

# When reciprocal violence turns into mutual acceptance

## A reflection on how dealing with hostile testing facilitated ethnographic production

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Reciprocal  
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acceptance

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### Abstract

**Purpose** – The purpose of this paper is to identify practices aimed at “passing the test” in fieldwork contexts characterized by reciprocal forms of symbolic violence.

**Design/methodology/approach** – It is based on an analysis of a fieldwork experience in an intentional community of activists inspired by anarchist ideas.

**Findings** – This study suggests that in a context of reciprocal violence, the researcher must qualify the specific threat that her presence poses and develop a set of behavioral practices aimed at neutralizing this threat in order to gain acceptance and gather valuable data. Three sets of practices – showing tenacity, disclosing oneself and adjusting while staying consistent – helped the researcher in crafting an acceptable status in the field.

**Originality/value** – Identifiable moments of hostile challenges should be addressed rather than avoided. They constitute indeed key gateways for understanding the culture and socializing processes of the observed group, and lead to relevant ethical questions regarding the ethnographer’s position.

**Keywords** Symbolic violence, Ethnography, Alternative organizations

**Paper type** Research paper

### The reciprocal violence of ethnography

After attending a rather long and tense meeting to organize the activities for the coming week, I go out of the room and join people on the porch. It is cold outside. Barbara, a woman in her forties who is repeatedly presented as an important figure of the movement and a proud anarchist, suddenly appears.

I greet her: “Hello Barbara, some people told me that you take care of archival data here ...”

Before I can finish my sentence she starts yelling at me: “Wait! Stop right now! I don’t like your project and I don’t want to contribute to it by any means. If your idea is to come here, to observe what is happening, and to adapt it to the business world [...] I will never help you do that!”

Feeling confused, I continue: “Ok, I understand. But I just wanted to ask you if you could take back the archival data that someone lent me [...]”

She seems really upset and yells back at me: “I don’t give a shit about the archival data they gave you! I will not help you, find someone else! If you can find someone here to help you, good for you, but I will not help you, because here, you are my enemy! You think that participative democracy within companies is good, right?”

I am now feeling uncomfortable and out of place. I stammer: “Well, yes, in some ways [...]”

She doesn’t let me finish and shouts: “Well I think that we should destroy the business world, so here, you are my enemy! And I cannot even understand how you can carry on such a project, this is totally [...] surrealist!”

She goes back to the meeting room and slams the door. (Field notes, November 25, 2012)

This interaction happened while I was doing fieldwork in a network of intentional communities inspired by anarchist ideas. As a PhD student in organization studies enrolled



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in a business school, I expected to face some challenges when I decided to investigate the organizing practices of an activist group devoted to prefiguring alternatives to neoliberal capitalism. My research project had already been officially accepted by the group based on the understanding that I broadly shared their values. However, I had underestimated the intensity and regularity of such tests. Based on this fieldwork experience, this manuscript investigates the nature of the reciprocal symbolic violence manifested in repeated moments of hostile testing and unveils the role such moments play in inducing relational transformations between the researcher and field participants, making the ethnographic production possible.

Ethnography is often described as an activity involving forms of symbolic violence committed by researchers against a group of people that they establish as their “field of study.” With an overt research design, the first dimension of violence resides in the distinction that the ethnographer creates (consciously or not) between her/himself and the subjects of the study by the simple fact of being in the field with the acknowledged and embodied status of the observer. This status is sanctioned by a title or diploma that is recognized in the broader society and confers upon the researcher the “right” to observe and study other human beings whilst maintaining her/himself at a distance from their everyday social and economic reality. Being able to escape the material necessities of everyday life while others cannot constitutes a basic line of distinction and symbolic violence committed by one social group against another (Bourdieu, 1984). This form of violence is encapsulated in the words used to describe ethnographic work. For instance, researchers refer to a “field” when describing a space in which people commit at least a part of their life and their self; thereby reducing and bracketing the life experience of others into a distant case, separated from their own reality. When observing illegal (Hughes, 1974; Goffman, 2009; Young, this volume); “gray” (Anteby, 2008); or stigmatized practices (Hudson and Okhuysen, 2009), the researcher can even put field participants at risk while enjoying protection from such consequences afforded by her or his social position (see for instance Zussman, 2016) – a problem that activists-ethnographers such as David Graeber (2009) try to mitigate by carefully selecting which data can be revealed and which should remain unspoken. At a more mundane level, every time the researcher takes notes, snaps photos or asks questions that disrupt the unfolding of life and events, the line of distinction between the observer and the observed is made salient. This ability to step both in and out of a social space makes the activity of the ethnographer suspicious (Anteby, 2013).

The second dimension of violence resides in the production of a written analysis from observing the practices of a group of people qualified as “others.” Traditionally in ethnology and anthropology, neither the negotiation of access nor the analysis of data directly involved the people being observed (Hughes, 1974). After what has been called “the reflexive turn,” scholars have paid increasing attention to the symbolic violence of written ethnographies (e.g. Van Maanen, 1979; Fine, 1993). However, the use of politically engaged (Graeber, 2009; Reedy and King, 2019; Deschner and Dorion, early cite); militant (Juris, 2007) polyphonic (Essers, 2009); participative (Clark *et al.*, 2009; Plowman, 2016) or collective (Gilmore and Kenny, 2015; Slutskaya *et al.*, 2018) methods of data collection and analysis is still marginal today within organization theory. Consequently, ethnography remains essentially concerned with the production of a cultural representation of “others” in their absence (Breuer, 2011). That is, ethnographers are engaged in a data manufacturing enterprise (Van Maanen and Kolb, 1983) that expects them to say what is happening in the field for those who populate it. As cultural representations cannot capture the essence of the observed group, scholars are left with the authority to disseminate a preferred interpretation which constitutes a form of symbolic violence toward field participants (Van Maanen, 1979).

Finally, the third dimension of the symbolic violence against participants is tied to the instrumentality of the relationships that are developed in the field. Inasmuch we try to be

reflective and to recognize the relational character of ethnography by developing relationships with field participants based on a symmetrical approach (Casas-Cortés *et al.*, 2008), on integrity and mutuality (Cunliffe and Alcadipani, 2016) and/or reciprocity and friendship (Tillmann-Healy, 2003; Deschner and Dorion, early cite), we cannot completely escape from our particular status as an observer. At some point researchers must put their observations on paper, thus creating a symbolic rupture (or at least a transformation) in the meanings of the relationships that have been constructed “in the field.” Hence, despite the personal attachment we might develop with the organization we study, the relationships are usually broken once data collection is complete, or damaged after the writing of a cultural representation of the studied group (Ellis, 2007; Breuer, 2011). Friendship ties that develop between researchers and participants can be perceived as artificial and manipulative, as they contain an inherently instrumental element. Researchers use indeed the knowledge gathered through “friendly” interactions in the production of their work (Ellis, 2007; Essers, 2009). As such, our ethnographic enterprise might be perceived as a traitorous activity (Van Maanen and Kolb, 1983; Fine, 1993; Cunliffe and Alcadipani, 2016) regardless of our feelings on that matter.

On the other side, scholars have started to engage with the difficulties they encountered in accessing the field, during and beyond the negotiation for formal access. Several scholars describe repeated attempts at discrediting the project of the researcher by pointing at controversies surrounding the field of study (Di Trani, 2008), or by disregarding “soft” sciences in comparison with “hard” sciences (Hamilton, 2012; Cunliffe and Alcadipani, 2016). Such moments occur when two competing understandings of the world are in conflict, requiring both researchers and respondents to negotiate the meanings of their work against the other (Vaughan, 2006; Hamilton, 2012). Researchers are particularly apt to face access challenges when: they are observing people engaged in morally ambiguous activities (Anteby, 2008, 2015); participants feel threatened as a social group (Di Trani, 2008); or the organization being observed displays a clear normative agenda (Ayella, 1990). More generally, ethnographers find themselves in a vulnerable position in the field because of their status as “strangers” to the norms and culture shared by the participants, who in turn perform acts of violence against this stranger by attempting to impose their own language, norms and cultural practices (Derrida, 2000). In some cases, the pressure to adopt cultural codes that might be totally alien to the researcher requires a temporal identity shift (Hunt, 1984; Kondo, 1986), subjugating her/him to the host’s code of conduct.

Although it might be surprising to face regular acts of informal testing and micro-games despite a formal agreement granted by the organization for conducting fieldwork, scholars have recently highlighted the inherently ambiguous nature of consent in ethnography (Roulet *et al.*, 2017). It happens, for instance, when the organization’s representatives grant unrestricted access to their organization without the full consent of the people that are going to be observed and interviewed, or when access is refused but the researcher finds a way to circumvent this denial (e.g. Anteby, 2008; Cunliffe and Alcadipani, 2016). Researchers can then be confronted with field resistance – that is, “any reaction that field participants collectively deploy to resist a research inquiry into their social world” (Anteby, 2015, p. 197). In this case, researchers are repeatedly tested throughout the time of inquiry.

If such moments of informal testing have been sporadically revealed, their exploration has not yet led to clear methodological suggestions. We argue that a part of the problem is in the framing of such encounters as mere “access difficulties” (see for instance Ayella, 1990; Bruni, 2006; Cunliffe and Alcadipani, 2016), and that unpacking the symbolically violent dimension of informal tests allows for a deeper reflection on the researcher’s position in the field. Understanding the threat posed by the researcher’s presence opens gateways into

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the observed group's culture and socializing processes. It also drives the researcher to take responsibility for his or her actions in crafting an acceptable status in the field. This manuscript engages in such reflections by analyzing the nature of the reciprocal symbolic violence that was present throughout a fieldwork experience. This is also an attempt at understanding the role of repeated moments of hostile testing in inducing relational transformations between the researcher and field participants, making the ethnographic production possible.

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Based on an analysis of my own experience of doing fieldwork in Longo Mai – a European network of intentional communities inspired by anarchist ideas – this paper shows that the first important step toward gaining acceptance inside the community was to unpack moments of hostile testing and qualify the threat I was unintentionally posing to participants. Three practices were then identified as helpful in neutralizing this threat and transforming hostility into acceptance (and, therefore, valuable data): showing tenacity, disclosing oneself, and adjusting while staying consistent.

The paper is constructed as follows: I first present the context in which access to Longo Mai was granted and the various tests I faced once in the field, and explain how I made sense of and qualified the threat I represented for the group. Next, I detail the behavioral practices I developed to redress this situation and neutralize such threat. Finally, I discuss the possibility of collecting valuable data by confronting and making sense of hostile testing.

### **Gaining access and facing hostility**

#### *Gaining "formal" access*

As a PhD student in management at a business school, I wanted to explore alternative organizing practices that could drive social change. Although I was not expecting to find utopic cases, I had a positive bias toward democratic and anarchist forms of organizing. I was hoping to find functional sets of practices that made it possible to foster the common good inside and around alternative organizations. Bearing that in mind, I started to explore potential fields of study through specialized books and websites. This is how I learnt about Longo Mai, a network of intentional communities of about 250 persons in European rural spaces living and working together without salary, hierarchy or written rules. A partially self-sufficient community, they manage many of their own activities such as gardening, canning, carpentry and sewing; and are politically active in a diverse set of struggles aimed at developing alternatives to neoliberal capitalism. I was able to find several books and newspaper articles about this community, which had been in existence for about 40 years at time of study. Here I found the contact information for one of the participants, Mary, who had been settled in Longo Mai since its inception. I wrote her an e-mail briefly presenting my research project and its aim. She seemed interested and asked me straight away when I wanted to visit them and how long I planned to stay. She also told me that she was going to present my project to their group for approval. Some days later, she sent an e-mail reading: "People here agree that you come and present your project. As such, we will talk about that together." And so, we planned the first visit.

A day before my arrival, Mary told me that she would be absent for my first visit and directed me to a friend, John, who was also settled in the community for several decades. Upon arrival I had trouble finding John, as people were dispersed throughout the 300 hectares constituting the main commune and were busy with different activities. When I finally found him, we sat down around a garden table and started talking about my project. He seemed confused, and laughed at my questions regarding data access and the planning of visits. As the communes were hundreds of kilometers away from my residence, I decided to scatter the fieldwork into repeated periods of observation, ranging from 2 to 15 days each, spread over 2 years. For practical reasons I also asked if I could

occasionally bring my baby daughter, which would allow me to stay for longer periods of time. He told me:

As we agreed on your project, you don't have to ask. You can come whenever you want, alone or with your family, and you can access all the documents you want! You just have to tell us in advance for the accommodation, especially if you come with your daughter, because we don't let children sleep in precarious inhabitations. Children have priority for comfortable rooms here.

To my surprise, I was not asked to present my project in front of the whole group; the e-mail and this informal conversation constituted the most "official" form of consent I could expect from the group.

Even if this first visit was supposed to last just a few hours, I ended-up staying for two days, accommodated in a room lent by one of the participants. I was directly invited to participate in the community's daily life. Hence, the participant-observation design emerged naturally from the interactions; non-participant observation seemed not to be an option.

### *Being subjected to hostile testing*

Not having been introduced to the whole group, I had to introduce myself and present my research project to each participant I met during my different stays. My project was not always well-received. Even though participants were generally very informed and valued reading newspapers and books, many of them were skeptical toward scientific research. Most participants shared the conviction that knowledge came from practice – or rather, that the sphere of knowledge could not be detached from the sphere of practice. Doing research was seen as an attempt to understand a social phenomenon intellectually, without taking part in it. As such, it was perceived as an absurd and useless activity. Casas-Cortés *et al.* (2008) have observed similar oppositional framing between intellectual thinking and "the real world" in contemporary social movements, and have urged social scientists to consider such movements as sites of knowledge-practice production. In the present case and consistently with an anarchist praxis (see for instance Maeckelbergh, 2011; Reedy, 2014), knowledge emanates from what is done in the here-and-now; it can neither be spatially nor timely distinct from present actions. Although most participants value reading as part of a learning activity, their skepticism toward scientific research points at the inherent limitation of trying to grasp and transcribe multiple forms of elusive knowledge (Torraldo *et al.*, 2018) in a written form. Juris (2008) highlights similar difficulties in trying to make sense and transcribe the affective experiences he and other activists felt when participating in counter-summit protests within anti-corporate globalization movements.

Yet, the greatest suspicion toward my research project stemmed from my affiliation to a business school. According to several participants, as my research project was backed by such a school, it would necessarily serve the interests of large companies. Even if I believe that my research might benefit alternative organizations and social movement organizations rather than large for-profit companies, I took this concern seriously. Indeed, we do not have a hand on who constitutes our audiences and what they will do with our research results. I think that there is an ethical problem in putting knowledge gleaned from anti-capitalist, non-hierarchical organizations to use in capitalist and hierarchical organizations, when the group from where this knowledge is gleaned expresses concerns about it. As is often the case with contemporary social movements (see Holland *et al.*, 2008), participants did not express a clear and unified view on this matter, making it difficult for me to make a decision about the proceedings of my research project. As a direct response to such concern though, I have decided not to use the data collected in this community to build a case study that would directly inform business school students of their practices.

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However, I believe that business schools' professors could have a role to play in transforming the values and practices in and around organizations. I am of course aware that when for-profit businesses have attempted to absorb some elements of democracy without being able to integrate their ontological premises, this has generally ended-up in the emergence of new forms of oppression (see for instance Vallas, 2006; Costas, 2012). Yet, I still believe that there is an ethical relevance in trying to teach students how to organize without hierarchy or domination. In this sense, knowing more about (and taking seriously) anarchist and democratic organizational practices might help in building forms of togetherness based on solidarity and equality in different organizational settings. The overall aim of my research is actually to detach the idea of "organization" and "organizing" from the notion of domination. There is a dangerously widespread belief that organizations need a structure of domination to be effective (Diefenbach *et al.*, 2012). I am convinced that it is possible to organize otherwise despite the numerous challenges and limitations that we observe in practice. I have chosen academia as a mean through which I participate in challenging the hegemonic hierarchical approach to organization and organizing by studying possible alternatives, and business schools as a platform to reach an audience that is directly concerned with understanding and practicing organizing in day-to-day activities – not only in for-profit businesses. This is on this ethical premise that I made sense of my research project in Longo Mai.

In front of participants' concerns and challenges, I repeatedly needed to justify the political significance of my project. Most participants were relatively ok with my presence after a brief explanation, but some of them remained doubtful or even opposed to my project. For instance, when I met Thibault, a 30-year-old man visiting Longo Mai and defining his activist approach as a "rejection of a system that is centralized around money," I had to defend my ambition to promote alternative forms of organizing in front of future managers inside business schools, as a way to participate in social change. Although he understood my point, he told me that trying to teach future managers about alternative ways of organizing was useless. In his view, managers "are conscious about all the negative consequences of their actions, but they don't give a shit, they will not change the system." Despite this skepticism, Thibault accepted my project, and agreed to openly discuss his experiences and motivations.

By contrast, Jenna, a 25-year-old woman settled in Longo Mai for several years, flatly refused to participate in my project. I approached her after participating in a demonstration co-organized by Longo Mai, where I found her particularly engaged. She told me:

I heard about your project. Do not count on me to answer questions or to do some interviews. I am against your project; as it is funded by a business school, it will be used by large companies.

After listening my point of view, she added: "Ok, so you are trying to transform the system from the inside. I understand your point, but I don't believe in it." Throughout fieldwork, she accepted my presence as we shared meals and other activities but to respect her standpoint, we never talked about topics related to my research project.

Reactions to my presence varied widely throughout the group. For some participants, my project was simply not a problem, but rather something uncanny and funny. It was among these participants that I found my main informants – some even consenting to record interviews. Such participants generally warned me gently when they perceived my behaviors as potentially problematic or out of place. During my first days of observation Thomas, a participant in his 50s and present since the inception of Longo Mai, gently warned me to avoid revealing my enrollment in a business school or mentioning anything related to "management." However, I did not want to lie about my affiliation; I preferred remaining honest. On another occasion Marvin – a 40-year-old born and raised in Longo Mai – came to me discretely after lunch to warn me:

I saw you answering your phone earlier in the lunch room, when it was full of people. You shouldn't do that, you know. Even if almost everyone owns a cell-phone here, it remains a kind of ... taboo object.

While such warnings were benevolent, that was not always the case. There were times when hostility toward my research was expressed more aggressively, as in the introductory vignette. Every Sunday after dinner, all participants gather for a meeting aimed at organizing the following week's activities and discussing any important matter. Visitors – at any time during the year there were between 5 and 20 visitors, with more attending in the warmer months – are asked to open the meeting by presenting themselves to the whole group. This practice is commonly unsettling and quite unpleasant for visitors, who have to explain the reasons for their stay in front of about 100 strangers who do not hesitate to challenge them. My first opportunity to present myself occurred on November 25, 2012, during my third stay. I did not bring my daughter on this occasion, believing that attending alone would leave me more opportunity to collect data and participate more freely in “productive” activities. There were about 90 participants gathered in the meeting room, and people were surprisingly tense on that day. The repeated criticisms toward my project left me embarrassed and uncomfortable presenting it to the entire group. I started: “I am [Carine]; I spent some time here last summer. My arrival was announced by Mary. I am doing research on alternative forms of organizing.” Suddenly, Rose – a participant in her 40s who had lived in the community for about 10 years – interjected: “I remember her, she is the one studying management!” This intervention cast a pall on my short presentation and I could see aversion on most participants’ faces. The interaction with Barbara described in the opening vignette happened just after that and constituted a turning point in my approach to fieldwork.

### **Qualifying the threat posed by the researcher’s presence**

Paradoxically, the most hostile testing I experienced in the field led me to address a set of questions and reflections that proved crucial both for understanding better the dynamics and organizing principles of the group being observed, and for crafting a method more inclined to promoting acceptance.

Being framed as an enemy by Barbara left me in a state of shock. I felt embarrassed and totally out of place. I was supposed to spend two more nights in the commune and I did not know what to do. But I did not have much time to think as Lucy, a participant in her 50s, appeared and offered me a ride to the hamlet of the commune where I was supposed to spend the night. Once in the room, I could not ignore what had just happened. I needed to make a decision: should I pursue this fieldwork or not? I wondered if I should just take my car and disappear. I did not know how many participants had an issue with my project and presence. Similarly to what has been observed by Holland and colleagues (2008) contemporary social movements are far less homogeneous than what they look like at first glance. They are often composed of a multiplicity of discourses and practices. As it was the case in Longo Mai, I was not sure of how many participants were in favor of my research project. Was it ethical to pursue fieldwork if most participants opposed my project? What if only one or two people were uncomfortable with it? Would that mean it was OK to stay? Along with these questions, I could not completely comprehend why I was facing so much confrontation about a project that had been approved by the group. As far as I knew, the community used consensual democratic processes to make decisions, and my project had been discussed and accepted before my arrival. Was I missing something? I thought that before leaving, I should at least try to understand what was happening and why it did not look like my project had been approved at all.

Hence, the experience of violence led me to be more reflexive about my status in the field along with making me more directly aware of the invasive character of the ethnographic approach. I wanted to explore organizing practices; but in an intentional community, such practices are embedded in participants’ whole life. There was no distinction between private and public sphere of life in Longo Mai; and even if I kind of knew that before entering the field, I had underestimated the difference between observing working practices within a

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formal company and sharing the intimate daily life of people in an intentional community. For instance, Helen, a participant in her 60s, said of a visitor who was not participating in any of the group activities, “I am fed up with people who don’t understand that Longo Mai is above all a living space.” The group regarded the receiving of visitors as vital to their political project as a way to diffuse their beliefs and practices, yet there were times when they felt violated. For instance, some participants disliked when visitors framed their movement as a “nice experiment.” They felt it was reducing and bracketing the meaning of their life. My presence as a researcher contributed to such dynamics.

However, this intrusive character was not enough to explain my being specifically identified as an enemy. Otherwise, all visitors would be. The problem stemmed more deeply from my status as a researcher from a business school. As highlighted earlier, some participants rejected research in general due to its very nature of distinguishing between the spheres of thinking and of doing, and of trying to transcribe complex, elusive knowledge in written form. Such criticism was deep-rooted in their worldviews. Longo Mai was organized around a prefigurative praxis, in which actions and thoughts were necessarily unfolding together (Farias, 2017b). As such, research – which involves the two-step process of first observing at a distance and then transcribing these observations in a written form – was running against their worldview. The fact that I allegedly held a worldview starkly different from theirs could explain some defiance, but not to the degree of regarding me as an “enemy” – that is, a threat to the group.

Their main objection to my project was situated at the political level. Even if the political claims of the participants were blurred and dispersed, they generally summarized them as a willingness to build alternatives to neoliberal capitalism. To use Barbara’s vocabulary, they aimed at “destroying the business world.” My affiliation to a business school made me part of the business world in their eyes. After all, I was being trained to educate future business leaders. In that sense, I was perceived as holding a key role in the nurturing of corporate values. This was fundamentally clashing with the group’s *raison d’être*. Yet, the problem was deeper than a simple difference in opinions and beliefs. Ethnography supposes that the group being observed somehow participates in the research project, by continuing to perform its daily tasks. This is the condition through which data are gathered. Hence, by opening access to a researcher, the group “participates” in the production of data despite a probable ambiguity or heterogeneity of consent (Roulet *et al.*, 2017). In my case, it means that the group was participating in the production of data that would be used – to their understanding – for the benefit of the business world. Put differently, I was making Longo Mai’s participants indirectly contribute to the development and well-being of corporations, while the political object of this group was to destroy them. As such, I was a political threat to the group, or an enemy.

To understand why so many people resented my presence after my project had been “accepted,” I asked Mary about how this “agreement” had been reached. She told me that she had understood and liked my project, but that “people grumbled” when she presented it at a Sunday meeting. She insisted on proceeding, promising to take responsibility herself. But, I wondered, why would Mary – who did not have any particular authority over other group members – commit to my project and take responsibility for it? Addressing this question led me to identify one of the most heightened tensions in the organization at the time of the study. There was indeed small group of participants who were willing to develop commercial activities to strengthen the self-sufficiency of the communes, while the majority believed that it would introduce an unhealthy consideration to the productivity of their everyday work (Farias, 2017a). Mary was part of this former group, as was Desmond – another main informant. If they did not make any explicit demands about my research – they even refused my proposition to present the results of my study, asking only for a copy of my dissertation – they might have had some

expectations regarding my presence in the community. Six months before the end of the observation period, Desmond suggested that I participate in some meetings aimed at finding common grounds with a larger group of participants to develop commercial activities while staying faithful to the group's values. I refused the offer as I believed it could jeopardize my relationships with other group members, and I was not willing to adopt an action-research design. Thus, even at a micro-level my presence constituted a political threat, as I could appear to be taking sides in an internal rift.

### **Passing the test: transforming hostility into acceptance**

#### *Showing tenacity*

After the interaction with Barbara I found myself thinking about leaving. However, upon reflection, the group's hostility did not feel right. I had the impression that participants' challenges were "unfair." That is, these criticisms were primarily based on my affiliation to a business school, as if it was defining who I was. For many of the participants, this affiliation was the most salient and perhaps only visible dimension of my identity – and this identity was stigmatized. However, I was not myself convinced that being affiliated to a business school shaped my behaviors and values. I did not recognize myself in the stigmatized view that field participants held of people enrolled in business schools. Even if I took seriously the political threat that my research project could represent for their group, I was – and I still am – convinced that my research would not benefit large corporations but could participate in developing non-oppressive forms of organizing. This led me to feel that the participants understood neither my project nor myself, and this was upsetting. I wanted to disrupt the stigma associated with my research project and to disclose my true self beyond the business school affiliation. Increasingly, I also began to feel that leaving at this stage would be cowardly. After all, I had been welcomed to partake in all aspects of the group's day-to-day life, and I had spent many enjoyable moments with a diverse set of participants. I had also observed other visitors being challenged about their chosen lifestyles, while being integrated in the commune's everyday life. Such hostile testing led me to dig further into this paradox.

The fact that access had been granted with the support of only a small group of participants was troubling. Hence, hostile testing led me to investigate further the ways consensus was practiced. Denis – a participant in his 30s settled in the community for less than a year – explained that it was unusual for the group to welcome someone with a background in management studies. He urged me to be stubborn and persist: "There are always people who will disagree, regardless of the project you carry on; but if some people accept you, then you should persist and continue." This statement echoed several others I had observed, in which decisions were taken without the consent of the majority. Following anarchist-inspired values, participants believed that they could not forbid anyone to act and to pursue his or her own project, as long as it did not require the implicit involvement of the entire group. As such, the meetings were more an occasion to share information and gather support rather than a decision-making structure. The process was consensual in the sense that anyone could expose her/his views, but at the end of the day, they were not waiting to reach a consensus before acting. The group simply required that the persons involved in a given project take the responsibility for it.

John further explained that at the inception of the movement, many people were willing to join in. As Longo Mai could not absorb all of them, the founders decided to test their courage and commitment with harsh challenges on the day of their arrival. This was a kind of rite of passage that has since been toned down into the Sunday meeting practice of letting visitors speak first. At that point, I understood that a part of the testing I was facing was a well-established socializing practice (see Farias, 2017b) aimed at protecting and redefining the group and its values. Showing tenacity was necessary to pass the test; it was

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a proof of my willingness to know and learn from them. The realization that part of this continuous testing was a component of their normal socializing process guided my decision to persevere.

*Disclosing oneself*

If a part of the hostile testing I faced was rooted in their socializing process and needed to be diffused by showing tenacity, the political dimension of the threat I was posing to the group needed complementary practices to be neutralized to make the ethnographic production possible.

When starting fieldwork, I wanted to present myself as a researcher to the group not only to develop an overt research design, but also because I was convinced that status would afford me a type of protection. I did not want to portray an unhealthy curiosity about their lifestyle, passing by as we pass through a zoo. I believed that the researcher's status constituted a legitimate excuse to engage more deeply with questions of alternative organizing and social change. As it is often unsettling to open up to a group as an outsider, I also expected this role to protect me from having to say too much about my private life. I was there for professional reasons; separating my professional tasks from my private life felt more comfortable and, I believed, appeared more appropriate. Put differently, I was trying to follow by the book the traditional injunctions of ethnographic methods – that is; maintaining professional distance to ensure the quality of data analysis – without taking into account the paradox that it created with regard to our necessary personal involvement (Anteby, 2013; Langley and Klag, 2019). This strategy proved to be completely wrong, as my whole identity was reduced to a highly contentious one. Moreover, there was no separation in the communes between professional and private spheres of life. By trying to “act professional” I was simply widening the distance between them and me.

For instance, as mentioned earlier I sometimes brought my baby daughter along, and this was initially a source of embarrassment when doing fieldwork. I thought it would appear unprofessional and constrain my capacity to perform participant observations. In most ethnographic tales, the ethnographer appears like a lonely explorer able to distance from his/her own private life for extended periods of time. I felt better when going alone to the field; I thought I looked more serious. However, I quickly realized that the conversations and relationships that I developed with participants were smoother in her presence, which made me appear more “human” and allowed people to generate random conversations about motherhood or education. Once I began sharing personal stories with the participants, they appeared more at ease. My daughter's presence forced me to blend my private and professional lives, showing a better fit with the group culture and opening up many other aspects of my identity that were much less threatening. It also signaled that I was confident enough in their lifestyle choices to share their everyday life with my child. Instead of constraining my ability to collect data, my daughter's presence allowed me to develop richer relationships with field participants and observe different aspects of the group's culture. When fieldwork was coming to an end I realized I had made some friends there – people who were actually happy to see me when I returned to the communes and in some cases kept in contact. Some participants were including me in their jokes as well as their leisure plans.

I finally understood that it would have been better to “show a human face” rather than try to hide behind the researcher status. Despite repeated warnings (Devereux, 1967; Fine, 1993; Anteby, 2013; Langley and Klag, 2019) the idea of maintaining “professional distance” in the field remains quite persistent when starting with ethnographic methods. This could be traced down to the ways we are understanding “professionalism” in general, but it could also reflect a deeply rooted mechanism of protection. When starting fieldwork we generally are outsiders – apart from auto-ethnographers (see Doloriert and Sambrook, 2012 for a review) or militant ethnographers (Juris, 2007; Graeber, 2009) – and thus

holding the precarious position of a stranger, at risk of facing hostility from the part of our hosts (Derrida, 2000). It might be beneficial to accept early on the possibility of meeting hostility in the process of doing fieldwork to reduce such protective mechanisms and to embrace the necessity of involving our self in the production of ethnographic materials. Disclosing my personal background and values had proven essential in reducing the distance and neutralizing the political threat my research project represented. It also helped me understand the importance of making friendship as an important socialization process for this group (see Farias, 2017b). Being confronted with hostile challenges made me re-think my position as a researcher, ultimately teaching me that exposing the motivations behind my project was not enough to gain trust. I had to disclose myself and blur the distinction between private and professional spheres of life to pass the test and develop trustful relationships.

#### *Adjusting while remaining consistent*

The last mechanism that allowed me to neutralize the threat and transform reciprocal violence into mutual acceptance was to partially adapt to some of the behavioral practices valued by the group whilst remaining consistent with my own claims. Following benevolent warnings, I adjusted my behavior to the preferences of the group. For instance, I stopped using my phone in public and began setting tables for more people than needed, as an open invitation to unexpected guests. On some occasions I began picking up cues about the appropriateness of a behavior, thus adjusting without waiting for warnings. Hence, I stopped taking notes in public after less than 2 h of observations. I also stopped asking participants if they were willing to record an interview after realizing that even with the more talkative people (with the exception of one or two informants), the recording machine was transforming the relationship and drying up the conversations.

But apart from these small adjustments, I stayed consistent with my broader goals. The participants wanted to understand who I really was – what were my convictions and values – and to discuss the potential limits and dangers of my project. I made the purpose of my project clear from the beginning and never changed my story even as my perspective was repeatedly challenged. Nor did I lie about my affiliation despite several warnings about the references to management or business. I was interested in alternative ways of organizing, and I believed that it was essential to have people supporting critical views of managerial practices inside business schools if we were to construct social change. Thereby, I managed to reduce the threat associated with my presence by engaging in intellectual and political discussions. Defending my viewpoints might have helped the process of mutual acceptance by making me appear as a trustful person despite the differences of opinion.

By adjusting my behaviors without changing my stated goals or values, I gained participants' confidence. This also helped me to understand that the group was not based on a clear and rigid ideology. They were open to accepting people with different backgrounds and opinions, as long as they adjusted to valued behavioral practices.

#### **Moving forward: hostile testing as key analytical entry points**

Since the reflexive turn, scholars have been reflecting on the symbolic violence committed against field participants and developing methods aimed at mitigating it (Hughes, 1974; Van Maanen, 1979; Fine, 1993). However, the reverse – violence committed by participants against researchers during fieldwork – has been addressed less systematically and has not yet led to clear methodological adjustments. This violence often materializes in the multiple and continuous tests researchers must pass to develop an acceptable social status in the organization they study (Van Maanen and Kolb, 1983; Anteby, 2015). In this special issue, we argue that the identifiable moments in which the researcher's ability to interact and fit in the field is of particular analytical relevance. To this end, we follow calls to relay and

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analyze our own stories (Devereux, 1967; Anteby, 2013). The present paper focuses on the hostile character of the tests ethnographers are recurrently subjected to and argues that analyzing moments of hostile testing is a key in crafting an accepted status in the field and for producing more reflexive ethnographic accounts.

In a context of reciprocal violence, passing the test translates to the ability to neutralize the threat we pose as researchers to the observed group. This neutralization process happens in two steps. First, it is necessary to dig into the root causes of the hostile testing addressed by field participants toward the researcher. Being confronted by open and repeated aggression pushed me to ask questions that proved crucial to better understanding my position in the field as well as Longo Mai's central organizing processes: how was the decision to grant me access taken? Who allowed me to investigate this organization and for what reasons? Are my supporters considered as deviant within Longo Mai? How many participants resent my doing research in their living space? Should I actually be here? In the present case, the threat perceived by participants was of a political nature. The presence of a researcher from a business school put the participants at risk of annihilating their political object – which constituted the *raison d'être* of the organization – by participating in a project allegedly aimed at fostering the well-being of large corporations – their political “enemies.” Internally, my presence could exacerbate the divergences between participants willing to develop commercial activities to increase self-sufficiency and those who rejected this approach. Following, I could be seen as an ally in the agenda of a sub-group of participants, and an enemy to other sub-groups. Taking instances of testing seriously and trying to qualify the threat the researcher represents in the field has proven essential for getting a deeper understanding of the field's organizational practices and beliefs.

Second, a set of behavioral practices have been developed to neutralize this threat and craft an accepted status inside the field. Displaying tenacity by pursuing fieldwork despite hostile testing was important in reaffirming a genuine interest in learning about the group's claims and lifestyle. This in fact gained me the respect of many participants. Disclosing myself by sharing personal stories and participating in activities helped in de-stigmatizing my identity. Participants got to know me more fully, permitting relationships based on integrity and mutuality (Cunliffe and Alcadipani, 2016). Finally, “adjusting my behaviors while staying consistent” with my broader agenda facilitated my integration into everyday community life, while re-stating an honest and consistent political vision. The fact that this political view was slightly different than theirs was accepted once they clearly understood it and saw that I could still adapt to their behavioral codes and question myself.

Crafting an accepted status for oneself in the field by neutralizing the threat stemming from our presence suggests that we can learn from the ways we disrupt the field, by focusing on instances of hostile testing. For instance, part of the hostility I faced allowed the group to re-affirm its political object. In this way, hostile testing exacerbates the value system of the group being observed, making it more salient and intelligible. Disrupting the field pushes participants to express their fundamental beliefs. Moments of hostile testing allowed me to better understand some organizational principles at play in the community. For instance, I was expecting to find a more structured group due to its 40 years of existence. As such, I thought I would be presented to the whole group on my first visit and that the formal acceptance of my project reflected homogenous consent. I also believed that my status as a researcher would provide me with a legitimate reason to be there and a safeguard against personal involvement in the field. I had not realized yet that I was entering a home in which participants were building their own life. They were much more diverse than I had expected, holding different views and purposes. In such a context, I could not be completely detached from the personal dimension of everyday interactions and had to disclose as much of myself as I asked from them. My request for “data access” had elicited laughter from John, as it revealed my disconnection from their reality and might have

reinforced some stereotypes about researchers. I was looking for formal structures to reduce the uncertainty of my endeavor, while Longo Mai's participants see human interactions as something necessarily unpredictable that cannot be studied. Their hostile reactions toward my affiliation to a business school also revealed their entrenched position against the practices that are commonly valued in the business world. Finally, disrupting the field sheds light on the integration and socialization processes at work in the community. As described earlier, all visitors were challenged by Longo Mai's participants, leading me to question what portion of the hostile testing I encountered was a part of the group's socialization process and how much was addressed to me specifically. Reflecting on the history of the community, I realized that challenging new-comers was a way to filter committed activists from curious visitors. Hostile testing was a rite of passage showing a form of "hostipitality" (Derrida, 2000) that proved necessary for the community to survive and structure itself as an alternative organization.

This study of a fieldwork experience in an intentional community of activists shows that crafting an acceptable status does not require a full adaptation to the field's norms of conduct, known as "going native." Field work by its nature sometimes disrupts, unsettles and even threatens field participants (Ellis, 2007; Anteby, 2015). They might respond by subjecting us to repeated moments of hostile testing. Such moments need to be carefully analyzed as they constitute privileged gateways to the field's culture, structuration processes and its untold behavioral expectations. Considering such moments as "difficulties" to access a given field of study (Bruni, 2006; Cunliffe and Alcadipani, 2016) may prevent us from reflexively digging into our own actions, decisions and ethical responsibilities in using strategies to start and continue our ethnographic observations of a given social group. Without providing clear and definite answers to the dilemmas associated with moments of hostile testing, this paper urges us to at least stop and reflect on them as they occur. Addressing such moments is a key for our ethnographic production for three reasons: they provide clues for understanding our position in the field and the potential symbolic violence that our presence generates, analyzing such episodes helps in crafting an accepted status and gaining confidence and they reveal specific cultural and socialization processes at play in the studied social group.

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