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Ethnography among 'experts': notes on collaboration and sabotage in the field

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ABSTRACT Based on an analysis of how an ethnographic research project was received by 'experts' in the field, a series of reflections will be put forward on ethnographic fieldwork and some aspects of anthropological knowledge. Developed on the assumption that the vicissitudes of ethnographic practice are central to the process of knowing the 'Other', this reflexive exercise focuses on how informants may turn into either the ethnographer's saboteurs or his/her contributors – and even co-authors. As spaces in the academic arena are shared – and somehow disputed – extremely conflictive relationships spring raising fears in the ethnographer, who may suffer direct sabotages in his experience on the field.

KEYWORDS: *collaboration, fieldwork, reception, reflexivity, sabotage*

Interacting in the field

This article[1] comprises a series of reflections on the characteristics of fieldwork, the relations with the subjects of study and certain general anthropological issues. The appeal to these aspects of reflexivity does not aim at nourishing narcissist post-modern trends; rather, its main goal is to profit from the 'behind the scenes' of ethnographic experience in an attempt to reach a better understanding of the 'others' being studied. Throughout the following pages, reflexivity does not constitute an excuse to embark upon a self-referential anthropology; neither is it used to question the validity of ethnographic authority or the knowledge supplied by the anthropologist. Reflexivity is to be deemed a starting point for analyzing how data are gathered in the field and how the empirical material obtained is conditioned by the researcher's presence. In that context, the natives' reception processes must be considered part of a fieldwork. In the words of Clifford (1995: 61), ethnography implies 'a constructive negotiation involving at least two, and usually more, aware and politically meaningful subjects.'[2] Clifford (1999) has also maintained that anthropology is not exclusively based on a set of 'delimited' fields, but rather on a 'series of journey encounters,' that is to say, a 'travelling residence.' This is all the more so whenever

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the field is multidimensional in nature, meaning that it is practiced largely in mobile, changing contexts. Clearly enough, the issues raised here are rooted in the criticism coming from feminist anthropology (starting in the 1960s) and postmodern anthropology (especially in the 1980s). The feminist trend was the first to highlight the significance of the dialogical and intersubjective aspects of the anthropological experience which may account for new perceptive experiences, particularly when fieldwork is carried out by women (Abu-Lughod, 1993; Behar and Gordon, 1995; Moore, 1994). In addition to underscoring gender inequality in diverse societies, this trend questioned classical anthropology and the pre-eminence it gives to notions of objectivity and male-centered ethnographic texts. Likewise, it emphasized both interaction processes in the field (which could be defined as the 'very complex negotiation of visions between ethnographers and interlocutors, collaborative and reciprocal approaches' [Lassiter, 2005: 90]), and the need to relativize categories such as subject/object, home/filed or native/outsider. It also focused on silenced voices and power relations in classical anthropological texts. Evidently, all these issues are also addressed by postmodern criticism, which, in the face of the complexities of the colonial world and postindustrial societies, has questioned the notions of ethnographic authority and scientific objectivity (Clifford, 1995; Marcus and Fischer, 1986). Thus, the intricacies of multiculturalism made it possible for the analytical focus to shift to subjectivity, dialogicity and complexity, and for the limitations of classical anthropological traditions to be questioned in order to give voice to the 'native's perspective.' This 'crisis of representation' was also brought about by emerging interpretational frameworks which – sometimes metaphorically, sometimes literally – interpolated hermeneutic exercises into literary criticism (Geertz, 1973).

Some of the issues discussed herein are related to 'self-anthropology', which Strathern (1987) defines as the type of anthropology that is carried out in the social context where it is produced. This concept is usually applied to anthropology as practised 'at home', which explores the features of the 'native' anthropologists and the social constraints which operate on them. In this way, Reed Danahay (1997) stresses the use of three self-ethnographic genres: native anthropology, ethnic anthropology (achieved by members of minorities who display a high degree of political compromise), and autobiographic ethnography. These approaches deal with

> … a set of epistemological conventions which have both reproduced and camouflaged key contradictions in anthropological practices. There is now a copious literature attesting to the distortions and contradictions involved in one of these efforts: the absence of the ethnographer as an active and embodied participant in the social relationships and situations described in published texts. (Amit, 2000: 3)

However, it must be borne in mind that 'native' anthropologists cannot take success for granted on account of their sharing the same social world with their subjects and that by no means does this position legitimize his interpretation and analytic insights. We could also question, following Motzafi-Haller

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(1997), well-known dualisms such as native-stranger. This author shows, through her experiences as a native anthropologist in Israel and an outsider in the south of Africa, that the categories 'native' and 'stranger' are sometimes blurred, vague. According to Motzafi-Haller, 'we are all, researchers and subjects, the products of our history' (1997: 217) because the experimented feelings of oppression could lead the researcher to write in a critical way, displaying a social and political engagement with the object. Thus, it may not be very useful – according to this view – to discuss the legitimacy of the 'native' anthropologist; rather, the emphasis should be put on the connections between the social and historical position of the researcher and the research agenda. Therefore, what would be truly significant is the social engagement of the researcher, rather than his condition of 'native.' Furthermore, 'the boundary separating the anthropological field and home, which has so often been demarcated by the metaphor of travel, has incorporated a presumption that 'home is stationary while the field is a journey away' (Amit, 2000: 8).

As Van Mannen (1988) maintains, ethnographies are written with specific audiences in mind, based on the competences, expectations and attitudes which the readership is assumed to possess. In general, the main audience for our ethnographic products is made up of experts (colleagues, teachers, leading figures in the discipline), readers keen on anthropology, individuals with a background in other social sciences (for instance, historians and sociologists), ethnography students, massive audiences and, certainly, the very natives who were studied, insofar as they are able to access the material and discuss it. Here lies one of the most important challenges for anthropology, a discipline over which the natives have rarely had any significant control. By fostering a textual opening of ethnographies, authors become exposed to risks and strict controls from the audience. Concomitantly, there is an increase in potential misrepresentations, misunderstandings and even tendentious and ill-intentioned interpretations (exegeses which academic readings are not exempt from). Hence, the 'popularization' of ethnographic texts, in the non-pejorative sense of the term, implies a major challenge for the anthropologist who understands that simplicity in exposition does not preclude depth in analysis. In this sense, Glazier (1993) claims that anthropological texts – just as any other discourse – have the potential to reach unexpected audiences and to engender unanticipated meanings in unplanned places, which leads him to advocate the acknowledgement of those impacts on reception as an integral part of the research process itself. The ongoing research presented herein is not mainly concerned with conventional field experiences primarily invoking the ethnographic authority of the *here and now*. In fact, this article is concerned with the past, with events which took place over 30 years ago; thus, it relies on the memories of the actors participating in the events. Although fieldwork has also been conducted under more conventional circumstances, most data offered by the informants is related to a traumatic past rich in symbolic content. It is precisely those research contexts that this article addresses – contexts so

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entrenched with daily work that the boundaries among categories such as 'field' and 'home', or 'native' and 'expert', become blurred. That is, the relations to be primarily analyzed are those established with those stories' protagonists, interlocutors who may be conventional informants but who also fluctuate among the roles of commentators, saboteurs and even co-authors. Needless to say, such individuals have adopted completely different attitudes towards this research project, which deals with a crucial part of their lives. In this case, the researcher's work is rendered even more complex since the informants are also colleagues within his own academic field. Indeed, this research is informed mainly by anthropologists whose careers will be objectivized in complex processes involving not only their personal accounts but also the political history of their country (Argentina), which, convulsed by unfulfilled revolutionary dreams, headed towards a pitch-black fate in which state repression from the mid-1970s shattered personal and collective dreams – and, in this case, considerably affected the development of the anthropological discipline.

Circumstances such as these – only some of which are planned – promote the development of a *public anthropology*, which, according to Lassiter (2005), supersedes applied anthropology and political activism. An anthropology done along these lines paves the way for a collaborative ethnography bringing together the researcher and his/her subjects of study in the production of ethnographic texts. Moreover, it contemplates the possibility of transferring that collaboration onto the writing process. Lassiter himself (2005) advocates for involving informants as straightforwardly as possible, co-authorship representing the maximum degree of collaboration among informants and researcher. Accordingly, the history of the American cultural anthropology itself cannot disassociate from this close collaboration between researcher and informant and/or interpreter. The case of Franz Boas and Kwakiutl native George Hunt – who even signed some articles in collaboration –, or Lewis H. Morgan's relationship with different governmental and non-governmental organisms (particularly The Bureau of American Ethnology), prove that the processes under discussion are part of anthropological knowledge, and not mere postmodernist or feminist findings. The same dialogic approach was favored by Marcel Griaule and Maurice Leenhardt between 1920 and 1930. In keeping with the French tradition of restricting fieldwork to a small number of informants, Griaule, throughout his dialogs with Ogotemmeli in *Dieu d'eau*, brilliantly captured the possibilities offered by a fluid interaction with a qualified native from the Dogon people (Mali). Although excluded from the official genealogy of French anthropology, Protestant missionary Maurice Leenhardt (1878–1954), during his more than 20 years among the Kanaks from New Caledonia, worked systematically with those qualified informants, whom he called *transcribers*. His habit of talking to social actors with proper names and distinctive personal characteristics, together with his harsh criticism of the colonial system, constitutes a hallmark considerably ahead of his time (Viola Recasens, 1987).

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Lassiter (2005) calls for a true *collaborative ethnography*, which, when pursued seriously, is much more than a bureaucratic recognition or a mere dialogic representation. Indeed, it is a multi-dimensional cooperation which directly involves the audiences we are dealing with. Without trying to diminish the possibilities of producing anthropological knowledge, this article posits that these relations established in the field configure unavoidable realities of conceptual analysis. Likewise, the development of new information and communication technologies, especially the internet, significantly facilitates contact between researchers and audience, thus favoring the relevance of commentary as an ethnographic genre. These platforms enable natives to read and speak, which substantially enriches the ethnographer's work. Fabian (2002) considers that this is a completely new situation which entails a radical change in ethnographic documents, as changes in the access to information can have a great bearing on ethnographic writing. Furthermore, he considers that the positions of authorship and audience are hence reconfigured and create new ways of writing.

A brief account of the political history of Argentina

This incipient research addresses the birth, development, dismantling and destruction of the anthropology course of studies at *Universidad Nacional de Mar del Plata*[3] in the 1970s (1969–1977). It has two main objectives: on the one hand, it aims at contributing to the knowledge of the disciplinary history or to the analysis of specific problems associated with Argentinean social sciences between the 1960s and 70s. On the other, it seeks to understand the political processes triggering an unparalleled state repression which consolidated after the establishment of the self-proclaimed Process of National Reorganization (1976–83), the last military *coup d'état* in the country's history.

Between 1930 and 1976, Argentina was characterized by the succession of civil and military governments, which resulted in blatant political instability. Military *coups d'état* interrupting civil governments (some of which had reached power through electoral fraud or the proscription of the majority party, namely Peronism) were often introduced as provisional governments bound to restore democracy. This did not prevent the Armed Forces from establishing themselves as legitimate actors in a national political culture which jointly validated military intervention in Argentinean administration. Nevertheless, the 1966 military *coup d'état* led by General Onganía established a new process – called 'Argentine Revolution' – which for the first time catered for the establishment of a long-term military government with no intention of restoring power to civilians. During Onganía's four-year term in office, repression of social protests escalated and different sources of popular unrest favored the increase of political radicalization in a significant part of the population, especially among youngsters. Given the social and political effervescence of the time, 'universities occupied a predominant role in disputes about national

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politics and, at the same time, were deeply permeated by them' (Barletta and Tortti, 2002: 107).[4] In that context, social sciences were directly influenced by political processes which conceived of the university – and consequently all academic disciplines – as another tool to reach the longed-for 'national liberation' (Barletta and Lenci, 2001; Barletta and Tortti, 2002; Buchbinder, 2005). A 'national mindset' was born which strongly influenced the social sciences with its harsh questioning of *scientism* and its suggestion that science should strive to transform society through revolutionary channels.

Less than four years after its irruption, Onganía's rather corporative political project collapsed before popular protests and internal conflicts in the army itself. General Lanusse, now in command of the army, sought to establish political agreements towards a democratic solution which, though free of proscriptions, had not yet determined the role of Juan Domingo Perón, the exiled septuagenarian popular leader who had been overthrown by a military *coup d'état* in 1955. By that period, a great number of politico-military organizations – many of which adhered to revolutionary Peronism – had already entered the public arena, sharply increasing their armed actions and their legitimacy among the population. In the 1973 elections, Peronism returned to power when Hector J. Cámpora was elected head of state. His eventual resignation would facilitate the return of Juan Domingo Perón himself, whom the military had banned from the elections. These events notwithstanding, guerrilla groups (both Peronist and non-Peronist Marxists) remained operative. They denounced the 'right-wing slant of the government', which deepened after the death of the elderly leader in June 1974 and the succession of his third wife, María Estela Martínez. During these democratic governments, a sustained growth of armed groups occurred (mainly *Montoneros* and the *Ejército Revolucionario del Pueblo*[5]); harsh factional clashes between rightist and leftist Peronists marked a period of political violence which laid the foundations for a *coup* which, in turn, would result in an ideological genocide without precedent in Argentina's political history. The universities witnessed many of these clashes, since higher learning institutions were the target of recurrent and systematic interventions and they eloquently expressed the tensions of those times teeming with utopia, death and violence. The imposed dictatorship, regarded as the bloodiest in Argentine history, took a toll of 30,000 'disappeared', according to human rights organisms. Specifically at universities, state repression, which had already started during the last stage of the Peronist government (Gil, 2008), was greater in humanistic courses of study, many of which were removed from the curricula of different universities. Suffice it to mention that in 1980 18 million books from the *Centro Editor de América Latina* (*CEAL*) were incinerated as part of the '*Operativo Claridad*' ('Operation Clarity'), whose aim was to censor, intercept and destroy «Marxist bibliography» (Funes, 2008) intended to disrupt the principles of the National Constitution. Those 24.5 tons of paper which burned in flames included books by 19th-century authors of the likes of Domingo Faustino Sarmiento y José Mármol. In the School of Humanities at the *Universidad Nacional de Mar del Plata*,

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several independent courses of studies – such as Anthropology, Sociology, Political Science, Psychology, Educational Science and Philosophy – were closed as from 1977. Specifically, anthropology courses throughout the country had varied fates. The Universities of Rosario and Salta witnessed the close of their courses, but some others managed to survive. Such was the case at the universities of Buenos Aires and La Plata, where the courses remained open, notwithstanding temporary lapses in enrollment and the expulsion of numerous teachers involved in social anthropology and progressive politics. Only the Social Anthropology course offered at University of Misiones (opened in 1974) was able to uphold its original approach and avoid the strict ideological control exerted by the military government (Bartolomé, 2006). Consequently, exile abroad was the alternative chosen by several prestigious scholars, young professionals holding postgraduate degrees from foreign universities and recent graduates from said closed courses, since not only were work opportunities completely shattered, but their lives were in real danger, all the more so for those militating in revolutionary political organizations.

A field and its approaches

Universities, as well as other institutions and social organizations, constitute a 'social arena' by reference to which some of the most relevant processes in the political life of Argentina can be better understood. It goes without saying that this article deals with a particular case possessing its own logics and institutional styles. Yet, it may be taken as a starting point to address broader processes at regional and national levels. Anthropology, as a course of studies, promotes the understanding of many situations framing the country. Furthermore, it reflects how generational dreams crumbled before a reality which neither catered for utopias of national liberation and socialist fatherland nor allowed for the success of more modest projects, such as the consolidation of discipline (Gil, 2007). As from 1969, anthropology was part of the academic offer at *Universidad Nacional de Mar del Plata*. It was the first time that a syllabus governed by social anthropology was imposed all over the country. Indeed, Argentinean anthropology was strongly controlled by humanist and archaeological trends, and diffusionist approaches from German anthropology were preeminent. A number of foreign scientists (such as Italian Jose Imbelloni or the famous Austrian archaeologist Oswald Menghin) prevailed in the local anthropological field and left little room for the development of the British, American and French disciplinary matrixes. With time, leading figures who mainly adhered to the *Kulturhistorische Methode* would be criticized for their political ideologies and branded as reactionaries and fascists. It was not until the mid-1960s that a generation of young graduate students from different Argentine universities and some postgraduates from foreign universities would introduce the most modern perspectives of social anthropology in Argentina. This discipline – alongside postcolonial criticism – accompanied the aforementioned

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radicalization process of the social sciences. The distinctive feature of this course of studies was its singular way of understanding anthropological practice, connected to a great extent to a series of moral and ideological precautions about disciplinary research. It was a highly politicized project which developed analytical tools aimed at fostering a critical attitude, especially against colonialism and the problems about the destiny of ethnographic research data (and that of social sciences in general). Almost unanimously, students strongly identified with their training and, although their relation with the institution was broken, graduates who stayed in the academic field completely vindicate the affiliation with their intellectual father (Eduardo Menendez, then settled in Mexico) despite the difficulty of establishing concrete bonds (for instance, in research programs) which go beyond the explicit recognition of a genealogic continuity.

In addition to more conventional ethnographic activities, such as interviewing direct or indirect participants of that anthropological course of studies, one of the first methodological steps followed in the research herein presented was to plan an intense archival research catering for the analysis of the different administrative acts of *Universidad Nacional de Mar del Plata*. Such an initial step was also intended to yield an overview of the predominant political measures and of the sociopolitical context of the country. Archives and other written sources have been precluded from ethnographic research by disciplinary orthodoxy due to their allegedly inadequate narrative and ethnographic value, their seeming artificiality and the hidden nature of the absent voices, partly because of their official character. Moreover, ethnographic archival work refers to past 'armchair' anthropology and to other disciplines (History), which in some way would be the antithesis of true fieldwork (Gomes da Cunha, 2004). However, working with archives which are not well-organized entails difficulties to access them. At the same time, it implies both rich and complex ethnographic work. Beyond the estrangement of relating to the institutional logics of a university such as *Universidad Nacional de Mar del Plata*, the contact with official documents from an educational institution provides us with both valuable and irreplaceable empirical material. Archives, as the means through which the state crystallizes and classifies knowledge, render the past partly accessible to future generations (Dirks, 2001). Anthropology is becoming increasingly interested in this type of varied documentary sources. Comaroff and Comaroff (1992) regard archives as dialogs – in Bakhtinian terms – highlighting their capacity to combine genres and to give rise to distinct voices as well as to a cultural and historical heteroglossia. Moreover, archives enable the anthropologist to observe breaches in groups which originally seem homogeneous.

As those archives are found and browsed, research lines are opened, previous hypotheses are destroyed by irrefutable evidence and research finds a pace of its own. As it is often the case with any anthropological research, the field may guide us in our findings, while imposing limits and revealing the complexity of the social field. Although this article is not aimed at establishing a 'true story' about what we may preliminarily call 'Mar del Plata's anthropology', the

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appeal to historical facts is strictly necessary to compare (not contrast) them to accounts of the actors themselves, who lived in a period both complex and rich, both tragic and frustrating. From the onset, this research posed a multidimensional fieldwork, tackled from different bases. More specifically, this article deals with a partially peripheral ethnographic activity, often complementary to conventional fieldwork. However, in some occasions such endeavor may become the most important work, since archival research places us in typical fieldwork situations. Indeed, it is possible to establish a dialogue with these documents, to discover alterity, ideological and discursive positions and seemingly hidden voices and, more importantly, to produce relevant empirical findings which may modify research goals. The ethnographic task, however, is anything but easy, as it is full of sabotages. Many protagonists, colleagues who sometimes tacitly regard themselves as competitors, refuse to speak; others go as far as questioning the epistemological and even moral pertinence of doing a research with these characteristics. Sabotage by some natives is a matter of vital importance to the ethnographer's work, especially in a contemporary world, where the ethnographer can meet natives as experts, or aspiring experts. Clearly, this is not a new issue for anthropology, although it has never been explicitly analyzed. On that score, reference must be made to Evans-Pritchard's discussion of the Nuers' attitude, which – unlike that of the cooperative Azandes – 'frustrates every effort to infer even the simplest facts and to clarify the most innocent practices' (1997: 24–5).[6]

On sabotages and other field problems

This ethnographic project was carried out 'at home', the researcher and his informants sharing the context of production. Some unplanned scenarios developed favoring the circulation of discourses about anthropology, which was already my object of study. The so-called 'at home' anthropology makes the researcher face the cultural continuity between what he himself produces and what his 'objects' of study do. This situation intertwines with a rather hard task for any anthropologist: that of explaining to his informants exactly what s/he does and what his/her aims are. Thus, anthropological texts may be discussed by the very subjects studied. Cultural continuity makes some actors think of themselves as experts and condemn any alien aspiration of discussing them when common sense discourses used to explain one's own actions are not reproduced. In this research I faced a troublesome 'Other' who questioned the moral and epistemological legitimacy of my interest in the 1970s. Without ever amounting to overt aggression, the degree of hostility showed by some 'natives' (whom categorically rejected the possibility of becoming informants) was significant. Many actors, colleagues who sometimes find themselves competing for positions within the academic field, refuse to speak; others explicitly oppose the idea that a research of this nature should even take place. Explanatory devices employed to reject the proposal of becoming research interlocutors

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varied greatly. Curiously enough, some 'protagonists' who had at first refused to remember ended up asking to be part of the interviewees. Others were adamant, invoking arguments such as: 'You can't talk about it if you weren't there.' Not even bothering to resort to any postmodern construct denying ethnographic authority, some local anthropologists deemed it impossible for someone of my generation (someone who was born in the 1970s) to reflect upon that period and decided to 'get down to writing our own story.' In no case was the rejection justified on grounds of triviality, an aspect which Gluckman (1989) had already identified as one of the main problems for a researcher who deals with facts widely known by most of the subjects of study.

I had a warm relationship with one of the anthropologists who had taken that course of studies. At a given point, I told her about my intention to write about the 1970s in Mar del Plata's anthropology. She had shown herself extremely willing to collaborate; she committed to share her testimony with me, and even told me some brief stories informally and cordially. Once anthropology as a course of studies in Mar del Plata officially became my empiric reference, her attitude changed substantially. After a period of silence, she expressed her refusal to be part of the object of study. First, she argued that personal feelings prevented her from recalling this significant time of her life. She then added that she would prefer to spare herself the frustration proper to bringing to memory her unfulfilled longings. At one point, she declared her opposition to anyone who wanted to 'meddle in those issues.' She sent me a lengthy email hinting at a confrontation that went beyond the refusal to become an 'informant', suggesting that her sole concern in the topic was that nothing should be written about her 'golden seventies'.

Once this research officially started, a series of events took place which changed my working scenario. I found myself sharing daily workplaces with some colleagues who were already informants and with others who had expressed their reluctance to participate in my project. After rumors of an agreement (which never materialized) with *Universidad Nacional del Centro* to reopen the course of studies almost 30 years after its termination in 1977, an anthropologist began holding meetings with local colleagues. Her second bulletin stated that the goal was to analyze 'different issues of common interest.' It also read: 'The first aim is to generate concrete actions to make our discipline more visible in the city, so as to start discussing the possible opening of the course of studies and other relevant issues related to our profession' (Tuesday 4 July 2006). In that context, three monthly meetings took place in which precise guidelines were established, even though Olga[7] would later maintain that 'we have taken a first step; we have agreed on setting up a forum on the Internet.' In one of those meetings, Alcira, one of my informants, had to face her old classmates' insistence on emphatically highlighting the collective nature of 'Mar del Plata's anthropologists.' For her, each graduate student in Mar del Plata had their own careers and their respective experiences were 'personal; nobody has to take care of mine, and neither do I have to take care of theirs.'

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The above mentioned bulletins, aimed at the '*antropólogos marplantenses*'[8] explicitly posed the need to write a text 'about the history of our course of studies, written by the protagonists of that story.' The remarks made by some colleagues about my research passed unnoticed, and they were once wrapped up with the trite phrase 'You can't talk about it if you weren't there.' Eventually, the meetings came to an unfruitful end, some rather aggressive emails going back and forth in the interim. Then, certain events took place in the political life of the university which catered for the explicit motion to reopen the course. As the university board approved the reopening of Sociology as a course of studies, the political atmosphere seemed favorable (an odd circumstance, perhaps never to be repeated) to continue with the so-called *historical restoration* process, which sought to right the excesses perpetrated by the 1976–1983 military dictatorship. Consequently, two colleagues (one of them 'part of that history') and I put forward a motion in the last meeting of the year, whose organization Olga had delegated to a grant holder. The meeting did not have a list of topics for discussion. (The previous sessions had not fulfilled the proposed objectives, such as the writing of the aforementioned historical text about the course of studies and the contribution of Mar del Plata's anthropologists in a Book Fair which would be held in October.) There we stated that if we acted efficiently and straightforwardly, the anthropology course could be successfully reopened; all it took was for us to work hard and present the project by the end of the year. We thus established weekly meetings in an attempt to satisfy the main demand of the university's management: reaching a consensus among local anthropologists.

Olga had never been contacted to speak about her memories as a student; however, in that new meeting in which the reopening was proposed, she made her position clear: 'you weren't there', she told me, and suggested I was not respecting a historical period in which many people died before having a chance to speak. The reopening proposal and the firm decision to achieve that objective intertwined with my research, as if they were two co-dependent issues. My role in the project caused resentment, since not only was I a '*pendejo*' [in English, a male 'young twerp'], but I was also 'disrespectful' for touching upon a topic which 'cannot be messed around with.' Both Olga and Irma made that clear at every moment, especially in the second meeting, during which they kept repeating phrases like 'those who were not there' and 'I didn't need to be *told* about it.' Olga is living proof of the enduring effects of the military process; she also incarnates a reactionary ideology typical of the School of Humanities at *Universidad Nacional de Mar del Plata*. Both Irma and Olga's discourses assume that past representations and antagonisms rule the present, implying that risks from the past have not disappeared. Indeed, they build a cyclic history in which a tragic past pervades the present, although, as some studies about social and political memory show (Fentress and Wickham, 1992; Yelvington, 2002), that past time which rules the perception of current times is built from the present. This 'watchfulness exerted in reference to the

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past' (Visacovsky, 2005: 294)[9] rendered my research evidently dangerous as there was a possibility that I might end up instituting my own version of the 1970s. In other words, each new project or job offer entails the emergence of references to the past. Old antagonisms ('those *facists* who never left and now stock us') are nowadays recovered and fully legitimated, the historical continuity of the military process pervading the university itself. In this sense, certain actors are labeled as 'enemies' on account of their alleged ideological affiliations, of their theoretical affinities, of how long they have held their posts, or of their being graduates from the *Universidad Católica*.[10] Olga quite often makes reference to a 'reactionary' epistemology, and Irma even labeled certain members of the university's administration as 'neonazis'. As Belmont (2000) argues, historical ruptures are brought about by men themselves, who enable repetitive cycles to articulate repetitive movements because the fundamental states which cannot improve are the ones which prevail. Nostalgia,[11] in this sense, emerges as an excess of self-communication referring to times lost. Different from linear time, in which the past is no more, cyclic time allows nostalgia to motorize small self-celebration rituals that aim at a mythical recovery of that past which *is present* and at the same time *isn't*. It also constitutes an account signaled by the 'heroic I' (Sahlins, 1997), whose relationships with society are historically projected and embodied in an individual's authority.

Accordingly, Visacovsky (2005) has reflected upon the vicissitudes in reception when the researcher writes about very sensitive topics touching the individual's 'inner self'. Drawing upon Malinowski (1998), Visacovsky analyzes what he terms 'sacred stories'. He compares these to primitive myths, in that they constitute living accounts high in moral content and potentially capable of controlling our behavior. In the case at hand, direct clashes with these 'expert' natives started as a result of Olga's implicit references[12] to my role as a promoter of the reopening and as a researcher of such an infamous period. She insisted in her remarks that 'those who had been part of the story could not be excluded', and for the umpteenth time she tried to undermine my authority to speak and act, as I was among 'those who weren't there.' This inevitably led to some arguments. At one point, Olga addressed me in the following terms: 'It is as if you were trying to take hold of history.' After some exchanges, Irma confessed again: 'It bothers me when people want to analyze our feelings objectively.' Whenever I quoted an official document from the university archives, she wasted no opportunity to interrupt me by saying: 'Nobody *told* me the story.' In previous meetings, she had already censored my work in front of her former classmates with arguments such as: 'What's up with all this washing our dirty linen in public?' Outside the meetings, she let one of her colleagues know about her opposition to what I was doing, to my alleged pretension to 'have it all', and sentenced once more that 'history's not to be messed around with'. Once again, Irma confused my research project with the proposal to reopen the course of studies. Having described me as

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'disrespectful', 'anxious' and as a 'twerp', she suggested that I was influencing the rest of the youngsters and that somebody ought to restrain me.

The expository stances adopted by these two 'saboteurs' – towards both the research and the formation of this team – evidenced a complementary strategy which constructed an authority based on their being 'historical' and 'experienced'. The 'historical' category implies the legitimacy of a vindicated affiliation with the course of studies which they belonged to in the 1970s, whereby they would be heirs to a genealogy starting with Eduardo Menendez, '*el Tata*', the intellectual father in exile. A second source for their 'legitimacy' comes from their being living witnesses of the history in question. Intimately related to this is the 'experience' category, which does not necessarily entail academic prolificacy (e.g. publications, postgraduate degrees), but the temporal continuity at *Universidad Nacional de Mar del Plata*, historically described as adverse, full of 'reactionaries', 'fascists', and even 'neonazis'. The expository strategies thus followed aimed at establishing a testimonial and generational complicity which would guarantee a greater authority, that is to say, an expository distance against potential interferences. This expository construction poses two types of enunciators who must listen and assume their subordinate roles in the field (for instance, in their posts as teachers). One of them is better represented by the category 'youths', while the other – relatively absent in the conflicts previously described – comprises those who did not graduate in Mar del Plata, that is to say, those who are not 'historical' and are often accused of lacking 'sound theoretical foundations'. Even though it did not represent a problem in the field, the gender variable deserves mentioning as a clearly visible one in the anthropological community. Most graduates from the anthropology course are female (32 out of a total of 39), a scenario that is mirrored by the teacher community in Mar del Plata. Moreover, with just one exception, the rest of the interviewed students who graduated from that course of studies and forged a career as anthropologists (in other cities both within and outside Argentina) are women.

Although the level of tension in other meetings decreased while the reopening project advanced, I could gather data, directly and indirectly, on the way in which some actors recall the 1970s. Whereas in formal contexts nostalgic and cyclic past references may establish a *community of suffering* (Das, 2003) based on exclusion (fascists, lukewarm analysts, youths who 'were not there' and thus cannot understand the topic, etc.), informal contexts led to a change of register. Some informants who have a closer relation with Olga and Irma told me several stories in which their memories about the 1970s in informal contexts were different. One of them often admits, sometimes publicly, that 'I wasn't aware of anything. Once I came to the university and found the door locked.' Another one every now and then recalls the 'big laughs' she had with her former classmates, or refers to more 'frivolous' aspects of university life than political militancy and state repression.

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Intermediaries, guardians, informants and collaborators

Notwithstanding the conflicts described above, contributions in the field largely outnumber sabotages. Contributions in some instances transcend the conventional roles of author (or researcher) and object of study, due to the same reasons why sabotage takes place – i.e. because this field fosters interaction with other experts, real or imaginary rivals competing for a place in a rather limited academic field. Cordial relationships in close cooperation with the natives were established not only among some actors of that academic experience during the 1970s – the 'historical people' – but also among several individuals who inadvertently ended up as informants. The latter, above all, acted as collaborators, due to their role as sources of empirical data essential to this research, viz. the archives from the university. By definition, archives from governmental agencies, such as national universities, are public. However, in order to have access to these documents, some administrative difficulties have to be overcome. For instance, the researcher must often obtain authorizations without knowing who has the authority to open or close his paths of investigation. As in the magnificent 'Before the Law' by Franz Kafka, common men depend on guardians to be admitted in legal bodies and have to hold out patiently even if they are never to be accepted. However, once those administrative barriers are overcome and formal authorization is obtained, the researcher has to interact with actors, that is, clerks working in offices related to the documental heritage, who will precisely allow him, or not, to do the planned archival research. Generally speaking, those employees are not used to people consulting those documents. First contacts with them are full of surprise, inconveniences and, in some occasions, sheer distrust. In addition, public offices are not equipped for an alien agent to use the documents. Moreover, the arrival of the researcher brings discomfort to workers who have to cope daily with lack of space. Be that as it may, once the 'outsider' becomes a *necessary evil,* the same people who distressfully receive the researcher ultimately become perfect intermediaries to sort out other types of administrative obstacles, allowing for quick access to previously forbidden areas. These intermediaries clear our doubts, guide us in our search criteria and alert us about the existence of documents that we have overlooked. Moreover, daily interaction may even foster friendship between the researcher and those employees, regardless of whether they get involved in the logistics of the research.[13]

Unlike what happened to some anthropologists, who studied sociology and political sciences simultaneously, the abovementioned intermediaries did not reject my research in any way. On the contrary, even those with heart-rending stories seemed enthusiastic about my work and even suggested incorporating their respective courses of study to the scope of my research. They have provided me with very useful information and even experienced a certain pleasure when referring to those past events, recalling new events as they recounted

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well-known stories. In some other cases, the contribution of the native-experts can be seen as a type of co-authorship. Whereas some 'historical people' who belonged to the anthropology course in the 1970s questioned and sabotaged this research, others showed signs of almost unquestioning collaboration. Cecilia has played a significant role in the process since not only has she offered her full cooperation to talk about the 1970s, but she's also accompanied me in all the research stages, supplying material, helping me in archival research, pointing out relevant names of which I was unaware, contacting new informants, suggesting hypotheses and research trends, publicizing my findings and even upholding the pertinence of the project. Certainly, Cecilia's was not the only case since Alcira, one of her former classmates, also became a devoted collaborator and even made some analytical suggestions which I thankfully capitalized on.

She was also ready to uphold the legitimacy of my work every time it came under fire and, together with Cecilia, she remains an active participant in this ongoing research, insofar as her memory – as well as those of other informants – recovers events, relations and processes which may prove useful to continue studying in depth the most complex and fascinating stages in Argentina's political history. Other 'historical people' – some living outside Mar del Plata – have also fully collaborated with me, both in our first contacts via email and during the interviews. The same could be said of the teachers interviewed, most of them eminent figures in Argentina's anthropological field nowadays. All in all, interviews with well-disposed interlocutors have become fascinating experiences, worthy of being investigated from many perspectives and disciplines. The way in which faces and eyes changed while memories revived, forgotten names emerged – some of them as a result of my browsing the archives – and happy and tragic moments were brought back to memory, enabled me to understand the vast complexity and ambiguity of a time of unfulfilled revolutionary dreams, of tragic personal and collective sequels, of irreplaceable losses, but also of happiness, frivolities and treason. As the informants uttered names which they had not heard in decades and thought they had forgotten (such as those of some teachers they would have preferred to forget) and as they recounted pleasant or tragic events (especially missing students and partners), the ethnographer was compelled to carefully balance his own conceptual and empirical interests with feelings, internal conflicts and even the need to elicit catharses from many witnesses of those days. It has been fairly common for certain initial reluctance – generally due to lack of time for an anthropological interview – to turn into an invitation to continue talking in the future and a desire to access future research results. More out of curiosity than fear of being published – and certainly not as a threat – one of the interviewees realized how intimate the stories were which he had confided in me and asked: 'What are you going to do with all this information?' Although I cannot predict the impact that my writings on the anthropology course of studies in Mar del Plata will have on them, these 'sacred stories'

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seem bound to evoke mixed feelings, most of them extremely adverse. However, I completely agree with the idea that 'this is not necessarily the way of the impartial and cold observant, of a voyeur reaching out to a voyeuristic readership, but rather that of someone who, experiencing the fear proper to understanding the value and significance of the «sacred stories» he comes across, does not elude the challenge of writing about them, even though he knows their potential consequences' (Visacovsky, 2005: 309).

As a matter of fact, the purpose of this article is to show how troublesome it may prove to carry out a research involving colleagues with whom the researcher shares everyday experiences. This project attracted both opposition and encouragement, sometimes for the same reasons. The natives' questioning does not lie precisely in personal fears regarding the eventual revision of their background as militants. On the contrary, negative judgments were rooted in their self-perceptions as referents in the local anthropological field and in their proclaimed positions as 'historical' and 'local.' Be that as it may, the fact is that no attempt has been made to shed light on the militant history of the field members, or to analyze their actions before, during and after the military *coup*. In general, they all militated in left-wing organizations (such as the Peronist youth and revolutionary Peronism) in the 1970s, maintaining progressive positions throughout time, although the field abounds in allusions to the poor commitment shown by some classmates who now advocate libertarianism. In this sense, the crucial point is to confront such 'holy stories.' The risks sketched above must be assumed and faced responsibly, so as not to contribute to the reproduction of simplistic frameworks which may emerge in the field to explain such traumatic past. At the same time, however, there springs the commitment not to banalize academic and militant careers or to give way to persecutory views in the presence of reciprocal accusations of 'lack of commitment' or even of direct collaboration with the military regime. Hence, it is necessary to ignore certain phrases, uttered during interviews and talks, which do nothing but express personal antagonisms. In all cases, then, the utmost care has been taken not to betray the trust of the 'collaborators' or that of the other interlocutors who agreed to narrate their personal and academic stories. In any case (at least for the time being), it is not possible to make any strong assertions regarding the convenience, or the lack of convenience, of undertaking a research project in the researcher's workplace, specially when it involves memories related to state terrorism. Faced with the difficulty of representing such complexity, the ethnographer may be accused of not taking seriously those processes of collective suffering. Furthermore, s/he runs the risk of being labeled as a 'reactionary' or a 'rightist accomplice'. Thus, this research involves a series of special precautions in view of the varied reception processes of different audiences (academic field, human rights organizations, official organisms) and all potential reprimands about the content of the articles. The 1970s and its concomitant illegal state repression are topics that captivate public opinion. Both are usually approached from stances precluding

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dialogical readings apt to account for the ambivalences of historical process and the great complexity of a time marked first by revolutionary fervor, and then by state terrorism. This poses the dilemma of touching on such a controversial topic; one that involves many delicate issues, such as the collective memory in Argentina. The researcher runs the risk of ceasing to be an 'acceptable' (Visacovsky, 2005) citizen, as he may be the target of morally degrading accusations linking him to the same old enemies (for instance, the fascists). This research deals with significant ideologies and identities as well as stories of missing and dead people, exiles, and, obviously, survivors.

Conclusion

A conceptual analysis of the fieldwork process leads us to consider central aspects of anthropological knowledge and practice. In fact, this reflexive reference to events related to the *here and now* of the ethnographer, to the way he obtains his information, to the relationships he establishes in the field and to the external conditionings of his daily work, constitute a significant and unavoidable part of the knowledge-production process. Even if physical integrity is not threatened at any moment – as when one works in violent contexts – when the ethnographer implements a research strategy mainly based on archival research and interviews to teachers and scholars, a different kind of fear may assault him; he may be worried about more than the possibility of being punched in the face. In this sense, sensitivities seem to gain a greater volume when one works with colleagues or when one discusses significant aspects of social and political memory. Colleagues, as 'experts' and competitors in the academic field, are capable of developing strategies of sabotage, discredit, calumny and even reprisals, which may jeopardize the researcher's continuity in the field.

In this project, my 'objects' of study were individuals with varying levels of power who defined themselves as unquestionable owners of history or irrefutable guardians of myths. I was thus faced with situations in which fear and precautions where equally extreme as I put forward my ideas and empirical findings. However, those actions of sabotage – foreseen to a certain extent – were significantly useful and their consequences were as productive as those of more favorable attitudes. Almost as importantly as the natives' collaboration, sabotage faced me with much more fluent ethnographic contexts than those framing the interviews I held with most of my interlocutors. The workplace contexts where the greatest opposition to my work was expressed shaped more conventional fieldwork situations. Those who rejected being 'objectified' ended up constructing significantly valuable situational frameworks whereby I could access their particular ways of exercising memory, their classification systems within the field and their concomitant strategies of power accumulation. Even though the illegitimate presence of a 'twerp' who intends to 'seize history' initially downplayed old enmities and personal resentments from their days

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as students, it also catalyzed the contribution of other 'historical people'. Specifically, those who 'were there' and intend to be neither 'local references' of the discipline (regardless of their academic backgrounds) nor the only legitimate heirs of an intellectual father in exile, were the ones most vehemently opposing those discourses aiming at discrediting research and the researcher himself. As 'part of that history', they also fluctuated between informants, intermediaries and co-authors. Their contribution has been and will be essential in this project.

By interrupting the genealogical continuity stemming from the course they took in the 1970s, my research and stance in the committee for reopening the course of studies only brought about conflict in a local anthropological space where positions had always been clear, at least for those who, shielding behind their careers and sharing mutual 'respect', bestowed upon themselves the role of legitimate local references in the discipline. Any other alternative, at least at a local context, only constitutes a threat to those 'careers' characterized by the eternal struggle against ever-present repressive forces. The project compelled me to interact with actors who appeal to a cyclic construction of history. A central paradox in their thinking was thus brought to the fore, since their interpretation of the past depends on circumstances and contextual relations, thus configuring a manipulation of ideo-logics (Augé, 1975), potentially leading to 'an orderly system of references for the understanding of events' (Augé, 1975). Ideo-logics enjoys a virtual coherence, pervades every social sphere, and exists only for the observer who can reconstruct it, since it is a theoretical discourse articulated by a society upon itself. Accordingly, 'the interpretative role of ideo-logics implies an expansion of its powers; it governs not only discourses, but also actions' (Augé, 1975: 127). Therefore, ideo-logics is an a priori interpretative framework, which defines strategic positions, determines individual roles, and regulates the right to speak in favor of those with greater privileges. By threatening to transform myth into history, my research represents a concrete threat to the survival of a mythical structure which feeds on daily action and thus achieves continuity (Toren, 1988). This project broke with the authority of the word available to a community of suffering demanding silence as a sign of obedience. A research of this nature shows the difference between a flexible conception of the past and an exercise of ideo-logical manipulation allowing any written and empirically sustained assertion to stabilize the past. And there lies the great risk.

NOTES

1. I would like to thank John Gledhill, Guillermo Ruben, Rosana Guber, Sergio Visacovsky and Germán Soprano for their careful readings and valuable suggestions of an earlier draft of this article.
2. Back translation from the Spanish version.
3. Mar del Plata is a city lying off the west shore of the Atlantic Ocean, in the southeast of Buenos Aires. It is the seventh largest city in Argentina (with about 700,000 inhabitants). Almost since its foundation in the late 19th century, it has

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been the country's tourist capital. Along its 39 kilometers of seashore, it offers enough facilities to accommodate twice its stable population in summer.

4. Translation from the original.
5. This organization emerged at the core of the Partido Revolucionario de los Trabajadores (PRT) (Workers' Revolutionary Party), frequently called 'Guevarist' guerrilla.
6. Translation from the Spanish version.
7. Real names have been replaced to safeguard the interlocutors' identity. Although some of them have not expressed a special interest in remaining anonymous, this precaution is aimed particularly at those stories involving colleagues in self-ethnographic situations who explicitly rejected this research projected.
8. The expression refers to all anthropologists who were born in, or studied in, Mar del Plata.
9. Translation from the original.
10. In 1975, the Universidad Provincial de Mar del Plata was transferred to the national administration, taking over the Universidad Católica de Mar del Plata, which depended on the archdiocese. After the military coup of 1976, the only courses of study remaining in the School of Humanities were those which had belonged to the Universidad Católica.
11. Belmont (2000) takes the four mythical categories developed by Lévi-Strauss which complete the communication modes. While nostalgia is an excess of communication of the person with himself and oblivion means lack of communication in the same sense, indiscretion is an excess of communication with others and misunderstanding implies failure in communicating with others.
12. The relevance principle developed in pragmatics shows that certain propositions have implicit contents that are not explicitly uttered by the enunciator. This means that every utterance in itself implies that it is the most appropriate one the speaker could have uttered in a given context, and that its processing will yield an information benefit greater than the processing effort. Grice (1975) holds that, during conversation, interventions by the participants are predictably related and obey a mutual interest, a common orientation, which means that participants' interventions share a common cooperative effort.
13. For instance, in the office where this archival work largely took place, employees had a farewell toast once my research finished.

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