

AUTOBIOGRAPHY AS ETHNOGRAPHY. DOING FIELDWORK AT HOME AWAY

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ABSTRACT. Ethnographic fieldwork as method of research configures a specific relationship between anthropologist and her object of research, whereby the epistemic position is one of mutual constitution. Knowledge is produced within and through a liminal cultural space between the researcher and her anthropological subjects, unfolded through interaction, transformation, and resignification. Based on my experience of fieldwork among fellow Romanian immigrants to Canada, this article discusses the tensions entailed by such reversal of the classic ethnographic situation. Autobiography precedes anthropology and survives it. Both the researcher and the researched are away, having already experienced the immersion into another culture in complex ways, during the process of immigration, and after the settlement in the new country. The stance towards fieldwork proposed here is one that upholds forceful and total engagement with the members of the community under study, which does not circumvent confrontation, strong emotions, or pain, but requires discretion, temperance, and prudence in its rendition. It also proposes the moment of writing as the primary ethical, ontological, and epistemological moment in the whole anthropological undertaking.

Keywords: ethnographic fieldwork, autobiography, writing, ethics, Romanian immigrants, Canada

Introduction: Entering the Topic and into the Field

This article is a chronicle of becoming. It documents the making of an anthropologist. That the anthropologist is also or strives to be a critical sociologist, a woman and a feminist of sorts, an immigrant from the former communist bloc to an advanced industrial country in North America and a transnational subject, are important details, which decide the shape of the story. The narrative bears little eventful material, but it nevertheless encompasses transformative moments of which the writing of the text itself is one, and begins with the very moment of its conception.

The Association for the Studies of Nationalities (ASN) annual convention, where I first presented a paper based on my ongoing social historical study of Romanian immigration to Canada, draws scholars from across the social sciences.

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It is a hub for political scientists, international relations scholars, historians, sociologists, political philosophers, linguists, and others sharing an interest in ethnicity and nationalism in Eastern Europe and Eurasia. My paper, organized around the notion of ethnicity as relational historical construction, started with an ethnographic description of the celebration of Pentecost in Windsor, Ontario's Romanian Orthodox Cathedral. This account of the single point where Romanians of different waves of immigration meet was intended to point to the paradoxes structuring my discussion of what ethnicity was and how it mattered.

During the ritual introductions of participants before the start of the panel, the discussant situated me within the discipline of anthropology, based on his reading of my paper. A rather rare occurrence in a space dominated by positivist political scientists and historians tackling fiercely contested national pasts and presents, my ethnographic passage aligned with the contributions of my co-panellists, which used similarly "qualitative" material: accounts of theatrical re-enactments of history through performative commemorations, and of lived history through cinematic renderings. The discussant had just published a collection of texts aimed to bring ethnographic approaches into the discipline of Political Science, which was to constitute the focus of one of the conference's book panels (Schatz, 2009). Therefore part of his comments to my paper concerned the ethnographic method I apparently used to assemble my object of study and of which I had given no details. In fact, disclosure of methods was absent in my paper, and very little was explicitly said about the stakes of its epistemological approach.

The first question concerned my "entry" into the field. Originating from the tradition of travel writing, accounts of author's first contact with her object of scrutiny have been incorporated into anthropology from its very beginning. For the canonical rules of writing ethnography, the episode of first arrival in the field still represents the legitimate single section of personal narrative in a body of objectified description. As such, it is a crucial part of the report and represents the place where the association with the "other" is laid bare. It provides elements of the landscape, in the larger sense, where the intersubjective space between the anthropologist and her subjects starts being built. This unfolding space of mutually transforming subjectivities through successive interactional passages of reflexivity and objectivation generates the events that will become ethnographic data. Authors assume and are forced into variable power positions from the very beginning, and consequently their stances on the possibility of "knowledge" of the other and/ through "writing" the other are shaped in particular ways. Images of royal arrivals (Firth, 1936), castaways (Malinowski, 2002), or contaminators (Lévi-Strauss, 1955) help us decipher the distance between fact and data, object and concept, lived experience and written ethnography (see Pratt, 1986).

Since my paper told no story of my entering the field, I was asked to provide one as a token of authenticity of the data, but also out of sheer interest in the methodological operations that had converted them into a structured

arrangement of concepts and relationships. But I could not oblige, since there had been no such event as a proper entrance, first contact, or genuine first encounter with the other. I was a landed immigrant from Romania in Canada studying landed immigrants from Romania in Canada.¹ I had been immersed in the reality of my object of research from the very beginning, by being and living. I had shared the same life in Romania during the communist regime and after. I had undergone the same process of remaking of the self while waiting for my permanent resident application to be processed (Culic, 2010). I had experienced anticipation, joy, wonder, anxiety, uncertainty, pressure, and strain in my new country, like all my fellow Romanian immigrants. My answer to interpellations about my fieldwork will always be self-referential. But my fieldwork was not, nor was the object of my research merely self-referential.

A peculiar fieldwork situation like this while perplexing may nevertheless uncover productive tensions in the practice of ethnography and force us rethink the relationships through which we conjure the ontology of our object of research. The strategies we employ to arrive at knowledge claims formulated in variable idioms of construction and discovery, representation and revelation, invention and rescue are shown from very distinct standpoints, engendering very distinct concerns. The junction between anthropology and autobiography takes here a form that *reverses* the usual epistemological and existential configurations between the ethnographer and the researched. As such this research may be portrayed as an odd instance of repatriation of anthropology as cultural critique (Marcus and Fischer, 1999: 111-36). The culture I investigate is my culture, and the agents' way of thinking and relating to objects are self-evident to me. But this anthropology at home is in fact taking place *away*. My subjects are immigrants, aliens in a new country and a new culture.

In the 2005 edition of *Welcome to Canada*, immigrants' official guide to their new country, the federal department responsible for immigration and settlement warned new immigrants of the risk of culture shock, and described its symptoms: "Your first year in Canada will be emotional and full of change. [...] Regardless of your situation, being a newcomer may mean giving up some familiar things for a new way of life. As a result, you may feel anxious or afraid, especially during the first few days and weeks" (Citizenship and Immigration Canada, 2005: i). All immigrants are explicitly alerted, in its 2010 edition, that "Canadian society and Canadian values may be different from what you are used to; some cultural practices in Canada may seem strange by the standards of your culture of origin. At the same time, some cultural practices in your country of origin may be considered unacceptable in Canada" (Citizenship and

¹ I immigrated to Canada as a permanent resident (landed immigrant) in December 2005. Since 2007 I divided my time between Canada and Europe. The ethnographic fieldwork reported here took place in Windsor, Ontario, during April-September 2009 and March-August 2010.

Immigration Canada, 2010: i). Not only are immigrants selected by the Federal Skilled Worker scheme reflexively refashioned through anticipatory socialisation with an imagined Canadian space, by subjective submission to state's and their own desires during the immigration process (Culic, 2010); they have to confront both the cultural image transported by the Canadian state in the implementation of its policy, and the version of Canadian culture they actually experience once landed and settled in Canada. They go continuously through successive episodes of re-mapping meanings and re-signifying facts. Such acts of objectivation and an existential liminality that may last for long periods of time open their practical reasoning to the elusiveness of things and the transposability of experience. But if the deep structure and the general script of this huge experience and transformation are the same, they nevertheless take variable actual shape for individuals. As one of my anthropological subjects worded a theme traversing immigrant talk, "Immigration gives the measure of you as a person".

Due to my always already being immersed in the reality of my subjects, forever constituted by it and constituting it, "entering the field" counts for the precise moment I took the decision I *wanted to write* about immigrants' experience and their lives, their thoughts, and their emotions. By this I wanted to arrive at some sort of self-analysis, starting from what was originally the grounding for postulating a *legitimate* assumption of an unconscious: "all the acts and manifestations which I notice in myself and do not know how to link up with the rest of my mental life must be judged as if they belonged to someone else" (Freud, 1989: 575). I wanted to tell the story of these immigrants and of their self-making throughout the immigration enterprise as much as I wanted to make sense of a life I had difficulty managing at that particular moment in time, where immigration was an episode that still needed to be explained. Hence, I embarked on an anthropologic research that implied psychoanalytic practice. Rather than take subjects' conscious moves to shield sensitive knowledge and unconscious defenses in interaction as obstructions to understanding, concealment was to be placed at the centre of ethnographic insight (Robben, 2007: 161). Interaction that claimed intellectual as much as bodily and emotional engagement was the only epistemological choice I could envisage for this.

Being in the Field: Engagement and Violence

My approach to data gathering in what was an emotionally charged setting for me and for my subjects thus departed from the model of the anthropologist as the "stereotypic stoic and faceless tabula rasa, which was to be the most receptive object of the analyst's inscriptions" (Robben, 2007: 163) or as "a sort of non-person, or more accurately a total persona [...] willing to enter into any situation as a smiling observer and carefully note down the specifics of the event under consideration" (Rabinow, 1977: 46). Empiricism or the use of methodology

as anxiety-reducing device made little sense to me. I was not going to look at and see practice as spectacle, by theorising distance into my conception of self and other, subject and object (Bourdieu, 1977: 1). Neither was I impressed by the announced “methodological” risks of narcissism, exhibitionism, self-indulgence, confession, or lyricism, which I relegated as technicalities to be dealt with in the artisanal “writing” part of the research. I was ready to engage into struggles with deceit, exaggeration, defences, disinformation, and resistances as much as I felt that I had to deal with repression in myself. That I thought this was the proper way to render the world of my subjects as a whole and to reach deep understanding there was no doubt. I placed myself alongside anthropologists who “exploit the intrusive self as an ethnographic resource rather than suffer it as a methodological hindrance” (Cohen, 2007: 111. See also Okely and Callaway, 1992; Davies and Spencer, 2010). At that point however I was striving to see for myself how it was going to be done.

Rabinow’s account of fieldwork unrolls incessantly shifting spaces of the world *between* the anthropologist and the others, predicated on interruptions and eruptions, on the constant breakdown of this ever liminal culture through which communication is made possible (Rabinow, 1977: 154). His assembling of data was often accomplished by “intruding beyond the boundaries which were acceptable and comfortable” (Rabinow, 1977: 130), “transgressing the integrity of [his] informants” and performing “essentially an act of violence [...] carried out on a symbolic level” (Rabinow, 1977: 129). Yet another experience of an absorbing violent field relationship with her Australian Aboriginal informant is reported by Marcus (1992), unavoidably so as it had been forged within state-orchestrated structures of racism, realized through all-encompassing surveillance. “What I had to learn was that there was no outside position from which to observe, and that giving up my personal power was an exercise of self-delusion that was of no use to anybody” (Marcus, 1992: 108). While liking one another may have helped building a relationship between the anthropologist and her subject, friendship could not have been possible, and the exchanges followed a pattern of “constant re-negotiation, anger, suspicion and deep mistrust,” where the “researcher was to become, like the informant, a resource-person to be manipulated,” who “had to try to relinquish the weaponry of cultural and race capital, to leave [herself] unarmed, naked and exposed” (Marcus, 1992: 108). These field autobiographies are permeated by uncomfortable sentiments of guilt, bewilderment, and exasperation. They record acts of confrontational extraction, and their counterpart in the physical and emotional work done by the anthropologist under a constant pressure for intellectual reshuffling. Rabinow (1977: 38) recounts this: “But one cannot engage in questioning and redefining twenty-four hours a day. The scientific perspective on the world is hard to sustain. In the field there is less to fall back on; the world of everyday life changes

more rapidly and dramatically than it would at home. There is an accelerated recognition of new experiences and their normalization.” Out of these chronicles of anthropologist’s projections and introjections, the personhood and wholeness of the people they evoke are nevertheless recuperated.

Violence is the running head of all reflexive epistemology grounded on the incorporation of the interactional, sensual, and emotional experience of fieldwork. While fieldwork stories relate caring attachments between the anthropologist and her subjects: moving friendships, solidarity, affection, love, and passion, and while the methodological injunction to give proper space to their voices has been widely internalized and given various expression, the appearance of symmetry is denounced every time. The anthropologist’s project always takes priority over the other’s project. Her questions are unsolicited, her probing into silences amounts to intrusion and pain (Hastrup, 1992: 121).

How was my own position within my immigrant culture, whose history I also shared, going to configure the anthropological position then? How did the fact that in my case *autobiography preceded anthropology*, and was also most likely going to *survive it*, change the ontologically and epistemologically troubling relations in the field? This was a space of self-objectivation, where everyone’s task at hand was to reinvent themselves for a new social space and a new life. Every success story and every failure were object to close surveillance and immediate feature in a self-writing urban culture *qua* history of the present. How would a “native” anthropologist fit in here and what would her “fieldwork” look like?

Bourdieu’s *Sketch for a Self-Analysis* (2007) and *The Bachelors’ Ball* (2008), published in English and read at the point of my conversion, helped me make sense of my ethnographic stance and also gave me elements for a system of reference to organize my biographical illusion (Bourdieu, 1999: 58-65). Bourdieu went back to his native village in Béarn to do research, after he had repeatedly appealed to his knowledge of Béarnais peasants’ world as anchorage to resist the spontaneous sociology offered to him by his Kabyle informants. His rethinking of his epistemological strategies turned into relentless efforts to objectify the subjectivity of the objectifying researcher. He took on to expose the difference that separated his familiar relationship with his childhood’s social world from the scientific relationship arrived at by casting an external gaze upon it, pulled together by instruments of objectivation such as genealogy and statistics (Bourdieu, 2007: 60-61). This intellectual and theoretical reorientation entailed social repositioning too. “The deliberate renunciation implied in this negative displacement within the [scholastic] hierarchies would no doubt not have been so easy if it had not been accompanied by the confused dream of a reintegration into my native world” (Bourdieu, 2007: 60). Conducted with the help of his father, in an intensely emotional atmosphere, the fieldwork recorded the return to one’s relegated past

accompanied by a “return, but a controlled return, of the repressed” (Bourdieu, 2007: 62). Research was carried out alongside self-analysis. The emotions elicited by reconnection, reconciliation, and recognition engendered by total involvement with his object of study, in Béarn as much as in Kabylia, were worked through in frantic collection of empirical data. Pieces of evidence such as a familiar sculptured door or the plan of his childhood house helped the reconstruction of both his self and his relationship with knowledge and its institutionalized forms.² The ethical and political thrusts of his work were grounded in close engagement with persons and objects, revolt against suffering and injustice, and struggle for autonomy by the objectification of the limits and conditions of freedom. The ambivalent relationship with the academic institution, grown out of a trajectory that combined high academic consecration and low social origin, was expressive of the profound internal tension between the self-certainty of recognition and the radical uncertainty towards its institutional source.

It was in the most serious yet unescapably self-ironic way that I claimed for myself a family resemblance. This is not the place to substantiate it by bringing birth records, family photos, and coming of age diaries to certify my ways “in the field” and to document the entangled history of my ethnography and my autobiography. I thought that the socio-analysis and self-analysis submitted to us showed not only that knowledge cannot be detached from the social conditions of its production; that emotions, when the anthropologist strives for critical distance and reflexive temperance, rather than corrupting knowledge and the context of its production, effect bodily and mindful dispositions that allow deeper access to the layers of the real and finer grasp of signification; and that personal and social trauma may be made into turning points in the expansion of the human self, occasions for illuminating misrecognitions, and steps towards reflexivity. They also presented us with a particular way of dealing with the intricate relationship between ethnographer, anthropological subjects, and knowledge. There is a permanent quest for objectivity and objectivation, which focuses on practices of both actor and researcher, relating categories of practical reason with the analytical categories of researcher’s practice.

What I also found in Bourdieu’s work as a whole and which took me some time to make sense of was a way to record ethnographic data that exposed the “best-kept and worst-kept secrets” (Bourdieu, 1977: 173) – the untold collective forms of bad faith maintained through the complicity of all, underpinning social life, or, in other words, the analysis and accounts susceptible of violating the privacy of the community and its right to keep its secrets. By using abstract concepts to render the very locality and particularity of practices and relationships,

² For a fine review essay on Bourdieu’s *Sketch* and *Bachelor’s Ball* focused on questions of self-analysis and socio-analysis see Pop, 2011.

and by an attentive dialectics of ethnographic detail, construction of concepts, and formulation of theory, the knowledge thus produced protected the privacy of individual agents and their integrity. Subordinate to his overarching project of objectivity and objectivation, this unswerving care, sensitivity, and sympathy for the people he worked with represented the subtext of all his writing. Most acute in the articles relating the research conducted in his native Béarn, Bourdieu avows it in *Sketch for a Self-Analysis*: “And the objectivist restraint of my remarks is no doubt partly due to the fact that I felt the sense of committing something like a betrayal – which led me to refuse to this day any republication of texts whose appearance in scholarly journals with small circulation protected them against malicious or voyeuristic readings” (Bourdieu, 2007: 63). This fundamental position towards fieldwork, research, and knowledge I read to myself as one that, while rescuing the humanness of all interaction on field, upholds prudence, discretion, and astuteness in its epistemic rendition. It also made me see the moment of *writing as the primary ethical moment* in the whole anthropological undertaking.

Writing Ethnography: Matters of Boundaries

The arrival at this point, surely, was much more tortuous and confused than the narrative here suggests. Much of what I tried to figure out during the first weeks “in the field” was how to reconstruct my relationships with the people that were already part of my life and had now become my object of research. I was keenly aware that this repositioning would necessarily imply change and I struggled to think this change through, for *them* as well as for myself. At about the same time I applied for funding for my newly envisaged research and completed the forms for Research Ethics Board (REB) clearance.³ Pondering about what “informed consent” should mean for my closest friends, family, everyday companions, and acquaintances became an obsessive and consuming activity. I felt that the instruments required by the REB and the terms in which clearance was granted pinned down the fluidity and the “order” of all interaction, and were at points quite absurd. Besides its particular materialisation in documents and forms with specific contents and rules of conduct, the clearance procedure undoubtedly served its purpose by the effect created in terms of dispositions of the mind and alertness of the person.

³ I was affiliated with the University of Windsor in Canada, which follows the common policy of ethical conduct for research developed by the Medical Research Council (MRC) now the Canadian Institutes of Health Research (CIHR), the Natural Sciences and Engineering Research Council (NSERC), and the Social Sciences and Humanities Research Council (SSHRC). The *Tri-Council Policy Statement: Ethical Conduct for Research Involving Humans* (1998) sets out the minimum for approaching research in an ethical manner. The second edition was published in December 2010.

My job as anthropologist stumbled at the very beginning due to one of my closest and most cherished friends. The way she relates to people is one that combines deep care and genuine wish to help with mutual and self-serving interest. My coming out as ethnographer and the discussion of the possibility to work with her as my research assistant were enthusiastically embraced and promptly integrated into a larger scheme that she thought would have served both our interests. She literally started to organize the field for me by enacting her own interpretation of what I should be doing, already approaching people of our acquaintance, and quite overtaking my own thinking about it. She also envisaged this prospect as a means to revamp her then crumbling social status by our “research association” and incumbent resources. The research she was lying in front of me would have cut out huge extents of the social life I wanted to grasp. Clearly I had not found the best terms to explain what my enterprise meant and how to pursue it. More importantly though, I had not considered fully how she would incorporate my ethnographic plans within her own life and momentary crisis, within *our* life at that moment really. The idea that I could separate the ethnographic-work from the life-work when the object of the *ethnography* was our shared *life* struck me as utterly unrealistic.

Out of anxiety-ridden nights and days of disquiet, I came to assume a particular ethical stance. I would not separate what were my personal and my scientific relationships with all the people involved, just like I could not disentangle practically and theoretically my autobiography from my ethnography. Drawing a dispassionate “objective” boundary between the personal and the professional in my ethnographical doings would not be a matter of concern as a principle. The way this boundary is conceived and constructed in the field and in the production of its account is crucial, and my *not* drawing it was a deliberate and momentous decision. In the approach of the construction of this boundary our ontological, epistemological, and methodological positions are formed and knowledge is shaped. I would not in any way detach myself from the people who were making me, and shut my scientific self from parts of their lives that were also part of my life. I was not going to spare them confrontation, challenge, thoughts, and emotions, just like I had not when they had been merely my friends. Nor would I constantly remind them in various ways that they were anthropological subjects as much as they were simply themselves in interaction with me. I could not use the flat terms outlined by the REB instructions to explain to them what I was doing, or present them the informed consent form, get their signature, and transfer to them the responsibility of all “data” ensued. Each of my close friends would have a specific scientific or ethnographic relationship with me just like we had a specific private one.

This is unquestionably a power position, but one that has been thought through. It is meant to engage and embrace the people and their reality, at times in ways they might not recognize or acknowledge. This power position

is armed with alertness, receptivity, and affection. It refuses the “attitude of moral indifference [that] has no connection with scientific ‘objectivity’” (Weber, 1949: 60) and assumes the symbolic and interpretive violence that comes with the conceptual, theoretical, and political version of community the anthropologist puts forth.

This relationship of power actualizes especially in the “writing” of the ethnography – when there remains only one genuine acting agent in the production of the story and of the reality. The anthropologist-author-scholar orchestrates the voices and organizes the plot. Practicing objectivism qua detachment in writing hides the substantiality of both the anthropologist and her interlocutors, and effaces the untidy space collectively authored to achieve understanding. Anonymity, omission, and disguise through various stylistic genres, hardly protect anyone or respect the privacy of the community. They slit the wholeness of field’s lived experience and obscure the epistemic and self-making journey the anthropologist and her subjects have accomplished, enacting yet another illusion of objectivity by distance and neutrality. “Anonymity makes us unmindful that we owe our anthropological subjects the same degree of courtesy, empathy and friendship in writing as we generally extend to them face to face in the field where they are not our ‘subjects’ but our boon companions without whom we quite literally could not survive. Sacrificing anonymity means we may have to write less poignant, more circumspect ethnographies, a high price for any writer to pay” (Scheper-Hughes, 2000: 128). Allowing subjects a dignified presence in the written reality is a matter of integrating them as full persons in the text. This means that their social historical constitution is recovered and anthropologist’s political and ethical standpoints in representing it are made clear.⁴

If ethnographies begin with a story of arrival which glimpses at a world of expanding subjectivity, shared time, and emergent insight, they never end with a matching story of departure. Perhaps this is because the anthropologist never fully leaves the field. She may keep in contact with the people encountered, revisit them, and follow their fate. Her body and mind may hold inscriptions that tie her to that experience in significant ways: a love relationship, a staunch commitment, a burning longing, or a professional interest. The field continues to exist and is reconstructed discursively through writing after the consummation of the experience. Fieldwork data are internal to writing and become through writing. The text is an exercise in epistemology: a rendering of objects and acts through the meanings attached to them; through the concepts built to grasp them into a coherent edifice; and through the theory that unites them into a

⁴ Max Weber’s exceptional essay “‘Objectivity’ in Social Science and Social Policy” (Weber, 1949: 50-112) stands as the classic statement on the relationship between empirical knowledge and evaluative judgments.

global discourse. The fieldwork is constitutive to the anthropologist not only through the transformations engendered by her engagement with her subjects in the field. It constitutes her through its narrative, which assembles actions and characters, weaving them into a plot only made possible because the anthropologist related to these actions and characters, and responded to them in a particular way. By assuming the point of this creation, the anthropologist should not fix their historical time. "Above all, we should not pre-empt the creativity of the 'others' within our own invention. By a narrow historicising approach we claim to have exhausted a moment. We have not; meaning is infinite, and the 'other' may have her own project which we should not violate" (Hastrup, 1992: 126, original emphasis).

If leaving the field might never happen for the anthropologist, her leaving is something that *happens* for the other. Ethnographies rarely talk of the anxiety brought by the anticipation of anthropologist's leaving or of the loss incurred upon the other by her departure. This loss however is real, and the ethnography stands witness to it. The writing of ethnography thus becomes necessary, as a means to integrate all worlds thereby ensued.

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