This article seeks to contribute to the debate on the challenges the anthropology of violence poses to contemporary anthropology. Due to its special thematic, theoretical and methodological difficulties, the anthropology of violence can be considered a frontier territory in the discipline, where agreed ways of going about ethnography are being constantly questioned. The theoretical frameworks available turn out in many cases to be insufficient, the research strategies established must be subjected to substantial adjustments, the rhetoric used to express our analysis needs to be particularly self-reflexive, and the knowledge generated must be returned to society in more effective ways, ranging from the usual channels of publication and distribution to other, more flexible, 'rapid response' formats. Overall discussion of the general characteristics of the anthropology of violence is followed by an example: the research the author has been carrying out since 2003 into exhumations of Civil War mass graves in contemporary Spain.

Aquest article pretén contribuir al debat sobre els reptes que l'antropologia de la violència planteja a l'antropologia contemporània. P er les seves especials dificultats temàtiques, teòriques i metodològiques, l'antropologia de la violència pot considerar- se un territori fronterer de la disciplina, en el qual es posen a prova contínuament els modes consensuats de fer etnografia. Els marcs teòrics disponibles ens resulten en molts casos insuficients, les estratègies d'investigació establertes han d'exposar-se a ajusts molt substancials, les retòriques amb què expressem l'anàlisi han de ser especialment autoreflexives, i la devolució a la societat del coneixement generat s'ha de fer més versàtil, des dels canals habituals de publicació i difusió fins a altres formats més àgils de «resposta ràpida». Després d'una discussió general sobre les característiques generals de l'antropologia de la violència de les últimes dècades, es posa com a exemple la investigació que l'autor està duent a terme des del 2003 a les exhumacions de les fosses comunes de la Guerra Civil, a l'Espanya contemporània.

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Ethnographies on the limit

Ethnographic versatility and short-circuits before contemporary violence[1]

In this text I shall defend ethnography as a weapon loaded for the future, a tool for research and analysis with a fruitful past and undeniable future possibilities, with great potential for the critical analysis of the changing circumstances of social and cultural reality, adapting to them with flexibility and rigour. A fundamental challenge for our discipline, as Gupta and Ferguson (1997) sustain, is the gradual reflexive and critical adjustment of traditional methods and subjects of anthropological study to a more and more complex reality that is global and interrelated, while being equally demanding (it must be said) of members of its own discipline and their analysts. Such an adaptation, for these same writers, requires a re-evaluation of the more or less formalized "hierarchy of purity" of habitual field sites, a re-evaluation that could be an opportunity to "reinvent the field", both in terms of methodol-ogy and location, of the anthropologist's positition. That is, faced with the transformation of research scenarios, the constant and parallel innovation of the theoretical and methodological frameworks we are used to becomes essential, including the ways we imagine ethnographic scenarios, and strategies and documentation of the return on knowledge. Ethnography has enough resources, flexibility, and rigour to go along with these changes, not only maintaining its "family feel" but likewise enriching and broadening its social relevance.

I will also defend the position that the anthropology of violence and the anthropology of social suffering

Keywords: Violence, exhumations, common graves, ethnography of the conflict, Spanish civil war

Paraules clau: Violència, exhumacions, fosses comunes, etnografia del conflicte, Guerra Civil Espanyola

have raised their profile significantly in recent years. They are complex horizons of the field that, precisely for the specificity and for the nature and variety of the theoretical and methodological challenges they raise for us, are in a position to become "frontier territories" of contemporary anthropology. Perhaps because we are speaking of "limit ethnographies", the study of violence and conflicts opens up new scenarios for research, requir-

ing us to re-evaluate other more classic scenarios, setting out new types of problems, confronting us with social agents in sometimes extraordinary and extreme situations, questioning our rhetoric and our ethical commitments and encouraging new modes of inter-disciplinarity. Along with this, we find ourselves questioning the terms and general conditions of debates concerning our methods, styles and repertoires of knowledge production. Just as some

defend the persistence of modified or restricted modes of on-the-ground fieldwork as a basic "sign" of the discipline, we now find highly articulated defences of "the anthropology at a distance", taking it as a legitimate way of placing the analytical lens over situations of extreme violence where it is impossible or highly unadvisable to be present on the ground, utilizing the comparative method and professional skill to articulate "anthropological versions" of situations we can merely get a glimpse at through the mass media (Robben, 2008). In this article, which is indebted to the important and now classic contribution of Nordstrom and Robben (1995), I will employ examples of my most recent fieldwork to make an evaluation of how some problems set out by the anthropology of violence and of social suffering might be useful in reflecting, from a more general framework, on the nature, limits and challenges of our work.

General Considerations on Ethnography

Let us begin with some general considerations on ethnography. Velasco and Díaz de Rada consider it a general methodological process that characterizes social anthropology, whose main "methodological situation" is fieldwork (1997). Hammersley and Atkinson, in contrast, understand ethnography as a "method or set of methods" of a fundamentally qualitative nature, where the ethnographer participates in the daily life of the people being studied. In their opinion, it would even be possible to speak of ethnography as "the most basic mode of social research", since it is the most similar to life routine (1994). For Marcus and Fischer it is "a research process in *which the anthropologist observes, records, and engages in the daily life* of another culture…and then writes accounts of this culture, emphasizing descriptive detail" (1986). Pujadas points to two basic meanings

■ Discrepancies over the visibility of scientific knowledge are relevant in the delimitation of ethnographic representation. A benevolent image, like the homage to a victim of the Civil War, is representative of the process of recovering historical memory.
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of the term: as a *product*, generally written although on other occasions found in a visual registry; and, in contrast, as a *process*, based on fieldwork (2004). For Pujadas, ethnography is one side of the so-called *anthropological triangle*, constituted on the other two vertexes by contextualization and comparison. Bryman for his part notes that the concept of *ethnography* has sometimes come to be assimilated by the text, which is the final product of the entire research process (2001). From the perspective of qualitative sociology, Willis and Trondman suggest we might conceive ethnography as a "family of methods involving direct and sustained social contact with agents and of richly writing up the encounter, respecting,

FIELDWORK, THE DIFFERENTIATING FEATURE OF ETHNOGRAPHY, BECOMES EVEN MORE SIGNIFICANT IN THE STUDY OF VIOLENCE

recording and representing at least partly in its own terms the irreducibility of human experience." In their opening "Manifesto" for the journal *Ethnography,* these writers propose the following characteristics: the importance of theory as a precursor, medium, and outcome of ethnographic study and writing; the centrality of "culture" in the research process; and the necessity for a critical focus in the research and writing of ethnography (2000).

Although we have already seen that there are certain research scenarios that, at least in some phases and for determined problems, make research in the field difficult and require strategies for "research at a distance" (Robben, 2008), all these writers agree that the "distinguishing mark" of ethnography involves the presence of the researcher

in the studied field; this presence quite logically carries with it a set of significant methodological consequences. An important characteristic of ethnology is that the researcher cannot control what is happening in the situation in the field chosen for the corresponding study, so that his or her presence ends up being fleeting. Another point in common amongst the mentioned thinkers is how they do not consider ethnography to be a closed research model, preferring to see it as "heterogeneous" like the objects of study it is applied to. For this reason, its practice puts the researcher in a position to utilize highly diverse techniques, adjusting and modulating the context of the study (Velasco and Díaz de Rada, 1997; Bernard, 1998). In this way it is an eclectic and reflexive practice that obliges the researcher to live out the research project within a kind of "methodological schizophrenia", or in a state of "explicit awareness", to use Spradley's term (1980), or in some type of "widened perception" (Peacock, 1989, cited by Velasco and Díaz de Rada, 1997). If we accept that the main instrument of research is the researcher, this latter should ideally be able to live daily life like any one of his or her informants, taking up the social practices analyzed in his or her routine, and even in his or her own body (Esteban, 2004; Wacquant, 2004). This experience should then be connected to the questions guiding the research, the roles played in the field and the techniques employed at every given moment. Further to this, immersion in the field, especially when long-term, requires the ethnographer to develop and cultivate a type of specific attitude towards reality, something Atkinson (1990) and Willis (2000) call *ethnographic imagination*, according to which it is necessary to keep up a dual focus, permanently communicating a global perspective on the subjects and problems studies, and the restricted, daily contexts we work

in (Hannerz, 1998). Or, as Eriksen titled his introductory book on the subject, it is a question of negotiating the tension between "small places"

FIELDWORK PUSHING TO THE LIMIT WILL PREDISPOSE THE ANTHROPOLOGIST TO BECOME PARTICULARLY CAUTIOUS

and "large issues" (1995). In summary, I would like to emphasize that ethnography requires specific, in-depth training, and it is always emerging; it can be understood as a process where feedback dynamics are set up between theory and practice, reality and text, research design and changing situations, field scenarios and the application of research techniques, between the researcher's position and that of informants, between the researchers and the readers of their texts, and so on.

Researching Conflicts, Violence and Social Suffering

I will now set out a series of problems related more specifically to the ethnographic research of conflicts, violence and social suffering. Anthropologists who have dedicated recent decades to these subjects seek metaphors and key words to characterize an evasive field, rife with dilemmas and trap doors, which sometimes can end up pushing the theoretical and methodological repertoires to the limit. In the introduction to the compilation of basic texts by Scheper-Hughes and Bourgeois, *Violence in War and Peace: An Anthology* (2004), the authors bring together some of the most commonly-used terms in Anglo-Saxon anthropology so as to get to the foundation of such highly precarious territories: *symbolic violence* (Bourdieu), *culture of terror, space of death* (Taussig), *states of emergency* (Benjamin), *banality of*

■ **Nowadays one of the great challenges for the anthropology of violence is to seek out the mechanisms of the imaginary networks of political terror in times of globalization.** GETTY IMAGES

evil (Arendt), *peace-time crimes, invisible genocides* (Scheper-Hughes), *violence continuum* (Scheper-Hughes and Bourgois, 2004), and, to close with one of the most successful, *grey zone* (Levi). At the opening lecture of the VII Congress of the Federation of Anthropology Associations of the Spanish State (FAAEE), celebrated in Barcelona, which dealt with the imaginary networks of political terror in times of globalization, Roger Bartra challenged anthropologists to open up the "black boxes" containing the structures of production, mediation and conflict resolution: "The black boxes of the 9/11 airplanes hold keys to understanding the imaginary networks of political power –and terror." (2003).

In a subsequent article on the anthropology of violence, Carles Feixa and I prolonged this technological metaphor condensing concrete, global experiences by proposing the *culturalist*

deciphering of cell phone SIM cards (*Subscriber Identity Module* cards), which as micro-electronic terminals of the Al-Qaeda network, triggered the March 11, 2004, bomb attacks in Madrid. Earlier Nordstrom and Robben had titled their book on research in situations of "violence and survival" as *Fieldwork under Fire* (1995). With more or less success, and with the risk of contributing to this rather shrill inflation of metaphors for practical and conceptual orientation through quick-sand ridden landscapes, I would like to use the images of a "minefield", of "ethnographies on the limit", in characterizing research on these subjects, furthermore applying them to a growing proportion of contemporary ethnographic projects. This conception of the ethnographic field as a tricky minefield, pushed to the limit of its energy, theories and methods, leads us as researchers of social reality to become extremely cautious, raising

the precision of our work, designing plans able to anticipate dangers and difficulties, modulate investigative distances and analysis, take on ethical dilemmas and conceive of strategies in the anticipation and deactivation of existing obstacles.

We have already observed in an earlier text that the recent rise of research on violence, conflicts and their consequences (sometimes brought together under the non-specific umbrella term *social suffering*) responds, according to quite a few writers, to a previous deficit in the discipline caused by more or less explicit connivance with the agents of such violence, straight jacketing the discipline from a theoretical-methodological perspective that led to "selective blindness" or "imperial nostalgia" vis-à-vis supposed "savages in extinction" (Ferrándiz and Feixa, 2004; Starn, 1992; Nagengast, 1994; Rosaldo, 1991). Writers like Starn

(1992), Scheper-Hughes and Bourgois (2001) and Green (1995) have been highly critical of the obscurantist practices they perceive in part of classic and contemporary anthropology, as developed in sites of conflict in relation to the forms of violence that were not classifiable as *tribal* or *ritual*, and where their presence was clear in studied societies. Starn for example, in his well-known article "Missing the Revolution: Anthropologists and the War in Peru", criticized the disinterest anthropologists specializing in the Andes had shown with regards to the (no doubt clandestine, though hardly invisible) expansion of such an important guerrilla group as Sendero Luminoso (Shining Path), during their fieldwork in the 1970s. According to Starn, the theoretical and methodological baggage of the time, together with a nostalgic vision of the *Quechua* communities as the residue of a Pre-Hispanic past with no ties to existing Peruvian society, made it inconceivable to speak of a clandestine political organization with massive and dramatic consequences like those then being prepared –thus making it nonexistent as an object of study (1992). Scheper-Hughes and Bourgois (2004) suggest that part of this "avoidance" could also be related to fears that analysis of indigenous modes of violence could exacerbate stereotypes of "primitivism", or "savagism", which in turn could encourage or justify violent reactions. Even so, they also point to something crucial in the restructuring of the discipline: it has been colonial and imperialist violence itself, similar to current forms of postcolonial violence and exploitation, that has historically produced many of our "subjects of study" since the discipline's beginning (just as Taussig pointed out in 1987). Some writers, like Green, insist on remembering that violence on a state level, or even situations that could be catalogued as ethnocide or genocide, have been

for decades the fundamental political stage of our fieldwork, without their being adequately incorporated into corresponding interpretation and analysis (1995). As Nagengast observes, in general terms and until relatively recently, anthropology had never been in the first line of studies on collective violence, terrorism and violence in state-related contexts (1994), in spite of all the data and debates we might be able to offer given our preference for field researchers and the comparative method (Sluka, 1992).

Short-circuiting Classical Anthropology

If it is possible to speak of a *short-circuiting* of classical anthropology, in recent decades a shift has been made towards a situation of particular interest regarding this previously ignored violence. The same increment in the visibility of violence (as we consume it through the mass media), together with new theoretical developments allowing us to set apart, distinguish, contextualize and relate different types of violence with greater precision, are fundamental features in its current popularity as a research subject. Here we come across what could be collateral damage of note: the overproduction, and as a consequence the possible "excess representativity" of violent aspects of human societies, linked furthermore to the demands of an "academic market" that is more and more competitive and inclined (especially in the Anglo-Saxon world) to a certain "spectacularization" of academic production. In the more traditional fields of study of anthropology of violence, amongst which there are those Nagengast has called *tribal (pre-state or sub-state) scenarios of violence*, where the interest lies in the analysis of violence of a "*practical, physical* and *visible*" type (1994), in recent decades many other research areas have been added, intensified, and balanced out that respond to corresponding social, political, economic

and cultural transformation linked to the impulses of globalization.

We are not only dealing with the appearance of new research scenarios, but also of the transformation of more classic areas in the discipline, in parallel with the expansion and progression of our methodological and conceptual instruments for confronting violence.

The recognition and analysis the ways of violence are produced and transformed into the new "sounding boxes", into the "fluxes of globalization" is also important for the anthropology of violence and conflict. In all cases we find ourselves in complex, multifaceted contexts, moving from the most intimate spaces of human experience to the most global processes, where conflicts and violence are not fixed modes of social action but rather practices undergoing a "continual process of mutation". It is not so much that they have changed their nature with globalization. Instead, existing tension in this historical moment, as found amidst the acts, uses, representations and analyses of violence, has transformed each one of these spaces of

> **GROWING SOCIAL, CULTURAL AND POLITICAL COMPLEXITY MEAN THAT CONFLICTS AND VIOLENCE ARE IN AN "ONGOING PROCESS OF MUTATION"**

social action, thus affecting the overall context where violent acts are executed, interpreted and analyzed. As Bernard-Henri Lévy observes with regards to 9/11, "the stock of possible barbarities, which we had thought to have run dry, increased with a never-before-seen variation. As always, as happens every time we believe it to be turned

off or asleep, when no one expects it, it goes and wakes up with maximum fury and, above all, with maximum inventiveness: other theatres, new front lines, and new, more fearful adversaries, the reason why nobody had seen them coming" (2002). It is clear that the expression of violence in the mass media is a fundamental feature of this process of feedback, not only because of what is shown or magnified, but also because of what is silenced, diverted, simulated or hidden.

Glocal Tension between Violence and Domestic Consumption

When it comes to anthropology and its more habitual areas of field study, this *glocal* tension between violence and its domestic consumption (Ignatieff, 1999; Echeverría, 1995; Feldman, 1994), between "traditions" and their new cybernetic expressions, does not only affect mass political violence but indeed any type of violent practice, including those that seem to develop in more domestic and local contexts, and thus in principle are apparently "less disconnected" to global flux. The international debates and campaigns developed in recent years in relation to clitoral ablation or stoning for adultery, and their growing, fundamental link to debates on human rights, have transformed social, cultural and political contexts where violence previously took place and negotiated its legitimacy and meaning. Thus even the kind of violence that was once considered *ancestral* in certain spheres (including that of anthropology) is now "transnationalized", taking on a new visibility. It is now made out of new forms, with social, historical, juridical and gender-based processes, finding form as –more or less– the seductive flag of the moment, to be waved by the world humanitarian community (Ignatieff, 1998 and 1999). The cause of addressing this violence will be infiltrated into the agendas of certain feminist groups,

or become more or less stridently adhered to in discussion of migratory flows. It could just as well oblige corresponding local authorities charged with the task of guaranteeing a pure identity and political and domestic power to create justifying discourses to respond to a globalized audience. Or, in the best of cases, it might lead them to break with the practice altogether (Ferrándiz and Feixa, 2004).

Many keys to the debate on the anthropological study of violent events can be found in sources like *Dangerous Fieldwork* (Lee, 1995), and in the articles brought together by Carolyn Nordstrom and Tony Robben in their *Fieldwork Under Fire* (1995), by Greenhouse, Mertz and Warren in *Ethnography in Unstable Places* (2002), and by Sanford and Angel-Ajani in *Engaged Observer* (2006). Robben and Nordstrom (1995) emphasize the "slippery" nature of violence, as well as its cultural character. Violence can be confusing and can lead to disorientation –it does not have easy definitions, not even amongst the social agents implicated. It affects funda-

mental and highly complex aspects of human survival, and has a huge role in the constitution of the perceptions of those involved. For these writers, the complexity of the situation can come to produce an "existential shock" in the researcher (beyond the "cultural shock" characteristic of the discipline), destabilizing the dialectical balance between empathy and distancing. In this situation, methodological difficulties are important.

To begin with, Lee appeals to common sense. We do not have to go to a conflictive site if the fieldwork to be carried out is dangerous in a given moment. These given dangers, which could include accidents, robbery, muggings, illness, environmental pollution and the like, had not been systematically studied, and were considered simply "little struggles" to be informally commented upon amongst colleagues. Lee goes on to set apart two types of dangers in ethnographic fieldwork: environmental and situational. The first refers to the dangers posed to a researcher because of the nature of the field chosen, as was the case with many phases of my fieldwork in Venezuela,

■ On the basis of a number of fieldwork projects, such as on armed separatist groups in Northern Ireland, basic guidelines concerning action and safety for anthropologists were begun to be set up. AGE FOTOSTOCK

where we had to "enter" into marginalized neighbourhoods controlled by youth gangs, and even by armed children. The second arises when the presence of the anthropologist gives rise to some type of conflict that could lead to an act of violence. It would seem reasonable that these potential dangers are fundamental in the design of research agendas and in choosing or discarding ethnographic scenarios.

As Lee has also shown (1995), just as in some situations the presence of the anthropologist can go against the informants, in others it can work as a "free pass" for them, since the social agents know that an act of violence taking place in the context or against a foreigner has a potential media or diplomatic repercussion, which could either be interesting or not to the different factions. On occasion, furthermore, some people who are "voiceless" or with "weak political representativity" in a given conflict could for multiple reasons be interested in accepting a relationship with an ethnographer. Sluka, basing himself on his field experience studying armed separatist groups in Northern Ireland, lays out a series of general principles meant to guarantee the safety of people involved in a study with a significant political and military component, including the researcher, but especially concerned about his informants. The first is to develop a reflexive awareness of the difference between "real" and "imaginary" dangers, which are quite often influenced by media stereotypes. Some of the points he raises include the previous calculation of danger, the need to diversify the subjects studied so as to reduce the public visibility of the most conflictive of them, the elimination from the agenda of incorrect questions or subjects, the setting up of safety and confidentiality measures with regards to compromising field materials (such as with recordings and photographs), the clear definition of the limits where

the researcher is willing to participate or not, or the search for financing sources for the study itself (1990 and 1995). Feldman, who like Sluka worked in Belfast, built his "field" with the clear idea that "in order to know I had to become expert in demonstrating that there where things, people and places I did not want to know" (1991). Lee points out that it is crucial when conducting fieldwork in conflict situations to avoid provoking any possible suspicion that you are carrying out a secret study –like with the case that took place in Northern Ireland in the 1970s, when an American anthropologist was

DELIMITATING THE CONCEPT OF "VIOLENCE" MEANS TAKING ON A BROAD VISION OF REALITY. DEALING WITH ANTHROPOLOGICAL RESEARCH ON VIOLENCE MEANS BEING AWARE HOW MULTI-FACETED IT IS

injured by the IRA– and it is recommendable for the researcher to take on the preventative role of "routine coward". In his study on Belfast, Feldman ran into problems managing the rules of spatial segregation between Unionists and Republicans. When he realized that the only social agents who could move from one space to another were the police and the army, he chose to not use these routes in his ethnography. Any violation of these spatial codes would be ethnographically absurd at least, if not a sign of "complicity". That is, he had to stay in control not only of what he said or asked, but also of where it would be "politically correct" for him to physically be at each and every moment in the city.

It seems clear enough that within this ethnographic framework, violence is presented as a multi-faceted subject of study with multiple edges. Unquestionably, there are radical differences between some research scenarios and others. Still, as a basic rule, to the degree that violence sharpens in intensity (to the point of reaching the extreme Swedenburg calls *treasonous field sites*, there on the *first line of combat*, where the virulence of social confrontation is so great that informants would not understand intermediate positions or field relationships with people or groups considered to be rivals (1995), the uncertainties and dangers to carry out research also increase, whether for the anthropologist or for the informants and communities involved in the study, in the short or long term. In the circumstances described by Swedenburg, who did his field research in Gaza, the ethnographer becomes necessarily "contaminated" or "tainted", often irreversibly, by the social relationships established in the field, closing many doors to him; in quite a few cases "participant observation" is neither desirable nor safe. As Lee too observes (1995), the ethnographer is in a delicate position, since the information flux is very restricted, the terrains of suspicion are rather heightened and it is not hard for a researcher and his or her sources to be taken as spies or possible informers. In most cases, the primary resource of work is what Horowitz calls "cognitive disagreements" or "metaconflicts" (1991), which expose us to wave after wave of seduction or rejection on the part of the different categories of agents in a given social field. Here a question without a single answer could be asked, deserving to be formulated assiduously before and during the research process: what constitutes, in each case, "good enough" field work on a type and context of specific violence? Lacking any sort of precise solution or model, the viability and quality of the project would be

related to the capacity to carry out the research project in a "form of chronic and critical reflexivity", from where it might be possible to permanently and dynamically rethink the ethical aspects of the study, the (scientific, militant) position of the researcher with regards to the subject of study and the social agents involved, the methodological decisions taken when working between victims and perpetrators of violence, or the prioritizing of participative data gathering on practices and/or imaginary worlds and representations of violence, to mention a few aspects.

In the epilogue written for *Fieldwork Under Fire* in 1995, Feldman pointed out that due to its difficulty, we were dealing with an "in transit" research site where activity took place on the limit. In his view, which I agree with, if it were possible to speak of a *new ethnology of violence*, it should not be moving towards theoretical and methodological orthodoxy if its task is to produce "counter-labyrinths" and "counter-memories" versus forgetfulness and terror. We are thus speaking of sophisticated critical analysis. In "spaces of death", and even in "low intensity terror and violence zones", the ethnographer's lenses of analytical certainty and the subjects he carries out his research begin to mix murkily, generating special types of problems, (dis)encounters and translations. On the other hand, if we continue with his diagnostic, the arrival of violent people, deaths, mutilation, disfiguration, traumatized people or those who have disappeared from anthropological discourse, necessarily had to open up rifts in research strategies and in the rhetoric registering these individuals' emerging presence. We cannot thus wait for continual or lineal paths in the ethnography of what are called *states of emergency*. With such antecedents, I will now set out a set of reflections on the ethnographic scenario I have been researching for a number of years: pre-

sent-day exhumations of mass graves from the Spanish Civil War. Through them I will show some examples of how the study of violence and the fields of uncertainty it generates might contribute to more general debates on a discipline in continuous movement.

Quick Response Ethnographies

In 2003, after finishing my research project on the María Lionza spiritist cult in Venezuela, I began to follow the process of exhumations of mass graves from the Civil War, in the context of debates on policies of memory in contemporary Spain. The image of a minefield raised earlier is especially adequate in reflecting the impact these exhumations is having on certain sectors of Spanish society, especially amongst the grandchildren of the defeated side in the war. There was a new awareness for many that the rural landscapes where some of them were still living and others spent relaxing summer holidays, in many cases contained abandoned graves and a diversity of repressive scenarios. On a high impact scale, this has been highly shocking for many, giving rise to social movements of a dimension transcending the local contexts dedicated to recovering cadavers, a movement that in its most recent manifestation began around the year 2000 (Ferrándiz, 2005, 2006, 2009, 2009b, 2010). The first question I raised was this: is there any reason for social and cultural anthropology to become involved in the study of suppressed memory, to explore the "black boxes" of repression, to dig into the victorious schemes of the winners of a civil war? Was there any point in looking at the shifting status of commemorative monuments, the residues of historical prisons and concentration camps, the movement and public and private management of skeletons and mass graves, the political, juridical and media life of unearthed cadavers? I believe so, and for a number of reasons.

First, because as a number of colleagues have shown (Verdery, 1999; Robben, 2000; Sanford, 2003; Díaz Viana, 2008), the analysis of mass graves and of violently mutilated bodies allows for a productive convergence of anthropological disciplines, including the study of violence, death, victimization, human rights, mourning, emotions and social suffering, memory, ritual, kinship, mass media, audiovisual production and art. At the same time, the exhumations and accompanying social, political and symbolic action taking place around them constitute ethnographic sites of "deep play", while at the same time being complex, demanding and extraordinarily fertile, condensing multiple processes running the gamut from the deepest emotions and barely perceptible gestures to media spasms and similar reactions from the realm of high politics (Geertz, 1992: 339-372).

The main difficulties I have found in this research project are as follows: the complexity and competitiveness of the preferred ethnographic space in the first phase of the research (the exhumations) and the lack of public knowledge

EXISTING VARIABLES RELATED TO THE EXHUMATIONS OF MASS GRAVES FROM THE SPANISH CIVIL WAR MAKE ETHNOGRAPHIC SITES MORE COMPLICATED

of the role of the social anthropologist; social and media pressure related to the return of knowledge; and policies related to the representation of violence. The exhumations are difficult ethnographic spaces to manage for all social agents present, including social anthropologists. Along with

the tension accompanying the gradual unearthing of the remains, the emotional presence of family members, the flow of images and gruesome details of the circumstances of the executions, we must add the lack of predefined protocols for interaction and behaviour, and, for many individuals present, the lack of established political, symbolic and emotional guidelines for dealing with such situations, which in many cases are only once-in-a-lifetime experiences (2009b). General rules for interaction, access to the remains and even "appropriate behaviour" are negotiated by some family members, associations and technical teams, especially those directly involved in unearthing remains, yet these guidelines do not always work and are not equally satisfactory for everyone. Within this complex plot, even though social anthropologists have the theoretical and methodological frameworks with

which to interpret violence and the desolate landscapes it leaves in its wake, we do not have the disciplinary training of (to give an example) forensic scientists, who are used to working in close quarters to these situations. In this case, proximity involves dealing with cadavers of people who met violent deaths and all the processes going along with their gradual visualization. In relation to the "existential shock" described by Robben and Nordstrom (1995), ethnography in this case necessarily needs to provide gradual emotional training (in any case an important part of ethnography as a discipline) so as to handle a highly charged ambience in a way that will remain relevant for the research process. Upon this ground, sometimes complicated decisions have to be taken regarding the idealness of an interview in a given moment, the filming or photography of a specific situation,

the selection of "informants" in a highly fluid and volatile social field, or the management of nervousness often brought on by the presence of "experts", journalists, politicians and activists on the ground. This latter situation could lead to a certain "research fatigue" or "coding and categorizing saturation" amongst some of the people in proximity to exhumations, subject as they already are to a high level of emotional tension arising from the mere appearance of bones and the dramatic re-creation of those tragic events (Clark, 2008).

With regards to the survival of the social anthropologist, in a "professional limbo" in amongst the various researchers working on various aspects of historical memory in Spain, let me make a few general observations (referring especially to the exhumations) that could be extrapolated to the dis-

■ **Emilio Silva, president of the ARMH, clearly understood the importance the presence of anthropologists would have during the exhumations of mass graves from the Civil War.** CORDON PRESS

cipline in general. Once I had chosen the excavations of mass graves as the "starting and anchoring scenario" of my long term research on policies of memory in contemporary Spain, I put myself into contact with Emilio Silva, president of the Association for the Recovery of Historical Memory (Associació per a la Recuperació de la Memòria Històrica, ARMH); a trained sociologist, he was able to perceive from the start the relevance of having anthropologists present. Silva was always open to seeing different specialists join efforts in analyzing and understanding a variety of aspects of such a multi-sided phenomenon. In spite of this, not everyone in the context of the exhumations immediately understood what a social anthropologist was or what he or she would exactly be "good for". Like the occasion the forensic scientist Francisco Etxeberria (Leizaola, 2006) commented to me, with a mix of curiosity, snideness and affection: "I coordinate a team, I find graves, there's the excavator, I identify bodies, do a technical report and give the body back to the family: what do you do?" He was not the only one who had doubts. With every exhumation, in almost every instance of coming into contact with those present, we began our ethnographic work by answering questions. What do we bring to these scenarios of violence? Did we know how to unearth bones or identify the disappeared? Could we provide psychological support? Were we working for the media? Should we be included amongst the "activists of memory"? What solutions would we offer to the respond to the suffering of the victims? Who reads what we write? What was our presence good for?

At the beginning of the process, when various associations dedicated to reviving historical memory started to sign agreements with universities or contact specialists to create technical teams to carry out the exhumations with more

reliable protocols, social anthropologists were quite often not included amongst the group of experts taken to be *absolutely necessary*. This was regardless of the fact that many of the things going on in these excavations have been an academic subject of interest for our discipline for decades and still are today, as I have mentioned previously. Nowadays, many descriptions of exhumations in the press speak of the presence on the ground of "historians, forensic scientists and archaeologists", though they rarely mention social anthropologists. This lack of public visibility for our work can be worrisome. If everyone does know more or less what is done by an archaeologist,

ANTHROPOLOGICAL WORK ON THE EXHUMATIONS SHOWS HOW A BACKGROUND IN CLASSICAL ETHNOGRAPHY IS NOT ENOUGH

a forensic scientist, a psychologist, a journalist, a politician or a documentary researcher, the terms *social anthropologist* or *cultural anthropologist* cause a degree of confusion. This confusion in turn often leads to "short-circuited expectations" amongst anthropologists and all kinds of informants. It has taken a long time for our presence to be considered timely and necessary, especially by means of our gradual specialization in gathering testimony, which to a certain degree has become our "ethnographic alibi" when analyzing other ongoing processes that are too long to explain with each unearthing, and to every person who asks us what we are doing there.

The process of giving and gathering testimony is not, on the other hand, merely a data gathering technique in

a context of participatory observation. Rather, it has an important "political" component for people who, as quite often happens (publicly or privately) break their silence for the first time in front of a digital video camera. This introduces a new factor of complexity into ethnographic work, no longer solely relative to the structure and meaning of the emerging communities of expression and listening. It also has to do with the handling of recorded material after the exhumations. Specialization in witnesses, for its part, makes us competitive with other professionals, especially with "parachuting journalists" who have fallen in from nowhere (when they do appear), since our expectations and strategies in obtaining information are as highly divergent as the "in-depth interview" or the sound bite with its juicy quotation. Alongside our full acceptance into technical teams, our range of action has also diversified notably. In other situations we have even come to coordinate exhumations upon occasion (Ignacio Fernández de Mata, at La Lobera near Aranda de Duero, Burgos, 2004; Julián López and Francisco Ferrándiz at Fontana, Ciudad Real, 2005); we have organized lectures and summer courses, and have participated more or less actively in associations and in very solid projects for the recovery of historical memory (Ángel Del Río and José María Valcuende, *All the Names* Project (Projecte *Tots els noms*)).

Faced with a subject such as this, it is essential to consider the issue of anthropology's social responsibility (Del Río, 2005; Sanford and Anjel-Ajani, 2006). In a project of this nature, so relevant from the perspective of social debate, people and collectives we work with will frequently require of us "results with an immediate return". This could occur with the exhumations themselves (on the part of family members demanding explanations or media looking for an expert opinion),

In ethnographic research on the exhumations, cultural anthropology has been necessarily driven to interact with other fields, like history, psychology and forensic anthropology. GETTY IMAGES

in public events where the procedures followed during the digs is explained, with the ad hoc rituals whereby the remains are returned to the family, in lectures in community centres or old-age homes, or in conferences organized by interested associations or political parties. Elsewhere I have insisted on how important it is that for specific subjects, like those related to violence and social suffering, anthropology be agile enough to turn itself into a "quick response" discipline (2006). This would not mean renouncing or not appreciating the importance of the discipline's most usual formats and cadences (even though they are themselves changing very quickly), but widening the repertoire. It would mean being able to diversify the discourses we transmit knowledge with for different kinds of aims and audiences, all the while (as we suggested at the start) modulating research strategies to properly comprehend rapidly

evolving problems, even those moving at a lightning-quick pace. If we are able to handle this challenge, perhaps we could then speak of a combined strategy of "ethnographic fluids" designed to deal with "slippery" problems (Delgado, 2007) by means of a "dialectic of surprise" of reciprocal illumination (Willis and Trondman, 2000), and of "multiple rhythms and formats of the return on knowledge" in the academy and in society. Just as has happened for years in our field, and as our institutions increasing require of us, the more we are able to go deeper into the registry of "quick response", the more we will be able to increase our relevance in present-day social debates. This will give us the capacity for critical analysis in a variety of contexts, whether in academic meetings, NGO board meetings or relating to the mass media, where we are often underrepresented or where we find it difficult to "translate ourselves" in relevant fashion.

The Ethnography of Mass Graves

When it comes to policies for the representation of violence, criteria involving "a dense context, reflexivity and a critical apparatus" are fundamental in the case of exhumations and historical memory, with the exception that in this case we are required to interact with —and construct ourselves in relation to— fields of knowledge as varied as history, psychology and forensic anthropology. So as create a more subtle understanding of the previous debate, I will offer two examples related to the digitalization process of historical memory, and, more generally, to the problems arising from audiovisual products of the ethnology of violence (Ferrándiz and Baer, 2008). Exhumations of mass graves give us very explicit images of repression, inscribed in the cadavers that are gradually brought into sight. The most recent cycle of exhumations has taken place in the context of the information

and knowledge society, and this is a key feature of their spread through the social milieu, in political debates and even amidst the judicial apparatus (Ferrándiz, 2009, 2010). As technologies for the digitalization of images become less expensive, with video and photographic cameras as well as cell phones, we are able to begin to think about how the new "site of memory" might be its digital expression (Ferrándiz and Baer, 2008). At the exhumation sites many people present have such technologies available, and there is an almost compulsive digital registry of everything going on, although with diverse motivations and visualization strategies. Even though there is a great variety of events, objects and people who might be "digitizable", maximum attention is usually given to the bones, and, more specifically, to the signs of violence left upon them. How can all these images be fit into the ethnographic discourse? How might they be able to modify understanding of the problem analyzed and the very structure of production of ethnographic knowledge? Is it possible to speak of the emergence of a new franchise in the "globalized market of horror and suffering" (Ignatieff, 1998: 29-37, 1999)? I will deal first of all with the use of these images in public presentations, and then in academic publications.

In my first public presentations using PowerPoint, I sought quite precisely to shift attention away from bone remains, in an attempt to show that, to a degree, "there was life" beyond the exhumations. I sought to show that what particularly interested social and cultural anthropology were the parallel processes of remaking social networks, the more or less spontaneous ritualization of mourning, the enunciation of past narratives in emerging contexts, and so on –all taking place not only within but also around the exhumations. In a moment of uncertainty regarding our

role as researchers in the process, this is what differentiated us from other specialists. While the archaeologists and forensic scientists worked the mass graves inwards, with highly technical protocols, social anthropologists (like psychologists) worked more "qualitatively" from the grave sites outwards. This difference could be clearly seen in lectures, talks and public interventions of any kind. Amongst images of gestures by family members, ritual offerings and old photographs, I would always show

THE CRITERIA OF *DENSE CONTEXT, REFLECTION* AND *CRITICAL APPARATUS* ARE BASIC FOR CASES INVOLVING EXHUMATIONS AND HISTORICAL MEMORY

some cranium with a clear bullet hole, in testimonial fashion, so as to refer to the impact "those" images had had as they came to light in contemporary Spain. I would not even spend very much time on the image.

In the majority of cases, I used images that had already been shown to the public eye by some high-impact news media outlet (covers of *El País* newspaper, for example), which allowed me to use them at the same time as a secondary source on the media expression of the process, reflecting on the shifting of the thresholds of tolerance regarding certain images documenting the violence of rearguard Franco-era repression. That is, I would use a selection of images (deliberately discarding those with more explicit or less media-friendly violence) so as to mark off the discipline in particular in relation to the "forensic style", even though my project deals with analysis of violence. On top of this there was a paradoxical situation. In many of these interventions, I participated with archaeologists and

forensic anthropologists, whose visual presentations, conditioned in turn by training in their respective fields, went in the exact opposite direction. After viewing a number of long presentations where the main characters were exhumed bones, a visual "complicity of style" (MacDougall, 1998) with the forensic scientists began to emerge, which in turn profoundly modified my understanding of the problem. As with the rest of the local, national and even international audience, I began "to get used to" seeing bones of executed individuals projected onto large white screens, just like what was gradually happening for me with the bones seen live in the graves, digitalized bones seen accompanied by measuring tapes, guiding arrows, technical terms, reconstructions of bullet trajectories, and so on. I came to realize that all of my caution and the limited mention being made of these images, lagged far behind the interest found in the technical process of recovering historical memory, and the degree of absorption (even saturation) there began to be in Spanish society and in other more globalized circles. It should be said that the number of television series with strong forensic content was not unrelated to this process, as they are turning into powerful, already "popularized" ways of understanding and imaging various criminal scenarios (Kruse, 2010). My study had to include, in a more relevant way, not only the bones as they appeared in the graves, but also the way they were digitalized by various social agents and elaborated by various kinds of specialists. Even so, in spite of having brought them more relevantly into the analysis and into my presentations (as we shall see), the limit continued to be marked by an ongoing fear a promiscuous, decontextualized use might have, leading to the banalization of historical facts and of the social suffering they these images still give rise to in the present, what Bourgois called the *pornography of violence*.

As a second example, let me consider a publication on the ethnography of mass graves (2006), where I was given the possibility of including various photographic images. At first they were to go inside the journal, but later on they were to be meant for its front and back covers. When I received the publisher's proposal, I felt rather uneasy. The image he had chosen for the back cover was a close-up photograph of two skulls with a bullet hole in each and their jaws out of place. The image was not only extraordinarily explicit, but it also had been taken by the photographer with a more aesthetic than documentary approach, using the angular light and shadows of the evening. It was a "magnificent" photograph. I wrote to the publisher to comment on the consequence of giving priority to an image like that, especially in the context of an ethnographic study, and particularly one in Spain. It was clearly the image with the greatest impact and highest quality, but

was it also the most representative? Did it describe the process better than the others? Was the best place to view such an image in an academic publication? Images like it were circulating in Spain in the mass media and in cyberspace, and were a fundamental part of the forensic reports and their PowerPoint presentations given before full auditoriums, as we have seen. For my part, I was willing to take on the debate concerning the politics of representation in anthropological discourse, though it was something that had to be done with a theoretical and psychological armature. In the end, this image for the back cover was replaced by another that was more benevolent with the brutal violence of the Franco-era repression, and undoubtedly more "comfortable" and representative of the process of recovering historical memory than the first: a wide shot of a grave once emptied, after a commemorative ceremony. In this case, in shifting from explicit violence to its ritualization,

the fear of trivialization through the spectacle of the process of recovering historical memory had imposed itself over high-impact imagery, privileging a kind of visual prudishness that other specialists we collaborate with would consider timorous. The disciplinary discrepancies regarding policies for the visibility of scientific knowledge are, in the cases of violence we have dealt with here, relevant in the delimitation and reconsideration of the limits of ethnographic representation. The double page, colour publication three years later of a very similar photograph, taken at the same exhumation by the same photographer for the *El País Semanal* report "Un tupido velo. 140.000 muertos invisibles" (A dark veil: 140,000 invisible deaths), written by Benjamín Prado (January 18, 2009), represented for me the confirmation of a new turning of the screw in the limits of tolerance towards certain aesthetics of horror in contemporary Spain. ∎

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NOTES

1. This is a shorter, updated version of "La etnografía como campo de minas: De las violencias cotidianas a los paisajes posbélicos" (Ethnography as a minefield: from everyday violence to post-war landscapes). In Bullen, M. and Díaz Mintegui, C. (eds.), *Retos teóricos y nuevas prácticas*, San Sebastián: Ankulegi, 2008, pp. 89-115.