

The method at the center: field notes of a psychosocial research from an ethnographic perspective

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Abstract: The article aims to discuss some effects of the use of the ethnography theoretical-methodological reference in social psychology research, especially the multi-sited ethnography. For this purpose, it takes the case of a doctoral research developed in this interface and discusses some of their issues and contretemps, as well as the choices and resources used to address them. It also seeks to present the criteria that guided the research design in general terms.

Keywords: social psychology, ethnographic method, multi-sited ethnography, methodology, fieldwork.

One forgets, however, that the means frequently transform the end; that every end has its own implicit means. One will never achieve an end with a means that are heterogeneous.

Agnes Heller

Tracing routes or *that which the field asks for*¹

For being based on the observation and prolonged experimentation of life in which it occurs (Sato & Souza, 2001), the ethnographic approach has guided an increasingly number of studies in the social sciences, broadening the path constructed in the midst of traditional methodological paradigms (Andrada, 2010). The purpose of this paper is to discuss some of the achievements in using ethnography theoretical-methodological reference in psychosocial studies, especially multi-sited ethnography, by means of the presentation of field notes of an investigation developed in this interface. We propose, therefore, to discuss the choices and resources used to deal with the central issue of the research and contretemps that have appeared during the fieldwork. However, it is not our goal to make general theoretical analyzes on the subject.

To deal with method issues, it is common to appeal to the etymology of the term which refers to the ideas

of means and choice². In general, the task is used to reiterate that there is not (or should not be) a place for absolutisms in this respect, but a permanent and sensitive ethical attention (Becker, 1999; Schmidt, 2008). In other words, adopting a research method is to trace a path among others in order to reach a certain goal. There will be no “best method,” except in relation to a particular end. Ecléa Bosi (2003, p. 55) summarized the question impeccably: “with regard to research techniques, they must be appropriate to the object: it is the gold standard. I do not know another one.”

Taken by these considerations, and based on previous experiences in the field of solidarity economy, we proposed a doctoral research with the objective of understanding the relations between work and politics in everyday life of *Justa Trama*, a self-managed network. (Andrada, 2013; Andrada & Sato, 2014)³.

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In order to discuss methodological choices, it is therefore necessary to present the objectives and the phenomenon that one wants to know. *Justa Trama* was proposed in 2005 by solidarity economy working women (Singer, 2002), the major part of them with low income, as a secondary cooperative⁴. The headquarters are located in Porto Alegre (RS), but the network congregates some six hundred people from seven different economic collectives from throughout the country; together, they comprise

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1 This article is result of studies performed during doctorate research. I had as contributors the disciplines *A dimensão cultural das práticas urbanas* (The cultural dimension of urban practices), taught by Professor José Guilherme Magnani (FFLCH-USP), and *O trabalho de campo na pesquisa qualitativa em Psicologia* (The fieldwork in qualitative research in Psychology), taught by Professors Leny Sato and Marilene Proença (IP-USP), in 2009 and 2010, respectively. Another article on the subject was published on *Ponto Urbe* electronic journal, published by the Nucleus for Urban Anthropology of the University of São Paulo (Andrada, 2010).

2 “The word *method* comes from the Greek *methodos*; composed by *meta*: by means of; and *hodos*: path, route; among other meanings” (Etymological Dictionary, 2008).

3 The research was developed in the Graduate Program in Social Psychology of the University of São Paulo, under the guidance of Leny Sato. It received Honorable Mention relating to the *Prêmio Tese Destaque USP2014* (human sciences category), and *Prêmio ABRAPSO 2013* (thesis category – doctorate), of the Brazilian Association of Social Psychology.

4 *Justa Trama* is mostly composed of women.

great part of the agroecological cotton production chain. In the arrangement of this network it is possible to find workers, men and women, from rural and urban environments, from various sectors of the economy, and activities such as agriculture, industry and handicraft. The set of collectives manufactures inputs – yarns, fabrics and props – that result in agroecological cotton clothing, bags and toys (Andrada & Sato, 2014)⁵.

First route: from psychology to anthropology

After defining research field and objective, we turned to anthropology to study in depth the ethnographic method, which had already been an important guide to investigate everyday life processes, the case of this research (Andrada, 2009)⁶. The echoes of what was experienced there – readings and debates – were so many that sometimes we marveled our territory (social psychology) and felt at home on foreign soil (anthropology). Later, however, there was increased need to return and find, in the lines of the psychosocial text, the bridges that led us to meet those aspects that most fascinated on anthropological lands. In this drive, reading Mary Jane Spink (2007) and Peter Spink (2008) was comforting, since they recovered, from the memory of social psychology, native experiences of observation in everyday life.

It is worth highlighting, however, what is precious and was found in anthropology. The wide and spontaneous possibility, usually conferred by the discipline, for appreciating the difference *as such*, for not imposing framing requirements to something which is not its own, had a great charm. It is a classic contribution from the area to the other social sciences. If, on the one hand, there are plurality and dissension within anthropology, on the other hand there is an invitation to encounter the difference without extracting it from its original place, without violating it with issues which are not related to it, and which end up lacerating it because they do not fit.

We have also learned from our anthropological colleagues that a reality is cultural because it was constructed and, therefore, it keeps on its soil the springs of its meanings, made and remade by its inhabitants when dealing with each other and with this soil throughout time. They teach that to reach these meanings, we must practice introspection – a retreat that places us somewhere between our place and that of the other – and remain there, suspended between two cultures (DaMatta, 1978). Stay there for the time necessary for the experience, dedicating

ourselves to it *from near and within* (Magnani, 2009), and then return to our starting point and establish the dialogues between “there and here” – between Geertz’s *experience-near* and *experience-distant* (1999).

In fact, this was an ethical-methodological concern that largely justified the choice of the ethnographic method (besides adjustment to the object): we wanted to avoid addressing *Justa Trama* working women by asking them questions which would seem to be artificial, in the name of the research. In a simple way, we would like that the experience allowed a respectful encounter between different people – which we are – and the construction of a shared meaning (Schmidt, 2008).

In a previous research performed with the seamstresses of the *Cooperativa Univens* (Cooperative) (Andrada, 2009), we noticed that the openness provided by the ethnographic method allowed the expansion of experiences possibilities in order to contemplate much of the idiosyncrasies and interests of both sides. And then the issues of method have gained centrality for us. In other words, the way we mean the fieldwork in the affluence of psychosocial and ethnographic perspectives: as a result of interpersonal relations and everyday choices in the course of these relationships, between researchers and people in the situation studied (Sato & Souza, 2001; Schmidt, 2008).

Certainly, it is not an easy or fluid process. In these studies, it was often necessary to slow down, stop, or even go back to re-talk about search terms. Negotiations, re-contracts, misunderstandings, nonunderstandings. All conjugated in the plural, as it is characteristic of ethnographic and participant studies (Becker, 1999; Sato & Souza, 2001). It is worth mentioning that this intense negotiation in the field was not only due to the method, but also to an intrinsic characteristic of the experience studied: the requirement of autonomy and political participation of its subjects, working women with appreciation for self-management in all relationships, including researchers. Also because of that, if we had approached them taking a set of questionnaires, the research outcome would have been another, probably more opaque (also a way of referring to object-method adequacy)⁷.

It is also known that the ethnographic method is characterized by being more demanding with the researcher (Sato & Souza, 2001), who will certainly take longer in the field when compared with other methods. He will feel lost, displaced, thrown into the unprecedented everyday life of the place – and worse, without knowing anything about its codes and lexicons. It is not for nothing that mundane issues gain immense importance in the early days in the field: what to eat or wear, where to store belongings, etc. (Sato & Souza, 2001). As Roberto DaMatta (1978, p. 24) observes:

5 For more information, please refer to: <http://www.justatrama.com.br>. We also strongly recommend the reading of the book on the group’s struggle history, written by one of its protagonists: Nelsa Nespolo (2014).

6 The mentioned studies also had the guidance of Professor Leny Sato, who has long studied the ethnographic approach in interface with the labor social psychology (Sato & Souza, 2001).

7 It will not be possible to approach in depth the passage rite and the negotiation process of the research in this article. We suggest the reading of the section dedicated to these themes in the thesis resulting from the research (Andrada, 2013, p. 58).

the oscillation of the existence pendulum for such questions – where I am going to sleep, eat, live – is not pleasant at all. Especially when our training tends to be excessively verbal and theoretical, or when we are socialized in a culture that systematically teaches conformism, this child of authority and generality, law and rule.

He will also have to dispose of any vestige of authority – no matter who the researcher is, what academic title he holds –, arriving in a foreign land with no surname or modifiers, except that of foreigner. And if he achieves some recognition there, it will certainly have been established due to the relationship with the people of the place, who know nothing of him beforehand, and who, at best, had a human-generic complacency about him, such as Cláudia Fonseca (1999, pp. 64-65) declares:

In this situation, the researcher, an intruder more or less tolerated in the group, no longer feeds the illusion of being “in control of the situation.” It is precisely here, when his malaise, his incompetence in local languages oblige him to recognize social dynamics that does not dominate well, that the anthropologist feels that he is arriving somewhere. . . . Then our “natives” finally begin to feel at home in our presence, they mock us or even ignore us, we go beyond the dialogues “just for the sake of appearances.” Nobody denies we are part of the reality which we research.

At the frontier: encounters and tensions between the focuses

We do not intend to explore in depth the relations between psychosocial and ethnographic perspectives; we have done so in previous works (Andrada, 2010). We want to highlight just a few aspects relevant to this discussion. The first of these is the plurality of divergences within each of these *territories*. In other words, there are many social psychologies (Farr, 1998) and many anthropology fields (Oliveira, 2000); therefore, divergent ways of operating the subjects of method in each of them. Our interpretation focuses on the encounter between specific readings from both areas and it is, in that sense, limited.

The social psychology from which we start brings light on the tension of identity that constitutes the discipline, and remains there: the intangible individual-society bridge, or the inseparable plots that interlace and dialectically constitute *people* and *world* (Andrada, 2010). To present it briefly and accurately, we use the beautiful Arakcy Martins Rodrigues’ (1978, p. 15) text:

If, for some periods, I tended to an intra-subjective explanation of man; if, in others, the weight of the perception of social determinations practically led me to give up psychology, as it has happened

with numerous social psychology scholars, I have sought here a balance in the search for an interactive explanation between man and the historically given social processes. . . . I also know that I occupy a place of frontier, claimed by various disciplines.

Gonçalves Filho (1998, p. 11) also uses the notion of frontier to situate the psychosocial approach:

Social psychology, this frontier discipline, is characterized not by the consideration of the individual or the focus of subjectivity on the separate man, but by the demand to find man in the city, man in the midst of men, subjectivity as a singular, vertical apparition, in the intersubjective and horizontal field of experiences. . . . The themes of social psychology fall precisely upon intermediate problems, which are difficult to consider only on the side of the individual or only on the side of society.

To illustrate the closeness between the strands of the social psychology and anthropology discussed here, let us see a definition of the ethnographic method derived from anthropology itself. In a text intended to discuss problematic approaches of other areas with this focus, Cláudia Fonseca (1999, p. 59) defines it: “The ethnographic method is seen as the tense encounter between methodological individualism (which tends towards the sacralization of the individual) and the sociological perspective (which tends towards the reification of the social”.

Here we speak of a kind of *folding*, or better saying, an *overlapping of two frontiers*: that which constitutes the social psychology itself, recognized as the frontier area in itself, and other resulting from its encounter specifically with anthropology. In other words, if there is something we can demarcate about this intersection, it is its frontier identity. For Boaventura de Sousa Santos, frontiers can be used to explain that which separates but also unifies: it is based both on limits and on their transgression (Santos, 2002).

And while it is not our goal, we must recognize the multiple tensions and differences between disciplines, which are neighbors, but which do not confuse with each other. It is necessary to distinguish the specificities between the psychosocial and ethnographic approaches and the limits of the approximation between them, without, however, incurring in patrimonialism positions that limit the circulation of knowledge and constrain the primacy of the object. However, it is well known that there are more exchanges between frontier areas, as well as the periodic need for redrawing the lines that shape the territories and give them identity.

For example, much of the problems identified by scholars in the approximation of these fields come from the risks attributed to interdisciplinary practices in general. Dangerous risks, as it will be verified. One of

them is the instrumental use of the ethnographic method in psychological studies without the care to properly place the subjects in their contexts, as pointed out by Fonseca (1999, p. 62):

We summarize the problem as follows: because of the central value of the individual in our society. . . there is, among our students, a strong tendency to isolate the individual from his social group. "Field research" is reduced to quasi-therapeutic interviews between only two people. There are scientific branches (of psychology and cognitive sciences) that provide guidelines for the analysis of such situation. . . . But in the current iconoclastic climate, these approaches are rejected by most of the old adepts for something more "open": the ethnographic method. The problem is that ethnography is not so open, since it is part of the social sciences and requires the social (political and historical) framework of human behavior.

The demand attributed to ethnography by Fonseca (1999), whatever it is, to interpret human behavior according to the contexts of its political and historical contexts, is also a premise of the social psychology that we practice. Basal works of Brazilian social psychologists, such as Arakcy Martins Rodrigues (1978) and Sylvia Leser de Mello (1988) are examples of this.

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In previous work (Andrada, 2010), we discussed *points of contact* between psychosocial and ethnographic approaches by examining two aspects: a) the peculiar displacement that both approaches require of the researchers towards the Other; b) the consideration of the research as "process of coexistence between people" (Sato & Souza, 2001), beyond the subject-object position.

Here we emphasize a particular point concerning the latter aspect. By postulating everyday life as "everything we have," Peter Spink (2008, p. 70) recaptures "the notion of social psychology as a social practice, of conversation and debate, of horizontal insertion of the researcher in everyday encounters" located in places or *micro-places*. The anthropologist Vagner da Silva (2006, p. 184, emphasis added) points in the same direction:

Thus, one of the possible answers to Malinowski's question about "what then is the ethnographer's magic with which he can evoke the natives' true spirit in an 'authentic view' of tribal life?" is that it consists in a "self-illusion" that this is fully possible, since there is no, we perceive more and more, natives' "true" spirit or an "authentic vision" of groups' life. Both are constructions made from the coexistence between people who observe and interpret each other, and there are no "final words"

other than those that are pronounced continuously in the uninterrupted flow of cultural dialogue.

In this sense, and from social psychology, Sato & Souza (2001) emphasize that not only techniques and methodological rigors will be the grounds of a good research, but the quality and ethics of relationships established in the field between researcher and people of the situation researched. Thus, in order for the encounter between different subjects, in the context of a research, to effectively produce knowledge and meanings shared in a non-violent or fetishized way (which would only affirm places and relations of power), care must be taken not only for dialogues and proximities but also for the silences and retreats, in intersubjective terms.

Multiple displacements of research: field notes and reflections

At this stage, or rather, in this dimension of research, I no longer find myself talking to *paper Indians* or symmetrical diagrams, but with people. . . . I am, therefore, submerged in a world that was placed, and after the research returns to be placed, between reality and the book. . . . It is experiencing this phase that I realize (not without being scared) that I am between two fires: my culture and another, my world and another. (DaMatta, 1978, p. 25, emphasis added)

In the research project that we took as the case in this text, the following proposal, objectively fulfilled, was read:

a case study based on broad ethnographic experience, composed of three stages: 1) first contacts with working women and the presentation of the proposal; 2) field trips to collectives belonging to the cooperative network, located in 5 (five) states of the country; and 3) return to some of them for extended semi-structured interviews with working women chosen from the visits.

But if they were not *paper Indians*, they still looked like a diagram – a freehand drawing, an intention drawn under the silence of the desk. It was necessary to leave, to hit the road that would lead to the working women of this collective that spreads across the country. And when finding them, as Ecléa Bosi (2003) says, we could start a common adventure. Because this group, which strives for self-management, would certainly be a co-author – not of the project, but of the research experience.

Between the planned and the experienced: field demands

The first moments lived with the working women inspired two great changes in the proposal of the research:

adjustments in the focus of attention and in the field travel route⁸. Studying in depth the everyday life of a network organized by a collective of more than six hundred working women spread throughout the country required a continuous effort of focus and synthesis before so many people, groups, contexts, diversities. The complexity of the object of this case increased interest and required, in a way, more work on the method subjects.

Regarding the focus of attention, it was necessary to translate the research objectives into everyday phenomena to be pursued in the field. This because searching for *relationships between work and politics* could be a formally distinct theoretical postulate, but it has showed to be tough and impalpable in the face of the plasticity of everyday life with the working women. We then tried to understand in the field how they reconcile the demands of work and management with those related to politics, what stimuli and obstacles they find in these processes and what resources they have developed to operate with them in such adverse conditions. We also wanted to know more about the historical process of network organization. Thus, the interviews were worth more than the ethnographic observations (Andrada & Sato, 2014).

It was also necessary to review the travel route in the middle of work. The initial idea was to follow “the cotton trail,” a common procedure among researchers of productive chains. In this case, the route is predefined by the production process: first the trip is to the collectives responsible for planting the cotton, then to the factory that spins and weaves, etc. But after the first trips, this path seemed an abstraction that was inconsistent with the object – the everyday political processes of the network. Instead of visiting the groups when separated, it made more sense to follow the encounters between them – the network political agenda –, which sometimes took us away from the formal spaces of the collectives on missions across the country and abroad (Andrada & Sato, 2014).

The main activities have been performed through 11 incursions in ethnographic immersion regime and six prolonged interviews. They are experiences of intense social interaction with network working women, in some two years and a half of fieldwork.

Each incursion required previous and subsequent work to its performance. A series of contacts, readings and surveys, particular to each locality and group visited, was a requirement for the field experiences to develop well, triggering the process of displacement even before departure. When returning, it was necessary to unroll the threads of the experience, with the meanings and knowledge proper to those places and people: re-reading of records made in small notebooks, transposing them

into an enlarged version to the field journal⁹. Thus, we grouped them into pre-categories and, finally, into *categories of meaning*, from repeated readings. This process (writing, reading, rewriting and re-readings) was one way of *decanting the field*. More than a generalizing procedural technique, this decantation refers to a process of progressive abstraction of the experience meaning towards the spoken, and of the subsequent extraction of the fundamental inputs from the produced narrative, those more related to the object of the research. In short, we sought to trace or evidence, between one trip and another, the next directions to follow in the research from the interpretation of the experience¹⁰.

This work reminded us of Peter Spink’s notes (2003, p. 38) on science as a narrative modality:

Much of what we call science, especially social science and social psychology, is the re-textualization of the other; the re-narrating of the already narrated. Academic re-narrating is a way of narrating the oral narrative conversation, visit, material, materiality, lost and found¹¹.

Among conversations and interviews

During the research fieldwork, the need for talking with some working women more closely has to be emphasized in the contexts of ethnographic experience and from the experience lived in it. That is, the interviews served as an auxiliary resource to resolve doubts and raise issues that had not been sufficiently elucidated. In particular, we wanted to gather from them aspects about the history of the network, issues that appear in everyday life as a *background* rather than a *figure*, therefore little thematized in the present.

More than interviews, they should be called *recorded conversations*. In some cases, they represented the continuity and deepening of reflections that we had with a certain working woman. In others, the proposal of a reserved interlocution, with a previous organization of subjects (although flexible), expanded, until to this point, a timid channel of communication, allowing a more accurate and sensitive contact with some people. However, in these cases, we also used the experience related to these people, and the experiences after the interviews have reciprocally benefited from the most detained contact, so that the separation between these conversations and the other experiences of the field does not seem adequate.

8 On the theme, we suggest the reading of the thesis item “O campo exige revisões” (Andrada, 2013, p. 106).

9 Choosing small notebooks for the field notes revealed to be important. It was necessary an instrument easily handled, which would allow fast records at several places (buses, vans, at a bar room or on the street, on the researcher lap). We suggest Magnani’s article (1997) on the theme.

10 About the research gross results: each day of incursion resulted in an average of eight pages of journal reporting. The six interviews (five individual, one collective) totaled about nine hours of recording.

11 Peter Spink is known as a good reader of Charles Taylor. On the theme, we suggest the reading of his *Compréhension et ethnocentrisme*.

The choice of the people to be interviewed followed criteria of representativeness, to guarantee the listening of working women of several collectives, founding partners and recent members, members of the network direction, but not only them¹². We also chose people with whom we had more meaningful contact during the incursions. We elaborated a general, lean and open script, based on the objectives of the research and ethnographic observations. Also, we elaborated some questions thinking about each interviewee, and, to do this, we selected passages from the field diary that were about the interviewee's experiences on aspects of which we would like to know more. In this way, they would come already informed on experiences shared with us.

Something remarkable in the interviews or conversations was the fact that we rarely used the script. That is, because of the time we had lived together, the interviewees knew more about what we wanted to know. Without exaggeration, sometimes they seemed to better dominate the research questions: "*you are going to understand later*" Idalina¹³ said once. Even so, knowing the questions does not mean that we dominate the answers. For that purpose, it is necessary the encounter and the dialogue between different beings, in addition to the work of interpretation of the experience, socially constructed, already mentioned.

Regarding the interviews, it is also worth mentioning their process of revision, which we have done together with each interviewee. The participants were invited for the activity with the promise of later receiving a hard copy of the transcribed conversations (what was done). Nevertheless, the main point was the will of revising the interview final text together with every interviewee, from their readings and impressions, something that we have already done in previous studies (Andrada, 2013). This process occurred more in some cases than in others and it was very significant for the research results. Remarkable memory of this stage was to receive the hand-delivered revision suggestions of Tauá's farmers, present in the chatting circles recorded in the field¹⁴.

The trips: displacements and unusual encounters

Motorcycle enduro in the *sertão* (arid region in Ceará state countryside) of Ceará; hot chicken soup and cold bath in the Josephine Sisters Center of Retreat and Reception of Fortaleza; taking the van up to Quixadá; and then the stop-over for the community *cajuína*

(kind of wine made of cashew). In Pará de Minas, we took a fashion class at the syndicate headquarters, ate *pamonha paulista* (paste made from sweet corn, from São Paulo) and Uruguayan lamb, but we were served with hospitality from Minas Gerais – and one of the best. We stayed in a beautiful place belonging to relatives of one member of the cooperative, with right to *piranha* (fish) soufflé and taster of native *cachaça* (distilled spirit made from sugar cane juice). A room for two people was made available for eight in Porto Velho, including me: "Just separate the box-beds and two of us can sleep on each!" – one of the working women said. In the improvised bath, the track pants turned into a towel. The beauty of the Amazon is breathtaking and the unaware researcher takes a burn of *cipó-fogo* (kind of caterpillar) as a souvenir. Just a small mark among the many left by field trips, as rich as diverse, such as the collective of *Justa Trama*.

The intention of the previous paragraph was photographic. Faced with the task of presenting field trips and the impossibility of describing the richness and difference degree between them, we seek a way of condensing them through textual portrayals¹⁵. As Roberto DaMatta (1978, pp. 26-27), we wanted to bring to light the "other side" of the field research:

the aspects that appear in the anecdotes and meetings of anthropology [or psychology], in cocktails and in less formal moments. In the stories that, as a tragicomedy, elaborate a misunderstanding between the researcher and his best informant, how hard it was to get to the village, the diarrhea, the difficulties of getting food and – much more important – how difficult it was to get food in that village of Central region of Brazil. These are the so-called romantic aspects of the discipline, when the researcher finds himself obliged to act as a doctor, cook, storyteller, mediator between Indigenous people and FUNAI staff, a solitary traveler and even a clown, using these various unsuspected papers for good performance of the routines he infallibly learned in the university.

It is possible that reading these reports causes strangeness due to the colloquial tone and the exposition of subjective aspects of the relationships in the field. If we do not believe in the neutrality of human actions – since there are always intentions and interests which motivate them – when research is discussed, we consider it a duty to reveal not only objectives and methods, but also the political-philosophical conceptions that guided the researcher in the field. In a way, they guide the interpretation of the study results (Becker, 1999; Heller, 2008). Spink (2003,

12 We asked the working women whether they would like to be identified in this text and, if so, how. All people have chosen to be identified by their names.

13 *In memoriam*.

14 This process has also occurred regarding the thesis. Working women from various groups have contributed with important reflections to the text, which we sought to incorporate as far as possible, with due reference.

15 We used photographs in the thesis as a resource to the challenge of presenting *Justa Trama*, inspired by the work of Leny Sato (2009).

p. 37) provides further arguments: “places, events, people, faces, artifacts, documents, impressions, excerpts, notes, memoirs, photos and sounds in pieces and pieces (many pieces). . . . Only he who is unaware may think that this is a neutral activity.”

As Roberto DaMatta (1978, p. 27) observes:

there is no doubt that any set of anecdotes related to the field research is a little imaginative way of placing on the dark side of the work its perhaps most important and significant points. . . . a very ashamed way of not assuming the human and phenomenological side of the discipline, with a childish fear of revealing how much is subjective in the field research, and the more focused the ethnologist is towards an idealization of rigor in the social disciplines, the greater the fear.

The field trip to Montevideo had a special meaning. It extended and made the meanings of research *field* and *theme* closer, as Spink (2008) discusses. It meant to observe the phenomenon-focus of the research away from the group spaces and the streets of my city. There, the native was the researcher¹⁶. With great satisfaction (and some astonishment) I found myself introducing the city, the codes and the rites of the place to the visitors, who gave me the “translator” credential.

Another field trip took us to Spain. We have been there for a week, in the house of a lovely long militancy couple. And together with fellows of the solidarity economy of Bolivia and Nicaragua, all foreigners in that field, we have gone to the spaces of work, education and struggle of Catalonia. In the end, the days would look like weeks and the world, smaller. Not for the long journey, but for recognizing the same phenomena so far from home, that is, the reflections of a same humanity that resists. The economic crisis approached the new-south to the old-north, which glimpsed a common direction: when the crisis is economic, the exit can only be political “y desde abajo” (and from below).

Finally, where it all began: I have been in Porto Alegre, headquarters of *Justa Trama* and the *Cooperativa Unívens* (collective founder of the network and scenario of the previous research), three times during the fieldwork. Although I recognized those airs, the incursions gained the prospect of being there, now in company of the working women of other collectives. Now the group was not only bigger but it was also different: farmers from Ceará and Mato Grosso, artisans from near (Porto Alegre), and far away (Porto Velho), and the colleagues from Minas Gerais and Santa Catarina, all joined in network to the seamstress of Rio Grande do Sul.

16 I was born in Montevideo, Uruguay. The thesis brings more information on the field trips (Andrada, 2013).

So many different incursions bring similarity: if the researcher moved away from her territory, *Justa Trama* working women did the same. In each trip (except the first one), other members of the network had traveled to get together there. Next to my suitcase, there were theirs. They also missed home and felt curious about the place. In addition to sharing medicines and toothpaste, we have exchanged impressions as travelers we were. It was common, therefore, the native of one trip to be the stranger in another.

Another curious phenomenon related to research: for as long as I was on the field, it was common to arrive and realize that I knew only part of the people who were there, and the other part of the group was new to me. This led to surplus demands: to re-introduce myself and the research, which created the uncomfortable feeling of always being “going to the field,” even after a year of work.

And sometimes I thought we were all on the field; or that the women carried the field with them; that everything was a great field, or great fields, in the plural. What we want to highlight is the idea that the *field, researcher and people of the researched situation seemed outside their traditional places, in permanent displacements*. This impression adhered to the reflections on the experiences as something very significant¹⁷.

When the field is plural and itinerant: observation on multi-sited ethnography

As we have seen, the collectives of *Justa Trama* are not united in a single continuous territory. This fact, together with the adoption of ethnography as a methodological reference, has given us a challenge: how to do ethnography when the field is plural and itinerant? In this process, the encounter with the proposal of the multi-sited ethnography¹⁸, divulged by George Marcus (2001, 2004), was an important finding¹⁹.

Traditionally, ethnographic studies are based on intense and prolonged immersion in a single locality, from the classical Malinowskian *mise-en-scène*. At the

17 The results of the research were organized in Cases on everyday life (based on ethnographic experience) and Narratives on the history of the network (supported by interviews). The everyday life philosophy of Agnes Heller (2008) was the theoretical reference of the study. The network proved to be a dialectic organization of work and political organization of resistance. It was also concluded that politics, in the routine of self-management of the network, can be understood as work. And the work can be taken as the object of political practice of the working women (Andrada & Sato, 2014).

18 We owe the reference to the multi-sited ethnography and its relation with the research to Prof. Magnani (FFLCH/USP).

19 Exponents of critical or postmodern anthropology, Clifford and Marcus (1986) wrote *Writing culture: the poetics and politics of ethnography*, iconic work of the movement. Directed to traditional aspects of ethnographic practice, as the naturalist ways of representing the Other and the indubitable authority of the ethnographer, the critiques proposed founded non-Eurocentric ways of otherness, based on premises as *reflexivity* and *complicity* (Marcus, 1995).

same time, according to the traditional perspective, the ethnographer uses other methods and techniques to establish relationships between the situation researched and its contexts, such as the consultation of documentary archives and works of “macro theoreticians” (Marcus, 2001).

There are other less usual ways of conducting ethnographic studies. Multi-sited ethnography, for example, was formally proposed by Marcus (2001) in the 1990s, but it has been practiced by several social science scholars for decades, such as Pierre Bourdieu (Wacquant, 2006)²⁰. Summarizing, it postulates the exit of the researcher from a given local situation to follow *objects* located in diffuse time-space (Marcus, 2001, p. 111): “This kind of research defines an object of study for itself that cannot be approached ethnographically if one remains focused on a single intensely researched locality.”

According to Teresa Fradique (2003), the model allows displacing from a paradigm based on a single place of investigation to the examination of territories with multiple anchorages, with the aim at finding the expressions of certain objects of study. This mobile ethnography has gained visibility in the last decades because it represents a powerful resource in the face of contemporary empirical challenges, typical of a more globalized and fragmented world. Certainly, the new forms of occupation of space and time in advanced modernity have provoked the re-reading of traditional anthropological objects, such as territoriality, diversity and cultural identity (Fradique, 2003; Sciré, 2009). For many ethnographers of critical anthropology, these phenomena required reinventing traditional ways of doing field research (Marcus, 2001).

For sure, multi-sited ethnographic studies have also been developed from the classical concepts of anthropology, as in the Marxist strands of the discipline linked to economics and history. But they became more frequent in the 1980s and 1990s in multidisciplinary objects projects, such as those which discuss artistic expressions in urban contexts, the media and the migration processes (Marcus, 2004).

However, according to the same author, the challenges posed to the imaginary of traditional ethnographic research also relate to what he called “self-esteem of anthropology”:

The fundamental problem here is to confront the policy of knowledge, which any object of field research involves, and the attempt of the ethnographer

to position himself in relation to this policy, making the place itself part of the investigation plan of the field research. Thus, since the 1980s, any critical anthropology deserving the name not only tries to speak the truth to power – power as conceptualized and theorized; truth as subaltern and understood within the everyday life of closely observed ordinary people – but also tries to understand power and its agencies, in the same ethnographically committed terms and in the same field research frontiers in which the subordinate is included. . . . this is what counts the most in the struggle to make contemporary field research more multi-sited and political. (Marcus, 2004, p. 150-151)

According to Marcus (2001) and Sciré (2009), the researcher interested in understanding local social and cultural changes related to other spheres should dedicate himself to the use of strategies that allow him to surpass the place and frontiers and promote connections on different ethnographic scales. For this purpose, Marcus (2001) defines at least six techniques, understood as practices built through movement and screening in different scenarios of a complex cultural phenomenon: *follow the people; follow the objects; follow the metaphor; follow the plot, story or allegory; follow life or biography; and follow the conflict.*

Nowadays, the proposal of multi-sited ethnography may generate anxiety among ethnographers: concerns about the limits of ethnography and the apparent reduction of the power of fieldwork, according to Marcus (2004). He argues that in most ethnographic studies many localities are already crossed in fieldwork, and therefore field and fieldwork conceptions should potentially presuppose a greater freedom of movement for the researcher through the space of his experience: “the field research norms need to be freed from the emphatic and vigorous *being there* of the classical imaginary” (Marcus, 2004, p. 149).

From social psychology, Peter Spink (2003, p. 28) also discusses the concept of field as a “complex of networks of interconnected meanings.” In this sense, the author affirms that it is a created space, although involuntarily, since the researcher will have to debate it and negotiate it to insert himself into its “webs of action”: “Field therefore is the argument into which we are inserted; argument that has multiple faces and materialities, which occur in many different places” (Spink 2003, p. 28).

Thus, even if we consider inspiring the contributions of multi-sited ethnography, we understand that it is not necessary to be under its methodological framework to be oriented with freedom and sagacity in the field and then follow our objects’ traces of expressions. Leny Sato’s psychosocial study (2007, p. 98) on street market is a fine example:

Realizing the network of social relationships that configures the street market has demanded to

20 Loïc Wacquant (2006, p.21) recalls the first studies of Pierre Bourdieu, developed concomitantly in Kabylie (colonial Algeria) and in his native village, Béarn (southwest of France), and states: “. . . Bourdieu may be regarded as a unique precursor of multi-sited ethnography decades before it was identified as a distinct methodological genre.” The author also indicates differences between the conceptions of contemporary multi-sited ethnography and that used by Bourdieu in the 1960s.

follow the threads and identify the “nubs” that weave their organization. . . . In this network, the flows of symbolic interaction are not guided by a center from which information, subjects of conversations and decisions emanate. The “threads” that connect people to each other extend in different directions. The network structure of social relationships and meanings could be later apprehended (Mayer, 1966/1987). The network protects and is built by the presence of several logics that are in one place, on a day of the week, and spread to several other street markets on other days and in other places.

Final considerations

We propose to discuss here some effects of the use of the theoretical-methodological reference of ethnography on psychosocial research, taking the case of a field research developed in this interface. Through the exposition of the everyday practice of research and its impasses, so determinant for its results and identity, we hope to have contributed to the defense of

the centrality of method issues when it comes to making and discussing research, especially the so-called *field research*.

In fact, in order to remain close to the object – the political phenomena of *Justa Trama*’s everyday self-management – I had to move through different material and symbolic sites, not only by the various network enterprises, geographically distant from each other. It was also necessary to move through an expanded field configured by the circuits of working women’s actions and interests, who go to meet their national and international political partners in several spaces and forums of solidarity economy. In this sense, the ethical quality of relationships established in the field has proved to be a fundamental condition for ensuring the smooth performance of the experiences, both in the research itself and in the experience, in general. Thus, we understand that the dialogue with anthropology, especially the perspective of multi-sited ethnography, has brought important contributions to the development of research in social psychology that we have tried to present in part.

O método no centro: relatos de campo de uma pesquisa psicossocial de perspectiva etnográfica

Resumo: O artigo tem por objetivo debater alguns efeitos do uso do referencial teórico-metodológico da etnografia em pesquisas de psicologia social, especialmente a etnografia multissituada. Para isso, toma o caso de uma pesquisa de doutorado desenvolvida nessa interface e discute parte de seus problemas e imprevistos, bem como as escolhas e recursos utilizados para enfrentá-los. Também procura apresentar os critérios que orientaram o desenho da pesquisa de modo geral.

Palavras-chave: psicologia social, método etnográfico, etnografia multissituada, metodologia, trabalho de campo.

La méthode dans le centre: les rapports sur le terrain d’une recherche psychosociale de perspective ethnographique

Résumé: L’article vise à discuter des effets de l’utilisation du référentielle théorique-méthodologique de l’ethnographie dans la recherche sur la psychologie sociale, en particulier l’ethnographie multi-située. Pour ce faire, on prend le cas d’une recherche doctorale développé dans cette interface et on discute une partie de leurs problèmes et inattendue, aussi que les choix et les ressources utilisées pour y remédier. Elle vise également à présenter les critères qui ont guidé le dessin de la recherche d’un mode générale.

Mots-clés: psychologie sociale, méthode ethnographique, ethnographie multi-située, méthodologie, travail sur le terrain.

El método en el centro: relatos de campo de una investigación psicossocial de perspectiva etnográfica

Resumen: El artículo tiene por objetivo debatir algunos efectos del uso del referencial teórico-metodológico de la etnografía en investigaciones de psicología social, en especial, la etnografía multilocal. Para ello, toma el caso de una investigación de doctorado desarrollada en dicha interfaz y discute parte de sus problemas e imprevistos, como también las opciones y recursos utilizados para enfrentarlos. También busca presentar los criterios que orientaron el diseño de la investigación de manera general.

Palabras clave: psicología social, método etnográfico, etnografía multilocal, metodología, trabajo de campo.

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