Being loyal to fieldwork: on building the ”contract of silence”

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Abstract
The aim of the present contribution is to analyze how relations of loyalty emerge between researcher and researched during ethnographic fieldwork and to defend a perspective against the principle of open science. I discuss methodological issues with respect to my several years of multi-sited fieldwork experience in various labs, research centers and medical institutions, during which I inquired into the design and use of exoskeletal devices. Exoskeletal devices are technologies applied to three fields of application: rehabilitation, industry and the armed forces. Their invention is the subject of high levels of economic and scientific competition. Given these constraints, I was compelled to develop "loyalty strategies", one of which I call the "contract of silence". I associate this category with an ethnographic exercise in how to address one's interlocutors during fieldwork. I conceive of this process as a result of consciously retaining the information obtained from interviewees that might endanger the position of the researcher in the field. Although a tacit contract with one's interlocutors during ethnographic fieldwork implies anonymity, certain sensitive fields and research situations require forms of auto-censorship and the control of published results. I associate these strategies with the fabrication of fieldwork secrecy.

Keywords
exoskeletons, contract of silence, loyalty, multi-sited ethnography, open science

INTRODUCTION

Opening Data: for What Context and for What Issues?

The question of open data has recently emerged in the European scientific landscape[1] and has raised numerous debates within the scientific community. In particular, anthropologists and sociologists working with ethnographic methods are engaging in new debates regarding the status of the data they are allowed to have access to and to use. Whereas in Anglo-Saxon research communities the concern with the anonymity and identity protection of informants has a longer epistemological history,[2] these questions are still mostly at an early stage in France [Forcadell and Laborie, 2020]. Nonetheless these developments are steadily acquiring a stronger political appeal.[3] In my position as a sociologist engaged in the practice of multi-sited ethnography, in this article I will focus on the reasons that made me be cautious with respect to sharing information about my own research. Based on this experience, in the following I will develop the notion of "contract of silence" and argue that in certain situations our fieldwork must involve researchers in explicit forms of censorship. In my specific case, in which I examine research on the design and use of exoskeletal devices that are still at the development stage, sharing collected data would obviously expose me to complex consequences both *during* my fieldwork as well as *afterwards*.

[1] https://data.europa.eu/en (access 23.03.2023)
[2] https://www.theasa.org/downloads/ethics/asa_ethicsgl_2021.pdf (access 23.03.2023)
https://www.americananthro.org/ParticipateAndAdvocate/Content.aspx?ItemNumber=1656 (access 23.03.2023)
[3] https://www.ouvrirlascience.fr/plan-daction-national-pour-la-france-2018-2020-lengagement-18-pour-un-ecosysteme-de-la-science-ouverte/ (access 23.03.2023)

Between 2014 and 2019, I conducted fieldwork in three countries (France, Germany and Switzerland) visiting seven sites and interviewing 46 persons, both users and experts, with experts being the greatest in number (N=33). Whereas users are mostly content to share data about their experiences and openly criticize this type of technology during interviews (or personal conversations), expert knowledge produced in labs in robotics is highly sensitive. Visiting labs and interviewing those who are directly involved in research programs to develop this type of technology proves to be a fragile exercise that needs constant ethnographic negotiations. Because exoskeletons are mainly developed for three fields of application– rehabilitation, industry and military – besides struggling to protect expertise from being disclosed between and among labs, one of the difficulties in opening data about my fieldwork to others concerns aspects of defense and security. As a consequence, exoskeletons prove themselves to be highly controversial technologies, and any attempt to "open" details about how they are designed or about their potential uses exposes the researcher to a variety of consequences. Having access to and discovering how fields of expertise are produced among the experts responsible for the design, as well as revealing the use of these devices led me to engage in what I call a "contract of silence". I understand the "contract of silence" as a methodological tool that guarantees confidence and trust during fieldwork. This is the notion I will argue in favor of in this paper.

With regard to this set of ideas, respecting and protecting one's informants by "silencing" details about the fieldwork compels the ethnographer to defend a position against a growing trend that advocates and supports the principles of open science. Arguments for the open science are numerous and cover such aspects as defending access to knowledge [Rentier, 2018], criticizing the knowledge economy and its associated patent models [Mowry, *et al*., 2004; Penin, 2020; Sampat, 2004] or ensuring better objectivity and the possibilities of replicating one's results [Forcadell and Laborie, 2020, 14; Penin, 2008]. Nonetheless such perspectives seem to leave aside disciplinary fieldwork contexts and the specific rules of these contexts that researchers need to comply with. Anthropology and sociology both explore fields that are often highly sensitive. Technological innovation is one of these. In this case, instead of helping the community of researchers to understand and objectify the data obtained with greater acuity, the principles of open science may lead to a variety of conflicts. Moreover, in disciplines such as anthropology and qualitative sociology replication is difficult, and objectivity includes a wide panoply of criteria [Strübing, *et al*., 2018]. As I will argue in the following, in order to gain knowledge about these sensitive fields, researchers in the social sciences need to develop an epistemological politics of silence, instead of adopting those of open science. In my own fieldwork, I had to rely on the "contract of silence" in order to ensure my being accepted on the sites I visited, and especially my being able to publish the results of my research.

1. ENCOUNTERING EXOSKELETONS AND CROSSING THEIR SITES

Inquiring into how emerging technologies such as exoskeletal devices are currently invented and used is a complex endeavor. As I will show in this paper, traversing robotic worlds is a risky journey and faces the ethnographer with fragile equilibriums. Exoskeletons are relatively new technologies that question our representations about bodies and their skills. Basically, these devices are being developed to respond to needs in three main areas: rehabilitation, industry and the armed forces. Acting in parallel to human limbs, they are intended to help users perform motor tasks such as walking in rehabilitation, practicing arm or finger movements, or carrying heavy loads in industry or the armed forces. They may be either actuated, and thus function with power (active), or passive. The latter category of devices is usually lighter in weight and worked by springs. Other prospective fields for the

use of exoskeletons include assistance with muscular-skeletal weaknesses due to old age and recreational uses. In the second case, they help users carry loads on their backs when hiking.
Approaching these devices in their materiality, as well as those who design them and their final users, means that many of the characteristics that common-sense perceptions associate with Marvel superheroes must be disappointed. On another level, however, it also means that accessing these fields compels the ethnographer to protect pieces of information, rather than have them disclosed. Because I wanted to understand whether the associations of these devices with cyborg figures in science-fiction movies and pop culture have any concrete foundations, I embarked on many journeys for several years between 2014 and 2019. I thus discovered many biases about exoskeletons' concrete functions and use and that the mixing of fact and fiction that creates specific expectations about exoskeletons has little to do with the current reality of these devices. In traversing the variety of sites to which I was allowed to have access, I engaged in the practice of what anthropologists call "multi-sited" ethnography [Hannerz, 2003; Marcus, 1995; 1998].
My time in the field was very clearly defined, and I had to capture the data rapidly. Prominent in my fieldwork was my constraint management. A main challenge was to observe the changes of the "exoskeleton" object at the various research sites, and to ascertain what consequences these changes have for how the users' bodies would further be defined and experienced. To observe how objects transform from one status to another is already a classical approach in ethnography [Appadurai, 1986; Niewöhner and Scheffer, 2008]. Yet, exoskeletons are not jewelry or commodities. Their close connection to the human body and their being sophisticated technological devices engages the ethnographer in a specific process of reflection about what scientific and material mutations exoskeletons actually produce. It was these mutations that I wanted to understand and that compelled me to develop fieldwork strategies adapted to the multiple facets of my object of study, among which was the "contract of silence".
In line with the principles evoked by [Marcus,1995; 1998] in his characterization of multi-sited ethnography, I was a "follower". I was not interested in intricate descriptions of sites or in how exoskeletons transformed bodies at each specific site. What I wanted to understand, rather, were the relations and active circulation between these sites, and how, despite being developed for a specific need of the human body (walking, arm exercises in rehabilitation or lifting and carrying loads in industry and armed forces), the exoskeleton will impact the conception of that very body, its abilities, and capabilities. Much in this experience was about comparing bodies in their attempts to overcome or deal with deviance and "normality", as well as comparing scientific strategies for dealing with these attempts. My object of study was, as [Marcus, 1995, 102] justly noted, "ultimately mobile and multiply situated". Consequently, I had to follow it. Thus, I came to follow people and things because they are the carriers of metaphors, plots, lives, or conflicts. More than anything, people are (at least to date) embodied, and what I was following was their bodies accompanied by this mesmerizing object. In doing so, I found myself following the expert strategies behind these technological devices and was confronted with specific restrictions and knowledge that I had to keep safe and confidential.
These many crossings made me discover that the devices I had followed during these many years forge three "corporeal worlds" [Butnaru 2023 forthcoming]. In this shaping process, one of the intriguing aspects that I found to characterize all observed areas for which exoskeletons are designed was their being protected by *secrecy*, a feature that I analyze in the following. Similar to other scientific cultures that compete for results on the wider academic and industrial market [Nelson, 2018; Petryna, 2009; Pollock, 2019], the work in the labs to which I had access is characterized by extremely competitive goals. Exoskeletons are developed by mixed teams, in which experts from the engineering sciences, neurosciences,

sports sciences, medicine and often physiotherapists meet. As these devices are products that will ultimately be offered for sale, the economic aspect is central. Hence, both the scientific constraints and the economic ones compelled me to respect "secret" borders that are highly relevant to expert cultures in general. To these, I need to name other sensitive fields related to the area of defense that involved me in developing strategies of loyalty. Based on these fieldwork experiences, I came to categorize what I will describe in the following sections as the "contract of silence".

2. PRESENCE LICENSES

In a recent text, "Empiricism and Its Fallacies", [Burawoy, 2019] explains the importance of ethnographer's loyalty towards one's own field in his defense of sociologist Alice Goffman. Goffman's study, *On the Run* [Goffman, 2014], an ethnographic study of a poor African-American community in Philadelphia, has stimulated numerous debates in the North American community of ethnographers, and not only there. Journalists and legal experts were equally involved[4] in either defending or attacking her study. A main point of criticism was that she took part in a criminal act during her fieldwork, a type of participation that Burawoy accepts and also defends in the name of scientific probity which ethnographers need to concede. Luckily, I was not confronted with criminal acts during my fieldwork; yet, the types of restrictions and censorships I needed to learn and integrate into my ethnographic practice were not easy ones. Indeed, I agree with [Burawoy, 2019, 52] when he notes that "ethnography is the study of the world in the time and space of the participant". Still, ethnographic research involves learning to cope with the specific restrictions and codes of the cultures being observed. Specifically, those scientific cultures I previously named, as well as the cultures of pain of users with heavy motor impairments, such as spinal cord injury (SCI) and stroke (CVA), or cultures of working bodies and the bodies of soldiers, require a highly demanding training of one's presence as ethnographer.

Because my fieldwork took place over several years in three different countries, I had to quickly adapt to expert cultures, as often I would visit in one month two or three sites that had projects in the three observed areas of rehabilitation, industry, and the military. Such shifts also confronted me with materially observable techno-scapes with diverse geographies and manners of practicing scientific research. In these crossings towards the various forms that my object of study took, I also changed. My research object transformed me in turn, asking me about ways of being and doing. As the military anthropologist Tone Danielsen notes, "anthropologists used to be rebels: the ones who travelled far, far away and studied 'the others' to set their own societies in relief. The hallmark of anthropology now is not the geographical distance we travel to do our fieldwork, but the use of a professional gaze, sense, and practices to give new perspectives to taken-for-granted, hard-programmed truths" [Danielsen, 2018, XV]. Although I am not an anthropologist, but a sociologist working ethnographically, my fieldwork likewise required me to forge "the use of a professional gaze, sense, and practices to give new perspectives to taken-for-granted, hard-programmed truths."

Crossing worlds of pain in rehabilitation and entering labs that conjoin the ghost workers of the devices, namely roboticists, as well as crossing the worlds of bodies at work, and especially those of bodies engaged in military activities, exposes one to a special training in fieldwork. Some time ago, Barth characterized fieldwork practice as an "extreme sport" [Barth, 2008, 11, quoted in Danielsen, 2018, 2]. Perhaps besides being an exercise in multi-sited ethnography, the type of fieldwork in which exoskeletons and their bodies engaged me

[4] A notable figure is Stephen Lubet, a law professor at Northwestern University, who Buraowy openly criticizes in the article I just quoted.

was one of extreme discretion. As Marcus notes, "multi-sitedness can emerge as a research space, not given by existing representations or understandings of processes, but rather as the mapping of a space or field of social action that is found in the field itself through closer work and collaboration with certain subjects" [Marcus, 2009, 186]. Collaborating with my subjects meant in some cases that I had to interfere in the contexts very little. I was thus at times compelled to engage in forms of 'discrete' ethnography in order to preserve both myself as a researcher and my interlocutors, as well as the concrete events I observed and learned about.

Globally, during all these years, I had to negotiate how my being an intruder and my disturbing the order of the place – and sometimes that of the more global institutional space and time in which tests, ethnographic visits or interviews took place – could best be tolerated. For example, before I started my fieldwork on a site where projects in rehabilitation robotics were being developed, the professor who was in charge of these projects explicitly warned me not to interrupt the doctoral students in engineering sciences working on rehabilitation exoskeletons. Their pauses were my pauses; but their pauses were also my opportunity to discuss their projects with them, opportunities I had to forego. These are examples of censorship within the site defined as such by the ways in which experts work, and thus by observable and material practices. Here I agree with the anthropologist Anne Pollock that "it still matters who makes knowledge and where" [Pollock, 2014, 851]. Although my observations were not carried in highly conflictual political areas as hers were, Pollock's research concerning pharmaceutical developments in South Africa, to consider instances of expertise and the places in which this expertise was produced engaged me in "how" knowledge emerged locally.

In this fragile exercise, during which I specifically had to respect "who" makes knowledge and "where", no interaction model between myself and the specific scientific culture of a location being visited was transferable from that site to another. Transferring this particular interaction model from one interviewee to another was even less possible. That our interaction patterns are generally precarious has long been an acknowledged fact in sociological research, Erving Goffman being one of the classic analysts of this phenomenon [Goffman, 1957]. Still, meandering among expert stocks of knowledge [Schutz, 1967] that are in competition but also prone to obvious risks, responsibilities, internal rules, and often penalties exposes the ethnographer to a specific vulnerability during fieldwork. Thus, besides being a "plural person" [Lahire, 1998], I needed to develop fieldwork strategies and techniques in order to ensure my being a "plural intruder". In these steady transformations, not only were my interlocutors in their status as ethnographic persons "bundles of relationships" [Weber, 2001, 489], I too became one of these bundles. My peculiarity, however, was that I was a near-by product, one that sometimes needed to keep still, to listen carefully, to remain on one side, to learn technological jargon quickly, and occasionally to help with small tasks during tests. During one of my visits to a clinic for motor rehabilitation, for example, I helped the physiotherapist "dress" the test person with the exoskeleton. In this context, being loyal to my fieldwork meant that I became an occasional participant in it, while still respecting boundaries. Some of these referred to "silencing" things I learnt and experienced on sites. Rendering these fieldwork details "open" and accessible would be an act of disloyalty on my part.

3. "WORDING" LICENSES: "LE SU DOIT ETRE TU"

In one of his analyses of the central Senoufo community of the northern Ivory Coast, András Zempléni discusses the emergence of the economy of the secret ('l'économie du secret') with respect to this population. Here, he explains that ethnographers as intruders are "secondary

addressees" in this process of the maintenance and protection of secrecy. During fieldwork we may have access to protected information, both as the main addressee, which was sometimes what I experienced, but also as secondary one. Zempléni calls this position of catching information in tidbits "secreta". As he notes, "it's the others – the addressees or potential intruders – who constitute these secreta – these 'fugitive looks' or 'sighs', these manners, these 'noticed' presences and absences"[5] [Zempléni,1996, 24]. Obviously my presence at the various sites was strictly supervised. The type of intruder I was gave me a stronger label as a "stranger" than ethnographers in more classic fieldwork may experience with respect to their acceptance by or living in a certain community. This situation of course has specific consequences. For example, besides recording and accessing information, one of the main challenges was how to deal further with the information I obtained during an ethnographic visit or interview while continuing to conduct interviews and access new observation sites. Indeed, what I knew sometimes needed to be kept silent: "le su doit être tu" [Adell, *et al.*, 2021]. Hence, my being a "bundle of relationships" required me to be occasionally silent about certain details when changing labs and confronting visions of how devices are conceived to work; it also meant to be especially cautious about how I formulate my published results.

One of the strategies through which I was able to ensure these specific forms of censorship was to use very general vocabulary or general descriptions about the devices and their answers to their users' needs. How I spoke or sometimes how I did not, and voluntarily refrained from divulging details I knew about in front of my interlocutors, protected not only their secrets. It also ensured me a form of neutrality that I needed in order to pursue my journey with exoskeletons and their bodies. Sometimes, wording "licenses" also meant that I had to adjust my own vocabulary in order to persist in worlds of meaning in which classifications bore other semantic loads. To adhere in such a context to the principles of open science would mean disrespecting my interlocutors and consequentially betraying my research object.

My engaging in this practice imposed forms of auto-censorship on me. My self-censorship in the field was not practiced because I wanted to deliberately omit details considered too private, emotionally laden or personal [Weber, 1991, 80]; nor did this voluntarily "bracketing" concern reflections on aspects that were deemed too technical for my sociological inquiry. I controlled the content of my questions because I was aware that transgressing labs and research sites might "discredit" [Goffman, 1963] me. Multi-sited fieldwork imposed on me the management of silence and "wording", as sites and the experts involved in their definition qua productive knowledge were preoccupied with ensuring the safety of data. Indeed, as Matei Candea remarks, censorship is an ethnographic reality. As he notes, "censorship implies the possibility of an ideally free, autonomous speaker. It also implies that there is a specific and identifiable location from which censorship operates: a specific individual, or more commonly a group, who has the power to intervene in and limit the expression of the censored agent" [Candea, 2019, 6]. The nuance I wish to make regarding my own experience of censorship and silencing was that I was the authority that did the censuring. In order to ensure relations of loyalty, respect the time and space I was allowed to experience, and have access to a technological object that was literally emerging in front of me, I engaged in an exercise of silence. From this point of view, I understand self-censorship during fieldwork as a productive manner of reinventing relations with one's own object of study. Additionally, I defend censorship as an ethnographic tool that contextually protects one's informants and the knowledge they provide.

[5] Translation by the author from : « […] ce sont les autres – destinataires ou intrus potentiels – qui constituent ces secreta – ces « regards furtifs » ou « soupirs », ces airs et ces manières, ces présences et absences « remarquées » [Zempléni,1996, 24].

Unlike rites of initiation that explicitly oblige the initiated to protect received knowledge and keep it secret, my position in the field and the forms of censorship I practiced enabled me to use other forms of expression and ethnographic shaping. I call this strategy "ethnographic loyalty." Here I agree with [Zempléni, 1996, 23] that the ethnographer certainly needs to become an "intrusion professional". By this I mean not only being accepted and tolerated in the field, capturing sensitive information and retaining it or not divulging it. In my own case, being an "intrusion professional" meant that I was not showing disrespect to these scientific and technological products or to the scientists involved in designing them. I was always openly informed that tests were producing innovative results. Hence, my loyalty to the field resulted from the practice of information retention and control. This exercise made me develop a methodological sensitivity during my fieldwork and led me to elaborate what I call here the "contract of silence".

Sometimes, my negotiating presence and discussions with interlocutors from different sites and countries, and my knowledge that these interlocutors were explicitly engaged in scientific and economic competition, often made me feel like "matter out of place" [Douglas, 1966/1984, 36]. [Douglas, 1966/1984, 36]discusses this concept in her study *Purity and Danger* within the framework of a wider analysis of uncleanness and pollution that gives an account of how the social order persists and how dangers are situated within this very order. As she pertinently notes, unity emerges through rituals of purity and impurity: "within these patterns disparate elements are related and disparate experience is given meaning" [ibid. 3]. Yet, nuancing Douglas's perspective of "pollution" and how such a mechanism contributes to how society functions globally, my transgression of borders involved especially traversing landscapes containing sensitive knowledge, and not just entering forbidden spaces and timescapes. Building relations of loyalty with my fieldwork was justified by these types of crossings and meant that I was accepted in my role as a "polluter", although sometimes the researchers I observed felt some discomfort at my presence. I recall that, during one test of industrial exoskeletons, one of the experts in sports sciences from the team who was measuring the motor strain on the muscles of the test person explicitly told me that the results were original and should be published soon. She openly showed her suspicion that I might transmit this knowledge or share it with other labs. I was thus "polluting" because I was under suspicion as being a potential source of information leakage.

Although access to sites was strictly formalized and controlled, and although my fellow researchers knew who I was and why I was spending time with them, and thus agreed to my being a "polluter", I was continuously confronted with an epistemological tension: that of keeping the secrets of the site I was visiting and of knowledge about the device I had access to. Besides keeping knowledge about the site, a second type of constraint was how I spoke or wrote about the specific thematic field I was learning about: whereas it is common to understand why experiments and the labs hosting them are bound by secrecy, military worlds are just as restrictive regarding information about technologies of defense. Since exoskeletons may be used either for logistics – especially carrying heavy loads, which is similar to what has been developed in industry – or for combat, and thus for defensive purposes, a further nuance in how my practice of what is "known should be silenced" emerged and forged my interactions in the field. As Christine Hine notes, "multi-sited ethnographers craft field sites with an eye to producing appropriate accounts for heterogeneous audiences […]. Rather than a pre-existing territory in the middle, there is instead an embodiment of tensions, in the ethnographer attempting to sustain a sense of meaning in the project out of diverse responses and accountabilities" [Hine, 2007, 657]. The sense of meaning that I was trying to defend and build up through my many journeys and stays needed to incorporate a variety of contracts of silence besides the tensions related to the type and quality of information. I had to respect both spatial boundaries and epistemological and political ones by learning the art of silence

while engaging in a specific fieldwork contract with my fellow experts, test subjects or more experienced users.
Unlike ethnographers dis-located in far-away communities and doing single-site fieldwork, I had to semantically dis-locate myself in a plurality of small-scale worlds, pursue other conceptions of corporeality and technology than those with which my own discipline works, and respect areas of expertise and needs. I thus engaged in exercises of censorship that involved not only how I translated my corporeal presence from one site to another, but especially how I managed information. Managing language, the knowledge it contained and its silences, I was compelled to develop further licenses than those involving my onsite presence in the flesh. All these constraints remained after I had finished my fieldwork. Contrary to the perspective evoked by Matei Candea, who notes that "ethnographers have proved that, [in the example he discusses, D.B.] anthropology could talk about anything, anywhere and in any way" [Candea, 2007, 170], the example of my fieldwork explicitly shows that ethnographers are not really allowed to talk about anything, anywhere and especially in any way about the facts they observe. They are sometimes engaged in an explicit politics of silence that they need to continue defending even if the fieldwork has ended. In doing so, ethnographers continue to demonstrate their loyalty [Burawoy, 2019, 52].

4. EXPANDING ON CENSORSHIP AS A FIELDWORK TECHNIQUE: WHY SOME LOCI NEED TO BE PROTECTED

In his discussion of multi-sited ethnography in the article I quoted previously, *Arbitrary Locations: In Defence of the Bounded Field-Site*, [Candea, 2007, 181]concludes with the following idea of relevance to this paper: "sidelong glances at other modes of knowledge production might help us experiment with our fieldwork and writing practices, in order to recapture the value of *not* knowing certain things". I would note that, besides *not knowing* certain things, specific sites compel us to *not showing* certain things, although as ethnographers we happen to know what they contain. Globally, anonymity and confidentiality [Kaiser, 2012] are general laws that ethnographers must follow to protect their informants. However, building secrecy, auto-censorship *during* fieldwork and censorship *after* fieldwork has specific epistemological and methodological consequences. The concern with opening up data for the purpose of allowing more transparency and scientific objectivity regarding how results are obtained ignores the complexity, tensions and political dimensions that some fields and their sites include. Some of the acquired information cannot be shared without endangering both the onsite actors and the ethnographer, both with respect to providing access to recorded data and in published materials [Becker, 1964].
Tone Danielsen describes this contradictory reality, which characterized some of her ethnographic experiences, in her study of the MJK, Norway's military special forces. She notes: "during the fieldwork, anthropologists are bound to obtain information they should not know. Interpersonal relations are continuous balancing acts, during fieldwork and when we publish. According to anthropological standards, it is an ethical problem to reveal the inner secrets of community. Some of MJK's … rituals and practices are not classified military information, but *locally considered secrets*" (emphasis mine, D.B.) [Danielsen, 2018, 17]. As I myself experienced, sometimes, the more global projects of scientific enterprises developing technologies in robotics or projects carried out within the world of defense were indeed "doubled" by forms of local secrets. Thus, in my experience, confining information proved itself to be a necessary methodological tool, forging the specificity of these very sites.
My understanding of censorship, and in my own case also auto-censorship *during* fieldwork, emerges from the locality of the secrets that I needed to respect as well as carry with me. Some of them still stay with me and will continue to do so. Nonetheless, I do not understand

auto-censorship as an extreme form of coercion. Rather, I suggest that ethnographers may use it as a constructive fieldwork strategy during their multiple journeys with their interlocutors in order to build and maintain relations of loyalty during fieldwork. In the end, the field and its locality surface during the ethnographer's presence at a specific site. Much of what we anticipate may be deeply contradicted by our findings in the field. Also, much of what we learn is elicited, responded to, and invented with us, the ethnographers. As Katherine Vicus notes in a recent article, "The Agonistic Approach: Reframing Resistance in Qualitative Research" [Vicus, 2008], there is a potential of conflict in the field that nonetheless needs to be revealed as constructive of the reality of research. The position I defend is that this potential, although needing to be acknowledged, described and analytically used to objectify meanings otherwise ignored, may not be rendered completely accessible to the public eye, whether to the eyes of our fellow researchers or our more general audience. Arjun Appadurai, notes that "the ethnographic project is in a peculiar way isomorphic with the very knowledges it seeks to discover and document, as both the ethnographic project and the social projects it seeks to describe have the production of locality as their governing telos" [Appadurai, 1995, 182]. In my own case, producing locality involved engaging in protecting information and in doing so producing forms of loyalty towards my field sites.

Multi-sited fieldwork, as I experienced it with respect to my own research, may not be conceivable without a variety of compromises that the ethnographer needs to steadily negotiate. Obviously ethnography is a fragile exercise, no matter what the loci. Still, some terrains have more political weight than others, their traces remaining present both *during* and especially *after* the research was conducted. As emphasized in the study by Forcadell and Laborie, one cannot engage researchers in the politics of open science without taking into account the juridical and ethical constraints [Forcadell and Laborie, 2020, 21]. Besides the strategies I have previously evoked, I also add censorship and auto-censorship to this list. As has already been shown, forms of censorship are generally related to sociological and anthropological scientific production, with the publication of one's results being the final stage and one of the most challenging [Weber, 2008]. Yet, I find that being confronted with expert cultures that are in competition, as well as with political arenas that may very quickly become sensitive to provided information – as, for example, with informants from the armed forces – recaptures the value of those fieldwork techniques and epistemologies that are primarily perceived as irritating or counter-productive. Although it seems surprising, silencing and not opening data may contribute precisely to nuancing nearness to and strong resonance with one's empirical object and ethnographic practice.

On another level, these examples show that sites are not only moments of discovery or entry points. Often, besides dictating to ethnographers how they access them, they also determine how ethnographers *leave* them. Relations of loyalty during fieldwork, their contracts of silence, and forms of censorship revalue the ethnographic experience of locality in a productive manner and prove themselves to be further nodes in analyzing both the temporalities and spatialities of doing fieldwork. In this line of thought, I argue that "the contract of silence" and its contribution to establishing relations of loyalty with interlocutors in sensitive worlds both *during* and *after* fieldwork reinforces reflections on how "the production of locality" [Appadurai, 1996, 182] gains further nuances. Secrecy and censorship emerge thus in these contexts as necessary elements to highlight negotiations of field practice.

Conclusion

As I have shown, ethnographic practice is often built on the necessity and possibility of compromising with one's interlocutors and thus with one's research objects. Being loyal to

one's fieldwork is sometimes inseparable from pursuing an economy of secret practices, as such an exercise may contribute to ensuring the integrity of data while also complying with research ethics criteria and confidentiality. Not divulging certain scientific details or discussing defensive strategies related to the design and use of exoskeletal devices is just as essential as neutral manners of speaking: for example, using standard and general terminology in order to ensure a more impartial tone for the questions being asked or discussions being elicited. Excluding details during fieldwork while traversing sites, as well as excluding details from published results, may prove to be a productive technique that in the end defends the fieldwork, its authenticity and the research community's rules that groups of experts share. It is due to these factors that principles of open science cannot be followed and that the fabrication of secrecy to which the ethnographer is committed is necessary. Contracts of silence are indispensable tools in building and likewise preserving relations of loyalty with the field. They also enter into ways of shaping modes of locality, since, as Vered Amit pertinently noted, "the ethnographic field cannot simply exist, awaiting discovery. It has to be laboriously constructed, prised apart from all the other possibilities for contextualisation to which its constituent relationships and connections could also be referred" [Amit, 2000, 6]. The contract of silence enters the panoply of these manners of accommodating.

Having acknowledged and practiced a strict "site management" [Sørensen, 2008, 322] that was imposed on me, I have therefore engaged in specific modes of relating while capturing and managing experts' secrets. As Marilyn Strathern remarks, "person-to-person networks that succeed by replicating the conditions under which persons relate to one another, work, as relations do, holographically. Their power is that interpersonal relations can take any scale, be productive at any order of encounter. […] they do demand time, energy and cultivation, and that is what is at stake" [Strathern, 1995, 29-30]. The fabrication of fieldwork secrecy shows precisely that fieldwork relations indeed take on any scale. Many of them include disciplining one's onsite presence and observing "wording" licenses. Many others include keeping secrets. Protecting sources of knowledge is inextricably related to obtaining valuable data for one's research. More generally, it contributes to the construction of the field and, in my case, to its "multi-sited imaginary" [Marcus, 2009, 184]. My defense of the "contract of silence" and of its associated regimes of censorship as manners of organizing, building, and maintaining confidence in the expert communities I interacted with and learnt about highlights the fragilities that the application of the criteria of open science would endanger. As Jean-Klein and Riles note, "if the ethnographer willingly serves as a kind of tool, she is a tool for the 'echolocation' of knowledge [Wagner, 2000], for allowing others to practice their knowledge on and through her" [Jean-Klein and Riles, 2005, 186-187]. I would add that ethnographers are also bound by "contracts of silence" and thus must be prepared to keep secrets. And this is precisely what the growing worlds of open science and open data need to carefully consider with respect to the practice of this fieldwork practice.

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