Jonathan Schwartz (Denmark)

INFORMANTS ANONYMOUS (IA)[*]

Abstract: Jonathan Schwartz is dealing with the problem of anonymity in anthropological research asking: why is the falsification imperative so important in anthropological "ethics"? Anonymity is considered as a norm in the contemporary anthropological research when disguising the true identity of informants is practiced even when there is no obvious reason for it.

Key words: informants, anonymity, anthropology, fieldwork, Lake Prespa region.

Imagine yourself confronted with a group of your informants, who suffered from being given false names and altered histories in your dissertations, so as to disguise their true identities.

To protect them, you might say in defense. "From what?" they might ask indignantly. In the interests of scientific objectivity, you might continue. "Is distortion really scientific? How so?" The members of IA would counter. No matter what you say to IA, the arguments would fall flat. Ethics of person and science of types are like irreducible numbers. Where is the common denominator? Most of us anthropologists would like to think that the informants "represent something" beyond their own personal selves. This is probably the case, and the IA persons would admit that as well. But why is the falsification imperative so important in anthropological "ethics"?

Why can't Ahmed be called Ahmed? Jens, Jens? Sara, Sara?

The trained historian in me has always been impatient with social scientific make-believe naming. Place-names and person-names should be rendered as perfectly as possible. If the name of a person spoken about in an action was unknown, this anonymity was part of the true story. Much of history is just this way: nameless, but no historian in his right mind would invent a name, if a person's name was known. Why do social scientists speak about "Cornerville" and not The North End of Boston? William Foote Whyte's book, "Streetcorner Society: the Social Structure of an Italian Slum" (1943, 1955) is intended to typify and thereby generalize young men's urban gangs. Danes today would say "second generation immigrants". Chick and Doc, his two main characters, his two types of leadership, did not accumulate symbolic, scientific capital because they were fictional names. Whyte's

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narrative science could just as well, if not better, have avoided any fictionality. White wanted to be a sociologist, however, not a historian. That's the difference that seems to make a difference.

Anthropologist's doing fieldwork in "primitive societies" had no scruples about naming exactly their chief informants, who could indeed be the chiefs in their own right. The chiefs maybe did not read, and their authentic names gave authenticity to the anthropological monograph back in Cambridge. If the chief could and did read, they were interested in the accuracy of details. The chief read the book as a personal history, not as a work of social science. To be included in a work of anthropology meant that one "went down in history", that is, one could be remembered by future generations, at home and abroad. The chiefs read the book, just like we do when we read an article by a colleague in our field. We look for our own names in the text and end notes. If our names and works are neglected or forgotten, we feel like joining the local branch of IA (Informants Anonymous).

Now I shall tell you why I am writing this, what led to it. In doing periodic fieldwork in the Lake Prespa region of Macedonia since 1977 and until 2003, I got to know several men whose livelihood was apple-growing and selling. Several of these men combined a salaried job with apple-farming. Substantial income could be gained from the apples, little from the salaries. Wages earned abroad were spent in orchard expansion and house construction. So far this is a piece of migration research. No names are needed. I could make up the place name to Appletown, or, to boast with a Macedonian conceit: Jabokesvo (Apple place). If I used a map, everybody would know this was the district on Prespa Lake, with guarded boundaries on Albania and Greece! So why to cover the real name?

Taking a risk, more to the profession than to the actual persons, I used the names of my male informants. I thought of them as historical persons in their own right. My informants were much older.

In my first book in which I write about Prespa Lake (Reluctant Hosts 1985), I use the names of several men who returned from migration abroad. I wrote short portraits of the men from Albanian, Macedonian, and Turkish communities in the Lake Prespa region. My intention was not to do distinct typing of each ethnic group. On the contrary, I was showing some of the common ground in the apple orchards, some of the experience of home-building after a return from life abroad, including America, Australia, Denmark, and Sweden. These men were usually at least ten years younger than I was, and yet their experience in migration and in apple-growing was vaster than mine. If not as heroic as Malinowski's "Argonauts of the Western Pacific", the Johnny Appleseeds of the Balkan did emerge as highly resourceful in my narratives.

If I dreamt about "the field" I appeared usually as a small boy or young man. In September 1979 one of the men, Kadri, took me hunting for doves in one of his fields. He gave me a chance to shoot at them. I missed both times. Secretly happy. When he downed a dove with the shotgun, it fell wounded to the ground. He asked me to wring its neck, which I did. Later in 1988 I dreamt an unlucky poem, in which the leading character is an Old Peasant, not Kadri who was fifteen years younger. Here is the poem as documentation:

Thirteen-line Dream with Old Peasant and Three Ducks

He is telling me that I must kill one of the three
ducks, two of them females, one of them male.
He is older than I, knows everything about ducks.
I follow his commands. He says I should kill
one of the females, as I suspected. I choose the one
towards my right hand, and just before I
reach for her, the male decides to mount her for the last time.
"Wait until he's finished," the old guy says.
I obey. Then, when it's over, I grab her by the neck,
close my eyes, and choke her with all my strength.
She kicks and flaps her wings, scratches me on my arms.
I am afraid, and the old man sees it. She is dead,
and he knows that I know nothing about life and death.

In 1985, Yugoslavia was still intact. Tito had died five years before, and most of my field work for "Reluctant Hosts" was done in a long summer of 1982, when I and my family lived in tents at a public camping place, Krani, on Prespa Lake. I had no reason to think in 1982 that inter-ethnic violence in Yugoslavia would erupt ten years later. Maybe I was "shy of conflict", maybe my informants didn't tell me the whole story, to protect me. In any case, I had little reason to protect them. They were civil, generous, and skilled persons whose experience could be told about without embarrassment.

When the war began in Yugoslavia, I was of course worried that it would not stop and that it would spread to the Prespa Lake region. My informants and friends would be involved, and many would die. I would have to face what Tone Bringa faced when she returned to her village in central Bosnia. The TV film: "We are all neighbors" (1993) is in the series "Disappearing Worlds", a poignant document of a remembered, lost time in the ethnographic field.

My second, detailed study of the Prespa Lake region was based primarily on a three-month field work in early 1995, published the year later as Pieces of Mosaic: an essay on the making of Makedonija. Here again I kept

the real names of the people I wrote about, because their history of not going to war with each other, I thought, should be told as best it could. That meant no fictive names. As persons who watched every evening the war in Bosnia on their TVs and who wanted no such war to come their way, I wished to show these persons the maximal respect of a historian and ethnographer.

Kadri, my coach in bird shooting, now become the central person in Pieces of Mosaic. It was in his yard that I observed, and slightly participated in, the laying of a mosaic path from the road to his front door. The symbolism of mosaic became for me the spontaneous and reflective finding of the right pieces to fit the right places. It was the making of the new mosaic in Kadri's yard that interested me, not the ancient Byzantine mosaic monument or the modern Canadian multi-ethnic mosaic.

In February Kadri was going to plant 130 Ida Red saplings in one of the family properties. I asked to come along and help. As usual, Kadri wanted to have fun while working. He had gathered eight men - Turkish and Macedonians he noted to me - to do the work, which took a whole morning, maybe five hours. Afterwards we went back to his home, where his wife and daughter served us a huge lamb dinner. This exchange of labor for a good meal is called a moba in Serbo-Croatian and a gilde in Danish. In Pieces of Mosaic, I included a photo (by Kirsten Rønne) of Kadri serving the lamb (s63). In October 1996 I and Kirsten returned to Prespa where I gave copies of my book to most of the informants/friends who made that book possible. Including, of course, Kadri.

A couple of years later I heard in Copenhagen (at a Prespa soccer match) that Kadri had recently died of cancer. The next time I went to Prespa I paid a belated condolence call at the home of his wife and daughter. Sevda, hid daughter, said to me how much she and her mother appreciated the photo of her father and what I had written about him.

My immediate thought was: what if I had given Kadri another name? How could I explain this alteration to his daughter? Science? Protection of anonymity…in case of an inter-ethnic war? Do paramilitary men read ethnographies before they go off to slaughter? So they can pick their victims?

I sometimes think that the trouble some colleagues go to to anonymize their informants serves as self-glorification of our professional importance. If we want to speak face-to-face with the persons in the field, than we have little ground to disguise their identities. If they ask for discretion, we owe it of course. Professional ethics, like ordinary ethics, depends upon mutuality and trust. This essay is in Kadri's dear memory.

Ilina Jakimovska (Macedonia)

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 ethnocentristic 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 know social circumstances, makes as 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 researches, 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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Karolina Bielenin-Lenczowska (Poland)

MY ANTHROPOLOGY IN THE REPUBLIC OF MACEDONIA[1]

Abstract: The paper is divided into two parts. In the first one, an author analyses her changing position in the field, highlighting a gender and maritial status. Positionality not only gives various access to interlocutors but also defines or even determines her subjects of research. In the second part of a text, a discourse on Torbeshi / Macedonian Muslims is analysed. An author observes, this group is exposed to strong propaganda from Turkish, Albanian and Macedonian perspectives. She argues, this propaganda is also grown by Macedonian scholars for whom Torbeshi are Macedonians who converted into Islam during Ottoman Empire, while there are very few research on their own changing and fluid identification.

Key words: positionality, gender, fieldwork, Torbeshi, discourse.

Having done fieldwork in Republic of Macedonia for ten years, I face with different methodological (especially ethical) problems during the fieldwork as well as with ethnographic issues researched by Macedonian scholars. This both questions I would like to describe and discuss in this paper.

As a student I carried out fieldwork in central and Eastern Macedonia (2000-2004), while from 2006 I have conducted research in western part of the country. I do not belong to any research team in Macedonia, nor am I financed by any Macedonian institution; I work as completely independent researcher, although I have regular contact with Macedonian scholars and use their publications. During the fieldwork I use Macedonian language, even while interviewing Albanians or Turks (I have very basic knowledge of Albanian). My latest subject regards labour migration of Macedonian Muslims (Torbeshi) to Italy. Probably my theoretical and methodological conclusions are nor unique for Macedonia, I do not however personal experience from the other places. Therefore the following statements are very subjective and personal.

The paper is divided into two parts. In the first one I will try to analyse my position as a foreigner and woman in the field while in the second, an academic discourse on ethnicity and national affiliation of Macedonian Muslims (Torbeshi) – a group whose mother tongue is Macedonian, while a confession – Islam.

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[1] My deep thanks go to an anonimous reviewer who pointed me several, very important issues related to Macedonian scholarship.

I. Status of an anthropologist in the field – three cases

At the beginning of a methodological reflexion it is worth to note that doing fieldwork in the Republic of Macedonia, alike in the former Yugoslavia and Bulgaria as well as in Eastern Europe, including my native Poland, consists of short visits in the place of research. It makes observing of everyday life almost impossible. In principle only interviews based on questionnaire are carried out, observation is more difficult to make in such short period of time. I try to change this tradition and spend as much time in the field as possible. Observations and writing regular diary are my most important tools.

Two questions have been significant for gaining my anthropological knowlegde: to be foreinger or rather to be Christian and to be a woman. While I was not declaring my religious affiliation, interlocutors aceepted a priori that if I am from Poland, I am a Christian. When I was researching Orthodox Christians, they highlighted my Catholicism, but for Muslims I was just "Christian". Actually, I tried to avoid express openly my religious and political views. Then, depending on my status (unmaried student or independent university scholar, and married mother), I had different groups of interlocutors with whom I could discuss various subjects. One of the most inspiring texts for preparing this paper is a chapter by Diane Freedman (1986) who analyses her position in Romanian village and its changes. At first she came with her husband, then she returned as a young widow and – finally – as a young woman, suitable for re-marriage. All this statuses gave her access to various groups of interlocutors as well as determined (or at least suggested) researched subjects. The second inspiration for this text is an essey by Kirsten Hastrup about her experiences during the fieldwork in Island. She writes very distinctly: "Sex of the anthropologist, elsewhere so inconspicuous in relation to other and much more marked differences, becomes a primary element in the local classification of the ethnographer" (Hastrup 1985: 96).

First case – unmarried girl

Jozef Obrebski in his best – in my opinion – essay "Scandal in the village" (2005) notes that for a long of time he had had a problem with the access to a subject: magical practices. Only non-everyday occurence allowed him to gain some knowlegde as people openly talked about magic, explaining this way an elopement of a young girl from the village. My very first ethnographic research is connected just with magical practices. For my BA thesis I was interested in healing activities within the Orthodox religious practice. So, I was interviewing especially elder women who were healing through praying or looking at the icons. This time I was about 21-22 years old and I was unmarried. Probably this factors allowed me to got some pieces of information about it. I was told, mothers handed on some of their knowledge

to daughters at their age of 14. Then, next lessons daughters get shortly before the marriage – mothers teach them how to deal with a new family and especially with mother-in-law. I was too old to get first part of the knowledge and unmarried, so I could know some practices but, according to my interlocutors, in this czase they would not have any magical power. Nevertheless, a magic is exclusivelly related to women, thus, probably, it was the reason Obrebski's informants were reluctant to talk to him about it.

The same status but in a completely different circumstances I had during my next research. For MA I invented more "male" subject – the cultural role of rakia. In traditional Balkan society women (especially girls; for elderly ladies there were some exceptions) did not sit at the same table with men and did not drink alkohol. Above mentioned Obrebski in his paper on social and customary structure of Macedonian village notes: "She [a healer – K.B.-L.] gains privilegies belonging (...) only for men. She can mingle with men at every public meetings (...) she can sit with them at the table, drink rakia with them (...) and even she may venture to use vulgar language that only men are entitled to" (Obrębski 1972: 209-210). When I was asking about this gendered "table division" in contemporary society I was told that the situation has completely changed. My interlocutors admitted nowadays there is no gender difference, women are feminists etc. However, once I noted in a diary: "Prilepec 17.09.2003. Hostess poured some rakia to her husband and to a neighbour [male, in Polish *sąsiad*], but to me and herself she gave some juice. All of us was drinking coffee as well". It is one of the best my example of importance of observation of people's practices, not only interviewing them.

Second case - "**profesorka**"

Some years later I brought students (predominantly female) to Macedonia. Althogether we spent in one municipality in Western Macedonia – inhabited by Muslims of different national / ethnic affiliations – ca. two months. A group of ten girls in a village caused numerous suspicions and rumours. We had a lot of problems connected with our sexuality. It was related to perceiving us as "alter" women, ergo Christians who are supposed to be sexually liberal. Such assumption about our faith was supported by our nationality (Pole means Christian or Catholic), dress, and – indeed – behaviour. Certainly we did not cover our heads like most Muslim woman, but we tried to dress very modestly (although it was very hot!). Then, we spent time in public, i.e. a male sphere, talking to people and drinking coffee in the bars. Automatically we were classified as "emancipated". Not only did our male informants cannot understand we were anthropologists and not turists, spies or prostitutes, but also they assumed we were more "sexually accesible" (it is a term of Kirsten Hastrup (1985), she mentions her problems with fishmen in Island) than *their* women, Muslim. Nevertheless, our

behaviour was evaluated as we were Muslims. In this place I need to make short diggression on gender division in this "Muslim Macedonia".

Alike in a traditional Balkan society, a strong division between male and female spheres takes place. It means, women and especially girls are hardly seen in public places, like cafes and bars. Most of them do not work at a salaried job, i.e. do not work outside home. They are responsible for keeping house and bringing up children. Girls are not allowed to go out without a permission or accompanying of a male member of her family. Therefore, cafes and bars are fulfilled by men, while women meet with their relatives and neighbours only in the houses or courtyards. There is a good word defining this situation – *shetanje*. Litteary it means "walking around", but contextually it can accept different meaning depending on an actor or rather on the gender of him/her. Thus, Macedonian bachelors are expected to *shetanje*, as they can present themselves to girls – potential spouses, while girls' *shetanje* was connected with their reputation and gained strong sexual overtone. Married women who *sheta* was condemned for spreading gossips. And, certainly, *shetanje* is forbidden for girls, they can present themselves only through male member of the family.

This time I was not a student, however. I was *profesorka*, that means a teacher, educated woman. Therefore I could get some access to the authorities of the village, like the head of local primary school. I interviewed him twice and got very interesting information, nevertheless I was still exposed to his very intimate questions and open date offers. Such situations forced me to use ethical-doubtful strategies: I was lying that I was married or at least engaged.

Third case – wife and mother

Next year and two years later I came to the field, already married and with a 10-months-old baby boy, accompanied by one female friend. We were renting a house next door to the hostess house. It totally determined my group of interlocutors. In principle, my contacts was limited to one family and I was treated like a fifth daughter-in-law of my hostess. Probably that is why I was treated not as a foreign woman but I included to the value system of the society and my behaviour was evaluated in its categories. Therefore, although only implicite, I was suspected about dissipation and being not a good hostess, wife and mother. What does it mean? I was travelling alone (without husband), thus I was meeting strange men. Then, I came with a little son, therefore I was very irresponsible. However, every day I was receiving a lot of pieces of advice how to bring up my child, including breast feeding. Certainly I had much easier acces to female part of the society, but actually I did not have an opportunity to interview men. For married men I was potential mistress while for bachelors I was simply not attractive.

II. Torbeshi in Macedonian scholarship

In Macedonian scholarship is common that particular ethnic groups are researched only by this groups' representatives (or by foreigners). That is, Macedonian scholars study above all their own group – Macedonians, both Orthodox and Muslims, while Albanians research Albanians, Turks research Turks etc.

Republic of Macedonia is a multi-ethnic and multireligious country. The largest minority constitutes Albanians (it is depending on statistics 20 up to 40 %), following by Turks, Serbs, Roma, Vlachs. After an armed conflict between Albanians and Macedonians in 2001 a Framework Agreement was signed in Ohrid and all ethnic and national groups in Macedonia was concerned as equal and called a "community". Therefore, nowadays in political discourse in Macedonia neither terms "minorities" nor "nationalities" are used. Other differentiation is religious – Orthodox and Muslims. In the latter group are Albanians, Turks, most of Roma and and the subject of this presentation – Macedonian Muslims. I will elaborate briefly a situation of this group in Macedonian society[2].

Macedonian Muslims, named also Macedonian-speaking Muslims, Isamicised Muslims or – most popular – Torbeshi – are people whose mother tongue is Macedonian, while confession – Islam. The name Torbeshi is used to describe all Macedonians confessing Islam in the Republic of Macedonia but this ethnonym originally referred only to those from the areas of Debar and Reka, and from the North West part of Macedonia. Because of its etymology, the term Torbeshi is quite often perceived as pejorative. There are several theories about the origin of this term, but the most popular etymology is related to a religious identity and describes the Torbeshi as those "who sold themselves for a sack of cheese" (or, in another version, flour): they are said to have sold their Christian faith to the Turks. Thus, in a scholarship either the term Macedonian Muslims or Macedonian-speaking Muslims, or Islamicised Macedonians are used.

The Torbeshi themselves are not unanimous in self-identification. In part they declare their affiliation to the Macedonian nation, in part they consider themselves as an autonomous ethnic group, while some derive their origin from the Ottoman Turks. Sometimes they declare themselves to be just Muslims (referring this way to a category of "Muslims" as a nationality – introduced in 1960. by J. Broz Tito) or Torbeshi as separate ethnicity. In the latter case the term Torbeshi does not have negative connotations; in contrary – is the only one used. Religion associates Torbeshi with Turks and Albanians. Therefore, depending on the political context, they are regarded either as Turks or as Albanians. Moreover, they start to declare themselves to

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[2] See more about this topic: Bielenin-Lenczowska 2008.

be Turks or Albanians while do not speak this languages. This new national shifts are connected with their living and being marginalized in ethnic Turkish or Albanian surrounding. Language and numerous customs or loyalty to Macedonian place them close to Macedonian majority. Thus, every group wants to incorporate the Torbeshi in order to increase the number of Macedonians, Albanians or Turks living in the Republic of Macedonia.

I will argue, the interest on research Macedonian Muslims is connected with their willing of incorporation into a Macedonian nation. Scholars as well as Macedonian politicians talk about Torbeshi's politicisation in Macedonia, i.e. Albanicisation or Turkicisation (eg. Svetieva 2003). But also Macedonians want to increase a number of their own nationality and include Torbeshi because of their language[3].

Under the Ottoman Empire, religion was the only important factor defining identity. The *millet* system segregated the population into Muslims and non-Muslims: all Muslims were considered "Turks" and all Orthodox Christians "Greeks" (certainly, we cannot relate those names with a modern notion of nationality). During the time of socialist Macedonia they were first treated as ethnic Turks, then (in the 1950s and 1960s) they were encouraged to adopt Macedonian national identity. However, only in the late 1970s more serious and coordinated attempt to integrate Muslims in Macedonian majority was undertaken and an organisation of Macedonian Muslims was established with the support of Macedonian branch of Yugoslav League of Communists who wanted to diminish the influence of the Albanians in Western Macedonia.

Nowadays, Islam in Macedonia is predominantly associated with Albanians. Both the Torbeshi and the Turks complain that they have been assimilated by the Albanian majority via the Albanian language used in worship in mosques, Albanian hojas and representatives in Islamic organizations, etc. Moreover, there is a tension between Albanians and other minorities linked especially with the social memory of armed conflict of 2001.

In Macedonian political and academic discourse regarding Torbeshi, this group is consider to be Macedonians who converted into Islam during the time of Ottoman Empire. When the state established an organization under the name "The Culture and Science Centre of Macedonian Muslims", its official line was: "Torbesh are local people, i.e. Macedonians; but the Ottomans converted them forcefully". In Macedonian official discourse, opinion about forced Islamisation is widespread – after all, in numerous academic publications about Torbeshi the term Islamised Macedonians is used. Ethnologist and Macedonian Muslim, Nijazi Limanoski, in his writings, understood Macedonianess through language, customs, origin and folklore

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[3] One of the very few distinguishing and reflexive papers on this issue is an essay written by Ljupcho Risteski (2009). Text is published in Polish but I hope it will be translated into Macedonian.

common to all Macedonians. Islam is in his opinion additional feature and had not deeply changed the social and spiritual life of the Macedonian Muslims. Islamisation and isolation of Torbeshi from the rest of the Macedonian nation is described as the beginning of a heroic path and one full of sacrifices for the Muslim population of Slavic origin on the territory of Macedonia, the end of which will be the recognition of the Macedonianess of this population by the rest of the Macedonian nation. Nowadays, only part of Macedonian Muslims agree with Limanoski. Rather they do not want to diminish the role of Islam in their collective identity. Moreover, there are strong struggles for proving their ethnic distinction. It is one of the goals of a Party for European Future (*Partija za Evropska Idnina*) established in 2005. Their representatives claim, their *nationality* is a Torbesh. They also try to show that they are descendants of the bogomils in the Balkans, thus not forced islamicised Christians and the term "Torbesh" means a person who carry bag (with books, as they were missionaries).

Conclusion

Issues and problems in the field presented in this paper are certainly not unique for Republic of Macedonia. It is obvious that foreign, and especially independent researcher has many problems with presentation her/his intentions and can be admitted as a spy, prostitute, tourist etc. It is well written in anthropological literature alike role of a positionality (gender, age, social status) in the field. As I showed, my changing status defined my group of interlocutors as well as determined – to some extent – researched subjects. My current professional interest, i.e. working with Muslims, regardless of their national affiliation, evoked suspicions among Christians who perceived me as pro-Albanian and pro-Muslim. Because of political tension between Macedonian and Albanians, I had to use some strategies in order to avoid presenting my political and religious views. The other problem is an academic ideologisation of the group researched – Torbeshi. Interest in research them in Macedonian scholarship is connected with their incorporation into Macedonian nation. The only official version of nationality of Torbeshi is that they are Macedonian who convert into Islam during the period of Ottoman Empire. Their declarations of different affiliation like Albanians or Turks are explained in categories of politicisation – Albanicisation and Turkicisation. It means, Torbeshi, because of their religion, are exposed to Islam or rather Albanian or Turkish political activities. But in fact also Macedonians, including scholars, grow the same propaganda.

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ACTION AND CONTEMPLATION

Abstract: This article is a draft for a personal training in reflexivity. New tendencies in Macedonian ethnology and anthropology, among other things, turn attention to certain aspects of cognition. Field research (understood as action), as well as observation, deliberation, dealing with one's own field work (contemplation), are perceived as an opportunity for facing oneself and for self improvement.

Key words: field research, reflexive ethnography, Macedonia, anthropology.

Starting from the '60-ties of the 20th century, anthropological science starts to focus upon the personal character of ethnographic research. It is a matter of descriptions and conclusions that the anthropologist makes after critical observation and analysis of one's own work, especially his field research of sensitive topics. One needs to deliberate how and how much does the researcher, having in mind his cultural background, influences the subjects of his field research, as well as the analysis and the presentation of the results. Also, a reversible character of this process has been noted, and this includes the influence that is exercised upon the researcher by his interlocutors, through the relations established during the research. Such contemplation of the ethnographer in regards to the used strategies, to one's own dilemmas, the solutions of certain problems, especially when it comes to his ethical attitudes and acts during the research, as well as the presented results, is called reflexivity.

During the early 70-ties, Nash and Winthrob (Nash and Winthrob 1972) published an analysis of the anthropological trend – a "self-awareness turn" towards the ways in which they construct the subject of examination, in which they define research problems, gather data and interpret them. During the 1990ties reflexivity has been adopted at a level of regular praxis, and its main axiom is that the good contemporary anthropological book or article should contain a confession-like introduction. Personal histories of the researchers, their identities, theoretical interests, the influence of the mentor, colleagues and the notions of the public to whom the ethnographer is addressing, the types of inter-human relations – all these individual topics are categorized in the class of "anthropological reflexivity", that becomes a legitimate/indispensable element of the researches according to Milenković 2006: 165).

Favorite elements of anthropological reflexivity include: the influence of the anthropologist upon the examined subjects, the choice of a topic and space, the transformative character of the field work upon the anthropologist himself, and accordingly to this, the differences that arise in the descriptions of reality by different anthropologists and other social actors (Milenković 2006: 164).

The reflexive approach has initiated a number of polemics among authors that belong to different sciences and schools of thought. All of them, through their pro and contra arguments, enriched the theoretical and methodological aspects of cognition. Anthropological articles treat reflexivity along with hermeneutics (Milenković 2006: 169), and even more often with ethics (Zebec 2009).

Reflexivity in Macedonian ethnology and anthropology still has a status of a potential. I hope that the articles in this issue of the journal will initiate a wave of reactions and constructive discussion and will present an affirmation of the reflexive practices in our environment.

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Since I am not really into the numerous theories, schools and conventions regarding reflexivity, I will try to give my contribution to the establishment of the reflexive method in Macedonian ethnological/ anthropological science through sharing few examples of reflexive thoughts that stem from my personal experience, related to research of ethnic identity. My primary idea was to conduct a research on the feeling of ethnic affiliation of people that live in Skopje, but originate from ethnically mixed marriages. Basic questions referred to what is their essential, personal ethnic i.e. national identity versus the formal, declarative one; which factors influence the creation and the changing of the expected multiple identities; how do the subjects balance between the one and the other.

Since Skopje is a multi-ethnic environment, I counted on the big number of persons whose parents belong to different ethnic groups. This proved to be true, but it was my first mistake. The high number of members of different ethnic groups created an even higher number of "combinations" of ethnically mixed marriages. Without taking into account the first mistake (typical for beginners), and that is the huge scope of the research topic, I entered a second mistake. It consisted of a selection of a wrong, anthropologically improprate method – a poll (!?). Although the poll questionnaire was detailed, voluminous, with a possibility to note personal attitudes, it could not give satisfying answers to the questions raised. The knot that I tied myself was solved through 1. Scaling down the scope of the research, through choosing to test the hypothesis of the multiple ethnic identity via statements of the descendants of the refugees from Aegean

Macedonia and 2. Using as a technique a detailed, semi-structured interview. These changes enabled me to realize the field research according to the plan (Павковиќ 2005).

I also made interviews with descendants of former children-refugees, that were victims of the exodus of the Civil war in Greece. Informants were 35-45 years old, and living in Skopje. They originate from ethnically mixed marriages – one parent is a Macedonian from Aegean Macedonia, while the other is from the country where the refugees were taken (Czechoslovakia, Yugoslavia, USSR, Poland, Romania, and Bulgaria). All of them were born in these countries and have spent their childhood or their youth there. This enabled to conduct the talks in a friendly atmosphere and to gain mutual trust in the general communication. The openness was additionally induced by the note that their statements will be published as anonymous. Most of the interviews were done at the informants' homes, and a smaller part at their working places. Five male and five female informants were included, but women were more eager and more competent interlocutors. They were pleased to be talked to, concerning a topic that they liked and that refers to the "Aegean question". They also expressed satisfaction that through the conversations, through re-telling their lives and the stories of their parents, in a way they succeeded to articulate certain previously non-defined thoughts and unclear feelings regarding their own identity.

These statements pleased me, since I experienced them as a proof of the good choice of the topic, the concept and the interlocutors, which after the lousy start of the research was inspiring. The pleasure was even greater since the researches confirmed that it is "normal" to have a fluctuating ethnic identity, and that this could be experienced as an advantage, which was in fact an intimate question of knowing myself, since my personal origin is from an ethnically mixed marriage. Almost at the same time I was disappointed by the refusal by few persons to participate in the research. Probably I felt their refusal as mistrust regarding my responsibility, ethics and competency. However, all these episodes, the pleasant and not so pleasant ones, are invaluable experiences in building the research profile of each ethnologist/anthropologist. Cabinet anthropology offers greater comfort in comparison to field research that is always uncertain and demanding. Maybe the answer is in their consequent implementation and unification.

The selection of statements that are presented from a relatively small number of informants (Павковиќ 2005) is a result of my preference of individual anthropology and perception of each person as an individual story (Ковачевиќ 2006). Thus the extended statements are given due to registering and representation of the multitude of the individually created meanings of the terms: ethnicity, nationality, affiliation, origin, culture and others. Still, due to the surprising similarities of personal histories (which in a major part developed in different, distant territories), which was a reason for subsequent

similarities in perception of the key terms, I could not resist to essentialize the statements and the generalizations of the conclusions.

<div align="center">*</div>

Although I share the doubts of Wolf (Wolf 1992: 50 and further on) regarding objectivity of reflexive ethnography (1. it is original only in relation to traditional ethnography; 2. it is not clear which is the target group; 3. personal character of ethnographic experiences does not fit academic standards and 4. experimental texts are valued more due experimentation itself than due to the new and different insight into the researched cultures), still I think that the practice of our discipline should walk its evolutionary path (even if it takes short cuts) due to continuity and gaining maturity.

I am skeptical about reflexivity due to general human vanity, and thus the vanity of the ethnographer/ethnologist/anthropologist. I think that the offered reflexive insight could be dosed and could be manipulated.

The biggest advantage of the usage of the reflexive method is gaining awareness on the modification of the cultural personality of the researcher after the field research, since each interlocutor/informant that enters our orbit carries a different and more complex world from any other that we can imagine. Hearing the details and subtleties of their experiences, their opinions and perceptions, we include a part of their world into ours. This is probably the biggest value and importance of reflexivity in field research.

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