Liminal belonging: routes into research

Pertenencia liminal: vías de acceso a la investigación

Cristina Pallí

Universitat Autònoma de Barcelona - cristina_palli@hotmail.com

Introducing pre-positions

When I finished my degree in psychology, I decided to start my PhD in social psychology, in particular, in a new field strongly influenced by Science and Technology Studies –social psychology of science (Domènech & Brown, 1999, 2001a; Domènech, Íñiguez, Pallí & Tirado, 2000; Íñiguez & Pallí, 2002). On joining the department of social psychology, I entered a research group led Miquel Domènech and Francisco Tirado, whose aim was to conduct an ethnography in a biology and biomedicine institution, the Institut de Biologia i Biomedicina (IBB). This is how my thesis became a 'laboratory study' –featuring humbly next to illustrious predecessors (Knorr-Cetina, 1981, 1995; Latour & Woolgar, 1979; Law, 1994; Traweek 1988).

During fieldwork, there were plenty of occasions to talk and discuss with members of the biology laboratory. Very often we would engage in a conversation similar to the following: "So, Cristina, you are a psychologist, aren't you? Good, good. But what on earth is a psychologist doing in a biology laboratory?" This is a tricky question that I used to half-avoid with the following answer: "I am doing an ethnography". And they would say: "a what?". So I offered always the same account: "imagine I were an anthropologist studying the Numba-Numba tribe in Middle Africa. I would go there with my notebook and a pen and I would observe their practices and rituals and would pose questions, in order to understand how they live and organise themselves, how they see their world. This is what I am doing, but in a biology laboratory". Of course, people showed surprise in front of the juxtaposition of elements: Africa, ethnography, tribe, science? Difficult to understand that science, our main institution for creating explanations of the world, may be in turn in need of any explanation whatsoever.

Quite unconvinced of the need for our presence there, scientists in the IBF would go on with the inquiry. "OK, you want to study how science works, but, what exactly do you want to observe?" And there they got me, honestly, I never knew exactly what to answer: "Everything –how you organise yourselves, how you see your world, and... you know, the way you organise yourselves". Poor people, they were supportive enough to say "oh, how interesting!", but I don't think they ever had any clue of what I was doing. This paper is intended to shed some light upon this question –a question that has taken me years to answer: what on earth is a social psychologist doing in a biology laboratory?

Whilst my academic and institutional affiliation was clear, the complicated crossing of disciplines informing my in-between position made it very difficult for me to define my theoretical approach and the particular topic of research. The challenge of the data analysis and the

richness of ethnographic relationships set me on the move in a way that not only confused me, but also affected radically the way my thesis was carried out. Instead of hiding these effects under the rug of the 'personal', and with the conviction that such factors are always already theoretical. In this paper I would like to describe the 'vectors of movement' that unsettled my path: some of the pre-occupations that informed my perspective, the turmoil in my convictions that fieldwork provoked, and how I came to focus my topic of research. Thus, rather than an attempt to make my position explicit, this is an acknowledgment of the field of pre-positions and moving relations that constituted me as a researcher.

Breaking through the rampart

But my story must start slightly earlier, before I entered the laboratory, at a time in which I was concerned with cultural encounters. While I was still an undergraduate I started a collaboration with the team of José Luís Lalueza, Isabel Crespo and Adolfo Perinat . We were engaged in an action-research project working together with a Gypsy community (Lalueza et al, 2002), situation which gave us plenty of chances to think about similarities and differences in cultural exchanges. If something became clear in this project, is that conflicting relations between cultures are usually much more related with inequalities than with symbolic boundaries (Crespo, Lalueza & Pallí, 2002), and that boundary definitions are often under struggle. For instance, whereas many non-Gypsy people oppose Gypsy culture to Spanish culture, many Gypsy people claims their rights as Spanish. They would articulate a complex belonging: attached to Gypsy culture, differentiating themselves from Spanish culture, but still linking to national belonging (Cerreruela et al, 2001). This was an interesting articulation that could not be apprehended with simple ideas of inclusion and exclusion –the typical Venn diagram would not do. This called my attention to the multiple possibilities of the way of imagining differences and similarities (Pallí, 2003). The challenge, then, was how to understand this difference without reifying them in notions such as 'boundary' –understood, that is, as a previous conditioning to the relation.

This concern with 'boundaries' was not irrelevant. While engaging in this project, I became more and more worried by the increasing problematisation in the media and in public discourses of immigration and cultural contacts (for an acknowledged discussion of this problem and an exposure of these new discriminating discourses, see Stolcke, 1995). Indeed, such discourses were (and are) bringing a new actor into existence: the cultural boundary. Cultures are presented as essentialist, bounded entities (symbolically bounded sometimes, physically bounded at others) that define in a totalitarian way one's belonging, to the point that when people from different cultures meet, negotiations are difficult and conflict is unavoidable. By routing cultures with physical, limited territories, this cultural discourse allies itself with nationalism, pretending that states are culturally homogeneous and denying both the complex networks of exchange and communication that constitute cultures, as well as all the open spaces of negotiation and creativity that relationship between cultures enables. Cultures are never incommensurable, invariable, closed entities which clash against each when members enter into contact (the so-called 'cultural shock'), but are always already engaged and configured by mutual relationships.

This discourse transforms those notions that imply affiliation and attachment, such as culture, community, group or identity into containers: they are imagined as bounded entities, usually linked to a place or territory, a bounded area with limits that distinguish an inside and an outside. But, above all, this argument proves problematic in that it legitimates exclusion linked to identities and differences: "if you are like us, you are in; if you are different, you are left out". To make it worse, the problem is not only one of entrance, but also of exit: such entities become our prisons, the practices and notions that we can never escape and that are supposed to determine our cultural life. Symbolic limits become ramparts. Understood in this way, I must agree with Michel Serres when he says that belonging is the devil of the world (1995, p. 202). Therefore, what appeared urgent to me is the search for a notion of belonging that does not go through the logic of the excluded third. Easy said than done, of course.

Negotiating boundaries

These are the type of thoughts in which I was lost before, during, and after the ethnography. If at first I thought that my ethnography in the laboratory would mean the end of my worries about identities, belongings and boundaries, it did not take me long to discover how wrong I was. One of the first things I could experience in the ethnography was a huge tension in my otherwise very kind relationships with the members of the IBB, regarding whether we were similar or different to them. Members of the IBF consider themselves researchers, and they welcomed me as 'one of them', another researcher. We were part of the same university and cultural milieu, used to inquire objects of study with the help of particular methods, and I myself shared with other 'native' Ph.D. students the efforts of writing a thesis. In a sense, we were so similar that I was in danger to take their vision of the world for granted. Even the words and notions they used so as to understand themselves had a sense of immediacy for me -'science', 'politics', 'money', 'economy', 'boss', 'pyramidal structure', 'hierarchy'... I entered a world that was so similar to mine, that it was somehow difficult to find the curiosity to interrogate it. My main work, then, was to achieve a kind of distance from a model of knowledge production that was too obvious and understandable for me.

And still, it was simultaneously so different in some respect! Their groups had a stronger hierarchy than ours, their concrete practices at the bench were completely new to me, and, especially, their attachment to truth and tradition was different. They felt themselves to be a small link in a bigger chain of workers for true knowledge extending from past to future. And, in this sense, this world appeared to be miles apart from the world impregnated by social constructionist premises in which I had grown as a researcher. What to do with this difference? How to understand it in spite of common identities? This was a tension that I experienced throughout the study. Similarities and differences, proximity and distance had to be continuously worked upon, without reifying neither difference nor identity: how to understand difference without falling into an a-relational notion of contact (i.e., distinct 'self' and 'other' which meet in a second moment). And how to understand contact without falling pray to the idea of communion (that is, the idea that we are always already the same)?

But this was not the only concern about boundaries. For if there was something that kept scientists in the IBF busy was a continuous preoccupation with the maintenance of the so-called scientific community. Indeed, every single element disturbing definitions was excluded as

polluted and polluting: if a work seemed suspicious, it was probably not guided by scientific hypothesis; if a method was too artificial, it was not based on scientific premises; if a person did not work the expected amount of hours, she did not have enough scientific vocation; if a boss did not read enough literature, she was not a real scientist; and if a young social psychologist bothers too much scientific identities with her questions, then she is surely not a scientist!

My first interpretation of these phenomena was that scientists are continuously engaged in symbolic work to eliminate from the community those elements which would endanger their identity. That is, 'boundary work' (Gieryn, 1983) that constitutes and reconstitutes boundaries as moments of inclusion and exclusion. In this interpretation the boundary is never a precondition of the community, but an effect of practices constituted and constitutive of collective being. Boundary work –acts of differentiation and of similitude (Latour, 1988). In this sense, belonging was not an act of inclusion understood as accepting or rejecting an element within or without the boundary, but was a moment of reconstitution of collectives and identities (Pallí, 2000, 2001).

Nevertheless, helpful as this first reconceptualization was, I was not fully satisfied with it. I did not have enough with showing that boundaries are mobile, continuously on the making, with constitutive effects upon the community. These images were still preserving too much the idea of the line: one can imagine the line as a flexible entity, changing, unable to prevent transfer, but the line is still there (e.g. Cohen, 1985). The community is still thought as a kind of container, even if with flexible borders, and belonging is still imagined as being in or out, as inclusions and exclusions. This idea is immensely spread out, actually: when we are admitted in a club, in a school, in a group, we cannot avoid a feeling of 'entrance' or, in the contrary situation, of being 'left out'. Something similar is at work when anthropologists talk of negotiations with 'door-keepers'.

Affecting constitutions

However, my experience as ethnographer engaging in participant-observation disturbed the container image of belonging, for in ethnography you do not have a feeling of crossing borders, but of an increasing entanglement. An entanglement that, I will suggest, is already implicit in the notion of participation: to become a part of, that is, to take part in 'native relations': one is enmeshed with those people one is talking to, discussing with and acting next to (even though I must write 'native relations' in quotation marks because 'native relations' are disturbed immediately as such by the ethnographer's presence). In an ethnography one works to become a member, and this work makes it quite visible that becoming a member is not a moment of entrance to a collective, but a moment of intensive constitution and attunement: the creation of a self that learns to see and smell how members see and smell, feel how members feel, joke how members joke.

Thus, ethnography is not a matter of reflection or cognition, but an illustration of what Shotter (1993) calls "knowing of the third type": an affection, a metamorphosis. You feel engrossed in imbricated and imbricating relations that alter you, moved by the pathos and ethos of the community –one is happy with them and one suffers for them, one gets nervous with them and because of them, offended by them. As such, the ethnographic relation sets you in movement –

both, in the sense of displacement or mobility and in the sense of transformation or motility (see Munro's work for this distinction, f.i. Hetherington & Munro, 1997). This is probably why ethnography has so often been conceptualised as a journey, a trip of alteration (Clifford, 1986, 1997; Pratt, 1986; Velasco & Díaz de la Rada, 1997). But this is, I claim, a movement that is not reversible –lest we resource to an understanding of 'travel' prone to male biases, the lonely traveller unaffected by the world he distantly observes (Wardhaugh, 1999). Even though this new belonging does not need to change your self completely, as if you gained a different identity, it does create a small difference, a minimal deviation, a kind of partial doubling of your-self that makes it impossible for you to see the world as before.

An added effect of this is the revelation of the precariousness of belonging: all the work involved in the continuous (re)constitution of selves that are attuned to a community. Whilst such awareness is not limited to ethnography, it remains true that the experiences gathered in the latter make the stability of 'home' shake, challenging "the happy phenomenology of home" (Sibley, 1995; see also Bhabha, 1994; Wardhaugh, 1999; Weber, 1998). It is in this sense that we can consider it a non-return journey; we never step in old, original selves. But ethnographic work brings to the fore not only the precariousness of home, but also of our-selves: we appear as entities in continuous (re)constitution that come into being or melt into oblivion: folds of exteriority, to put it with Foucault (1990, 2000), Deleuze (1989) and Blanchot (2001). In this sense, I think this ethnography posed a challenge that I could only partially pick up: how to understand the emergence of selves while at the same time struggling to unthink oneself.

Simultaneously, the awareness of these previous and sometimes still valid belongings creates a feeling of doubling, of multiplicity: one feels constituted by different voices and practices, as if one's self was more than one. Which provokes another puzzle: how to conceive of this multiplicity? We do not imagine our self like a society of different, multiple selves that enter conversation with independent existences, so to speak; we feel a stronger connection between these partial beings. But we are neither, pace Mead (1934), an integrated unity that ensembles without tension partial selves into a coherent totality. We do feel a vector of multiplicity, we feel stronger than we usually admit what Mead himself called 'cleavages', as when we are aware that others speak in us, and that there is something other in ourselves.

In short, what appeared also was the need to reconsider what Strathern (1991) calls our Western mathematics, that is, the idea that our basic unity is the one. The way we usually imagine quantity makes space for competing images: whereas small entities are unproblematically imagined as one, big entities such as society or institutions allow two alternatives. Either they are thought to be composed by the juxtaposition of many small ones (and then we have individualistic, atomistic notions of society as a sum of individuals) or big entities are imagined as big ones bigger than its parts (such as in Durkheimian ideas of society, for instance; society as an organism that encompasses individuality to work as one). Hence, the difficulties to think belonging as something else than inclusion! According to common views, to belong means to become one of these unit-elements that constitute the one, or to melt into the oneness of the collective. In both cases, though, the entrance into an 'inside' is perceived as necessary. However, this is precisely, as we have said, the perception that ethnography disturbs. Hence, a suspicion started to grow: that a reconceptualisation of belonging needs to

go hand in hand with an understanding of multiplicity (in the Deleuzian sense, as opposed to the multiple). This was, then, another worry informing my concerns.

What I am clumsily trying to explain in this section, then, is that being an ethnographer -a participant-observer- involved a process of ontological transformation of my self, and a continuous challenge to common-sense views upon what it means to be-with, to be part of. Indeed, participation emerged as 'becoming a part' resonating with other parts without being reduced to sameness. A kind of being attuned to something other that transforms you by bringing you in accord. An affection that entangles you with those with whom you are in relation (Brown & Stenner, 2001), and that should not be dismissed by saying that emotions are individual epiphenomena. Thus, while observing and suffering all these changes in my-self, I started to realise that by analysing my own transformations as a researcher, I had at my disposal a material through which to think a different way of belonging.

Extending relationality

What to do with all these thoughts and feelings that I felt accumulated, waiting for me to make sense of them? All these impressions made some interference with what I started calling 'my data', meaning a recollection of procedures and protocols with bacteria, proteins and genes, as well as organisational stories going on in the lab. Interference in the sense that I felt that, before analysing the empirical material I had gathered during fieldwork, I needed to pay heed to all these transformations experienced as a result of the relation to something other than myself. To this task I dedicated my MA project (Pallí, 2000). The undesired consequence of this was, however, that I had 'my data' lying around for a couple of years, not being able to tackle them. This inability to deal with the empirical material was accompanied by the incommodity of not being able to answer to the question: why a biology laboratory? All I had been analysing until that point was related with identity, belonging, community... All well and good, but I could have drawn exactly the same conclusions visiting any other community, or even analysing media discourses on cultural contact. What was the specificity of this ethnography?

It was not until very recently, on preparing the viva of the thesis, that I realised that, inadvertently, I had created a division between 'relationality' and 'data' –which, implicitly, reintroduced a division between 'emotionality among people' vs. 'cold things such as labs and proteins'. As if the data had nothing to do with relationality, as if fieldwork relations were not already data. This division was blocking my path, and it would probably have done it longer had I not felt the urge to deliver: many people were waiting for the analysis of the data (among them, my ethnography group but also all those members of the IBF whom I had disturbed with my questions). Thus, I felt indebted enough to start with the analysis of the empirical material. Despite not quite knowing what I was looking for, I decided I would let the data do the job for me and went on.

After some time I realised that my concerns with relationality and those of members of the IBB were not that different. People in this institution discuss the ways in which the group organises their 'being together', continuously the balance between individual contributions and collective achievements. The relations between the boss and the members of the group seemed to require continuous elaboration. There were narratives circulating around to make sense of the

tense relationship between science and politics, science and economical resources, science and the media or science and society. They spent an incredibly amount of time constructing links between laboratories so that their results, theories and doubts did not remain local, but could circulate in global networks; and whereas they were proud of their strong position nationally, they did not cease to reinforce international collaborations making contacts through the world. They had regular workshops within the group to make sure that each member's work was known by the rest and they had to be attentive to what other laboratories did, in case they could help each other; as they put it, their interdisciplinarity was their main strength, but this meant that they had to invest continuous efforts to make it work. Which was quite easy at some points, and not that easy at others. At the time we were there, they were learning to connect the work of experimental laboratories with the results obtained in bio-computing laboratories, and these attempts, interesting as they were, proved not that straightforward.

All of a sudden what became clear to me was that all these worries and endeavours could be read as work they were doing upon their being-together and their being-with-others. Or, to put it differently, that relationality was already a concern of them as much as of mine. Or, if you want to put all the blame on me, that my concern with relationality could be extended to the empirical data as much as to the reflection of the fieldwork experience as such. What is more, the empirical data themselves had a lot to show me about this topic. All the abovementioned examples showed a concern with moments in which entities, parts or bits and pieces came close to others; they were moments in which people discussed what was affiliated with what and what remained disconnected, what should come together and what should be kept separate. In a way, one could say –and this is how I came to see it- that these were moments of creation of belonging, moments in which attachments and detachments were performed. Moments of encounter between things that are imagined as different but which nevertheless enter in connection constituting attachments –moments of connected heterogeneity. This can either be the case of two entities that are linked together –as science and politics; biologists and psychologists; objects and subjects; the collective and the individual. Or of one entity that is divided by transformation –our self before and after an ethnography; the same protein after a purification and after computer simulation; our past, present and future self; or even the case of science as practiced in local settings compared to science grown global through international collaborations.

Following my previous concerns, I started thinking about these encounters of heterogeneity using as a tool the dichotomy self-other, precisely as an attempt to challenge the very division itself; that is, as an attempt to show how such dichotomy was always a result of a relation, and not a condition previous to it. If one does not focus in selves and others as entities, but on the processes of their constitution, then a much more ambivalent, ambiguous space of the in-between emerges (Anzaldúa, 1987; Bhabha, 1994; Fine, 1994; Star, 1991), where ontologies are still on the making and relations show their creative potential. However, the danger of such denomination is that one has to make continuous efforts to exorcise the shadow of reification; indeed, such an expression runs the permanent risk of situating 'selves' and 'others' as independent entities that only in a second moment come to be related. My aim was rather different: how to make sense of relations assuming neither a previous a-relational difference nor a field of identity and homogeneity.

Hence, I progressively became interested in what we could call 'moments of alteration' or 'becoming other'. Moments of transformation in which the dichotomy 'self-other' does not represent 'selves' and 'others', but rather, can be taken as a vector of alteration, an arrow describing a movement of transformation, a movement towards the outside (a fold) that reveals the presence of exteriority constituting us. Thus, thinking selves and belonging in terms of becoming and alteration is a way to account for the presence of otherness within the self –an absent presence constitutive of ourselves that challenges the idea that we are self-sufficient, a closed entity. Alteration, the presence of otherness in one's self, stops the circularity of being from ever closing into a circle, and forces us to acknowledge the opening of being and being-with-each-other (Bachelard, 1994; Deleuze, 1989; Duque, 2001; Foucault, 1994; Heidegger, 1971). In this way, becoming-other disturbs those limiting boundaries which we bring into being when we imagine belonging as an excluding enclosure.

To make sense of this wide range of situations in which we experience attachments -our being-together and, with it, our being transformed by something other than ourselves- I explore the notion of liminality. Liminality not only in the classic anthropological use as a moment of ambivalent non-structuration between two normal states (Turner, 1994), but also as a way of thinking about this particular creative moment proper of relationships, in which parts are imagined to connect to parts partially, bringing novelty into the world.

Mixing traditions

In order to think about liminality in this amorphous way, I have resourced to different traditions. And this is, probably, one of the most conspicuous features of the thesis: the mixture of authors, proposals and theoretical perspectives. But whilst I have shamelessly mixed, my mixtures were not random –not always random. From social psychology I took the interest in identity, belonging, selves and community, as well as preoccupations with reflexivity, positions, and the focus upon narratives and discursivity. But the very nature of the study I had in my hands forced me to explore other directions. First, because at the end of the day I am a psychologist worried about sociality using anthropology's methods doing an ethnography among biologists within the tradition of science and technology studies! And second, because to analyse liminality and moments of alteration without leaving one's place sounds unreasonable. How could I think of encounters and becoming-other without mixing disciplinary orthodoxy? We could say, then, that the topic itself forced me to some extravagations through unknown territories.

Next to social psychology, an important but ambivalent affiliation was the plurality encompassed by the label science and technology studies (STS). In particular, I was quite attracted to the provocations of Actor-Network Theory (Callon, 1986; Domènech & Tirado, 1998; Latour, 1987, 2005; Law, 1991), but I did not want to construct a universe of "actants" and "obligatory points of passage". So I took the challenges I found interesting, and dismissed the rest, in a good illustration of "translation, trahision" (Law, 1997). STS contributions helped me reject the division context-content, and extend constructionist insights beyond the realm of words, in order to show how scientists bring to the world a new entity. Likewise, these studies accompanied me in the attempt to challenge dualities and dichotomies, an exercise that I have tried to sustain, with more or less success, throughout the whole thesis.

Since I was doing an ethnography, it felt only natural to turn to anthropology for help. Looking back I can say now that this move was quite important, because it allowed me to gain the kind of distance that I was in need of, if I was to offer something other than a nice description in native terms. It allowed me, if you will, to exoticise home; to disturb the naturality of taken-for-granted assumptions of my own world (Augé, 1998). Moreover, it gave me tools to analyse a scientific community as any other community, and scientific practices as any other type of practices. Importantly, then, anthropology, next to STS studies, helped me put the Western privilege of science into parenthesis. But to challenge privilege does not equate to gainsaying specificities. Therefore, Isabelle Stengers' (1997, 2000) work proved effective to balance this last move.

A special inspiring source within anthropology has been Marilyn Strathern's (1991) Partial Connection. This notion refers to a way of imagining parts that relate to each other, creating a novelty (allowing the actualisation of something new) which is more than the parts alone, but that nevertheless do not resolve into a unity or a completion –thus challenging our Western perception that there is no alternative between one and multiple ones. This notion allows to conceive partial relations –that is, a type of relation that allows the perception of connectivity and of difference, a relation that makes space for a perception of non-relation. Difference is both an effect of a relation and simultaneously one of its conditions of possibility, suggesting a particular way of articulating presence and absence. If this sounds paradoxical, the effect is intended: the notion of partial connections is an attempt to challenge the either-or logic impregnating much of social thought, by cutting in diagonal oppositions and dichotomies.

Liminal science

So, what comes out of this mixture? And, more to the point, to what extent is my work related to science and members of the IBF? One possible answer is to suggest that, if the practices of this group have rendered themselves so well to my analysis of boundaries and folds, this might have to do with the fact that science, at least as it is practiced in the IBF, can be conceived as a liminal endeavour. As I have tried to show, the IBF, as a locus of production of science, inhabits the boundary. We entered the lab looking for the place where science is produced, just to found ourselves in a complex tissue of liminal relations: science inhabits the tense and partial relations between labs, groups, and personae. But, above all, if science is liminal, is because it inhabits the delicate boundary-fold that links and separates simultaneously objects and subjects. This last claim may require some elaboration.

There is no denying of the intimate entanglement that links scientists with their objects of study –and they themselves are quite aware of it. In this sense, it is still worth insisting that there is no reality apprehensible independently of our practices –no given separation between object and subject. However, one thing is to defend that objects are constructed through practices and languages impregnated by our cultural prejudices, and another, quite different, is to think that the relation subject-object makes the object redundant. Or, differently put, to claim that the object is predetermined by the subject's position within a particular community. This is a position that a scientist could never sustain, because it would be tantamount to self-destruction, a denial of what they understand as their vocation –to tell of the object.

This is why, when I arrived at the IBB with my social constructionist accounts on convention, power and interpretative repertoires, they kindly pointed to me that I might be missing the point - my object of study talked back: they started calling me Psycho-Killer. I was facing a dilemma: I could abandon constructionist claims and proclaim the Kingdom of Reality –this would have been a type of conversion that they labelled as 'seeing the light'. Alternatively, I could dismiss their perception of their practices and swear that with the invention of science, nothing new had entered the world.

Nevertheless, given the cruelty of the nickname, I was faced with a new question: if I am Psycho-Killer, what am I killing? Was theirs a complaint for the loss of their privilege to talk on behalf of reality? Perhaps. Importantly, however, whilst many of the scientists were quite willing to accept the amount of interpretation, negotiation and power relationships within science, there was a general puzzlement towards the distribution of agency of my social constructionist interpretations: if there was nothing else than society in science, then what on earth were they doing there? A puzzlement, interestingly enough, that I started to share. My own version of science stopped at the bench, whereas it was precisely the bench the locus I was confronted with and couldn't make sense of. One should not misunderstand my claim here; at no moment did I find social constructionist contributions irrelevant, on the contrary. But they were somehow insufficient to understand how their proteins, to name just one of the main characters of the ethnography, came into being –was constructed or engineered into being. Thus, my own incapacity to make sense of their practices at the bench made me consider seriously their complaints as rather ethical –word which is, as Stengers reminds us, linked to ethos: what if I was killing the very specificity through which they understand their being?

The whole thesis can be read as an attempt to understand this missing "little what", that is, what they felt my description was missing, and how to make space for it without legitimating science in terms of rhetoric of truth (Ibáñez, 1995). And to do this, I turned to them for inspiration –to my object of study and their version of their relationship to their object of study. For what applies to the relation between a biologist and her protein may also be valid for the relation between a biologist and a psycho-ethnographer. This is, I think, one of the presents they gave me. They showed me that science has to do with courage to deal with a risk: the risk of becoming entangled with the object that constitutes you, while you constitute the object with the power to say no -that is, with the power to say something different from what you say. Thus, the invention of science lies in a particular object-subject relationship that allows for a radical distribution of agency.

One can believe that reality is pre-existent to human practices and as such it enjoys an existence independent of the activity of researchers: the division between subject and object is a given. Alternatively, one can insist that objects are constructed through practices and languages impregnated by our cultural prejudices: there is nothing significant in the object that it is not predetermined by the subject's position within a particular community, and in this sense, any pretence of division is a fake. From same to other or from same to same, locked in a dichotomic discussion. However, there is a way to focus the problem avoiding this split choice: we can imagine subject and object partially connected, celebrating the gap as both, a possibility and an effect of this relation.

A possibility, since it would make no sense to try to understand the object if there were no gap to cross; without it there would be no possibility to construct new knowledge. Only if we manage to cut our-self from the world mixture in which we are embedded can we interrogate what we perceive as other. Differently put, the object must be accepted as other/different/distant. Nevertheless, at the same time, research upon this object is not possible without becoming intimately enmeshed with it –after all, the object is constructed, invented and not 'discovered'; distance has to be worked upon and created anew to stimulate its abridgment –an effect. Hence the relationship between object and subject is, to use Foucault's words, simultaneously "a tear and a bow", a cut and a link. Which means that science is not to be found in the subject and its community (neither in its normative version, as a set of norms, rules and methods, nor in its cultural version, as reducible to processes of interpretation). Nor, for that matter, in the object (in this mythological 'out-there' reality). Instead, we could say that science lies in the effortful establishment of a particular type of relation and consequently, it inhabits neither the object nor the subject, but precisely the space in-between, the abridgement between subject and object.

Paraphrasing Deleuze's (1989) interpretation of Leibniz, we could say that subject and object are different but indivisible, always already involved in a mutual constitution. They extend each other, in the sense that each of them becomes a background for the other; the background against which, from which and with which the other actualises itself or gains possibilities of being. And this is another way to imagine belonging. A belonging that does not require containers, and that it is better expressed with prepositions such 'of'. A preposition that, like our idioms of belonging, suggests the double meaning of attachment and property. Genitive case: a generative relation of co-property understood as affiliation. The object is of the subject, as much as the subject is of the object – an exchange that takes place as a mutual realisation of extended capacities: we extend each other's possibilities of being.

And this is, perhaps, an open path for us to recreate belonging. A way to convince ourselves that we do not belong because we qualify as members and are 'admitted', as if we could posses a right to enter home and rule it, excluding those who are not perceived as the same. The encounter narrated in this thesis can perhaps offer a new image: that we belong because we inhabit each other's hospitality; because we participate of each other, we take part of (we become a part of) partial entanglements, that enable us to exchange gifts with each other -gifts of existence that set us in this spiralling move of extension of which we never return.

References

Anzaldúa, Gloria. (1987). *Borderlands/La Frontera*. The New Mestiza. San Francisco: Aunt Lute.

Augé, Marc. (1998). *A Sense for the Other*. Standford, CA: Standford University Press.

Bhabha, Homi K. (1994). *The location of culture*. London: Routledge.

Blanchot, Maurice. (2001). Lo extraño y el extranjero. *Archipiélago, 49*, 80-86. (Original published in (1958).

Brown, Steven D. & Stenner, Paul. (2001). Being affected: Spinoza and the psychology of emotion. *International Journal of Group Tensions, 30*(1), 81-105.

Callon, Michel. (1986). Some elements of a sociology of translation: Domestication of the scallops and fishermen of St. Brieuc bay. In John Law (Ed.). *Power, action and belief: A new sociology of knowledge?* (pp. 196-233). London: Routledge & Kegan Paul.

Cerreruela, Emilio; Crespo, Isabel; Jiménez, Raimundo; Lalueza, José Luis; Pallí, Cristina & Santiago, Ramón. (2001). *Hechos gitanales. Conversaciones con tres gitanos de Sant Roc*. Barcelona: Servei de Publicacions de la Universitat Autònoma de Barcelona.

Clifford, James. (1997). *Routes. Travel and translation in the late twentieth century*. Cambridge, MA: Harvard University Press.

Clifford, James & Marcus, George E. (1986). (Eds.). *Writing Culture: The Poetics and Politics of Ethnography*. Berkeley: University of California Press.

Crespo, Isabel. (2001). *Cambio cultural y desarrollo humano en contextos minoritarios: el papel de la mujer en una comunidad gitana*. Doctoral thesis presented at the Universitat Autònoma de Barcelona, Bellaterra. Access at http://www.tdx.cbuc.es/TDX-0123102-155728/

Crespo, Isabel; Lalueza, José Luis & Pallí, Cristina. (2002). Moving communities: a process of negotiation with a gypsy minority for empowerment. *Community, Work and Family, 5*, 1, 49-66.

Deleuze, Gilles. (1989). *El pliegue*. Barcelona: Paidós. (Original work published 1988).

Domènech, Miquel & Brown, Steven. (1999). *Science? Technology? Society? Shifting terms with the social psychology of science and technology*. Presented at the XII General Meeting of EAESP. Oxford, 6th-11th July 1999.

Domènech, Miquel & Tirado, Francisco Javier (Eds). (1998). *Sociología simétrica. Ensayos sobre ciencia, tecnología y sociedad*. Barcelona: Gedisa.

Domènech, Miquel; Íñiguez, Lupicinio; Pallí, Cristina; Tirado, Francisco Javier (2000). La contribución de la Psicología Social al estudio de la ciencia. *Anuario de Psicología, 31*, 3, 77-93.

Duque, Félix. (2001). Los humores de Heidegger. Teoría de las tonalidades afectivas. *Archipiélago, 49*, 97-119.

Fine, Michelle (1994). Working the hyphens. Reinventing Self and Other in Qualitative Research. In Norman Denzin & Yvonna S. Lincoln (Eds.). *Handbook of Qualitative Research* (pp. 70-82). London: Sage.

Foucault, Michel. (1990). *Tecnologías del yo y otros textos afines*. Barcelona: Paidós/I.C.E.-U.A.B.

Foucault, Michel. (1994). *Hermenéutica del sujeto*. Madrid: La Piqueta.

Foucault, Michel. (2000). *El pensamiento del afuera*. (M. Arranz Lázaro, Trans.). Valencia: Pre-Textos. (Original work Publisher, 1966).

Gieryn, Thomas F. (1983). Boundary-work and the demarcation of science from non-science: strains and interests in professional ideologies of scientists. *American Sociological Review, 48*, (6), 781-95.

Heidegger, Martin. (1971). Building, dwelling, thinking. (A. Hofstatter, Trans). In Martin Heidegger. *Poetry, language, thought* (pp. 145-161). New York: Harper & Row.

Hetherington, Kevin & Munro, Rolland. (1997). *Ideas of difference: social spaces and the labour of division*. Oxford: Blackwell & Sociological Review Monograph.

Ibáñez, Tomás (1995). Ciencia, retórica de la verdad y relativismo. *Archipiélago, 20*, 33-40.

Íñiguez, Lupicinio & Pallí, Cristina. (2002). La psicología social de la ciencia: revisión y discusión de una nueva area de investigación. *Anales de Psicología, 18*, 1, 13-43.

Knorr-Cetina, Karen D. (1981). *The manufacture of knowledge. An Essay on the Constructivist and Contractual Nature of Science*. Oxford: Pergamon Press.

Knorr-Cetina, Karen. (1995). Laboratory Studies: the cultural approach to the study of science. In S. Jasanoff (Ed.). *Handbook of Science and Technology Studies* (pp. 140-166). London: Sage.

Lalueza, José Luis; Crespo, Isabel; Pallí, Cristina & Luque, María José. (2002). Socialización y cambio cultural en una comunidad étnica minoritaria. El nicho evolutivo gitano. *Cultura y Educación, 13* (1), 115-130.

Latour, Bruno. (1987). *Science in Action: How to Follow Scientists and Engineers through Society*. Milton Keynes: Open University Press.

Latour, Bruno & Woolgar, Steve. (1979). *Laboratory life. The social construction of scientific facts*. London: Sage.

Law, John. (1991). *A Sociology of Monsters*. London: Routledge.

Law, John. (1994). *Organizing modernity*. Oxford: Blackwells.

Law, John. (1997). *Traduction/Trahison: Notes On ANT*. Published by the Centre for Social Theory and Technology, Keele University at http://www.keele.ac.uk/depts/stt/jl/pubs-JL2.htm

Pallí, Cristina. (2000). *Jugant als límits: Selves, Others & Monsters en el viaje etnográfico*. Master project presented at the Universitat Autònoma de Barcelona, Bellaterra.

Pallí, Cristina. (2001). Ordering others and othering orders: the consumption and disposal of otherness. In Nick Lee & Rolland Munro (Eds.). *The Consumption of Mass* (pp. 189-204). Oxford: Blackwell Publishers/The Sociological Review.

Pallí, Cristina. (2003). Communities in Context: Undefinitions, multiplicity and cultural difference. *Revista Interamericana de Psicología/Interamerican Journal of Psychology, 37*, 2, 307-324.

Serres, Michel. (1995). *Atlas*. (A. Martorell, Trans). Madrid: Cátedra. (Original work published 1994).

Shotter, John. (1993a). *Cultural politics of everyday life. Social constructionism, rhetoric and knowing of the third kind*. Buckingham: Open University Press.

Sibley, David. (1995). *Geographies of Exclusion*. London: Routledge.

Star, Susan Leigh. (1991). Power, Technologies and the Phenomenology of Conventions: On Being Allergic to Onions. In John Law (Ed.). *A Sociology of Monsters* (pp. 26-56). London: Routledge.

Stengers, Isabelle. (1997). *Power and invention: situating science*. (trans. Paul Bains; Foreword by Latour). Minneapolis: University of Minnesota Press.

Stengers, Isabelle. (2000). *The Invention of modern science*. (D.W. Smith, Trans.) (L'Invention des sciences modernes. Paris: La Découverte). Minneapolis: University of Minnesota Press. (Original work published 1993).

Stolcke, Verena. (1995). Talking culture. New boundaries, New Rhetorics of Exclusion in Europe. *Current Anthropology, 36*, 1, 1-24.

Strathern, Marilyn. (1991). *Partial Connections. ASAO Special Publication 3*. Savage, Maryland: Rowman & Littlefield.

Turner, Victor. (1994). Betwixt and Between: the Liminal Period in Rites de Passage. In Victor Turner. *The Forrest of Symbols. Aspects of Ndembu ritual* (pp. 93-111). New York: Cornell University Press. (Original published in 1967).

Velasco, Honorio & Díaz de Rada, Angel. (1997). *La lógica de la investigación etnográfica. Un modelo de trabajo para etnógrafos de la escuela*. Madrid: Trotta.

Wardhaugh, Julia. (1999). The unaccomodated woman: home, homelessness and identity. *Sociological Review, 47*, 1, 91-109.

Webster, Wendy. (1998). *Imagining Home. Gender, 'Race' and National Identity, 1945-1964*. London: University College London Press.