jackson 2010).

The most prominent parts of myself that repeatedly surfaced were race and gender. I am a white, upper-middle class woman from the Netherlands who conducts research with and about predominantly nonwhite, South African men. My gender and skin color continuously differentiated myself from my informants. I do not want to analyze gender as a fixed personal identifier, but I cannot deny the prominence of gendered interactions and perceptions that surfaced as a woman conducting research in a world dominated by men. Although gender influences any research, it weighs heavier when researching police institutions due to inherent masculinities exerted through a male-dominated environment (Huggins and Glebbeek 2003; Hunt 1984; Marks 2004; Westmarland 2001).

At the outset of the research, I was warned about “hustling” (Easterday et al. 1982, cited in Lumsden 2009, 504), and I expected sexual advances to

be an obstacle. Although advances were made, they did not impede the research. In fact, I was frequently alone with males in their vehicles and I rarely felt unsafe in those situations. In concurrence with Marks (2004) and Hunt (1984) and in contrast to other literature on women studying male institutions that predominantly highlight the “gendered stumbling-blocks” (Huggins and Glebbeek 2003, 384), I show that my gender was mostly advantageous rather than problematic.

As a woman, my informants ascribed two opposing images onto me: a potential sexual partner and a comforting listener. I continuously shifted within these two extremes and adjusted the role I inhabited accordingly. When interviewing owners, I was purposely naive and subordinate, and when attending sessions at the shooting range, I felt expected and pressured to act tougher and like “one of the guys.” Occasionally I was flirty and at other times non-sexual. As my informants regarded grief and distress as normal feminine emotions, I often purposely acquired a nurturing role and delved into my emotions to encourage my informants to share theirs. I sometimes therefore displayed certain attributes deliberately, and at other times, they were evoked naturally and identifiable only in retrospect.

Method

Although I employed various qualitative methods in my research, I focus here on participant observation among the armed response officers. I define participant observation as “a method in which a researcher takes part in the daily activities, rituals, interactions, and events of a group of people as one of the means of learning the explicit and tacit aspects of their life routines and their culture” (DeWalt and DeWalt 2002, 1). I conducted participant observation to analyze the daily endeavors and interactions that constitute the occupational culture and policing style of armed response officers within the larger private security industry in Durban, South Africa. Although I accompanied and conducted interviews with other members of the industry (such as managers and technicians), clients, and public police officers, the core of my data collection originates from participant observation with armed response officers.

I gained access to the companies by directly approaching the owners and/or managers and requesting to accompany their armed response officers. Although two companies refused my appeal for safety reasons, the remainder complied after signing liability and confidentiality agreements. During twenty months over three years, I spent approximately 750 hours with armed response officers in their vehicles. These 750 hours were spread across four different companies to represent the diversity of the armed response sector:

an international-owned company, a Durban-based company, and two community-based companies.

During their twelve-hour shifts, I focused on understanding the world of armed response and establishing rapport with armed response officers through continuous open interviews and informal conversations. All of the armed response officers were aware of the purpose of my research, and when possible, I also informed other research participants, such as clients and police officers. I did not wear the company's uniform, and I placed a business card of my university on my bulletproof vest to clearly show that I was a researcher and not an employee of the company.

When accompanying armed response officers on their shifts, I carried a notebook and jotted down keywords that focused on events, such as *car chase*; themes in conversations, such as *trauma*; and my own emotions, such as *anger*. I also carried my tape recorder while on duty, but as it was practically impossible to record all twelve hours of each shift, I recorded conversations that I then deemed to be important. After each shift, I wrote up elaborate field notes that I brand as *mosaics of data*, including jotted down keywords, fragments of transcribed interviews, and detailed field notes written afterward. Unlike Murray (2003), I did not distinguish between personal, observational, or methodological notes. I believe that such a categorization disregards the interconnectedness between emotions and method, between emotions and knowledge. Rather, personal accounts and methodological issues blended in throughout the notes as an interwoven ingredient of the empirical data, resulting in "messy texts" (Denzin 1997) that voiced various facets of the research process.

Another prominent technique that I have employed throughout the entire research process is the frequent rereading of my field notes, both in and out of the field. It was initially a way to remind myself of the research data, yet it has provided the additional advantage of tracing the developments of my emotions, my relationships with my informants, and my analytical insights. After my visit to the field in 2011, I reread all of my field notes again and particularly focused on my emotions and behavior in the field to try to uncover the distinction of that experience.

According to Beatty (2010, 440), "the evocation of feelings is an exercise in imaginative recovery." I concur that recapturing and reexperiencing those identical feelings is impossible, as a retrospective gaze on an experience may evoke other emotions and yield another analysis than those originally conjured. However, I also argue that a backward-looking analysis established over time is fruitful exactly because our perspectives have changed. Time has allowed me to acknowledge how my emotions are interconnected to my

participation in the field, how my feelings and moods influenced how I acted in the field, and how my actions simultaneously evoked other emotions. Crapanzano (2010, 56) argues that participant observation is about “the need to be critically conscious of what one is doing as one does it.” However, certain situations, such as those discussed in the following sections, do not always provide space to think about what one is doing; very often, one merely acts. A retrospective gaze grants the opportunity to understand and analyze how our feelings steered our actions. I therefore recommend researchers to value and utilize the retrospective gaze and continuously reread and reanalyze their field notes throughout the entire research process.

To illustrate the emotionality of participation, this article presents three different modes of participation that I inhabited along the participant observation continuum, as the two extremes of nonparticipation and pure participation (DeWalt and DeWalt 2002, 18-19) did not exist in my research. Other scholars have made similar categorisations, such as Murray (2003), who differentiates between different roles in the field, and Hage (2010), who discusses the analytical, emotional, and political modes of participation. The three modes of participation discussed here do not refer to different roles or personal identifiers, but they examine the actions I undertook in the field during particular (violent) encounters. The first mode is *active participation*, where I actively and willingly stepped into my research setting by moving beyond that of the researcher and concordant to my research population. The second is *reluctant participation*, where I participated rather unwillingly, less actively, and differently than my informants. The third is *passive participation*, where I deliberately tried to inhabit the role of observer.

Active Participation: “It’s Good You Were Here and Stepped In”

In May 2010, I was on night shift with one of the community-based companies, and we heard that a cousin of Michael, one of the employees, had just died of an asthma attack. Out of compassion for Michael, the owner of the company and I visited the residence. When we arrived, the whole family was present and extremely upset. Feeling like an intruder into this private matter, I remained outside in the garden. We were told that she was already dead, so I was surprised to see that several family members were calling an ambulance in the hope of saving her. In the Netherlands, I had completed a basic first-aid course after the first fieldwork period, including mouth-to-mouth resuscitation, so when Michael asked the crowd for help to resuscitate her and nobody responded, I stepped in to assist.

With an adrenaline rush and agitating nerves, I approached the victim. She was lying on the floor in the middle of the room and encircled by approximately twenty family members who were staring at me and begging me to save her. When I kneeled down beside her, I felt that her body was ice cold and that she had no pulse. My training told me that she was already dead, but my lack of medical qualifications and the pressure of the family members forced me to perform mouth-to-mouth. As soon as I placed my mouth on top of hers, it was like I had tasted death; a horribly foul and rotten taste slipped from her mouth into mine and a wave of vomiting reflexes engulfed me. Although I was guided by an experienced person on speaker-phone and assisted by someone else, I was outright petrified and nervous about aggravating the situation. The fight against vomiting and expressing my own fears was overpowered by the mechanical act of resuscitation. When the paramedics arrived after what felt like hours, I felt I could finally breathe, as if I, myself, had just been resuscitated.²

After the situation settled down, I resumed the night shift, but I could not shake off what had happened and continued having gag reflexes. I also became angry with my informants for not being able to perform the resuscitation themselves, and I started demanding (rather rudely) that the owner provide such training for all of his employees. Similar to incidents described by Campbell (2002) and Punch (1986), when I got home early in the morning after the shift, I went to sleep, but woke up a few hours later, drenched in sweat, to vomit. For weeks I had recurring nightmares of the woman's face floating out of a bathtub and coming toward me, on the verge of vomiting over me. Simply thinking about that incident brings back that atrocious taste into my mouth.

This incident is an example of active participation, where I actively stepped into the field and dismissed the role of the researcher. As discussed by Winlow et al. (2001, 546), it was a situation where I had entirely "forgotten that the reason you are there is to conduct research." Active participation also occurred when I encountered female victims. In almost all instances, I felt that female victims became more comfortable when I arrived. One particular incident took place during a night shift in December 2009. An elderly white woman was attacked while sleeping and was held at gunpoint, beaten, tied up, and robbed by three black men. When we arrived at the scene, the woman's hands were tied up; she was partially naked, and she was shaking and crying excessively. The armed response officer I was with tried to release her, but this made her more hysterical. She kept looking at me, which I interpreted as an indication that she wanted *me* to assist her. So with her visual signs of consent, I untied her. When her hands were loose, she quickly

grabbed mine and refused to let me go. When others, such as the police, arrived at the scene, she declined interacting with anyone else, except me. For the next few hours, I became a go-between between this woman and everyone else.

In this episode, I also willingly stepped in and acted similarly to my research participants. With both incidents, my participation was steered by feelings of moral responsibility, a desire to help, and a perceived need to hide my feelings of fear and act like a “professional.” Active participation refers to actions whereby the distinction between “the researcher” and “the researched” diminishes, particularly in the eyes of the researched. For my research participants, my participation was defined as a willingness to help them, and I was now on “their side.” With the first incident, I intervened on the basis of my limited medical skills, and the research participants highly appreciated and valued this, as the quote “It’s good you were here and stepped in” depicts. For them, I performed a service on behalf of the company and thus “worked” as an employee. The community’s expectations of the company had changed due to my presence, and my informants hereafter assumed that I needed to be present at every medical emergency, despite my protest.

With female victims (particularly, white female victims), I became an active participant in my research setting by operating as a sort of counselor and filling a gender void, a role I was able to assume as a woman. After this incident, many armed response officers from this company requested me to assist when female victims were involved; I had thus assumed a role that was thereafter repeatedly ascribed to me. Actively participating thus transformed me from the foreign, female researcher analyzing their work into the capable and helpful female “team player.”

The field notes of these experiences are mainly filled with descriptions of my emotions rather than of the larger research setting. The first three phrases I jotted down when we resumed the night shift after resuscitation were *death*, *foul taste*, and *nerves*, and the elaborate field notes written afterward primarily focus on my emotions and contain little observation of the behavior of my informants, such as the interactions between the armed reaction officers and family members. Hunt (1989) and Nilan (2002) had similar experiences. During her research on the police, Hunt (1989) discusses a specific case of witnessing death and how her field notes of that case, written afterward, are not reliable and contain false information. One could argue that active participation thus decreases our ability to analyze the research setting.

However, I argue that these experienced emotions are empirical data. I felt a moral responsibility and a desire to help, yet simultaneously pressured to perform in a certain way and as a failure for not saving her. These emotions

shed light on the experiences of my informants in the line of duty, and by experiencing them firsthand, space for discussion emerged. A few days after trying to resuscitate Michael's cousin, I told several informants about vomiting afterward and the recurrent nightmares. They first laughed at me or remained silent, but weeks afterward, two informants shared similar experiences after seeing a dead body for the first time. Intriguing discussions about coping with traumatic incidents and death followed. Through my actions and emotions, active participation thus provided a deeper understanding of the emotional experiences of my informants, thereby consolidating rapport and creating space for relational interactions.

Reluctant Participation: "You Don't Have to Shoot; Just Hide!"

On May 12, 2010, I was on night shift with Lushen, the owner of a small company, and two of his "volunteer" friends. During a coffee break with several police officers at a gas station, we heard about an ongoing hijacking taking place close by. Within seconds, we soared into our vehicles and raced off. All three of them cocked their guns, and Lushen drove hastily and commanded me to put on my bulletproof vest, a difficult task in a speeding car taking sharp curves. They were very excited, saying things like "This is going to be a good one" and "Finally we get some action." While driving, they instructed me to jump out of the vehicle on the opposite side of the suspects when we came to a standstill and to seek cover next to the car's engine. It was only then that the danger of the situation kicked in; my heart started beating excessively, my mouth became dry, and I nervously scratched my face and head. A large part of me wanted to jump out of the car and run away, but I felt that there was no turning back.

While trying to absorb their instructions, the car suddenly stopped and I heard gunshots. But I had no idea where they were coming from and who was shooting. I got out of the car, crawled to the bonnet, and curled up tightly in a fetus position right next to the engine with my arms covering my head and ears. Shots continued to fire and at one point I was sure that a bullet was not far from my head, thundering over me like a rocket. And then unexpectedly, there was complete silence and the putrid smell of gunpowder lingering in the air. I remained still and realized that I had held my breath the entire time. For the rest of the night shift, I chain-smoked in the hope to calm my nerves.

Although I had previously experienced violence in the field, this was the first life-threatening situation. I asked myself whether I should have stayed at the gas station when news of the hijacking came through. Although no data

are worth my life, it is unfortunately not that straightforward. It is often unfeasible to outweigh the value of data with the potential risk. Although one may become more adept at detecting signals and calculating risks over time, certain things are unavoidable or unforeseeable. Perceptions of what constitutes danger changes over time, and bad luck cannot be incorporated into such precalculations (Lee 1995; Sluka 1990).

In the armed response sector, violence is not a daily endeavour, and the majority of the days consisted of hanging out and chatting. However, the potential for danger is always present and therefore very unpredictable. Similar to my informants, I risked exposure to such circumstances when I decided to step into their vehicles, thereby making it impossible to avoid danger and dissociate myself from violence and those perpetrating violence (Hunt 1989; Marks 2004; Lee-Treweek and Linkogle 2000; Rodgers 2001, 2007). As Liebling and Stanko (2001, 424) state, “researching violence means we choose to (or become obliged to) explore the dangerousness of violent groups or settings.” One must therefore try to attain a realistic account of what the fieldwork entails and be certain of one’s choice to pursue it.

I increasingly asked myself why I had selected this topic. Am I a researcher who wants to “seek out danger” (Westmarland 2001, 532) and who thrives on the “business of thrill seeking” (Winlow et al. 2001, 537)? In relation to Clendinnen’s (1999) notion of the “Gorgon effect,” is there something wrong with me that finds violence-related topics intriguing? Researching violence is often regarded as something exotic, on the edge, and a way of “breaking new ground” (Lee-Treweek and Linkogle 2000, 10), where bravery and novelty are compulsory attributes. Although the thrill of violence was certainly not my preliminary motivation, one’s inspiration changes along the way. Boundaries that I had initially set continuously shifted. In all truth, the research was addictive and frequently incited adrenaline and feelings of invincibility. On days where “nothing” happened, we sat around in anticipation for the next prescription of action and adrenaline. It is a world that one gets easily drawn into and where everything thereafter is boring and mundane. I clearly recall taking a writing break in June 2010 to write a fieldwork report for my supervisors. I experienced withdrawal-type symptoms; the serenity and lack of action made me restless, I had difficulty concentrating, and it took days before the constant need to be ready and alert disappeared.

Many researchers discuss the need to take distance from the field to reorient, unwind, and cope with the mental and physical burden on the body (Ghassem-Fachandi 2009; Huggins and Glebbeek 2003; Hume 2007; Lee 1995; Sluka 1990). Although this was my initial plan, it turned out to be more difficult to realize, particularly during my first fieldwork period. Riding on a

“researcher high” of collecting fascinating data, I did not slow down, but I pushed myself further. It was only after a visiting colleague expressed concern about my hair loss, weight gain, and the dark circles around my eyes that I realized I needed to leave the field. It thus required an “outsider” to confront me with the physical burden of the research. I therefore recommend maintaining frequent contact with colleagues about the fieldwork experiences, as their “outside perspective” is helpful, or perhaps even necessary. Additionally, I advise supervisors to visit their researchers in the field to see firsthand and discuss the emotional process of conducting such research.

Thereafter, regularly leaving the field allowed me to recharge my batteries and distance myself from the research setting, but it also increased my reluctance to continue. As was the case during my visit in 2011, it was challenging to step back into that social world after residing in a safer and more comfortable one. Yet all of these emotions—being excited about the progress of the research, experiencing withdrawal-type symptoms when taking breaks, feeling forced to leave the field, and reluctantly reentering the field—are empirical data. They not only shed light on how I experienced doing the research and the interconnectedness of my emotions and actions, but they accentuate a fundamental difference between my informants and myself: unlike myself, they are not able to take a break when the physical and emotional burden takes its toll. I therefore concur that taking breaks from the research setting is essential; besides providing distance, breaks provide the chance to examine the shared and unshared experiences with one’s informants.

I argue that the shooting incident is an example of reluctant participation. This is not only because I reluctantly participated but also because I did not directly take part in the shooting and acted differently than my informants. As an active participant, I had filled a void and willingly participated; in this case, I had acted differently due to the *lack* of certain skills, as the quote “You don’t have to shoot; just hide” indicates. However, I was a participant by merely accompanying them, and my presence forced my informants to act differently, such as giving me instructions. I therefore refer to this as reluctant participation—not engaged in “the praxis of violence” (Rodgers 2001, 3) but a part of its manifestation and interpretation.

Not actively participating allowed me to make more observations of the actual incident. A few of the first jotted-down keywords were *shooting*, *danger*, and *instructions*. The more elaborate field notes describe my fears, but they are primarily extensive observations made *right* after the incident, something I would have missed if I had arrived half an hour later. In concurrence with Nilan (2002, 383), the “dangerous” events are not necessarily the most insightful, but rather it is the “data collected after the event or in quieter

moments which turn out to be the most evocative.” However, to utilize the value of “the quieter,” one must be present at the “dangerous.” This is particularly so in building rapport with our informants. Ghassem-Fachandi (2009, 6) refers to this as “interrupted reciprocity,” a specific type of rapport that is established between the researcher and the researched through an encounter with violence that allows bonds to be established more easily.

In contrast to Rodgers (2001, 2007), I luckily never used force or coercion in the field. As women are regarded as harmless, powerless, and in need of protection (Lumsden 2009; Westmarland 2001), I did not have to impress my informants with the use of force, and it did not matter that I did not know how to shoot properly or dismantle a firearm. The perception that I needed protection, as a woman, prevented me from actively participating in such cases, and I purposely took advantage of this.

Unfortunately, my informants were occasionally too protective. During a night shift at a public event, an unknown man passed by me and whispered something vulgar in my ear. Although none of my informants knew what he had said, the look on my face said enough. Kenny, an armed reaction officer, immediately approached the man, screamed at him, and punched him in the face. Out of extreme guilt and responsibility, I intervened and convinced Kenny to let it go, which he fortunately did. For days afterward, I became increasingly worried when other informants voiced support for Kenny’s behavior.

This is another example of reluctant participation: I was the cause of the use of physical violence on behalf of my informant, although unwillingly and unintentionally. With Kenny’s “intervention,” my field notes immediately after include my apprehensions and feelings of guilt, but field notes written on numerous occasions afterward are based on conversations with informants about issues surrounding honor, group loyalty, and group cohesion that stem from this incident.

However, with both incidents, I had placed myself in “situational danger,” danger that “arises when the researcher’s presence or actions evoke aggression, hostility, or violence from those within the setting” (Lee 1995, 3-4). It forced me to question whether it was ethical to continue with the research. When researching violence, we become a part of the violence that is being studied and are therefore complicit in the repercussions (Ghassem-Fachandi 2009; Robben and Nordstrom 1995). The question posed by Liebling and Stanko (2001, 422), “Can research on violence lead to violence and what happens if it does?” kept surfacing. What if Kenny had not stopped and the situation had escalated? What would have happened if I had been injured during the shooting? Although I had signed a liability agreement, what implications would this have had for my informants? Although researchers inherently

influence their research setting, when researching violence, our impact can be disastrous, but where do we set the boundaries?

Passive Participation: “You Were Just, Like, Looking at Us!”

My first experience of existential shock, shock that concerns “a disorientation about the boundaries between life and death” (Robben and Nordstrom 1995, 13) is deeply ingrained in my memory. It happened on a Friday morning while standing outside the company office. During a cigarette break with several armed response officers, a call came in about the presence of two suspects in someone’s yard. Everyone threw their cigarettes onto the floor, started screaming at each other to hurry up, and hustled to their vehicle. I quickly grabbed my vest and hopped into the front seat of Siphos car.

While driving toward the premises in question, we heard over the radio that the suspects were apprehended. When we reached the site, Siphos told me to stay in the car. I could see that eight armed response officers were standing around two suspects, who were lying facedown on the ground with their hands cuffed. For the next few minutes, they repeatedly hit the suspects with their batons and kicked them while the suspects howled with pain. More and more members from the community came toward the scene to watch. The armed response officers continued going at it hard; they thumped the suspects, screamed at them, preached to them, and accused them of stealing from innocent people. I felt extremely uncomfortable to be sitting in the car—almost as if I were hiding—especially when I realized that bystanders were looking at me. I felt responsible, as if I were one of them, partaking in this violent act.

They then picked up the suspects and I finally got a full view: they were bleeding excessively on various parts of their bodies, and their clothes were torn. They dumped the suspects into the back of two pickups, including the one I was sitting in. The car wobbled from the beatings and I heard the landings of their feet onto the suspect’s body. The sound of the suspect’s screams and grunts penetrated my ears. Siphos stepped into the front seat and asked me whether I was all right. He then said, “Ach, all that violence—” interrupted by a smirk. “But I told you we were tough and know how to hit.”

Suddenly everyone got back into their vehicles to drive approximately one hundred meters further up the road. They took the suspects out of the cars and threw them onto the ground. They then told me I could get out of the car. For the next twenty minutes, they continued to interrogate them and hit them in a playful manner. Two armed response officers cocked their firearms, pointed them at the suspects, and then threatened to shoot them, followed by laughter

by everyone. They teased and provoked one another, and some used this as an opportunity to show off their fighting skills. At one point, one suspect made eye contact with me and I instantly looked away, unable to bear the pain expressed in his eyes. And then it dawned on me: here I was, standing on the other side of the road, watching a group of armed men in uniform screaming at, smacking, and kicking two suspects. And they seemed to enjoy it. I was absolutely disgusted by what I saw. I craved for them to stop. Everything inside me screamed, yet I did not make the slightest sound.

Although my participation here was certainly reluctant, I differentiate this case as passive participation for two reasons. The first is based on my behavior and the conscious decision to *look* at what was happening. Although repulsion dominated my mind, I was intentionally watching the scene unravel and observing various details, such as the group dynamics and inherent power relations. I purposely put on the researcher cap by, for example, talking to nearby community members to learn their perspectives. The key phrases that I jotted down were far more extensive and focused on the behavior of my informants, such as *hitting*, *hierarchy*, and *showing off*. The field notes written afterward include my feelings of aversion and discomfort, but there is much more description of the interactions that occurred at that moment. The second difference concerns the way the research participants identified my actions. For them, I was an observer. They told me afterward that they felt watched by me, as the quote “You were just, like, looking at us” portrays. This is in contrast to the shooting, where they had labeled me as a participant.

This experience of existential shock primarily came from seeing my informants use violence in front of me. In anthropological research on violence, the majority of the research focuses on victims rather than perpetrators (Clendinnen 1999; Robben 1996; Rodgers 2001, 2007). Although researching both “groups” is emotionally distressing, I argue that there is a distinction between listening to someone talk about violence he or she experienced and witnessing someone *being* violent. I found it easier to take distance and implement emotional shutdown with accounts of violence from the past. Witnessing violence is more confrontational; it affects us in the here and now, and it demands an instant reaction.

However, passive participation does not imply less responsibility, and I continue to question whether I did the right thing. Should I have intervened and told them to stop hitting the suspects? Was my silence unethical and was I now complicit to their actions? These questions linger in my thoughts, and fellow anthropologists have articulated concern over my actions. Let me try to elucidate them. First, I was initially overwhelmed and felt numb; all I could do was look. As this subdued, I started to feel scared that an emotional

outburst on my behalf might unleash more anger within them. Second, I felt that an intervention would not cease the violence. I knew beforehand that this company was known for its coercive and violent measures. Siphó's comment, "But I told you we were tough and know how to hit," and accompanying smirk highlight the possibility that I was their audience and thereby influenced their performance of violence. I am certain that my presence as a woman exacerbated certain masculinities and triggered a particular type of macho behavior. With Kenny's intervention, the influence of my presence has an unquestionable gender dimension, as Kenny felt that he had to protect me as a woman. However, I am also convinced that my presence was not entirely decisive, as they exhibited this type of behavior before my arrival and after my departure.

Third, I was new to this company and I did not want to jeopardize my position. As they had granted me permission to analyze their occupation, knowing that physical violence was a part of the package, I felt that expressing my judgment was simply inappropriate: I was there to analyze their actions, not judge or change them. Research on violence inherently posits a researcher into degrees of judgmentalism (Liebling and Stanko 2001; Rodgers 2001). Any act of violence is "ingrained with moral tension" (Hume 2007, 151) and contains a "deep moral bias" (Rodgers 2001, 3) on behalf of the researcher. However, in concurrence with Murray (2003), I believe that the ultimate goal and loyalty of our research is for our profession, however difficult recognizing this may be. In this episode, acting as the researcher was prioritized over a need to express my emotions and moral standpoints; recognizing the potentially valuable data rose above the desire to escape or change the situation. This is in contrast to active participation, where my emotions were privileged. Being the onlooker (in their eyes), rather than the arbiter, opened up doors, because I had now seen their "ugly side" and they felt free to "let loose" thereafter. By not judging them or intervening, I had passed a test somehow. As Crapanzano (2010, 65) states, tests are "a way one's informant learns something about you."

However, my judgments were palpable in my field notes, and they continuously pierced my attempts to comprehend certain situations. The struggle to remain open-minded and nonjudgmental was particularly demanding in my relationships with my informants. Similar to others (Hume 2007; Lumsden 2009; Pickering 2001), I disliked many informants and dreaded accompanying them during their shifts. To make matters more challenging, some were also the most valuable and interesting. The truth is that I developed close relationships with many of my informants, and I missed them when I left the field. Similar to Pickering (2001), I questioned whether I shared some of their

characteristics, what my enjoyment of their company revealed about my personality, and how exposure in the field had changed my viewpoints of violence. Undergoing these contrasting emotions, relentlessly thinking about my standpoints and personal characteristics, and negotiating between opposing emotions are emotionally draining.

Racist and sexist sentiments also influenced my dislike for several informants. Hearing informants discuss certain racist ideologies infuriated me. Sexual jokes and vulgar remarks about a woman's physical appearance or sexual acts were common practice, and although I generally ignored them, they made me feel uncomfortable. It became increasingly difficult when they "walked the walk." One example is Nick, who told me that he sporadically visits prostitutes while on duty. This became alarming when he discovered he was HIV-positive (after I encouraged and accompanied him to get tested) and continued having unprotected sex with prostitutes. I was fully confronted with this during a night shift in 2009 when we suddenly stopped at the side of the road so he could see a prostitute. When she arrived, the two of them disappeared behind a bush to return a while later. Not only did I feel uncomfortable and unsafe while sitting alone in the vehicle, but I also felt disgusted for the remainder of the shift due to the sex act and the possibility that he had passed on HIV. I was troubled with questions of loyalty, toward both his wife and the managers of the company.

Contrary to the previous incident, I discussed my feelings of aversion and disapproval with Nick due to our existing relationship. At the start of my research or when researching a new company, I did not disclose my feelings, but I questioned my informants about their behavior: why they acted in such a fashion, what they believed it accomplished, and their emotions at the time. Nevertheless, by merely posing these questions, my judgment and dissimilar understandings of morality surfaced. However, as time passed, I became more comfortable in voicing my beliefs, and my informants increasingly questioned me about them.

However, rather than stating that what they were doing was wrong and consciously entering a "hierarchical relationship" of morality (Becker 1967, 240), I concentrated on providing alternatives and explaining that such behavior was not common in my social environment back in the Netherlands. By purposely focusing on the "foreign self," they perceived my questions as less judgmental and understood why I probed into their justifications. But more importantly, through my various modes of participation, I gained insight into the complexity of their decisions and the reasoning behind their moral standpoints. Toward the end of the fieldwork, my judgments softened and I increasingly sympathized with their behavior and divergent perspectives.

Rather black-and-white notions of “violence is bad” no longer molded my moral framework; it had become rather gray and I increasingly understood why my informants were violent in certain cases and why they believed it was the right thing to do. This change in my stance developed due to the various modes of participation that allowed me to enter a certain level of understanding that outright aversion and incredulity would not permit (Becker 1967). Thus, the various types of participation and the passing of time were crucial in disclosing my own feelings that made the emotional side of their occupation (and their personal lives) accessible empirical data.

Concluding Remarks

In this article, I have discussed three different modes of participation in the field, both reactively and deliberately, and the interwoven emotionality. Although I do not intend to underestimate or trivialize the emotional turmoil associated with such research, I argue that emotionally charged experiences should be analyzed as crucial components of the gathered data. Although it is straining for the researcher and the researched, participation obliges us to experience emotions—and when understanding them, reflecting on them, and analyzing them, we acquire a more enriched grasp of the research setting.

If I had not participated—if I had declined to assist female victims or give medical assistance, if I had stayed at the gas station when we heard about the hijacking, and if I had remained at the office when everyone rushed off to apprehend the suspects—my research data would be fundamentally different and certain analytical insights would be absent. Not only would my understanding of what they do be different, but more significantly, my understanding of *why* and *how* they do would be incomplete. The first key to this is participation: similar to ethnographic fieldwork on other topics, understanding violence occurs only if one is a participant of its manifestation. The second key is acknowledging the emotions that steer and emerge from this participation, regarding them as empirical data, and centralizing them within one’s analysis. The third key is to recognize the emotionality of participation: our emotions influence the way we act in the field and our actions conjure emotions.

The emotionality of participation also shapes the relationships with our research participants. My informants’ perspectives and experiences of my actions differ in each mode of participation. Through active participation, I was seen as a helpful and useful team player; with reluctant participation, I was seen as a part of the group who needed its protection; and with passive participation, I was an observer and bystander. Witnessing how I felt also

influenced the manner in which my informants viewed their own emotions, and vice versa. It is thus not only about how I felt and acted but about how my research participants interpreted my actions and emotions, what feelings this evoked within them, and the data that emerged from these intersubjective and relational dialogues and encounters.

Furthermore, through the various modes of participation, transgression occurred on several levels and gave access to larger meanings of the research setting. For example, I argue that I placed myself in potentially dangerous situations when I decided to conduct this research, although I could not foresee everything. Similarly, when deciding to work as an armed response officer, they expect such situations without being able to predict them. They also have boundaries that are pushed further by the adrenaline rush of the occupation. Just as I struggled to maintain categories of “good” and “bad” behavior, they grapple with defining their moral framework. Just as I question the ethics of my decisions, they also query their actions. The emotionality of participation is thus not only about “I was scared, so I understand that you were scared,” but it provides an analytical lens into larger processes of the entire research setting.

Although I have distinguished between these three different modes, I want to stress that they are not fixed forms of behavior but are fluid actions that are interconnected and easily overlap from one mode into the other. For example, convincing Nick to get himself tested for HIV was a form of active participation, and sitting in the car while he visited a prostitute was a form of passive participation. Yet both were essential in building rapport with Nick and discussing our shared and divergent emotions. Similarly, Kenny intervened to protect me because I had actively and reluctantly participated during other incidents, which gave him the impression that I was one of them to protect. Each mode of participation is therefore equally important; the modes are not aligned within a hierarchical structure. Rather, there is constant negotiation inside the researcher between each mode—often with friction—and this, in turn, generates further emotions (Hage 2010). Different modes of participation thus influence, mold, and give birth to each other as intricate parts of the research process. This article is merely a beginning, and I aspire to uncover other modes of participation in the future.

Furthermore, I do not intend to show how research should or should not be done in this article; in fact, I continue to contemplate whether I should have acted differently on numerous occasions. Yet, I simply cannot change what happened. The only thing I can do now is analyze how my emotions shaped my actions, understand the consequences of my actions in the entire research process, and scrutinize how all of this this influenced the analysis I have

made of the research setting. And although this occurred in retrospect, I am certain that this was and is an extremely fruitful and insightful process.

I therefore hope to encourage other researchers to reflect on their experiences and ascertain other modes of participation after reading this to further understand the dialectic between emotions and method—the emotionality of participation—and its vital role in providing insight into the world of our research participants.

Author's Note

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Notes

1. See 2010 Annual Report of the Private Security Industry Regulatory Authority (PSIRA) in South Africa (www.psira.co.za).

2. Michael's cousin did not survive and the paramedics declared her dead shortly after their arrival.

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Author Biography

Tessa Diphhoorn is conducting her PhD research at the Department of Cultural Anthropology at Utrecht University in Utrecht, the Netherlands. She is currently a Marie Curie Sustainable Peacebuilding (SPBuild) Fellow at the Conflict Resolution Unit at the Faculty of Arts and Social Sciences at Sabancı University in Istanbul, Turkey. She completed a BA in Cultural Anthropology in 2005 and an MA in Conflict Studies and Human Rights in 2006, both at Utrecht University. Within these studies, she conducted research in Guatemala and Sri Lanka respectively.