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**THE NOBLE FIELD-WORKERS: ON POTENTIAL SIDE EFFECTS  
OF A METHOD**

*Abstract:* This article is an auto-reflexive analysis of the potential side-effects of the overuse of the field method, and the absence of a critical attitude towards it in ethnological research, especially in context of past and current Macedonian ethnology. Improper registration and processing of materials, their treatment as private ownership by certain individuals, their “filtering” in a sense of choosing only those field data that support the proposed hypothesis, and field work that is being conducted only in well-known social environment for the researcher, are potential obstacles for a productive use of this method. The text also promotes a deeper look into the theoretical instruments that are available for analysis of the field material.

*Key words:* ethnology, field work, method, Macedonia, authenticity, Boas, Feyerabend.

In the collection of articles “Ethnology of the imminence – poetics and politics of contemporary field research”, Ivan Lozica speaks about the polarization theory vs. praxis, in the context of Croatian folkloristic and ethnology, where field work is implicitly categorized in the latter section: “Theory and praxis/field are binary juxtaposed; theory is the spiritual (abstract) pole, while the field is the material (concrete) pole” (Lozica 2006 : 253), he says, commenting upon the historical development and the contemporary situation of ethnology in his environment. The development of Macedonian ethnology, due to a number of historical, political and other circumstances, can hardly be compared with the ones of the neighboring and regional ethnologies who, due their longer traditional and different treatment of this discipline at their territory, can more systematically detect phases and methodological “schools” that have shaped its evolution. There is almost no difference between “field-researchers” and “theorists” in Macedonia, since even those works that have a strong theoretical frame, and are not only simple re-telling of the collected material, still containing field research as an empirical basis of their analysis. However, in the context of Macedonian ethnology, one can raise few ‘hot’ questions related to field work:

- Has the professional profile of the Macedonian ethnologist been related to the one of a foreign traveler who, loaded with dictaphones, cameras and a pile of notebooks arrives riding a donkey to isolated

villages and registers the “exotic” ethnographic material that is doomed to extinction?

- Can an article that does not contain field references be considered professionally relevant? On the other hand, how much field material is *enough* for a certain research?
- Where is our “field” in contemporary circumstances? How much is the virtual field real, and how much can the real field be ambiguous, fluid?
- What are the methodological challenges faced by contemporary Macedonian ethnology?

Personal preparedness for field work, in the context of Macedonian, but also regional research tradition, is considered an essential part of education and craft of a professional ethnologist. Students are trained to structure a questionnaire, to “search for informants”, to approach interlocutors, to lead the conversation and to transcribe the recorded materials. The undergraduate curriculum of the Institute of Ethnology and Anthropology in Skopje includes an obligatory subject Field ethnology, that is taught in all four years of studies, and presupposes a collective stay and work on the field, which is usually taking place once a year, in May. Students develop a habit to relate their own professional identity to the image of an ethnologist who is not only physically prepared for 'survival' in different natural habitats, but who is above all a good “swimmer” in different social contexts – he has well developed communicational skills, he is patient, adjustable, empathic and “close to the people”. Such a self-constructed image spills from the academic area to the wider social context, so even the ones that are outside the ethnological ‘guild’ consider the field work of an ethnologist as essential for his profession, the same way laboratory work is considered ‘normal’, but also obligatory, for a chemists or a biologist.

Such a stereotype of the “noble anthropologists” (paraphrase of the stereotype of the researched noble savage man), who not only is obliged, but also ‘burns of desire’ to stay at the field, during the history of the discipline has often, purposefully or not, been deconstructed. The posthumously published field diaries of Malinowski, i.e. the racists and ethnocentric attitude of their author towards the researched “subjects” that they contain, have initiated a scandal in the frames of anthropology at the time of their appearance. While ones have tried to distance themselves from the “hate speech” of Malinowski, saying that he is an exception (*enfant terrible*) of the anthropological profession that suffers different psychiatric diagnosis (from being obsessed with sex to hypochondry), others have found that his diaries express a need for purposefull catarsis from the feelings that he, as a ‘normal’ member of the European white-skinned family, could not fully repress (L. K. Hsu 1979: 530). Avoiding the influence of such feelings upon his ethnography, he uses another, intimate discourse to express and canalize them. The analysts of his diaries have counted even 69 examples of different

levels of aversion towards local population, from being irritated to being angry and hateful (L. K. Hsu 1979: 518). At one point he even promotes the idea of their extinction: “At moments I was furious at them, particularly because after I gave them their portions of tobacco they all went away. On the whole my feelings toward the natives are decidedly tending to *exterminate the brutes*” (Malinowski 1967 : 69, italic in original).

After such statements, one simply wonders why Malinowski tortured himself doing what he obviously deeply hated, to be at an unknown terrain, among savages. The explanation is given by him – contemplating for a moment on the sense of his field work, he says: “I see the life of the natives as utterly devoid of interest or importance, something as remote from me as the life of a dog. During the walk, I made it a point of honor to think about what I am here to do. About the need to collect many documents. I have a general idea about their life and some acquaintance with their language, and if I can only somehow “document” all this, I'll have valuable material.” (Malinowski 1967 : 167). The need to register and to document, to *prove* something based upon field material – this is exactly what motivates ethnologists and anthropologists to travel and stay at the field, even when they don't really like it. “I hate traveling and explorers”, says another classic of anthropology, Claude Levy-Strauss, at the beginning of “Tristes Tropiques” (Levy-Strauss 1992: 15). If he did not tell us that himself, we would probably never have guessed how this man, who has devoted his life exactly to traveling and exploring, felt during his field trips.

When it comes to older authors, such openness seems controversial and scandalous is considered a symptom of ethnocentrism and racism of early anthropology. However, in contemporary anthropology auto-reflexivity of the researcher – describing his or her physical sensations and inner feelings during the field stay, even when those are an expression of distance and repulsion towards the researched context – is considered a “normal”, even an obligatory part of his/her work, a corrective of ethnographic narration, which in this way becomes more personal and more authentic. One of the recent examples, that refers to the experiences of a foreign anthropologist in Macedonia, is the book of Ilka Thiessen, “Waiting for Macedonia: Identity in a Changing World”. At a number of points in the book she describes the feelings initiated by different situations in which she participated with her informants, from those of empathy and friendship, to such that were not pleasant at all: “While I lived in Skopje, male friends came to visit me, not for a coffee and chat as I thought, but in order to tune in to the different pornographic movies that were playing. My loud protestations were heeded but not understood. They did not understand why I was upset, as their understanding was that I was from Germany and porn was a normal thing for me to watch. Also, I was their fiends, so why was I not ready to share the freedom of living on my own and my television, when they surely could not

watch such films with their parents and little sister at home.” (Thiessen 2006: 131). This facing with one’s own feelings serves as basis for analyzing the connection between consumerism and identity of young Macedonians, who want to prove themselves as modern and liberal, especially in the eyes of the woman-researcher from the West, where they would like to belong. Such and similar auto-reflective notes that refer to the field experiences are not merely illustration or footnote to the anthropological text – the same as ethnographic anecdotes, they become factual (and lively) confirmations of a certain statement, something that we experience or define as a proof, even if that was not the primary intention of the author.

The absence of a critical stance towards field material, when it is taken as an indisputable “proof” of how things are, is one of the dangers that are valid not only for younger, but also for more experienced researchers. The danger is at least twofold - if the hypotheses are established beforehand, interviews could be conducted in a way that supports them. Also, the material could be “filtered” in a way that suits our needs, marginalizing or fully avoiding the contradicting cases. Such a process of conscious/unconscious manipulation with “facts” is surely not characteristic only for ethnological science, but science in general. Still, the self-awareness of the ethnologists that this is a source of a potential danger, and his/her critical stance towards all methods used, especially the field one, which is a *differentia specifica* of the profession, is something that should be developed during the studies and cherished as a part of the professional praxis. Otherwise articles are doomed to be either simple retelling of the things heard on the field (in a type of a travelogue), or to be totally free of a personal attitude of the researcher, who in fact promotes the attitude of his/her interlocutors, without incorporating them into a theoretical frame, without interpreting them, commenting upon them, explaining them. Blinded by “authenticity” of the collected materials, and tired of the field routine, the researcher sometimes does not have the strength to confront them.

But who is in fact the owner of the material that we collected? Its treatment as “private property” by the researchers who collected it, and the relation towards this phenomenon of other researchers, is also an important ethical and professional issue. When an author quotes a statement of an informant, the footnote usually says: “Own field material”, which points to the fact that the author himself/herself conducted the interview in question. Although it seems as perfectly ‘normal’, such a formulation still implies a certain “ownership” over the material. It is surely “our” material, in the sense that we conducted the process of its registration, and have influenced, one way or another upon its content and flow, but this does not mean, in any way, that we have the copyright upon it - that we are the only ones who could use it. Although no one openly talks about it, the utilization of “other people’s” field material, for a certain research, in absence of a personal field

engagement, is not approved, even if the names of the ones that collected the original material have been correctly quoted. When I worked upon my dissertation “The Body in Macedonian Traditional Culture” (2009), I incorporated field materials of younger and senior colleagues, that were either unpublished up to that moment, or were not analyzed in the context of this topic. Although no one objected to this, I still felt a moral and professional obligation not only to quote the names of the ones whose material I used (which is perfectly normal), but also to “justify” my deed saying that it is a matter of a new “reading” of those materials. Besides, although there was no real need for it, due to the huge amount of related materials, I went to the field myself, so that I would not be accused of being lazy or not professional in this sense. In the context of Lozica’s statement, no one wants to be considered neither as an exclusive theorist, nor as an exclusive “practician” (i.e. field worker). The former stereotypically presupposes dealing with “higher”, abstract topics from the cozy atmosphere of the warm home, and the latter – roaming the roads, sleeping under a tent or in village houses, often having an unpleasant contact with the material – the dirty, cold and disagreeable. The dichotomy theory (abstract) : praxis (field, material) discussed by Lozica thus gains new dimensions – the theorist is a pure mind without a body, while the field worker is all physical, always equipped with a bottle of water, a wind jacket and tracking shoes. As every dichotomy, this one is also a caricature. Still, one always recognizes while being on the field the ones that like it and the ones that don’t. Even those colleagues that in principle want to go to the field retell about “border-line” situations, when they could not accept the field challenges. For one of our professors, for example, such was a situation when some village women offered her to try a pie from a metal dish from which everyone ate, while most of them had mouth herpes. Even the most “noble” of the ethnologists become un-noble at such moments.

Ethnologists from Macedonia rarely do field trips outside their country, especially not such that involve distant cultures and environments. This makes us more or less similar to regional ethnologies – ours, as well as the field adventure of our colleagues from, for example, Serbia, Croatia or Bulgaria, at first glance cannot be compared to the ones of Western travelers to Solomon Islands, Mali or Tibet. Still, even our field researchers bring a dose of excitement, or at least present a step outside the daily routine and an encounter with something new, and thus more or less strange (Pleše 2006: 119). One of the main reasons is lack of finances for traveling and longer stay abroad, but also our professional orientation to deal above all with our own culture, in all of its local forms. This is not negative in principle, but it impoverishes us, leaving us short of valuable experiences, that could be lived through only as complete outsiders in a certain culture. It is a fact that even in our own environment we are often the “Others” in relation to our informants - due to our education, sometimes due to our age, marital status, gender, and

even the language that we use, if it comes to a less familiar dialect, slang, or language that we are not fluent in (Albanian, Vlach, Roma etc.). Still, the geographical proximity to home, and the fact that we are in more or less well known social circumstances, makes us sufficiently relaxed and secure, which we might not be in different situations. On the other hand, Macedonia was, and still is, a “Mecca” for foreign researchers, whose insights of our own culture sometimes surprise us, sometimes make us angry, but very often enrich us with knowledge that we would otherwise never attain, due to our status of insiders, burdened with previous information and experience of the culture to which we belong.

As seen above, the classics of anthropology sometimes do not prefer to go to the field, but they still treat field work, that presupposes physical dislocation, as an indispensable method for gathering data on the researched culture. In the historical moment when this attitude was promoted this was completely justified - the systematical collection of ethnographic material was only beginning, and industrialization threatened to destroy a huge part of traditional practices, which had to be registered or otherwise would be erased from the collective memory forever after. But today the need for long-distance traveling and dealing with “classical” terrain is getting smaller. The relativization of the border between “virtual” and “real” terrains still does not imply relativization of reality and the relevance of researched problems – although allegedly separated from geographical territories, with a computer IP-address instead of a street and a number, with fake or real identities, the inhabitants of the cyber space cannot be separated from their mental and physical “rest” from the real world (Pleše 2006: 122). As such they, their opinions, feelings and everyday problems are as important and “authentic” research topics for ethnology as the opinions, feelings and problems of an inhabitant of Mariovo or Tresonche.

In this particular historical moment for Macedonian ethnology, especially if its ambition is to become anthropology (not only through a simple change of the term “ethnology” with “anthropology”, but also in an essential, conceptual sense), one needs to dismount the donkey – to start, on one side, with strengthening the theoretical basis of our research, something that could lead us in completely new, unexpected directions of reading the existing (“our own” or “someone else’s”) field material, and on the other side, to conquer new research territories, not so classical terrains, where processes important for our discipline happen on daily basis. Putting on disposal of the existing field material (in a form that would be easy to search and use), the critical attitude towards the field method, its deconstruction without an intention for destruction, as well as a deeper dealing with theoretical tools that are available, should be a serious challenge for Macedonian ethnology.

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