

DEALING WITH “LIVED EXPERIENCE” BENEFITS AND LIMITATIONS

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Summary: In this paper we discuss various experiences we have made during fieldwork. Firstly, we will have a closer look at the categories “field” and “place” in general. As the field in our case is derived from a policy approach on the European Union level – a comprehensive political strategy addressed to the neighbouring countries - and as the fieldwork is additionally embedded in a research project drawing on different disciplinary approaches - we clarify in how far the classical notions of “field” and “place” can be applied in our case. After having reconstructed how we can frame sites of our research, we will assume that the process of building rapport between researchers and researched significantly determines the research process and we will reflect on questions of gaining access to the field. Therefore, the analysis will focus on questions of accessibility to the research field and the relationship between the researcher and those being researched, especially focusing on the role that “lived experiences” (BERRY 2011) on both sides may play. Furthermore we analyse our strategic approach working in binational teams to decrease the initial gap between local insiders and us as outsiders and on the other hand deal with a moment of limited access to the field.

Zusammenfassung: Dieser Artikel befasst sich mit verschiedenen Aspekten der Feldforschung. Zum einen wird grundsätzlich die Kategorie des „Feldes“ näher betrachtet, zum anderen stehen die Fragen des Feldzugangs und des Verhältnisses zwischen Forschern und Beforschten im Mittelpunkt der Analyse. Da das Feld in unserem Fall aus der Europäischen Nachbarschaftspolitik, einem umfassenden politischen Programm für die Nachbarländer der EU, abgeleitet ist und unsere Untersuchungen außerdem einem interdisziplinären Projekt zugeordnet sind, reflektieren wir zu Beginn, wie in diesem Fall die für Ethnographie und Geographie klassischen Begriffe „field“ und „place“ verwendet werden können. Anschließend werden wir uns ausgehend von der These, dass die Art des während der Feldforschung entstehenden Verhältnisses von Forschenden zu Beforschten und ihre jeweiligen „lived experiences“ (BERRY 2011) von entscheidender Bedeutung für den Forschungsprozess sind, näher mit Erfahrungen bezüglich des Feldzuganges beschäftigen. Hierbei reflektieren wir einerseits den Versuch, diesen Zugang aktiv mittels binationaler Teams zu beeinflussen, während andererseits ein Moment analysiert wird, in dem der Zugang zunächst nur begrenzt gelang.

Keywords: Social research, Commonwealth of Independent States, fieldwork, qualitative interview, binational team

1 Introduction

“...recognize[s] that you can't really say it all; all analyses, no matter how totalistic their rhetorics, are partial.”
(MARCUS 1998, 37)

“That ethnographers personally have a hand in the construction of ethnographies is typically understood. Less agreed on, however, is how ethnographers are to reconcile this influence as it relates to deeper, more personalized considerations for ethnographers.” (BERRY 2011, 166)

Our article deals with experiences made during fieldwork in Belarus, Ukraine and Moldova in 2012 and 2014, where we researched the local effects of the European Neighbourhood Policy (ENP), which is a policy approach of the European Union (EU)

towards our countries of research and others.¹⁾ Even if either the time frame or the budget of the research project allowed for a “fully fledged” ethnography, presupposing traditionally stays in the field of one year or even more, we tried to come as close as possible to the participants and to apply different ways of doing research there, e.g. participant observation, conducting in-depth interviews and informal talks, taking field notes.

¹⁾ The field studies were conducted in the framework of the research project “Within a ‘ring of secure third countries’. Regional and local effects of the extraterritorial engagement of the European Union in Belarus, Ukraine and the Republic of Moldova” which is being carried out at the Leibniz Institute for Regional Geography in Leipzig/Germany from 2011 until 2016.

While we each went into the field on our own (Dorit to Ukraine and Belarus, Helga to Moldova), we discovered upon our return that we had encountered similar difficulties and experiences related to the ways we chose to approach our field. We will analyse moments that made us think on the one hand about the fragmentariness of our fieldwork and on the other hand about the influence that we, but also our participants exercised on the relation due to the circumstance that we as “subjective persons [...] are implicated within research practices” (BERRY 2011, 166). Questions raised within our research team were for example: In how far have we willingly or unwillingly shaped the research field and the access to it? But also, which role did our own “lived experience” (BERRY 2011, 166) play in the field? So we want to focus on episodes that made us think about how we should conduct our research (see ELLIS 2007, 13), which happened particularly during or after encounters we were less satisfied with.

The matter of fragmentariness, addressed in the above quote by MARCUS seems worthwhile thinking about as our research topic reads rather abstractly at first sight. We want to discuss in which way we can use the notions of place and field so common in ethnographically inspired social geography to describe where and what you study: How can we define place and field when the topic is the local effects of some encompassing EU policy in comparison to the classically explored village, a town or a smaller social group? These considerations will also contain a reflexive element because the decision for a research topic and for a certain location to study represent subjective acts of selection that influence what you will be able to find out about the certain topic. In the following sections (one and two) we consider more general notions of place and field together with ideas on multi-sited or un-sited ethnographies in order to apply them to our own research topic.

Then we turn to the question of how to gain the necessary and desired access to the different places of our field. Here an emphasis will be laid on those more challenging experiences that deviate from what is depicted in the many manuals of ethnography, where in the end close, friendly and trustful relationships are created between researchers and researched. On the one hand we will present the strategy of working in a binational team as a trial to anticipate certain limitations inherent in any subjective research, on the other hand we will analyse the influence of limitations in building rapport on our side and also on behalf of the participants that is closely related to what BERRY calls “lived experience” (BERRY 2011, 166).

The presence of the researcher, in the sense that his or her subjectivity influences the research process, can occur in different ways: Especially when we were not perceived as researchers or when questions concerning our role and aim of the interview were explicitly raised during interviews, we wondered about our impact on the material we can gather and the information we can obtain. Fulfilling that role presupposes taking diverse decisions to establish oneself in the field, starting with how we dress, which language we choose, which interview location we propose etc. We want to make some of those moments transparent in which we, by the way we behaved, influenced the field we study. This also means accepting their reflection as “complex, knotty, and uncomfortable processes” (BERRY 2013, 211–212) which can lead to interesting insights which consequently should be seen as a part of the generated data because they can contain information about the field one tries to research (MADDEN 2010, 60).

To sum up, in our article we will look at our own practices of research ethics and disclose our internal decision process regarding the research design. In times of research projects funded by third parties with a limited duration and budget only short and intense field stays are possible in many cases and therefore consequences which arise due to these terminated circumstances need to be analysed in detail. As research projects often follow an interdisciplinary agenda (including ours) scientific disciplinary borders become fuzzier and therefore the reflection on the use of theoretical terms is of high relevance. Consequently, in the following section we will shed light on the terms “place” and “field” applied in our research context.

2 Place and/or field – how do they come into being and where is the researcher?

“people construct place, places construct people”
(HOLLOWAY and HUBBARD, geographers, 2001, 7)

“Humans are place-makers and places make humans” (MADDEN, anthropologist, 2010, 37)

Before starting field studies, we had already taken a number of decisions to work on our research questions: in general, the effect of the EU’s external engagement aimed at the Eastern neighbouring states. Some of them had consequences for *where* we would go to do research. Very long ago, we decided to study the three countries Belarus, Ukraine and Moldova because they are treated within a homog-

enizing frame of the EU, namely as the “Western Newly Independent States” (WNIS). Later we decided on the number of EU-funded projects whose local effects we want to examine, three of them being organised in all three countries. Beforehand, we had initiated first contacts to those officially in charge of the projects, many of them residing in the capitals, even if the majority of project activities was carried out in locations outside the capitals. We travelled to these locations, yet not because of the locations themselves, but because they represented the sites where the projects we wanted to study were taking place and because we were interested in the experiences of the people affected by these projects. And of course each of us organised a place to live during the period of stay. Altogether, we experienced a multitude of locations where we carried out our research, often without living there for longer. Does that constitute a field, and if yes what kind of field? Another reason for which we strive to delineate the notions of place and field is that our research project itself crosses several disciplinary boundaries: It is located at a geographical institute, with a sociologist as a coordinator, two research assistants with a background in cultural studies and referring to ethnographic methods, so potentially different understandings of these two core notions are at play in the micro-cosmos of our project.

Before we turn to the concept of multi- and unsited fields, we take a look at more conventional usages of the notions of field and place: in order to speak of the “*wheres*” in a research project, disciplines seemingly prefer different words, such as place in geography or field in anthropology. These notions however have long begun to migrate across interdisciplinary boundaries, for example due to the increasing role of qualitative methods in geography, like ethnography, and the wave of spatial turns in social sciences (see DÖRING and THIELMANN 2009). Our main observation is that while in methodological literature on ethnography both concepts appear, yet are distinguished with a priority on the field, in literature on qualitative geography they are used in another way: Here, place represents the more prominent notion, and field is mainly used in connection with fieldwork as being the location(s) where research is carried out. More importantly thinking about place starts from the assumption that place is something constructed by human beings in both disciplines (see the introductory quotations to this section). These constructions may happen on different levels, e.g. politically, symbolically, or materially. The topic we are looking at in our research project, can serve as an example:

The political offer on behalf of the EU goes hand in hand with a spatial construction by deciding over who is eligible for taking part in the neighbourhood policy. From the EU perspective Belarus, Ukraine and Moldova are “put into one pot” as the “Western Newly Independent States” suggesting a certain degree of homogeneity which in many respects is not the case. On the other side, the neighbouring states also construct space by accepting or denying the offer of cooperation and integration. An alternative construction, projected upon the same area, is Putin’s Customs Union which underlines that spaces are often contested, sometimes even fought over like currently in Ukraine. In any case they are constructed by trying to attach meaning to them.

Place thus is nothing you can take for granted or something that simply exists, which also applies to places we study: Why is one project carried out in a specific region of the country and not in another? Why are a lot of projects concentrated in the national capitals? And who exactly are these human beings who make place? Here things start getting interesting because it turns out that up to a certain point, geographers as well as ethnographers agree that place is produced by the people they want to study. However ethnographers simultaneously seem to make another turn by introducing an additional category, namely the field to emphasise their own role as researchers in producing his/her place of research so that we also have to ask: Why did *we* decide for choosing some projects that are *not* carried out in the capitals? Our aim was to study aspects of everyday life and focusing on non-capital places implies we assume that capitals are not representative for average everyday life, at least not in centrally organised states. So we too attach meaning to places by choosing them and not others. Writers of manuals from both disciplines largely agree on the very basic definition of place as space which is made meaningful by people being active in place in different ways (see e.g. CRESSWELL 2004, 7; MADDEN 2010, 37). For geographers it is precisely the interconnections between people and place which come into the focus of interest. The core assumption is that “humanity has to exist in place” (CRESSWELL 2004, 50) and therefore, place is interesting to study. For ethnographers it is the “human condition” as such (MADDEN 2010, 39) and in order to study it you have to go to places where you hopefully find cases of the “human conditions” you want to study: “Place and purpose have to intersect” (WOLCOTT 1999, 39). The role of place is described as ambiguous and changing over time. One of the changes concerns the shift from place to

problem-centered ethnography: On the one hand it was important *where* you conduct your study, on the other hand the main criterion for choosing a certain place was that *people* there should be as different as possible from where you come from so you decided for one place or another because of the people and not because of the place:

“I think it essential to recognise not only the importance of place in the evolution of ethnography but to recognise as well that until recently it did not matter where the place was *as long as it was dramatically different from one’s own*. Such difference, rife with implicit contrast and the courageously anticipated strangeness and challenge to cope, was built into the choice of place without anyone having to ask.” (WOLCOTT 1999, 24, emphasis in the original)

“There is no reason to select a site that is difficult to enter when equally good sites are available that are easy to enter. In many cases, you *will* have a choice – among equally good villages in a region, or among school districts, hospitals, or cell blocks.” (BERNARD 2011, 256, emphasis in the original)

Up to here, place appears as something researchers of both disciplines determine as a research setting for more or less pragmatic reasons. It represents the choice of the researcher and therefore contains elements of definition on his or her behalf.

While developing sets of research questions and therefore limiting the scope of research interest, the category of field plays a crucial role. This term refers to a construction of the researcher since it “provides an interrogative boundary to map onto a geographical and or social and or emotional landscape that is inhabited by a participant group” (MADDEN 2010, 39). MADDEN reminds us that ethnographers (like researchers of all kinds) are humans and consequently have to be conceived as potential place-makers: “However, ethnography turns someone’s everyday place into another very particular sort of place [...] Ethnographers create a thing called a ‘field.’” And: “It’s an old ethnographic cliché that there are pre-existing ethnographic fields out there awaiting discovery, all one has to do is walk into them” (MADDEN 2010, 38). To decide for certain places and not others as relevant for the field means at least a selection determined by the researcher (on appointing a setting see WARREN and KARNER 2010, 61). Furthermore, this often implies that not an entire place or places are being studied but only portions or sections of places (ibid., 61). Vice-versa, a field can consist of multiple places, as long as they are tied together by a common research agenda, respectively “the notion of a field [...] is not solely reliant on geographic

space, but rather informed by interrogative boundaries” (MADDEN 2010, 53). As we will see in section four, it is possible that participants revolt against the way the researcher designs and limits the field.

To sum up: Place is referred more strongly to the place-making of those we wish to study. We explore their places, or try to understand them or the interactions between them and their places. In contrast, the notion of field stresses the role of the researcher in place-making in the sense of choosing a specific site, to which we as researchers ultimately add meaning by thinking of it as an appropriate place to research. The ways we describe and delineate it may not be congruent with the ways other people relate to it. Recapitulating the mentioned examples of places - the cell block, the school, a village - they represent places with relatively clear boundaries and exist prior to the interest of the researcher in it. The point of departure seems to be either a certain sort of place or people or their behaviour, a particular problematic or the ways of dealing with it represented in these places.

In our own research project we focus on local effects of a policy approach initiated by the EU. The EU as a supranational actor designed this policy for a certain geographical area: The Neighbourhood and the WNIS are spatial constructions that are not uncontested (see e.g. GREEN 2012, 296 on Russia’s refusal to be member of the ring of friends), which nevertheless we adopted to decide where to research. By addressing research questions concerning this policy we construct our field of research (interrogative boundaries), however this field remains abstract in the first instance. Only later we decided for concrete places to do research as we determined concrete EU-funded projects carried out under the framework of the ENP to include in our study. Ultimately, our field consisted of up to six EU-funded projects in each country, so we conducted research in many places (villages, town halls, offices of project coordinators and NGOs, but also cafés and restaurants if these localities were preferred by our participants, and in the daily surroundings in which we lived, paying attention to political advertising etc.). The multiplicity of locations meant that we were not able to cover all relevant places and also not cover the whole period of the official EU project duration (two to four years). This is why we will consider questions related to multi-sited or unsited fieldwork in the following paragraph and especially the question of fragmentariness, which is at stake in our case all the more as we did not spend the classical “at least one year” in the field. Therefore, questions of how to gain access

had to be resolved as quickly as possible and at the same time constituted a nearly daily task given the changing settings we visited during our research.

3 What kind of field is a policy and how are places found in projects?

What kind of ethnographic field do policies constitute and how can we research them? According to several authors, studying a certain policy easily ends up with the challenge of studying a “multi-sited” or “un-sited” field (see MADDEN, 53–54; WATSON and TILL 2010, 123–124; HANNERZ 2003) creating the challenge of defining what exactly a policy is:

“On closer examination, however, policy fragments – it becomes unclear what constitutes ‘a policy’. Is it found in the language, rhetoric and concepts of political speeches and party manifestos? Is it the written document produced by government or company officials? Is it embedded in the institutional mechanisms of decision-making and service delivery? Or is it (*pace* LIPSKY 1980) whatever people experience in their interactions with street-level bureaucrats?” (SHORE and WRIGHT 1997b, 5)

With reference to the ENP some of these fragments could be strategy papers, speeches, institutional mechanisms like that of conditionality to make the countries adapt more rapidly, the sections of the external politics of the addressed countries that deal with the negotiations about the ENP with the EU, the different projects carried out under the ENP ranging from adapting the legislation to organizing student exchange schemes or the Europe Day on 9th May. Each of these activities involved different actors, some of them from the EU, others from third countries, most of them representing only a part of what is included in the ENP, let alone the many political fields that the ENP touches upon (economy, juridical affairs, education, research, transport, migration, environment, regional development etc.). No matter what focus is taken when studying the ENP, one is firstly faced with an externally initiated political framework, applied in other contexts, like Moldova, Ukraine and Belarus. Secondly, any analysis of the ENP has to be selective, given its complexity.

What we want to study are the local effects of the ENP in these three countries mentioned above by looking at six concrete projects implemented in each country under the ENP. Given the first characteristic of the ENP (external initiation and local implementation), we think that MARCUS’ approach to multi-sited ethnography is a fruitful one because he

frames multi-sited ethnography as a mode of examining the circulation of cultural meanings (MARCUS 1995/1998, 79) which also touches upon the distinction between “lifeworld and system” (*ibid.* 80). On the one hand this allows for bringing together different accounts of the same subject (MARCUS 1989/1998, 37), the analysis of documents presenting one possible kind of account and concrete experiences in projects presenting another account of what the ENP can mean. On the other hand, his perspective fits well because the ENP is often perceived as a trial of achieving “Europeanization beyond Europe” (SCHIMMELFENNIG 2012), a trial, though, to make sets of meaning if not circulate but spread to other countries, i.e. a policy to adjust countries outside the realm of the EU to EU member states (without incorporating them completely as member states).

As one part of our research is dedicated to the contextualisation of the ENP we analyse core documents such as speeches, policy/strategy papers etc. The main focus however is on the local effects this policy has in the addressed countries, referring to the concrete mode of implementing the policy via the projects we chose. Given our interest in the influence of the policy on the daily lives of people in the countries addressed by the ENP, an approach inspired by ethnography seemed likely. With both SHORE and WRIGHT (1997a) as well as MARCUS (1998) we can legitimate our research agenda because according to them such an approach carries the potential to find out something about the mechanisms at work in the system side of the ENP (the EU) by looking at a locale that is penetrated by that system (MARCUS 1998, 39). With the underlying question of “how do policies ‘work’ as instruments of governance, and why do they sometimes fail to function as intended?” (SHORE and WRIGHT, 1997a, 14; very similar see MARCUS 1998, 52) they both refer to the interplay of different societal levels (population, project coordinators, politicians, media, city vs. villages) in negotiating external impulses. What MARCUS makes more explicit however, is that this perspective intends to overcome the dilemma that anthropologists always strive to say more than they actually should be able to according to the limited, fragmentary material they have (on totalistic rhetoric and the spirit of holism in ethnography see MARCUS 1998, 36–37). So to put different accounts together, juxtaposing them, letting others say it, may all be regarded as attempts out of this dilemma (*ibid.*). Instead of presenting the global or the “whole” as something fixed, ethnographers should try to find out something about the ways and mechanisms by which this system oper-

ates and which forces lead to its alteration (MARCUS 1989/1998, 34-35). This “something” is admittedly only a partial insight: “being [...] in different sites [...] one learns about a slice of the world system” (MARCUS 1995/1998, 98). On the other hand one might say that our field as the entity we constructed, and that was partly reconstructed in interacting with people we met during the fieldwork, became that slice of the system we want to understand and in that sense gained the quality of our research entity. It becomes the small whole we look at from different perspectives, yet in order to do so, we had to be able to adapt it while on site which requires the ability to also include other material or data than those generated via classical methods. This process includes analysing EU press releases, local press, speeches and information material produced within the projects we studied, the projects’ websites as well as the reflection of the email correspondence with those we interviewed after going home etc. According to HANNERZ, precisely these “skills of synthesis may become more important than ever” (HANNERZ 2003, 212) underlining once again the constructed character of any field under study.

To sum up: Unlike traditional anthropological studies, based on long-term research within one group of people, ideally in a remote site, suggesting completeness in contrast to selectiveness (HANNERZ 2003, 207), studying a policy field means including different sites, dispersed locally, presupposing different logics on behalf of the people researched and emphasizing other methods than in traditional or classic anthropology. These different levels of analysis will also produce different qualities of data (MARCUS 1995/1998, 84), what apart from that has to do with the ways the researcher is located by the participants. Therefore, we tried different ways of gaining access to the field paying attention to “the self’s locations in culture and scholarship” (BERRY 2013, 212), which we carry around with us but which are also projected upon us by others. Both can influence the way the research process unfolds.

In the following chapters we will describe two aspects of gaining access to the field and how we tried to position ourselves while we were positioned by others, too. In section four, we will analyse our experiences of conducting qualitative interviews in a binational cooperation as there is only little discussion on field assistants in the scientific discourse (GUPTA 2014, 397). This approach can be seen as a rather planned, strategic approach on our part. In contrast, we will deal in section five with a case in which the perception of ourselves as researchers was

surprisingly negative and proved to have a limiting effect in the first instance, provoking more reflection later on.

4 Entering the field as a binational team – benefitting from shared lived experience

When entering a foreign society and applying ethnographic methods, two general ways of conducting interviews are possible: First, to perform the interview with the help of an interpreter or translator. Second, the researcher carries out a non-native conversation in the subjects’ foreign language (WINCHATZ 2006, 84). Although we are both able to communicate and conduct the interview in the foreign language of the addressed countries, we decided for several reasons to work in a binational team, i.e. a foreign researcher and a local collaborator, during our field stays. Essentially, while conducting qualitative interviews the interviewer defines and tries to keep the focus on a specific topic, but it is up to the respondent to determinate the interview’s content. Therefore qualitative research is to a high extent unpredictable and fluctuates, the relationship between researchers and researched is constantly under construction (MCCORKELE and MYERS 2003, 204). By intending to conduct qualitative interviews a change in terms of power relations can be stated as well. Although the researcher appoints his or her fields of interest, it depends on the members of the field as to whether he or she gets access to it. Regarding the accessibility of the field, questions of trust, likeability and the ability to communicate our research interest play a crucial role.

In order to conduct interviews we as researchers followed the basic principles of scientific practice which obliged us to identify ourselves including our profession and affiliation, the purpose and the conditions (e.g. the anonymous and confidential maintenance of collected data) of the research project (see WARREN and KARNER 2010, 33f.). After the introduction, which is characterised by transparency and the voluntarily given respondent’s agreement the investigation can be launched. Beside this formal and indispensable disclosure of the research objective and the therefore emphasised distance between object and subject of the research the researcher can nevertheless apply different strategies to bridge the created gap. Thinking of strategies to close this gap as quickly as possible appeared important to us since we did not have too much time at our disposal to become familiar with the field and vice versa.

Conducting interviews together with a local colleague and thus forming a binational team with him or her was a strategy to overcome the experienced distance between researcher and researched and to anticipate limitations resulting from a potentially perceived distance. As there is less literature about research teams and the implications in qualitative field studies (DÖÖS and WILHELMSON 2012) we intend to elaborate on the implications of entering the field with a local collaborator in more detail. Although LEWIS and RUSSELL point out that fieldwork "requires the researcher to immerse him- or herself fully in the chosen field of study, learning the day-to-day and extraordinary stuff of social and cultural life by 'being there'" (LEWIS and RUSSELL 2011, 400), they recognise that there are often "fragmented contemporary forms" (ibid.). Due to a lack of time and money our field stays were limited to three months. As one solution to respond to these circumstances we intended to work with a local PhD student. In our case the temporary colleague assumes a double function: As a collaborator he/she is included in the research project and as a member of the society of the countries he/she is also a part of the research field being described. Recognising cultural communities as a heterogeneous, fragmented, flexible and a constantly altering web of relations, we follow BRUBAKER and COOPER that subjects position themselves as they refer to certain features which construct a groupness and at the same time constitute "[...] a sharp distinctiveness from non-members, a clear boundary between inside and outside" (BRUBAKER and COOPER 2000, 10). With the help of categories such as nationality, citizenship, age, gender, religion etc. subjects are able to formulate membership or non-participation in a certain community. In our case, both an inside and outside perspective are combined within the interviews: we being from the EU and our participants being citizens of states outside the EU. This dichotomy was mentioned explicitly throughout some interviews, even if it was not always done in a disadvantageous manner. From an auto-ethnographic perspective, however, presenting ourselves as a team with members from both "sides" can be interpreted as a conscious location of ourselves (see BERRY 2013, 212) in the actual cultural context, an approach of displaying existing personalised relations between members of different contexts. The decision for binational teams was an effort to make research more meaningful (ibid.) in the sense that we expected conducting the interview in such a team would lead to a more open and fruitful atmosphere than

working alone. When starting the interviews with a self-introduction, the local collaborator positions him-/herself by mentioning the place of residence, the current occupation as a PhD student in a certain discourse which consequently could appear as an offer to identify and integrate with the interviewee. As the native field assistant is employed by a foreign research institute he or she appears as an in- and an outsider at the same time. This binational team combines the insider with outsider status, therefore sameness and difference are interconnected within the research process. As the foreign researcher constructs the research field while choosing it his/her distance also legitimated the conduction of research, which has at least two dimensions: one dimension is that as a stranger you may ask those more "naïve" questions a local cannot ask since he or she is supposed to know the answer. Secondly, as a stranger (and especially as somebody from the West) you sometimes enjoy strange but helpful privileges, e.g. in certain contexts you are treated in a more friendly manner than locals, your passport may be controlled faster etc. In our case, it was our local colleagues that pointed us to one such privilege by often asking us to initiate the first contact with the desired participant with the explanation that the chosen respondent "can't refuse a foreigner's request" (minutes from fieldwork in Belarus), and indeed a conversation was never denied. The acceptance of the research interest also appeared in contrast to the experience of another independent local researcher, who conducted research in the same field and whose interview requests with the same respondents were refused. The local researcher told us the following assumed explanation with respect to the refusal "they don't have any benefit from a talk with me." (minutes from a talk in Minsk 2012). In this context it would be interesting what benefit the respondents expect from an interview with a foreign researcher, a question we cannot elaborate on here, however.

One of the benefits of working in a binational team was that during the interview the native-speaking colleague guaranteed an atmosphere of free and open conversation. References to common experiences like having watched the same Soviet movies, knowing the same jokes or anecdotes given by the respondents was the most obvious evidence showing that the "lived experience" (BERRY 2011, 166) must not influence the research in a negative way but can even be useful in constituting an open setting. Whereas the local colleague was reflected as an insider in connection with a shared cultural background, the foreign researcher can raise basic or es-

sential questions and scrutinise certain statements, a phrase or the specific use of words to get further explanations (WINCHATZ 2006, 90).

The following quote shows how the different cultural background is perceived by the respondent and which positive implication it has regarding the interview:

Mr. R.: ‘Mhmm do you know the word kolkhoz?’

Dorit: ‘Yes, of course, I know!’

Mr. R.: ‘What does it mean?’ [Respondent tells a joke as an explanation]

In this quote the respondent is questioning the researcher’s knowledge about a specific term introduced during the Soviet era. And by doing so he expresses a distance between the cultural backgrounds. Although the researcher answers affirmatively the respondent nevertheless continues to explain the term with the help of a joke. In this context the more detailed answer is one effect of this distinction between researchers and researched. Additionally, a mixed team to work in the research field blurs the boundaries of a monolithic research perspective as there is neither a pure single inside view nor an outside one. This was also evident regarding the reflection on interviews. All notes are a dialogical product of the researcher team as we shared and contrasted our observations.

An interference regarding the research process while working together with a local interviewer is the possible emotional attachment regarding sensitive issues. In one case the local interviewer was so emotionally affected – he felt in his own words “insulted” by the “exaggerated” harsh critics of the respondent regarding the local authorities – and consequently he could not continue the interview. To sum up, the attendance of a local partner can on the one hand support the fieldwork but on the other hand endanger the equilibrium of the conversation as well and challenges the impartial scientist’s attitude. In the mentioned case we took over the interview.

A second strategy for positioning ourselves was to highlight common elements and aspects with the respondent. While Dorit had stayed in Belarus prior to the research project for a year, it was only Helga’s second visit to Moldova after a short term stay the year before. Therefore, Dorit

could emphasise prior stays, substantial experience in the country, she shared deeper and “first hand” knowledge about certain societal, political developments with her interviewees. To put it methodically, this can be interpreted as revealing something personal (MCDOWELL, 2010, 162) or as a partially shared lived experience (BERRY 2011). In any case, this degree of knowledge and competence may be activated to signal empathy or to reduce power differentials, especially in cases where the researcher finds himself in the position of the subaltern. That way, it was easier for Dorit to naturalise and authorise her attendance in the field as she could better include herself into the addressed society due to contacts established beforehand. All this supported her authenticity and helped build up trust between researchers and researched. First of all we recognised the importance of previous experience in the addressed societies as the respondents constantly asked: Is this your first time in our country? And they were very pleased and interested in any link between their home countries and us. In this context the phrase “as you know” [“как Вы знаете”] often mentioned during the interviews can be interpreted as the acknowledged connection between the researcher and the field or/and as a supposed connection or/and as a manner of politeness, nevertheless it marks a moment of inclusion. It can also be interpreted in the sense that the participant, from what he/she has come to know about us, has drawn the conclusion that it makes sense to tell us more, since we are able to follow and understand him/her. The contrasting remark would be: You do not know how it is and you cannot imagine what it is like, consequently, it makes no sense to continue the conversation. SEYMOUR points out:

“Emotions, reactions, feelings, and the past experiences of researcher and researched, although seemingly locked within the individual, inevitably affect each other and influence the conduct of the research and its outcome.” (SEYMOUR 2007, 1189)

Common experiences or a common stock of knowledge about the local context may represent a good basis for conducting interviews. The exchange of personal details and information was the fundament to establish a mutual understanding and trust so that the link between the researcher’s biography and experience with the research field may gain high importance. Notwithstanding, this potential cannot always be activated and the relation may remain more distanced.

5 Gaining entrée despite negative lived experiences of the others

“Participant observation fieldwork is the foundation of cultural anthropology. It involves getting close to people and making them feel comfortable enough with your presence so that you can observe and record information about their lives.” (BERNARD 2011, 256)

Anybody who even only “borrows” ethnographic methods (WOLCOTT 1999, 41f.) needs to add his or her presence to a place inhabited by others, those we want to research in whatever dimension, because an ethnographic approach still means very much “being with people [...] in their time and space” (MADDEN 2010, 32). This addition not only includes the spatial relocation of the researcher but also a social one as LÉVI-STRAUSS points out in what concerns social hierarchies (LÉVI-STRAUSS 1989, 105, quoted in SIDAWAY 1992, 403f.). As we have illustrated above, this relation may be influenced positively in the case that researcher and participant share some stock of common experience and knowledge, especially, when the researcher can refer to a degree of commonality and local competence due to having “really” lived there for a longer period. In another instance of our fieldtrips, the “tricky politics of the knowledge acquisition” (MADDEN 2010, 60) turned out much more complicated, however.

In the case we turn to now, the relationship rather suffered due to lived experiences of the participants and even despite a common background in social sciences between researcher and participant. The case developed as follows: After we had contacted the gatekeepers of a French donor organization they intermediated a contact with their branch in Moldova whose staff is in charge of the project we were interested in. The staff working in Moldova stays in close contact with the organization’s head office in the EU country, thus we hoped that the intermediation of the contact could contribute to building up trust that would ease further contacts. Prior to our visit in Moldova, we indeed had exchanged a couple of friendly emails with the staff in Moldova, in which we had already presented our research project, given an outline of our interest and roughly agreed on the period of field stay to make sure that they would be available for some interviews, since people working for international organizations are often out of the country. Once in Moldova however, things proved to be much more complicated than expected: After the initial meeting had to be postponed several times, one staff member asked for the interview questions

in advance. From the methodological point of view it is of course difficult to lay open the exact questions in advance, nevertheless it is a legitimate request that can build trust, so in this case we decided to send a rough list of the points we wanted to touch upon in the proper interview. When we met after another couple of weeks for the in-depth interview, the other staff member confronted us with severe doubts concerning the seriousness of our project, concerning the relevance of their own project and a distrust vis-à-vis social research projects in general (see HERBERT 2000 for the discussion of similar reservations):

- 1) *How can you claim to come to generalizable results if you do so few interviews?*
- 2) *And what we want least is to read another report in which Moldova is portrayed as the country with the most serious alcohol problems in the world!*
- 3) *Don’t you see that our project in itself is much too small to say anything about your topic – we are not relevant at all, go and ask another project, funded by the government! (All italics taken from minutes from memory in Moldova)*

The reservations touch upon two aspects: The first is our professional qualification, that is, our identity in the field, since all points contain critique concerning our empirical approach. Their reproach was that we cannot come to representative results and that we chose the wrong case. The second point has to do at the same time with a prior experience with external research, so interestingly, here enters the lived experience again, yet that time on the part of the participant. Due to a negative prior experience with external research, they want to avoid the risk that another inadequate representation of Moldova is produced. Clearly, in this case it might have been tempting to not disclose our complete identity as researchers to not affect field access, an aspect ELLIS reflected on (ELLIS 2007, 7). For instance we could have argued that we will also conduct a representative survey. But it was too late, since from the initial contact with the French parent organization and given our institute’s emails, it was clear, who we were, and of course we had expected that our openness, that resulted also from an anticipated distrust vis-à-vis researchers, would on the contrary be rewarded with trust.

Concerning the aspect of our professional identity, we can interpret their criticism as being a typical example of what MARCUS describes: In any contem-

porary field there will be people who know as much as you do or who want to know the same things and whose discourses overlap with the researchers' (MARCUS 1995/1998, 97). Basically, this might constitute a similar stock of commonality like the common knowledge about daily life and societal issues in the country of research as in the example above. Yet the problem was that our methods did not belong to their "equipment", the overlapping was, so to say, too small, perhaps from their perspective there was not any overlapping at all: They conduct surveys and generate statistics to analyse migration patterns in Moldova as a basis for designing projects in local development. We, too, want to analyse this topic, yet applying different, qualitative methods, not aiming at representative results, which discredited our undertaking from their point of view. Our initial hope, that in this case, the common ground in social research and interest in the same topic might attenuate prejudices and scepticism *vis-à-vis* some strange foreigners posing strange questions, did not come true. The situation was aggravated by the fact that they had experienced how external scientists cast a damning light on their country, constituting a decisive part of their personal lived experience with being represented by others, impacting evidently on how they encountered us. It is exactly what MADDEN describes as the "tricky politics of knowledge acquisition in ethnography" (MADDEN 2010, 60), only, that we were not confronted with a "potential problem" (ibid.) but with a real one: how to convince them to participate (the suggested government projects were no alternative because they are not financed by the EU), and how long should we try to convince them? What can we find out if there are so many doubts on their side?

We spent a lot of time talking about how we work and how they work. One of them still keeps in touch with the local state university and was obviously interested in an eventual career as a political scientist and perhaps therefore he was also the one more open towards our ideas in the end. We emphasised that we were not here to do anybody any harm with our research and that we do not work for any governmental or EU body because even if they did not ask explicitly people asked us several times questions like "Is this your private initiative or are you preparing a report to Brussels? (minutes from a talk with a professor in Belarus). Consequently we repeatedly stressed that we are affiliated with an independent research institute and that all collected data will be used anonymously and confidentially. Yet, when we offered to retreat completely, expressing our regret because of the many interesting aspects of *this*

project, the one who was more open, but who was not the head of the project, offered us to talk to some of their beneficiaries. And as if our offer to retreat had convinced them of our harmlessness, from then on talks became more open.

When they took us by car to do interviews with their beneficiaries we learnt during the trip that the project was in trouble at that point in time, difficult negotiations with the EU delegation lay ahead. Regardless of whether their hesitation also had to do with these difficulties, they created a bigger picture of how their project is embedded in the actual political and social context of Moldova, its competitive situation with other projects, those run by the government, the decreasing interest of the people in this kind of project due to the high number of similar projects etc. After all, all their hesitation and also the difficulties within the project revealed something more general about the situation of projects in this domain and thus the broader context. At the same time, their critique concerning our approach made us rethink our approach to their project.

Their critique consisted mainly of not being able to share the way we added meaning to their project and of the focus on their project alone being too narrow. We were up to take it for something it was not for them – while we were convinced that their project is of high relevance for our purposes they had a completely different point of view on this. This episode helped us to realise that we needed to re-contextualise our research agenda and consequently initiated a process of recurrence regarding the construction of our field of research. Finally, what they expected or wanted us to do was to be more thorough in understanding the background, e.g. the political implications of the topic for Moldova and to be careful not to present their project as isolated, which they feared would easily distort not only their project but potentially create a false picture of the whole complex. Consequently, on our second field trip we included interviews with other relevant institutions. We thus made an effort to learn from their critique (ELLIS 2007, 13 and 17) trying to see the bigger picture. When we met for another interview with the staff of the project during the second fieldtrip in spring this year, the atmosphere was completely different. We could talk much more openly about aspects of competition with other projects, the dissatisfaction with their situation, the general situation in Moldova and also their disappointment with the way the EU supported them. They were even more open than the last time and took us to many more beneficiaries than the first time, spending a whole day with

us, inviting us again and again to their office and to do a radio interview together with them to lend their project more visibility – which this time we refused to not interfere with our own research nor to harm the anonymity of those we had talked to.

6 Conclusion

In our paper we reflected on the use of the terms place and field under two specific conditions: Firstly, our research was not designed as a comprehensive ethnography with a time frame classically extended up to a year or two spent in one place, “the village” or another seemingly well circumscribed set of places. Secondly, our initial interest lies in the effects of “a policy” – namely the European Neighbourhood Policy – which is not necessarily tied to any specific place given the ambiguity of the notion. For these reasons, the fact that any field of research, and consequently the decision for certain places where the relevant aspects may be studied, is a construction of the researchers appears all the more obvious. Even after having decided for projects as the right places where effects of the policy in question can be observed, it turned out to be difficult in some cases to identify concrete places in a spatial sense, where you could “hang out” (not to think of “deep hanging out”, see GEERTZ 2000, 110; WOGAN 2004; MADISON 2012, 19f). In that sense, our research was characterised as partly un-sited fieldwork as there were not those places we could pass by every day and at the same time partly multi-sited due to the different places where actions of the projects took place. The constructed character of the field as a composition of many sites also means becoming aware of the fragmentariness of the potential findings, an aspect that is central in the approaches to multi-site ethnographies. Findings in the different sites should however not be neglected but seen as a way to understand a part of a bigger system from many perspectives.

As we gained contrasting experiences regarding the entry to the assigned field of research, we furthermore clarified how the collaboration within the interview process was initiated on a meta-level. Entrée may be eased if you know the country very well and cooperate closely with a local, but it may also be more difficult when your participants partly share your professional qualifications. This means that not any shared “lived experience” has a beneficial effect. Rather, we agree with MARCUS that it depends on each setting how you are positioned and how you can position yourself in a changing site: “In

certain sites, one seems to be working with, in others one seems to be working against, changing sets of subjects.” (MARCUS 1995/1998, 98). It is a matter of negotiation that sometimes may take more time but that also leads to unexpected and valuable insights. As long as a consent for researching can be ultimately reached, it thus seems worthwhile accepting even difficult negotiations since they help to rethink former assumptions and to come to a more appropriate analysis in the end. Reflecting these specifics and restrictions is an inherent part of the research results. Consequently, they are part of the process of knowledge production in general.

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