
**APPENDIX I: BACKGROUND, FIELDWORK, REFLECTIONS**

**Beginnings**

The original idea to do this project was not mine, or not mine only. It came up in conversations with friends in Valencia, while I was living in Madrid but visiting Valencia often. On each visit I met and talked to people in the neighborhood association of my old barrio there. Once in a while we discussed my work on gender. And at some indefinable moment, there was a collective spark, a common idea: Wouldn't it be interesting to study the gender aspect of the neighborhood movement? And wouldn't I be the right person to do it?

For the purpose of preparing such a project, a group was formed in 1991, consisting of three local women activists plus myself. We had several meetings to discuss how to go about it. We agreed to focus on women. Not just women's committees or feminist activities but anything that women did in the movement.

The group at first wanted me to visit some forty or fifty federations, but after I had explained my anthropological preference for one or two, we reached a compromise of six. This decision turned out to be methodologically sound, but originally the reason I agreed was that I wanted the project to be truly collaborative.

We had long discussions on which federations to study. We discussed such criteria as north-south, big cities-small towns, types of local economy, federations with long experience versus relatively new ones, very successful federations versus ones with problems, federations under PSOE or IU influence versus more independent ones, and so on. We tried to pick federations that were different on several counts. Madrid and Valencia were big cities, Vigo and Cordova medium cities and Elda and Linares small towns. Both big cities and Cordova and Elda had well-established federations, Vigo and Linares newer ones. Women participated a lot in the big cities and Vigo, less in Elda and little in Linares and Cordova. Elda, Vigo and Linares were PSOE-influenced, albeit to varying degrees, Cordova was very much IU, and the two big cities independent. There were internal conflicts in all of them, but Elda was an example of a rather harmonious place. The big cities had varied economies; Vigo was industrial while Cordova was not, and Elda and Linares were both industrial but with very different economic structures. One of the big cities was geographically central, the other coastal. One of the medium cities was in the north, the other in the south. The two small towns were both in the south but in regions that were culturally different, especially as to gender.

As the group had predicted, all of these federations agreed to receive me. I also spoke to representatives of several more federations.

One of the women in the group, Cristina Verdugo, had good contacts at high levels of the movement, so through her I was invited to present the project idea at two meetings in Madrid and one in Tenerife (cf. chapter 5). It was also through Cristina that I was introduced to the CAVE president, the CAVE secretary and a few other persons who helped me get fieldwork going.

The other two group members were Julia Arjona and Rosa Ibáñez. All three have been of great help through the years, keeping me updated on trends and events in the movement and serving as sounding boards for my tentative conclusions.
Main fieldwork period

As outlined in chapter 1, the main period of fieldwork was 1994–1995. The project would not have been financially feasible if it had not been for the small apartment I had in Madrid. It served as the place for planning, preparations, transcriptions of notes and tapes, sorting of other empirical material and initial analysis work for about half the time. The other half was spent in the other five places, about one month in each place. Elda and Linares were first, then came Vigo and Cordova, and finally Valencia.

In each place, I participated in such things as meetings, demonstrations, group discussions, parties and local festive events, friendly talk with activists. I walked around the towns and cities, trying to obtain a feel for how they functioned spatially and observing gendered aspects of space and movement. I read local newspapers. I sat in federation and association offices reading their archives. And I interviewed, of course: movement activists and ex-activists as well as other local political persons such as party or union activists. I collected whatever movement material seemed significant, such as pamphlets and posters, bulletins and letters. I gathered statistics and other information on the towns and cities, from published studies where they existed, otherwise from municipal offices.

While staying in each place, this took all my time. Most of the work with the notes and all of the work with the taped interviews was done in the intervening periods in Madrid or later. The same went for reading the written material gathered.

I interviewed both women and men. But I took women's point of view as my main study object. I listened to women, participated with them, thought about their problems.

The chronology of each visit was similar. The initial contact was made from Madrid (in three cases with the aid of the CAVE secretary, in Cordova and Valencia through my own contacts). In each place (except Madrid), a meeting was arranged for me upon arrival, either with the federation assembly or the federation board or directly with a special women's grouping. This was my bridgehead, where I explained the project to key persons, and it invariably led to invitations to visit local associations, either to speak there, too, or else just to sit in on regular meetings and/or to meet activists. The details varied as well as the number of associations visited (cf. chapter 9–12), but in no case did I content myself with contacts only at the federation level.

In Madrid, my starting point was the neighborhood association in the barrio where I lived, Malasaña. I wanted to use the periods in Madrid to follow one association more closely throughout the year, and to choose the one I already belonged to seemed the natural way to go about it. Besides, it had been a very energetic and significant association in the Madrid movement. Unfortunately, in 1994–95 it was going through a difficult period, with low participation and diminishing influence in the barrio. But that was of course also one aspect of movement reality that was interesting to observe. For a while I contemplated switching to a different, more active association to follow throughout the year, but there was really no time for that. Instead I complemented data from Malasaña with interviews with women leaders from other barrios and at the federation level.

Back in Sweden in 1995, other professional responsibilities took up most of my time, so the writing up of this project had to wait. When my workload eased in 2002, I decided to visit each federation again. There were other advantages than mere updating with this, more on that below. Between September 2002 and February 2003 I lived mostly in Valencia, from where I prepared the visits and where I worked with the material between visits, much as I had from Madrid in 1994–95, and where I followed more closely the activities of the association of the barrio where I lived, Benimaclet, a very active and successful one.

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2 Special thanks go to the women of the associations of Carabanchel and La Elipa who offered their collaboration on this count.
3 Support from Umeå University, Sweden, and a small grant from the Magnus Bergvall Foundation made the revisits possible.
Revisits

To revisit field sites has long been an established anthropological method. Revisits are fruitful because they usually result in better rapport and deeper understanding. But there are also ethical reasons. The people studied should be given an opportunity to comment on the descriptions and interpretations that the anthropologist has produced before they are published.

In my case, the time lapse between the first fieldwork period and the main period of writing was also a reason to revisit. Moreover, I had actually promised to return and report.

The revisits were repeat experiences in most aspects. I contacted the federations ahead of time to set appropriate dates; upon arrival I usually gave a short talk on my work in a general open meeting or in a federation meeting or in a specially called women's meeting. Again I was received with warmth and generosity. Doors and archives were opened, meetings and interviews were set up, I was allowed to sit in on board meetings, and so on. As a whole, the revisits were both humanly and intellectually satisfying.

To update myself on everything that had happened during the eight years between visits was evidently impossible. People told me a lot about it, but I treated that as bonus information. The main purpose was to report on how I had gone about constructing my interpretations and to observe activists' reactions to them. As expected, the discussions became lively. I made it clear that I would ponder all opinions and incorporate them into the final text, if I could agree, but just as I had a commitment to the people who had given me information and to the movement as such, I also had a commitment to my profession, so I was not offering them the right of censorship. This was understood and respected.

The most interesting information turned out to be the reactions around uncomfortable topics. The lines of conflict in the movement stood out clearly. This is reported especially in the empirical chapters 7-12.

Choices in fieldwork

One year of fieldwork is considered acceptable anthropological practice. But in this case it was divided among six different places. Excepting Madrid, I spent only about a month in each town and about a week during the revisits.

My best knowledge comes from Valencia, because of longer time depth. For one thing I could compare with my earlier fieldwork there, 1982–83. For another, at the end of fieldwork I sold the apartment in Madrid and bought one in Valencia instead, and since then I have visited there regularly. And I am a member of the neighborhood association in the barrio of Benimaclet, so during the intervening years I have obtained continuous information. My fieldwork in Madrid was supported by my previous long-term knowledge of the movement there.

In the other four towns, my fieldwork was necessarily more superficial. The methodological choice to spread fieldwork over several locations is becoming a trend in some anthropological quarters, and it is becoming necessary as anthropology takes on new study objects which are better defined in other ways than by physical space (Clifford 1997, Hannerz 2001, Marcus 1995, Thurén 1999b).

To make such delocalized anthropology yield good fruit, however, we must be reflexive not only about our personal presence in the field and the various influences on our interpretation, but about the theoretical connections between our field data and our analyses. Anthropology offers a kind of knowledge not easily available through other methods, and this knowledge results from the "thickness" of our data (Geertz 1973, esp. chapter 1). Anthropological methods must not be trivialized (Thurén 1999b). Without thickness, no anthropology.

In this study, the strength of data lies in their extension in both time and space. There were many interviews, many situations, many informants involved, many different issues raised and compared, and all of this extended over six very different towns and cities. The constant frame of it all was the neighborhood movement. Therefore the thickness of my analysis is to be found not in any one place or in any one series of events, but around one topic, namely the overall participation of urban Spanish women in this special kind of political context. The methods of the project were designed accordingly, and the main period of data gathering was complemented with a time
perspective over a period of eight years, with letters, visits, phone calls, friendly conversations, participation in congresses, etc. and finally the revisits. Throughout the whole period, I have stayed in touch with the women of the initial work group and with a number of other key informants.

Self-reflection

One important methodological specification in research is the calibration of the instrument of observation, which in anthropology means the anthropologist herself. Who am I to be doing this study?

I am not a neutral researcher. During fieldwork, I presented myself as belonging to the neighborhood movement, and this was true. The movement has been important in my own life, and analyzing what it means to other women, I have arrived at an interpretation that fills me with emotion, even though fieldwork and analysis have made me feel some doubts about the future potential of the movement.

As to women's participation in public life, I am of course not neutral. I am a feminist, which means among other things that I think it is dangerous for women to leave politics to men. Not because there is anything wrong with the way men do politics (that might be so or it might not, but that is another issue and one which can only be tackled when/if women have also monopolized politics for a few centuries), but because it is dangerous for any social category to abstain, as a category, from influencing collective decisions.

I have been a feminist by conviction and reading as long as I can remember. For many years I have also belonged to various feminist organizations, both in Sweden and Spain. My knowledge of the Spanish feminist movement comes from participating in a feminist group in Madrid from 1985 to the early 1990s, from following closely the work of a colleague on the feminist movement in Madrid (Sundman 1999), from participating in many feminist congresses, courses and meetings in Spain over a period of more than twenty years, and from regular reading of Spanish feminist publications for just as long.

I suppose I must explain about my national identity. I was born in Sweden to Swedish parents and spent my childhood there; later I traveled around, married a Spanish man, lived in the USA for four years, then in Madrid for over a decade before moving back to Sweden, only to spend the following decades, up to the present, moving back and forth. My children grew up in Madrid. Many of the most important events in my life have taken place in Spain. But my professional career has been almost wholly Swedish. I like to define myself as bicultural, not just because I feel at home in both countries, but also because they have about equal emotional weight for me. It would be impossible for me to choose between them. Luckily I do not have to – due to legal vagaries, I carry both passports.

While doing fieldwork for this study, I felt I lived, reacted and thought as a Spanish woman and as a former activist of the neighborhood movement. I let it happen, because it felt right and because it made my rapport with informants easy and authentic.

As a Spanish feminist, and as a former housewife in a peripheral barrio of Madrid, I feel deeply wary of any reinforcements of traditional patterns. The threats of backsliding are dangerous for all women, therefore also for me personally. One must hold them at bay without relaxing for a second, because strong forces stand ready to press all improvements back using any opening.

As a member of the neighborhood movement, I feel as wary of attempts to neutralize it, diverting its energies, for example occupying it with harmless courses, as I do of attempts to instru-

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4 This is not the place to offer a definition of feminism. It is a much too large and varied movement for any concise labeling, and in its academic guises it has developed a complex set of debates (as discussed in a large number of texts, e.g. Basu 1995, Beltrán et al 2001, Benhabib et al 1995, Evans 1995, Kaplan 1992, Nicholson 1997, Threlfall 1996, Thurén 2003). When I say that I am a feminist, I mean basically that which almost all feminists agree on: that the gender order one lives in is a gender order, i.e. a social and cultural fact, not a natural and permanent given), and that one would like to contribute to changing it in a direction of less hierarchy, i.e. away from "patriarchal" social arrangements. I find this to be a political stand I need to take in both my home countries. Within Spanish feminism I further define myself as a socialist feminist, which means that I consider it important to combat other social hierarchies as well, especially those related to class, and I do not share the convictions of so-called "difference feminists". Cf. chapter 5.
Fieldwork and norms of interaction

It is basic anthropological knowledge that in the field one must adapt to and as far as possible adopt local interactive styles. This is usually a process of trial and error. Even when the anthropologist is familiar with the context to be studied, the norms for conversation, body language, courtesy, etc. must be taken into account when designing fieldwork methods, and reflected upon when interpreting data.

To adapt to the local style of interaction in Spain requires not pretending not to have opinions. The interaction dispositions of most Spaniards are critical and oppositional. The dominant interaction style is friendly and contact seeking, but consensus is not sought; it is rather a conversational norm that one should offer examples and illustrations, personal anecdotes and personal opinions, whatever one is talking about. Anything else would be interpreted as boring and perhaps suspiciously reticent. The value to be accomplished is not consensus but excitement, and knowledge of other people and opinions (Fant 1989 and 1993). I had to adapt to that norm. So I

5 There are good historical reasons for this acceptance, cf. Thürén 1988, but intellectual and political reasoning must sooner or later move beyond the dualism of "progressive" versus "traditional" thinking, as opposition to Francoism recedes in time.

6 The issue of how "nativeness" can be defined and how different degrees and kinds of it influence fieldwork has not received as much attention as it should. Early inquiries (e.g. Jackson 1987) were not very subtle, directed as they were to legitimating anthropological research "at home". Postmodernist authors have mostly focused on other aspects of fieldwork encounters, such as hierarchy and representation, taking quite for granted that the anthropologist is some sort of outsider. Feminist anthropologists, and also other feminists using ethnographic techniques, have discussed the issue mostly in relationship to the intersections of various hierarchical dimensions, usually gender, class and "race". Narayan (1997) and Shami (1997) are honest reports of personal experiences. Many general method texts (e.g. Okely and Callaway 1992) contain some relevant discussion. Clifford (1997) asks some sharp questions. But the issue still awaits a concerted debate in the light of recent theory.

7 To clarify in relation to expansive and cautious modes of management of meaning (cf. chapter 3): people who tend to prefer the expansive mode may be more critically minded in any conversation, but people who are cautious in the sense of upholding, not criticizing, established meanings are just as oppositional and critical in interaction style. Form and content is not the same thing. It is perfectly possible to shore up accepted positions in conversations sprinkled with oppositional phrases: "no, I don’t agree because...", "that is not what my grandmother said..." etc.
asked leading questions, declared opinions and gave examples of what I was after in the questions. This was experienced as normal behavior and did not make people adjust their ideas to mine.

One complication, however, was that there were several competing discourses inside the movement and I could not avoid showing which one I preferred. In the interview with Pilar Navarro, for instance (chapter 13), it can be seen that I use such key words as struggle, solidarity, not give up, etc., that marked me as of the reivindicativo tendency. But Pilar already knew that about me, through earlier encounters and common acquaintances. If I had used any other kind of expressions with her, she would have been disturbed, and I would have felt deceitful and manipulative. With people I did not know beforehand, I acted as neutral as the situations allowed, but I would, also in accordance with local norms, tentatively try out some key words of my own preferred discourse after a while. Another norm in Spanish conversation is that it is interesting (not aggressive) to try to convince others of one's own opinions.

These norms produce lively conversations. The risk, so often discussed in social science handbooks, that interviewees would say what they thought I wanted to hear was negligible. Usually, any Spanish conversation contains a mix of clashing and parallel ideas. If an interviewee disagreed with me on something, he or she would probably agree with me on something else. Discourses overlap and the movement universe is crowded with issues and events that are interpreted in myriad ways. In one way or another, we would find ways of creating the culturally desirable balance between excitement and agreement.

And I took care to live up to yet another norm or disposition: that of not letting one's sides down. The willingness to tell personal anecdotes and declare controversial standpoints and argue strongly for one's opinions should not be confused with risk-taking. Most Spaniards have fine-tuned sensibilities when it comes to what one can tell to whom. Nobody leaves his or her soul wide open.

It is always hard to know how informants interpret an anthropologist. Most people I met seemed to feel some initial insecurity about how to deal with a "researcher". But this feeling would not be as strong as in most other contexts, since movement people, at least the leaders, were used to meeting outsiders and explaining movement matters. In most encounters there was some deference towards me as a "teacher" to begin with; while introducing the project, I could notice this in body language and facial expressions, and it was sometimes verbalized. One example of this is when people addressed me by the formal pronoun usted. As I explained in chapter 5 (note 15), it was overshadowed by other considerations as soon as I said that I was a member of the movement.

In any case, the impression informants had of me did not affect very much what they said about the movement. Most of my data come from group interviews or from participation in collective situations, especially association meetings. In such contexts, my presence soon faded into the background, as the other participants concentrated on their business. I hardly ever noticed anyone glancing over at me to check my reactions; they were too busy for that. In general, movement activists were proud of what they were doing, so they had little need of justifying their actions and opinions to me. Moreover, movement values include the idea that a person should be independent, active, even rebellious. Deference is not near at hand.

One effect, however, that I am sure was produced by the fact that I am a female researcher was that people made an effort not to appear machista. In other words, the extent and depth of anti-machista convictions may have been somewhat exaggerated.

**Choices in writing**

Throughout the book, as often as possible, I have included information on what I said and did and how I think the others present interpreted me, and how that might have influenced what they said and did. This is necessary to validate ethnographic reports. In one chapter, on Linares, I have gone further and told more of a personal tale. That tale should be read in the light of what is said in other chapters and vice versa. It is impossible to present all the complex and fragmented layers of human interaction faithfully in linear writing. So, I have allowed myself to experiment a little in one

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8 See for example chapter 7, where I asked women in Valencia about resistance from husbands with the aid of examples from Linares. The use of leading questions is recommendable in other cultural contexts, too, according to Kvale (1997). As long as you take the context into consideration in the analysis, of course.
chapter. The reader is invited to read that chapter as a key to what is between the lines in other chapters.

That said, informants' words are more important than mine, of course. I report them as much as space allows. They reflect what the informants think can be said and should be said in a given context. Therefore, the context for each utterance should always be considered, and that context consists not just of the participants and the setting but also of what has just been said, and what debates are going on among these participants, in that setting and in others in which they participate.

Ideally all situations used for analysis should be described in minute detail. Since that is impossible, at least a number of situations should be carefully described. Consequently, I have selected some individuals, some situations and some events to be described as narratives, which is the only (even though never perfect) way to contextualize reasonably well. The main criterion for selection of these examples has been variability, in order to give the reader insights into the range of people and situations present in the movement. But the variability described remains within what movement members would consider "normal", i.e. not too singular and not too far from that which surprises nobody.

I wish I could report on just a fraction of the wealth of information I was given, on movement history, on local struggles, on political strategies, on complex debates and reflections on what the movement has been, is, might become.

But informants do not have a monopoly on truth anymore than the anthropologist does. So what they say must be contrasted, compared, discussed, scrutinized. An anthropologist must take responsibility for selections, angles, emphases, themes. Ideally the reasons for the many choices made in telling the stories should be told as stories in themselves. But this would take us into an infinite hall of mirrors.

To avoid getting lost and yet not hide the choices made, I have presented data in varied ways: according to place, according to themes of academic debates, according to informants' issues, according to feminist worries, and others.

This presentation of varied narratives is also a way to dress academic analyses in accessible language. It is a feminist ethical concern to avoid hierarchical relationships in research. There is no avoiding the fact that you take upon yourself to try to understand some others who are probably not very interested in you. That implies asymmetry in the meeting. But not necessarily hierarchy. To worry so much about avoiding hierarchy in research situations that you end up thinking more about informant-researcher relationships than of the social problems you set out to understand – that is indeed to fail your informants, because it will not further understanding, either in the immediate situation (which will become insincere) or in the long run (Davies 1999:41, Okely and Callaway 1992).

There is more to research than story telling or reporting. Anthropology mediates between different constructions of reality. Theory can be used as an aid in thinking about changing social circumstances and new possibilities (Calhoun 1995:9). During fieldwork people often asked what I had "found", what my "results" were. I interpreted such requests as a wish for some inspiration in their own reflective work. Movement activists were striving to go beyond the evident, and they thought an outside observer could be of help in that effort. So I have tried to take their realities, churn them through a mill of theories, send them on a merry-go-round of comparisons, then to return to them and see what we have learnt on the way.

To ban hierarchy is not the same thing as to ban difference. To intrude on informants' lives with the excuse of doing research, only to repeat what they already know, that would be to abuse them.

A feminist anthropology aims to translate the circumstances and discourses found in the field into terms other than their own, in order to create new knowledge. This is to put theory to practical use. It is not disrespectful, as has sometimes been argued in feminist, post-colonial and postmodernist debates. It would be if you say or imply that your categories can encompass theirs but not vice versa. But as long as you keep your mind open to experiments in the opposite direction, you are not placing your own thinking in a higher rank.

For a feminist it is impossible to reject all metanarratives, as an orthodox postmodernist would recommend. That would be incompatible with feminist critique and perhaps with any critical
intention (Calhoun 1995). The intellectual politics of anthropology have long been methodologically relativist, but that is a different matter (Agar 1980, Bourdieu 2003, Davies 1999, Mascia-Lees et al 1989, Thurén 1993). The effort is to put our own opinions, worldviews, perspectives and categorizations in parentheses, in order to understand what the people we study think and feel. The reasons are both ethical and instrumental: In that way we respect them and only in that way can we understand what they are doing. We want to describe the variations in human thinking and action, and to do that we have to meet them with as open a mind as we can muster. That is the purpose at a first level, where one must bracket one's own thought habits as far as possible, but other steps follow where the brackets can and must be lifted. The long-term purpose is not relativist.

In many ways, the ideals of good ethnography are absurdly impossible, full of contradictions and sky-high ambitions. But they serve to point out the direction of an ethically sensitive and intellectually fertile effort. I set my course according to the highest ideals, stumble along around the contradictions and errors and then report the trip in a spirit of humility and sincere wonder at the things learned along the way.