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<https://orcid.org/0000-0002-6194-7623> (2017) Reflecting on My Positionality as a Multilingual Researcher. In: Conteh, Jean, (ed.) Researching Education for Social Justice in Multilingual Settings. Bloomsbury, pp. 105-120

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Chisato Danjo (2017)
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In Jean Conteh (Ed.) Researching Education for Social Justice in
Multilingual Settings: Ethnographic Principles in Qualitative
Research. London: Bloomsbury

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Chapter 5

Reflecting on My Positionality as a Multilingual Researcher

Chisato Danjo

INTRODUCTION

In autumn 2011, I visited for the first time the site where I was planning to conduct ethnographic research. This was, in fact, the first time that I conducted ethnographic fieldwork; I had only recently embarked on a doctoral programme following a year of Master's studies in the United Kingdom which had interrupted my career as a primary school teacher in Japan. Keen, however, to connect the practical experience I gained as a teacher with my newfound interest in sociolinguistics, I conceived a project that would examine the multilingual practices of pre- and early-school age children born to Japanese-English intermarriage parents in the UK across two field sites: a Japanese Saturday complementary school and at home. An investigation of linguistic phenomena in a 'natural' setting, meant that whatever utterances would eventually constitute the data to be analysed, these would originate from real-life interactions – among the children, between children and teachers, children and parents, or between myself as a researcher and my participants. Sharing the broader interests of 'linguistic ethnography' in understanding social and political contexts and the nature of power relationships through the analysis of linguistic interaction (Blackledge 2011, Creese 2007, Rampton 2007, Tusting and Maybin, 2007), I was thus interested in observing and understanding both *practices* – i.e. the way multilinguals use language – and *perceptions* of language use – i.e. how they think they use language.

My fieldwork eventually lasted for 16 months, and consisted of fortnightly visits to the complementary school and monthly visits to the family homes of a number of selected 'core' participants. During the fieldwork, I collected a variety of different data from different sources: field-notes from ethnographic observations, audio-recordings of naturally occurring linguistic interactions, interviews with teachers and parents, e-mail and diary exchanges, and institutional policy documents. These data were dominated by one conspicuous presence, that of myself as a 'participating' researcher. It is this 'presence' that I wish to elucidate in this chapter by understanding my positionality and role during the fieldwork, not only from my own perspective, but also from that of my research participants.

Such concerns, of course, did not, and should not, emerge at the end of my research project. As I will show in the following, one's position as a researcher and the way this position is perceived by participants has great influence on almost all aspects of the research, and as such, active reflection on these issues must follow each stage of the project. Especially in ethnographic research, where data analysis often runs alongside data collection (Spradley, 1979), it is of utmost importance to understand how your presence as a researcher, and the participants' perceptions of you, may have influenced – or may still be influencing – the quality and

significance of your data. Before I begin to set out my own experiences and reflexions on my positionality from the very first encounter with 'the field,' I will provide a brief theoretical context to the main concepts at the centre of my current analysis.

POSITIONALITY AND REFLEXIVITY: AN OVERVIEW

Ethnography, as a social scientific method, emerged in early-twentieth-century anthropology as a preferred approach to describing and understanding societies of which little was known. It required – as it still does – that the researcher become fully immersed in the everyday life of a given community in order to be able to give an account of that community from the perspective of 'the researched.' By participating in social life, experiencing what the 'natives' did and felt, the researcher was expected to gain a holistic insider understanding of the examined people. Despite the depth of understanding conferred by research in this tradition, early ethnographers, maybe under the influence of the then prevailing positivistic and reductionist approaches to the study of social life, rarely considered critically the impact of their own participation on the communities they studied and on their findings.

A self-reflexive interest in the researcher's role emerged in the second half of the twentieth century, but often with the aim of compiling rigorous categorisations of the researcher's roles, so that s/he could clearly acknowledge their position. For example, taking into consideration the researcher's distance from participants, Gold (1958) proposed four roles that researchers could assume as observers: 1) a complete-observer; 2) an observer-as-participant; 3) a participant-as-observer, and 4) a complete-participant. Similarly, based on the researcher's involvement in activities in the field, Spradley (1980) differentiated between 1) non-participation, 2) passive participation, 3) moderate participation, 4) active participation, and 5) complete participation. While these taxonomic exercises did little to foster a better understanding of how different levels of involvement and positionalities affect the data, emerging debates around insider/outsider statuses and their respective benefits and drawbacks moved these questions centre stage (e.g. Schatzman and Strauss 1973).

Awareness of these issues became more conspicuous with the so-called 'reflexive turn' in anthropology, the discipline most closely associated with participant-observation (Clifford and Marcus 1986, Hymes 1999, Rabinow 1977, Ruby 1982). The concept of *reflexivity* facilitates researchers' continuing awareness and assessment of their own position in the social world and its impact on their research process, project design, data collection, data analysis, and the consequent findings (Finlay and Gough 2003). Importantly, *reflexivity* requires a 'critical' reflection on the researcher's situated position, involving the researcher's own social and cultural assumptions and practices underpinned in the research process.

Questions regarding the 'positionality' of the researcher can be traced back at least to post-war inquiries in sociological epistemology, which broadly asked whether 'insiders' or 'outsiders' ('natives' or 'strangers') have better access to

culturally coded information (Agassi 1969, Merton 1972, Nash, 1963). The question originated in the surge of social psychological interest in concepts like 'strangeness' or 'foreignness' and the social construction of these categories, as social scientists were trying to explain the origins of the radical ethnocentrism and xenophobia which dominated the first half of the twentieth century to devastating effects (Berger and Luckmann 1967, Schütz 1944). Stemming from this line of inquiry, perceptions of racial and ethnic distance have remained dominant categories in the analyses of researcher positionality and effectiveness (e.g. De Andrade 2000, DeVault 1995, Hawkins 2010); but the palette of social factors of possible significance has broadened considerably to include, among others, religion, language, class, profession, age, gender, motherhood and marital or relationship status (Adams 1999, Bolak 1996, Ceglowski 2000, Chen 2011, Ergun and Erdemir 2010, Freedman 1986, Naples 1996, Sherif 2001, Winchitz 2006, 2010). Together with the diversification and fragmentation of the attributes to be critically reflected upon, there is an increasing realisation that the insider/outsider dichotomy is itself much richer in complexity, with various intermediary positions.

These changes were driven not only by the postmodern paradigm shift, but also by ethnographers' intensified interest in their own (sub)cultures or societies. Despite the expectation that the latter would ultimately resolve the insider/outsider dilemma, the question of 'how native is a "native" researcher – as pertinently posed by Narayan (1993) – is still a valid one in ethnographic research. It is broadly this question that the present chapter aims to provide a subjective answer to by reflecting on my own ethnographic experience (Danjo 2015). During my fieldwork, I found myself sharing ethnic, cultural and linguistic ties with the research participants, factors which I considered essential for the success of a project investigating multilingual practices. Yet, as Narayan (1993: 680) pointed out, 'given the multiplex nature of identity, there will inevitably be certain facets of self that join us up with the people we study, other facets that emphasize our difference.' Given the nature of my research, it is particularly the role of *language* which I will focus on in my discussion of the 'multiplexity' of researcher positionality. Taking forward the idea that when reflecting on our positionality as ethnographers, instead of presuming rigid insider/outsider juxtapositions, we should view ourselves 'in terms of shifting identifications amid a field of interpenetrating communities and power relations' (Narayan, 1993: 671). I shall thus focus on the on-going and moment-to-moment negotiations of positionality as a multilingual researcher, and discuss what impact these issues have had on my data collection procedure.

NEGOTIATING POSITIONALITY BEYOND THE INSIDER/OUTSIDER DICHOTOMY

Martin, Stuart-Smith and Dhesi (1998: 110) define an insider as 'someone who identifies themselves as a member of the community and is in turn recognised as a member by the community,' who shares 'the community's culture which at a surface level manifests as, for example, skin colour, language, dress, knowledge, neighbourhood, as well as at a more fundamental level, such as consciousness,

belief and value systems.' By this definition my ethnic, linguistic, cultural, and national identities would have granted me an 'insider' position in the field. Furthermore, my experience as a primary school teacher in Japan would have been a clear advantage when negotiating access to the field, especially in the complementary school context. However, while in certain contexts these attributes did in fact take primacy, in others they were trumped by 'other facets of self' (Narayan, 1993, p. 680).

Negotiating access

On my first visit to the complementary school I was equipped not only with an initial research idea, but also with an awareness of Japanese social norms which are notorious to permeate interpersonal relations at all levels. The Japanese 'community' in the part of England where I was planning to conduct my research was rather small and enclosed, but I also enjoyed the privilege of having a friend from another area of life who was acquainted with a member of the school, who on her part could introduce me to an influential member of the community. This initial contact was crucial, as it was through him that I was then introduced to one of the chairs of the school and could make a formal appointment for an 'interview' to clarify my status and aims.

My 'insider' knowledge also helped at this 'interview.' Although the complementary school was based in the United Kingdom, the documents I was required to bring with me for the interview were a Curriculum Vitae and a personal statement in standard Japanese business format, significantly different from the ones used in the UK. We started the interview by exchanging our business cards, and the conversation took place entirely in Japanese. The interview felt rather as a test of my 'Japaneseness' than one of my 'researcher qualities,' and it required an understanding of norms of verbal and nonverbal politeness expected of formal job interviews in Japan. While for the interviewer it may have felt like the most natural encounter, for myself as the interviewee it was rather like a quaint social masquerade.

Although it seemed to me that my 'educationist' experience was making a good impression, and that the interview itself would generate trust in my research aims, the negotiation did not succeed on that occasion. The school chair told me that they would be happy to offer me a position as 'a teacher,' but not as 'a researcher.' There seemed to be a group of people who felt uncomfortable with the idea of being 'researched' and opposed my presence in that particular role. That a stranger would be observing their actions and making detailed notes on them was understandably an unsettling feeling for them. For me, on the other hand, it was a realisation that my 'insider' qualities could only take me that far, and that I was still a stranger, an outsider, in that world.

Although the first answer from the school was negative, and I was contemplating the prospect of having to radically rethink my research, subsequent email exchanges with the school let me believe that there may still be an entry point. I continued to attend the school's open public events in order to meet with members of the community face to face and start building relationships with individual families and potential gatekeepers. I had a positive progress in this process as I became close with a few families, some of whom showed an interest in

participating in my research. This slow negotiation process lasted for six months, and finally another opportunity arose when the school offered me an assistant teacher position in the nursery, and allowed me to present my research project to the parents. At the following parent meeting, I obtained permission from both the parents and the teachers to conduct my fieldwork at the school.

It is widely acknowledged by ethnographers that gaining access to a setting or community is probably one of the hardest tasks in their trade (e.g. Hammersley and Atkinson 2007). Once gained, however, access is not unconditional and final. I was aware that the school community only 'kindly' offered me permission to conduct my research at this stage, but this did not mean that I gained their firm trust. During my research, therefore, I had to be careful to minimise my interference with the normal flow of events at the school and further develop my interpersonal relationship with members of the community. The process of access negotiation has only made me aware of how much of an outsider a female student without a child is in the context of the complementary school.

Initiation as process: the parents' case

It is commonplace in the ethnographic literature that in order to obtain access to the 'insider' view on things, the researcher must undergo an 'initiation' process, whose length and terms may often remain unknown to the researcher. Once passed, however, '[y]ou have crossed, somehow, some moral or metaphysical shadow line' (Geertz 1973: 413). In Clifford Geertz's famous account, this passage occurred rather suddenly and dramatically; after ten days following his arrival with his wife in a Balinese village to conduct their fieldwork, during which the locals – with the only exception of their hosts and their close relatives – treated them as non-existent, they attended an illegal cockfight. Despite the apparent conviction of the locals that the event would pass under the radar of the official government, police arrived at the scene, and the participants fled. The anthropologists escaped with the locals, finding refuge in one fugitive's courtyard. By the following day, the villagers' attitude towards the 'strangers' changes drastically, and they become the centre of attention, a sign of acceptance earned by having participated in a shared experience and proved allegiance to the villagers rather than the policemen (Geertz 1973: 412-416).

Such experiences, of course, are heavily dependent on the social context of the field, but what should be noted is that there is considerable difference between gaining initial access and obtaining an 'initiated' status by which participants accept you as a member of the community. In Bali, being accepted meant being teased by the villagers; this – as Geertz notes (1973: 416) – 'was the turning point so far as our relationship to the community was concerned, and we were quite literally "in".' In my fieldwork, such a feeling of being 'in' arrived rather late in my fieldwork and in a much less dramatic fashion. It went, in fact, unnoticed by myself until a closer examination of my field notes towards the end of my research. By then I had somewhat accepted that not being a mother myself it would be impossible to achieve a true 'insider' status, if such status at all existed. Nevertheless, comparing excerpts from my field notes, one recorded in the first month of my fieldwork, the other after the eleventh month, shows that my position has changed considerably:

During a break at the complementary school, I was asked by mothers about my previous profession – a primary school teacher. After a chat, one mother told me in a playful tone that it would be nice if I could baby-sit their children while teaching them Japanese, so that they could have some free time to meet up with each other. (Adaptation from field note; May 2012)

Recently, a few mothers have often been asking me to join their private lunches and dinners. I feel much closer to the mothers nowadays. (Adaptation from field note; March 2013)

Comparing the first excerpt with the second, it becomes obvious how the relationship between myself and some of the mothers changed over time. In the beginning my 'ex-teacher' and 'student' status was emphasised, creating a certain distance between me and the 'mothers.' In the second excerpt, almost a year later, mothers seemed to accept me more as a peer. However, the advantages offered by certain aspects of my 'self' were evident from the very beginning. As an accredited and experienced 'teacher,' I shared the same status of respectability as any other teachers at the school (in fact, my formal accreditation was often a factor pulling me into power-knowledge structures that I actively needed to evade in order to conduct my fieldwork). Yet, as a student outsider, I could perform tasks in positions that were being negotiated not only by myself, but by my participants. Having noted down the 'playful tone' in which I was asked to baby-sit is not only a trivial addendum, but represents the linguistic tool used in the negotiation of my positionality by the participants. It was meant to provide a safe passage way between my professional and personal statuses, and the careful acknowledgement of both is evident in that the request was not only to 'baby-sit' but also to 'teach Japanese.' At the same time, the reason given for the playful request – 'so that *they* could have some free time to meet up *with each other*' – spontaneously reinforced the separation between myself and *them*. The request, on the other hand, could also be interpreted as a test, as an 'invitation to cockfight' in Geertzian terms, and one through which I could consolidate my relationship with the participants and enter the second field in which I was interested to conduct my fieldwork, that of the family home.

As we built up our relationship, mothers seemed to become more comfortable to share their time with me, and the interview data I obtained was also changing its character, becoming more emotional and personal towards the end of my research, often going beyond the scope of my original project. This also shows how certain topics that may have a significant relevance for the data only emerge after strong relationships were built between the researcher and the participant, proving the efficiency of longitudinal and immersive studies. In this section, nevertheless, I aimed to highlight how the 'initiation' process, that would ultimately grant the researcher access to such valuable emotional data, is often prolonged and unremarkable, yet clearly marked in linguistic, bodily and other forms. This 'initiation' however, still only allows entry to certain domains and

participants, and as my research involved people in very different positions and relationships to one another – mothers, teachers and children – my positionality was inevitably negotiated on a moment-to-moment basis during each encounter.

POSITIONALITY BEYOND CONTROL: PARTICIPANTS' INTERPRETATIONS

Above I discussed instances when my positionality was being 'negotiated' more or less with my active participation, and how certain aspects of the 'self' positioned me at different points on the insider-outsider continuum. In the following, I turn to examples of when my positioning takes place not only without my active involvement, but contrary to my own perceptions of my positionality as a 'multilingual' researcher. I argue that these are the processes to which we as researchers must pay careful attention, as they may go unnoticed while deeply affecting the data we collect.

Since I was interested in the language practices of multilingual children and parents across time and space, I paid careful attention to my own language use in the field. I decided not to ask my participants to use any specific language during our interactions, but that I would accommodate my language use according to theirs. While at the complementary school my observations focused on children – some of whom were bilingual children of intermarriage couples with more-or-less stable lives in England, while others were children of Japanese professional expatriates who only spent a few years on work postings outside Japan – my interests in linguistic phenomena in the family context focused more on intermarriage families.

Many intermarriage families in this study were following a One Parent One Language (OPOL) policy, which aims to help children acquire more than one language at an early age, by demanding the use of strictly one language to each parent (Park 2008). Namely, the 'Japanese' parent restricts their language use to Japanese when communicating with their children, and children are likewise required to use Japanese to that parent. This creates a similar language environment to that of the complementary school, where a Japanese-only policy is more or less enforced, but differs in that the children are required to use another language to the non-Japanese parent. Such policies, while they have their educational motivations – the usefulness of which, however, I challenged in my thesis (see details in Danjo 2015) – nevertheless created several difficulties in my research. Here I will focus only on those challenges that relate to my positionality as a researcher who shares ethnic and linguistic characteristics with my participants.

Appearance tells more than it says: the children's case

At the beginning of my fieldwork I came across an interesting phenomenon, which proved a challenge later on in my data collection. Several mothers related to me how their children tended to adapt their language use based on perceived ethnic and racial traits. Kumiko remembered how once she was conversing with a Korean friend in English in the presence of her children. Although the children

witnessed the linguistic interaction between the two adults, Kumiko's 4-year son addressed the Korean woman in Japanese, as unaware that she did not speak the language. Similarly, Emiko recalled how her children also chose to use Japanese to 'east-Asian-looking' women (Adaptation from field notes, May 2012).

These observations by Kumiko and Emiko highlight how children construct ethnic and gendered perceptions of their interlocutors and adjust their language choice according to these. The gendered nature of these mental constructions was reinforced by the One Parent One Language (OPOL) policy, since in most of the intermarriage families in my study it was the mothers who were the Japanese speakers to whom children were expected to use Japanese.

Consequently, my child-participants, almost without exception, used Japanese language to me – an East-Asian looking woman - during my fieldwork, and it was very difficult to challenge such 'routine' language practices that my participants had already developed in their daily lives. Thus, the 'insider' characteristics which were supposed to ease my data collection, proved to be at times actually working against my research aims to collect data on naturally occurring 'multilingual' practices. Chances were that what I was observing was not 'natural' behaviour at all, but one shaped by my ethnic appearance. A clear indication of this was when one of my child-participants disciplined her younger brother that he should use Japanese to me after he addressed me in English several times (field note, November, 2012).

The above examples suggest that it is essential for researchers to first understand the research participants' 'routine' language practices before attempting to predetermine their own position. I am not arguing that participants' interpretations of my positionality are definite and fixed; rather, as will see below, their interpretations are dynamic and fluid, changing throughout the fieldwork period. However, it is important to be aware that a researcher's predetermined position in the research planning process can be easily challenged if it does not comply with participants' routine practices. This also vividly highlights that certain social categories in which the researcher may find herself could have more significance than others, and it is the participants who will ultimately determine the researcher's position in different situations.

The researcher as a Japanese language resource: the parents' perception

I also observed similar attitudes and practices on the part of parent participants. During the fieldwork, it became evident how the Japanese mothers were desperately seeking Japanese resources (e.g. Japanese speaking persons and communities, Japanese media and teaching materials) in an English-speaking society. Parents actively create opportunities for their children to be exposed to Japanese language through, for instance, local Japanese communities (e.g. toddlers' groups, story-telling and reading groups, complementary schools) and regular visits to Japan. In family contexts where access to Japanese language is fairly difficult, my status of 'Japanese' and my home visits were often perceived and welcomed by parents as a Japanese language development opportunity for their children. As such, parents often expected me to serve as a Japanese linguistic resource. The mothers often told me enthusiastically that their children seemed to enjoy my visits very much, and that they tended to use Japanese more often

following my visits. Some parents even instructed their children to use Japanese to me, despite the fact that I had informed them about the nature of my research, and asked them not to do so.

It is also noteworthy that during my home visits, I was rarely provided with the opportunity to engage with the English speaking parent – mostly the fathers – and a distance was maintained between myself and them. One reason may be, as mentioned above, that my presence was regarded as a Japanese linguistic resource. Some Japanese mothers tended to arrange my visits while the English speaking father was absent, so that the language spoken in the household during my visits would be only Japanese. On the other hand, non-Japanese speaking fathers may themselves feel uncomfortable being at home during my visits, which appeared to increase Japanese language interaction among the family members, thus excluding them.

As the above examples show, the way in which both parents and children perceived my position and role had an impact on my research. Which dimensions of identification have significance on practices depends on each participant's routine practices, and therefore, it is impossible to plan comprehensively in advance. Participants have the liberty to perceive the researcher very differently from what the researcher's self-assumed position is thought to be, and sharing a number of traits with the participants often makes discerning the disjunction between the two perceptions even harder to recognise. I would argue, however, that paying attention to such issues not only helps avoid inaccurate interpretations of your data, but brings up broader questions that are increasingly the focus of ethnographic investigations. Through identifying and trying to elicit 'routine' practices, we can gain a deeper understanding of participants' values and beliefs. This argument is similar to those advocating the advantages of having an 'outsider', 'non-native' status even in linguistic research where the deepest understanding of the researched language is often preferred (Chen 2011, Winchitz 2006, 2010).

ONGOING LEARNING FROM ACTUAL INTERACTIONS

Compared with the complementary school context where I had a distinct and official role as an assistant teacher, my participants were less constrained by formal factors in their perceptions of my role during home visits. One example of this was their different ways of referring to me. Although some children usually called me *sensei* [teacher/Ms.] at home, just as they would at the complementary school, others adjusted their form of address. For instance, Naomi (aged 7), who usually addressed me as 'sensei' in the complementary school, often called me just by my first name outside the school (e.g. in a public park or at the supermarket). Depending on the addressee, context, and her intentions, she seemed to perceive me as playing a different role, and in one occasion, Naomi even introduced me as 'mum's friend' to their neighbours. The reference to me as 'mum's friend' may also imply my closeness to her Japanese mother from her perspective. Although she generally used Japanese language to me, she was more flexible on mixing Japanese and English in front of me, especially outside the home, which was another indication that she tended to see me less as a 'teacher' or enforcer of a strict language policy. Awareness of this contextual positioning was a key discovery during my research, and as consequence I tried to create further opportunities for

myself to interact with my child-participants outside the school and the family home contexts, and thus gain access to more varied data.

Some of the children in my study actively tried to understand my position in respect to language proficiency. For instance, Naomi and Tsugumi asked me directly if I understood English, in order to be able to categorise me as either a 'monolingual' or a 'multilingual' and adjust their linguistic practices accordingly. Another child, Kyoka (aged 7), asked me several months into my fieldwork if she could use English when struggling to find words in Japanese (field note, February 2013). After gaining my explicit approval, she seemed more flexible in her language choice, using both Japanese and English. Although such cases when I was directly asked for permission to use a certain language were rare, many of the children in my study tried to test whether they were allowed to use English in front of me by actually using English to me. Despite the fact that Japanese mothers who strictly followed an OPOL policy would instruct them to speak in Japanese, through such actual interactions, children gradually tested my position in that respect. As I was less inclined to impose upon them the use of any one language, children gradually reverted to a more 'natural' multilingual practice in my presence, and this rapport also meant that my positioning in their respect changed from that of a 'teacher' to one they felt closer to them.

CONCLUSION

There is an increasing number of ethnographic studies examining the way in which 'familiar' sites are researched, in which researchers share, for example, ethnic, cultural, or linguistic background with the research participants (Martin-Jones 2012). Conducting research in such conditions has many implications for the research process, and it is generally accepted as advantageous to be highly familiar with the research language, preferably at a 'native' level. However, as I have also tried to show in this chapter, it is important to remember that such closeness to the research object has an impact on the research process, and requires an even more critical self-reflection than research in 'unfamiliar' settings.

First of all, no matter how familiar you are with the people and field where you plan to conduct your research, the entry process as a researcher inevitably casts a degree of 'outsider'-ness onto you, which will then be present in the background of all your consequent interactions from which your data emerge. On one hand, the researcher becomes part of the daily practices of their participants. In my field experience, due to my ethnic, linguistic and cultural position I have undeniably contributed to the reproduction of such 'routine' practices (e.g. extensive Japanese language use during my visits) despite my intentions. But on the other hand, the researcher and the participants are constantly constructing their relationships and the positions they occupy in respect to that through actual interactions in the field, and accordingly, the quality of the data – the way they talk about themselves – as well as their practices – the way they behave – could be in continuous flux. Notably, such on-going negotiations usually would have already started at the access negotiation stage.

I am not arguing that there is therefore no need to 'plan' your own research carefully before the fieldwork; planning your research aims and intentions in advance enables the researcher to reflect on what is actually happening in the field as opposed to their initial assumptions. On the contrary, the researcher – and even more so the 'native' researcher – should actively and continually consider her position and status, and even plan in advance for ways in which such a reflexive activity would be adopted throughout the research project. An uncritical pretence of 'insider' status can do more damage to the research than the difficulties caused by being an 'outsider.' As discussed above, regardless of one's status, there usually is a process – or a clear watershed event – of *initiation*, which, especially in familiar settings, can go unnoticed or unreflected-upon. There is little a researcher can do in this respect, yet awareness of her position at different times and in different settings of the fieldwork can help her identify the sort of questions she can pursue in each. Importantly, one must not assume that superficial national, ethnic, racial or linguistic characteristics we might share with the participants will overshadow the various other situated differences that make up the 'multiplexity' of our identities.

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