

# Participant Observation, Inclusion, and Language Skills

## On the Authenticity of Anthropological Fieldwork

DOI: [10.54742/tabula.2020.1-2.05](https://doi.org/10.54742/tabula.2020.1-2.05)

1. In both domestic and international anthropological and ethnographic research, fieldwork is one of the most substantial instruments to collect information and has become, often openly, the primary source of legitimization of an anthropologist as a researcher. [1] In modern anthropology, the completion of fieldwork provides an anthropologist with the right and the opportunity to contribute to the academic discourse inquiring into a particular problem of ethnographical relevance. Therefore, we should not only examine under what circumstances, using which methodology and in what amount researchers collect texts, observe procedures and participate in rituals, we also need to discuss how the data gathered during fieldwork contribute to the accomplishment of specialist studies.
2. Reports on the circumstances and methods of fieldwork in monographs and studies focus primarily on supporting data collecting and study writing authority and authenticity (Casagrande 1960) in order to prove that the authors find some (not necessarily the only) interpretation of the social understanding of the community in question (Crapanzano 1986:51). It is much less common upon demonstrating the circumstances of ethnographic collecting activities to indicate what restraints the collector's presence and the fieldwork location represent in the accessibility of information. In this article, critically approaching the literature on fieldwork methods, and with hindsight to my fieldwork experience in Yakutia, I intend to discuss three issues of empirical data collecting that raise questions on the epistemological status of field studies in anthropology.
3. The first issue focuses on the connection between the procedure of anthropological fieldwork and the writing of academic studies. The completion of fieldwork is not a precondition for an author to discuss issues of anthropological relevance, after all. We can find precedents for the omission of fieldwork not only among the classics of anthropology but there are also present-day cases, for example, anthropological descriptions based on imaginary fieldworks, while authors of summarizing theoretical works, upon discussing a particular anthropological issue naturally rely not only on their own collections but also on data published and processed by others. So the question is rather *why* and *how* the completion of fieldwork authenticates the opinion of an anthropologist, and by what means an anthropologist creates this authenticity.
4. The subject of the next issue is precisely the problem of creating authenticity. Upon describing anthropological fieldwork (in addition to indicating the data collecting techniques applied), researchers occasionally refer to the moment (or even ritual) of acceptance or inclusion, i.e., the event, following which the anthropologist is taken into the confidence of all or particular members of the community

studied. From the moment of creating this harmony, the anthropologist's observations are less exposed to distractions or influences caused by the shyness of locals or by the various techniques of conscious concealment. To put it differently, the data collected by the researcher seem more authentic and accurate from this point on. In what follows, I will argue that in many cases, the anthropologists' reports on inclusion should be regarded as acts of rhetorical self-legitimization rather than genuine turning points (in terms of epistemological interpretation).

5.

Finally, I will discuss a third aspect of the authenticity of fieldwork, the pitfalls of using a foreign language. Ethnological fieldwork, in the vast majority of cases, is performed in the language of the local community, which requires the knowledge of that foreign language from the anthropologist. While in the methodology of anthropological fieldwork, mastering the language or the particular linguistic register of the local community has already been a fundamental requirement for some time, many anthropologists still tend to work using the assistance of interpreters and translators. At the same time, there is little to no methodological literature on carrying out field studies with limited language skills. Furthermore, language skills are just necessary, but not the only ingredients of well-grounded fieldwork, and even familiarity with the local community's language (not to mention its limitations) may give rise to numerous problems and misunderstandings during fieldwork. Bringing examples of the collecting processes in Sakha language during my fieldwork in Yakutia, I will demonstrate the opportunities and limitations of fieldwork performed in a foreign language.

## Fieldwork and Participant Observation

6.

The connection between the *act* of fieldwork and the *result* of ethnographic processing is a close one, but by no means seamless (Fabian 1983:71–73). This fact is what the English terminology of anthropology is reflecting upon when using the term *ethnography* with a double meaning: on the one hand, as the general 'descriptive' introduction of the studied community; on the other hand, as the respective process of data collecting (Sanjek 2010:243). The two meanings are in a specific relationship. The point is not simply that the former (the act) defines the latter (the result) since it is usually assumed that their relationship has a successive nature, much rather that they mutually define and legitimize each other. [2] After all, it is not without some indirectness that fieldwork material becomes an ethnographic description, it always involves the academic tradition and taste that will determine even the conditions of articulating valid anthropological questions. This tradition can determine such narratives of interpretation as the problem area of the lineage as a political entity in Africa or of the shaman-hunter relationship in Siberia (Herzfeld 2001:22).

7.

The question in this relationship is how legitimacy is established through the completion of fieldwork and the processing of the information collected. In this legitimacy relationship, it is not surprising that fieldwork is occasionally regarded as a provisional rite de passage in anthropology (Wattson 1999:2), by which young anthropologists are initiated into the group of experts and specialists before the scientific community. Other researchers regard fieldwork as the distinguishing mark of anthropological research in contrast to other social sciences like sociology (Amit 2000:1–2).

8. The relationship between fieldwork and anthropological data production can by no means be regarded as permanent. It aligns the norms and rules for carrying out fieldwork with the interpretational framework of anthropological epistemology characteristic of a particular era. Roger Sanjek, for example, describes the model of acquiring anthropological knowledge based on three factors. From his perspective, the three factors of knowledge production are *comparison*, *fieldwork*, and *contextualisation*. These factors establish the process of acquiring anthropological knowledge based on historically different roles, creating research methods and schools. According to Sanjek, the working method of stationary fieldwork as represented and legitimized by Bronislaw Malinowski was preceded by a school hallmarked by the method worked out by Franz Boas that focused on the comparison in space and time of the fullest possible reconstructions collectible from the so-called key informants. The effort of collectors working according to this method to garner reconstructions is well characterised by the note of Boas, in which he complained at a potlatch that he could not find an informant he could continue his collecting process with (Boas 1969:38). Contextualisation, the third element of the epistemological triad outlined by Sanjek is preferred by the interpretative research method of anthropology that follows researches emphasizing the role of fieldwork. The assertion of contextualisation in the course of fieldwork creates a situation in which the anthropologist, rather than merely being present at the place of the data collecting process as some objective spectator, he also interprets, creates, and reinterprets data *immediately* and *on the spot* through his personal involvement (Sanjek 2010:243–244).
9. Neither of these approaches excludes the validity of the other two during the data collecting process, and although it is participant observation based on stationary fieldwork that creates the dominant narrative of interpretation for anthropological fieldwork today, a wide range of other qualitative and quantitative collecting methods can be applied by ethnographers in fieldwork (combined with participant observation where necessary).
10. The relationship between fieldwork and ethnographical processing does not only depend on the fieldwork methods selected by the researcher but also on the intended role of empirical data collected during fieldwork as used by the anthropologist in his academic argumentation. From the aspect of the argumentation and proof procedures of ethnography, fieldwork is nothing but a case study in a given and relatively limited time-frame, after all (Gerring 2007:2–3). While argumentation and proof by case study as a method is standard in social sciences, it raises several issues. On the one hand, it needs to be explained how general observations can be made on the basis of a case study which, by its very nature, contains too many variables and too few examples, i.e., how the findings of case studies can be connected and compared. On the other hand, from the aspect of ethnological research, it is even more important to ask how a standard methodology for the implementation of case studies can be developed that allows the comparison of the data collected. A standard methodology in anthropology is difficult to apply since every community, and every field represents different options for the researcher. This methodological difficulty could also lead to the view that, since there are no uniform methodological considerations for case studies or they can be interpreted very freely, anthropology and ethnography based on case studies (fieldwork)

should be classified as non-systematic, non-rigorous, and a less scientific social science (Gerring 2007:7).

11.

From the epistemological aspect, fieldwork, rather than defining an ethnographic/anthropological *method*, sets the *objective* of the research within the limits of the respective research area. This also means that information, data collected through fieldwork may not necessarily be comparable. For the more intensive fieldwork is and the more methods the anthropologist uses for his collecting activity, the more difficult it becomes to find two ethnographic descriptions prepared using a similar methodology. Of course, this also entails the danger of ethnographic texts describing separate worlds that researchers can only define the *internal* principles of (cf. James Hockey–Dawson 1997:5). Another problem is the unrepeatability (irreconstructability) of anthropological case studies in practice.

12.

Anthropological research tried to provide a variety of answers to these undeniable epistemological problems, of which perhaps the consolidation of the data collecting methodology of participant observation is the most important one. The primary focus of participant observation is for the researcher to get close to the subjects of his research and cooperate with them to collect information about them. Nevertheless, this behavioural strategy may occasionally entail deception and pretence (Russell 2006:342) that, upon processing and reviewing recorded data, all ethnologists need to be aware of.

13.

Based on the above, it may have become evident that participant observation, rather than being the sole valid method of anthropological data collecting, is just a concept, the validity of which is not less limited than that of any other data collecting method. Nevertheless, this is the method that typically provides the authenticity of anthropological fieldwork. The development and legitimacy of the participant observation method is a historical phenomenon that, after establishing the professional community of ethnographers and anthropologists, soon divided observers into professionals and non-professionals. According to this paradigm, real researchers understand the culture of 'savages', of 'others' differently (more deeply) than travellers, missionaries, colonial officers or merchants (Pelto, P. – Pelto, G. 1973:241; Ben-Ari 1999). Such a paradigm establishing legitimacy is usually associated with a person (a 'founding father') whose personal credibility can be used to support the validity of the method (Pratt 1984:27).

14.

The paradigm of participant observation and stationary fieldwork is associated with Bronislaw Malinowski and the school of functionalist thought that established the legitimacy of this method (Dewalt et al. 1988:262–263), although anthropologists, for example, Frank Cushing among the Zuni people between 1879 and 1884 (Evans 1997:718–719) and even sociologists had already employed a similar method before (Russell 2006:346). The paradigm of participant observation based on stationary fieldwork does define not only the employed methods of fieldwork but also its duration since, according to Malinowski, the ideal length of anthropological stationary fieldwork time has grown to 1-3 years. However, the normative nature of the paradigm should make the ethnographer writing about his fieldwork, including myself, cautious, especially if there had been doubts about the reliability of two or three years of fieldwork even before Malinowski's fieldwork. Perhaps the best articulation of this idea was given by representatives of Christian churches who were primarily not data collectors but often spent two or

three decades in non-European communities and complemented their church-related anthropological tasks with personal interest and fieldwork. Quoting Methodist missionary Lorimer Fison, Robert Henry Codrington, performing research in Melanesia, expressed his views on the possibilities of fieldwork as follows: 'When a European has been living for two or three years among savages, he is sure to be fully convinced that he knows all about them; when he has been ten years or so amongst them, if he be an observant man, he finds that he knows very little about them, and so begins to learn' (Codrington 1972:VII). So it is only after two or three years of staying with a community that we believe we know everything about the group or people in question; after ten or more years, we usually start to realize that we actually know very little. Upon analysing Maurice Leenhardt's fieldwork in New Caledonia, James Clifford represents a similar attitude (Clifford 1980).

15.

The mode and duration of fieldwork practice are, therefore, not necessarily limited to the mode and duration of *stationary participant observation* considered legitimate in anthropology, but also according to a significantly different and considerably longer practice, whose representatives are often suspicious of the results of fieldwork carried out with the accepted methods and duration (McKnight 2002). 'Fieldwork' carried out over a significantly more extended period of time (lasting 10-20 or even 30 years), simultaneously assuming that the person in question is not only present in the community as a data collector [3], does not belong to the data collecting methods accepted in anthropology or in general.

16.

In the case of Siberia, however, the situation is reversed. The traditions of Anglo-Saxon anthropology have hardly affected the methods and possibilities of ethnographic data collecting there. Different forms of financing, as well as the administrative methods of the tsarist government, have created a new type of fieldwork in Yakutia: that of the exiled researchers. For the vast majority of their ethnographic descriptions are based on living with the observed group for at least a decade. These fieldworks were triggered by the tsarist government's practice of exile, which I will discuss in detail below. The majority of 19th-century data collecting activities of ethnographic nature concerning the Sakha originates from European military officers, revolutionaries, doctors, and writers sent into exile for 10-20 years. Almost all the authors of 19th century ethnographic works have spent a long time in Siberian exile (Jochelson, Hudjakov, Seroshevskiy, Pekarskiy, Levental, Vitashevskiy), but during their stay in Siberia, they were naturally not in a position to devote all their time and attention to record ethnographic data. Fieldworks of this nature are often characterized by data collectors focusing on description and the production of data, where the result of their collecting activities often took the form of archives, folk poetry collections of several volumes or a collection of museum objects [4]. These researchers perceived their task as a kind of translation activity, often coupled with creating actual dictionaries and grammars (Troshchanskiy 1902) (cf. Clifford 1980:520).

17.

My fieldwork, of a duration and method complying with today's socioanthropological data collecting standards, is not compatible with data collection processes regarded as *traditional* in Yakutia, either in terms of methodology or in its duration. We need to note, however, that participant observation, which is far from being widespread in Yakutia, is not merely one of the fieldwork methods but has become the almost exclusive method employed in fieldwork and is even regarded occasionally as a synonym thereof (Dewalt et al. 1988:259). In connection with his

own experience, Malinowski, the most prominent representative of stationary fieldwork wrote about this in the chapter entitled “Proper conditions for ethnographic work” as follows:

18. “Soon after I had established myself in Omarakana (Trobriand Islands), I began to take part, in a way, in the village life, to look forward to the important or festive events, to take personal interest in the gossip and the developments of the small village occurrences; to wake up every morning to a day, presenting itself to me more or less as it does to the native. I would get out from under my mosquito net, to find around me the village life beginning to stir, or the people well advanced in their working day according to the hour and also to these a son, for they get up and begin their labours early or late, as work presses. As I went on my morning walk through the village, I could see intimate details of family life, of toilet, cooking, taking of meals; I could see the arrangements for the day's work, people starting on their errands, or groups of men and women busy at some manufacturing tasks (see Plate III). Quarrels, jokes, family scenes, events usually trivial, sometimes dramatic but always significant, formed the atmosphere of my daily life, as well as of theirs. It must be remembered that as the natives saw me constantly every day, they ceased to be interested or alarmed, or made self-conscious by my presence, and I ceased to be a disturbing element in the tribal life which I was to study, altering it by my very approach, as always happens with a new-comer to every savage community. In fact, as they knew that I would thrust my nose into everything, even where a well-mannered native would not dream of intruding, they finished by regarding me as part and parcel of their life, a necessary evil or nuisance, mitigated by donations of tobacco.” (Malinowski 2000:46–47.)
19. Malinowski did not write that he was making his observations with simultaneous involvement in the events, carrying out the same practical tasks as the locals; his morning walks and contemplations, as well as his observation work, were rather just phenomena of accepted and habitual separation within the village community. [5]
20. It is not even as a new method of fieldwork that anthropological research has legitimised participant observation linked to Malinowski, but from an epistemological point of view (Roldán 2003:143). For the novelty in Malinowski's fieldwork in the Trobriand Islands lies not in the fact that he spent a longer period of time in the researched community than other Europeans making ethnographic notes or that he knew the local language better than other researchers, but rather in the way he focused his fieldwork on providing an answer to a particular question, and that he answered the research question in his ethnographic publications based on the data he collected. Following that, fieldwork was not intended to provide a detailed description of (or an introduction to) the life of a particular community anymore but became closely linked with the process of ethnographic writing. From that point on, in addition to theoretical knowledge, the credibility of ethnographic writing was to be supported by concentrated fieldwork as well. Consequently, the quality of fieldwork also determines the quality of the ethnographic work to be produced; therefore, anthropologists strive to distinguish their own life in the community from 'mere' presence also in an epistemologically relevant way. That is, an anthropologist has to justify somehow why he can see and understand more about the life of the community he is studying than for example, the local missionary or a fellow worker of a foundation or a relief organisation. This self-

interpretive approach assumes a hermeneutic relationship and unity between the subject of knowledge (the anthropologist) and the object of knowledge (the person or community being studied), which the anthropologist should establish during fieldwork (Josephides 1997:17). The personal and, at the same time, epistemological credibility of creating hermeneutical unity is established by the moment or sequence of events of 'inclusion' in the community that resembles a breakthrough.

## The Moment of Inclusion

21.

The assumption that after a certain period of time, the collector's presence becomes generally accepted in a community, and this acceptance turns the tide in the relationship of collector and community at the moment of creating respect and harmony (*rapport*) is a topos in anthropological descriptions. Such an interpretation of the collector's presence may lead to the appearance of descriptions of the act of inclusion, of acceptance, as a kind of breakthrough in the data collecting process in the fieldwork accounts of anthropologists (Dewalt et al. 1988:269). Perhaps the best known of these is the flight of Clifford Geertz with the cockfight participants from the police in Bali (Geertz 2001), but many other anthropologists have experienced these situations as a kind of *initiation*. Telling the story of initiation is one of the instruments of establishing anthropological credibility, which, however, may not be more than one of the rhetorical tools employed by anthropologists for persuasion (Crapanzano 1986:53) that does not make its approach credible by itself (Becze 2008:186–187). Thus participant observation often changes into the observation of participation (Dewalt et al. 1988:269), lulling the anthropologist into the belief that his own situation and presence are identical with those of the other members of the community studied. A far more sceptical interpretation of gaining rapport is formulated by Bernard Russel, who thinks this turn should rather be construed critically: 'Gaining rapport' is a euphemism for impression management, one of the 'darker arts' of fieldwork' (Russell 2006:369). The need of researchers to create harmony in an atmosphere of distrust in strangers often results in a cunning and well-thought-out data collecting and behavioural strategy (Nojonen 2004:168–173). Researchers' strategies to establish and gain rapport are not about cooperation, rather about the dominance of the ethnologist performing fieldwork, after all. (Bornemann 2009:238). Nothing illustrates this statement better than the fact that communities, which have been permanently subject to intensive fieldwork in the last hundred years, have become explicitly dismissive of the presence of anthropologists by now (Deloria 2007).

22.

While during my fieldwork in Yakutia, I was an active participant in events where the local village communities appreciated the fact that I stood by them in occasional conflict situations, I think they have always done so in the knowledge that I would not be obliged to do this, because (as an *outsider*) it is not my duty. It is well illustrated by the case when some secretaries of state representing the government of Yakutia visited one of the field sites I worked in the spring of 2004, and in a public forum, they intended to find out from the locals what problems they see in their daily lives. Of course, the village residents received several promises at the forum, some of which have never been held. As a person believing to understand some of the community's problems, I rose to speak and asked for additional teaching equipment and education materials for the local school. My request was eventually fulfilled, and after a visit to the capital city Yakutsk, I could

return to the school with several boxes of textbooks, tapes, and CDs. In this situation, I have defined myself as someone representing the interests of the village, which was also confirmed by the community, but neither then nor later did I think that the community regarded me as one of their own members because of that. On the contrary, any respect that I had in the community or any acceptance of my presence was rooted in the fact that I did not belong to them originally. As a consequence, locals excluded me from the discussion of specific issues, even if at other times they opened up to me with great enthusiasm.

23.

On another occasion (giving the impression of inclusion and common fate) I visited a small lake belonging to the neighbouring village (Tiit-Aryy) with two people I knew in the village community, where I conducted fieldwork to see their nets and check the catch. Maksim and Anatoliy had installed the nets a week before, and every other day they got on their motorbike to check the nets and collect the catch. On one of these occasions, I accompanied them. Maksim and I paddled in together, with Tolya waiting for us on the shore. By the time we finished collecting the catch, three men from Tiit-Aryy arrived. Maksim told me immediately that we will have to leave the catch (the result of our work together) behind, since the fishing rights to the lake theoretically do belong exclusively to Tiit-Aryy residents. After paddling back to the shore we had a short smoke together and handed over the fish to the men from Tiit-Aryy. We took out the nets and headed back to our village. On the way back, Maksim indicated that the only reason it did not turn into some more considerable trouble was me being there. That is, my mere presence as an outsider changed the situation, regardless of the fact that the men from Tiit-Aryy knew me well, we had even been on fishing trips together. The adventure of getting caught together did not create inclusion but emphasized my character as an outsider.

24.

Based on this and many similar examples, I think that inclusion and integration can never reach the point when an anthropologist can argue convincingly that he took his records after gaining some inclusion and mutual harmony, which makes his notes and his own person in the activities he performs authentic. After all, anthropological data collecting, as James Ditton argues, fakes interactions (Ditton 1977:10) and makes the experience of blending in impossible, since in every critical situation (if he is free to act therein) the anthropologist is present differently, with different intentions from the locals. [6]

25.

Accordingly, not only do I regard my ethnographic description as the voice of an outsider, I can also interpret my presence as a fieldworker exclusively as that of an *accepted outsider*, allowed to participate in certain events and excluded from others. In my opinion, the fact of acceptance does not legitimise the statement that I can look at the community as it were *from within*. I think the *marginal native* status postulated from participant observation (Lobo 1990) cannot be established and neither have I become a marginal member of the communities I worked as an anthropologist. That is why the attainment of the internal or ethnographic approach idealised in qualitative research (Steger 2004:31) should rather be regarded as a topos of the self-reflexive research method. Consequently, participant observation should only be interpreted as an observation made in a participation of marginal position, where recording the events could never be accompanied by active participation. It means that upon getting hold of collected data, I never participated in a situation in the same way as my informants, and while I was



part(icipant) of the situations in question (hunts, hay work, games of cards), I did not see these situations 'from within'. I was not 'included' in these communities, but the locals gave me more or less freedom to enter certain situations and gave me the mandate to collect and use certain types of information. However, in all these cases, they were aware of why they were doing so, and there were clear reasons for them to involve me in certain situations. Accordingly, not only was I welcome to join their sports teams but explicitly invited; I was invited to hunts, dinners, and games of cards as a result and acceptance of my activities there (that were important to them), since they were aware of the fact that these community occasions were important to me from the aspect of data collecting. The writing process for the ethnographic work about their own village (what they expected from me) seemed a valuable and interesting activity to them as well.

26.

The reciprocal nature of these situations was often obvious: in one of the villages, I helped Lyubov in translating English texts, she invited me to dinner (which in this village was known to be associated with an interview) in return. For Vienna I posted parcels and letters in the faraway capital city, Yakutsk, in exchange she kept a diary for me for four months. I gave extra English lessons to Kolya's son, in return for which his father took me hunting. They never tried to hide these reciprocal relations; on the contrary, they were fully aware that they *gave* me something important by helping my work. On such occasions, a friend of mine in the village, Lyuba from indicated to me, 'what follows is true ethnography.'

27.

It was also quite common that they tested my knowledge, inquiring about the meaning or sense of certain things or perhaps consciously deceiving me. It is well illustrated by a particular rest during a joint hunt in one of my field sites, when Gennadiy started to talk gobbledegook, claiming to speak Evenki. When I told him it was not even close to that, he just laughed and said he was just curious how well I knew them. Valentina from the saem village had a strange attitude towards my fieldwork. She used to be an ethnographer by profession, and was the head of the local cultural centre at the time of my stay in the village. Whenever we met, she interviewed me about the progress of my collecting activities and often called the information I acquired from my informants a lie. She kept telling me there was no ethnographic material of any value in this community, and whatever was there to be found was irrelevant to an ethnographer from a distant land. When she was drinking, however, she contacted me regularly and took me out to a variety of events to show me what she considered authentic, 'true' ethnographic phenomena. On one of these occasions, she took me to an ice fishing event of a small group of villagers, on another occasion to a meeting of the club's management, and once she took me to a communion. Gennadiy even made a joke of Valentina not regarding my collecting activities authentic and told her once that in his wife's clothes, using an ice-breaker spade he performed an Evenki shaman dance for me in the potato bed. Following that, Valentina's indignation was subject to jokes for days.

28.

So to me, the relationship between the ethnographer and the locals can be understood as an exchange, where the community involves the collector in a common game rather than accepting him into the community. The playfulness of the relationship was often explicit in one of the field sites I worked. In the other village, where I started my work in 2002, however, I found the rules of the game far more serious and strict.

29.

In the field, it soon became clear that my informants in Yakutia were very well aware of the point and significance of ethnographic fieldwork, as well as with the opportunities that the presence of an ethnographer from Europe may offer to them. The 'field' is not at all isolated from the researcher's own environment and discourses (Caputo 2000:20–21). Ethnographic descriptions will eventually reach the informants affected (Brettell 1993; Keszeg 2005). The residents of one of the villages I worked were familiar with and kept in evidence the book of Nikolaev about the Evenki living in Southeast Yakutia (Nikolaev 1964), as well as the dialect collections of Myreeva and Romanova (Romanova–Myreeva 1964). They criticised the work of a Spanish anthropologist who spent a week in the village earlier, saying he had not been able to perform sufficient collecting work among them.

30.

The fact that village communities have their own well-established idea of what ethnographic/anthropological descriptions are about and that the villagers are not just 'sources' but also consumers of ethnographic works creates a new kind of relationship between the collector and the community during fieldwork. I often experienced that local residents consciously wanted to communicate, to disclose something to the public through me. Occasionally, this has led to a certain 'competition' between informants to make sure whose opinion influences me more. The theory of anthropological fieldwork calls it a 'methodological trap' when a collector, rather than describing a phenomenon, becomes a *spokesman for the community* (Coffey 1999:32).

31.

I think one of the often-overlooked factors of present-day fieldworks is precisely this new type of relationship, in which the studied community consciously (and in a way that seems valid to them) delivers an ethnographic representation to the collector, i.e., during interviews, the informant communicates with the collector in a way that complies with the requirements of the ethnographic attention desired by them, manipulating the conversation accordingly. Locals often censored data I collected that did not comply with the objectives of their self-representation. One of my main helpers commented on my notes on a crime in the village as follows: 'This is not true, I don't even see why Roma told you this. It's really sad. You are not going to put it in your article, are you?' Part of this control was that many of them wanted to read or listen to the notes about them. That is why I considered it important to keep the majority of my notes in Sakha language and not in my native Hungarian. They often invited me to events of ethnographic relevance that they considered worth presenting, like roadside and fire sacrifices, deliveries of animals, the meeting of the local seniors' club, etc. The leader of one of the villages I conducted field study asked me to write a short summary of my work on the basis of the things I saw at the village, as well as a proposal about 'the boosting of community life.' So the collected materials also reflect what the community could use me for and to what extent I became their suitable spokesperson. This is an important aspect of the game between the community and the collector, as well as a measure of the collector's immersion in the game.

## Texts and Language Skills

32.

That is what makes the discussion of another important question of data collecting and fieldwork important at this point. According to one of the theoreticians of

the anthropological collection process, Daniel Sperber as anthropologists we can perform three actions with the material collected upon completing our fieldwork: we can describe the event or data (*description*), or, moving beyond this, we can reproduce something (*reproduction*), or interpret a phenomenon (*interpretation*) through it (Sperber 1985:11–12). Description, however, is a process fraught with problems in the first place. Let us take the simplest method of description, interviewing. According to the theory of anthropological interviewing it is not at all irrelevant, in what status the interviewee will answer the questions raised. In this situation, they can formulate their own (subjective) experience (*respondent*) or deliver some community information, knowledge (*informant*) (Levy–Hollan 1988:336).

33. To illustrate that such a distinction may not always be helpful in interpreting the materials collected during fieldwork, let us examine an interview excerpt from my own fieldwork:
34. My grandpa often told me that he was born in Önör, but we have never lived there. His father had resided at the settlement we live in now – perhaps he went to Önör in his old age to marry a younger maiden, and thus my grandpa was born there. My grandpa was D’ögöör, and his nickname was D’ögörsöö. He was a funny person. He constantly made fun of others. Therefore his other nickname was funny D’ögörösö. He passed away when he was 90 years old; until that time, he did not lose a single tooth. He was a very clear, neat, and healthy man. He was a great runner also. I saw him running when he was older – many young people could not run as fast him.
35. In this text it would not make any sense (and may not even be possible) to separate the parts where the informant delivers information based on his own experience and the parts he delivers as community knowledge, a distinction like that has no relevance at all. Accordingly, in interviews, reports on personal life experience and accepted value judgements based on community concord do not represent opposing, well separable systems. To put it more accurately: personal and community spaces are not displayed by separate voices during verbal expression. [7] Therefore, local views arising from fieldwork data should not be captured in the dialectic relationship of personal and community sphere set against each other, [8] but rather in the language game (Sprachspiel) of individual contributions (in the context of common action and verbal contributions; Wittgenstein 1998:21). Accordingly, the collector's objective should not be to filter out from fieldwork data, from recorded texts what the *objective* meanings of these statements are or what monolithic *emic model* they represent, much rather to establish how these contributions, in spite of their differences, create an opportunity for a common language game, how they form a functioning real-life system.
36. Since the distinction between the statuses of individual contributions does not represent a priority task and neither is it important to (re)construct some model from the materials collected, the question may arise to what purpose and in what manner the texts collected as part of the fieldwork should be used, if they neither provide an 'insider' approach nor refer to some objective meaning accepted and known in the studied community. Since the final result of fieldwork typically takes the shape of texts (Dorson 1964:1), first, we should clarify how in what fieldwork situations and communicative frameworks the anthropologist captures these

texts. The types of texts can be determined not only by the emic text classification system but also by the collecting situations they were recorded in. The anthropologist contributes to the creation of texts learned or recorded during fieldwork to varying degrees, after all. Sometimes he is only present as a listener when the texts are spoken and only affects the delivery of the texts by changing the composition of the audience. On other occasions, not only the subject and the structure of the text, but the entire communication situation is created by the collector, like in the case of filling out a questionnaire (Sanjek 2010:247–248). Nevertheless, the creation of both text types implies some cooperation between the parties, i.e., the existence of a situation, in which the information flow between the parties is not one-way (highlighting the problematic nature of the opposition of collector and informant), and where questions and answers mutually affect each other.

37.

“Language,” as generally accepted in anthropological thinking since Sapir, “[...] is a symbolic guide to culture” (Sapir 1949:162). The members of the studied community maintain symbolic representation systems through the language, from which Robert Prus came to the conclusion that he should regard his informants as *ethnologists*, rather than individuals, units maintaining and repeating this symbolic system: ‘Relatedly, although the notions of people associated with images, categories, and physiological qualities may be useful in many respects, as also are the senses of difference implied in terms such as individuals, persons, personalities, and characters, the term *ethnologist* has a particular relevance for those in the social sciences’ (Prus 2007:671). Based on this consideration, an *ethnologist* is in fact the ideal anthropologist, who possesses the full competence of the local symbolic and communicative system. Consequently, ‘the memories that *ethnologists* possess are very much representations (and linguistic records of sorts) of the realms of community life in which these people (and those who preceded them) have participated’ (Prus 2007:679). According to Prus, language as the primary modelling system determines the content of the collecting activity. To me, however, language is primarily not so much a coherent modelling system in the village communities of Yakutia, but rather a tool used by the locals. Accordingly, in my perspective, the contributions of local communication are not expressions of the organised, well-structured sets of ‘intersubjective meanings’, but actions that only seem to make sense in the respective pragmatic context. That is to say, with the help of the texts recorded during fieldwork, I have not aspired to describe a system, but to learn as much about the conditions and ways of using these texts as possible. In order to achieve this, I had to be familiar with several registers of the language of the respective village community.

38.

While the ability of the researcher to perform direct verbal communication in the field may be regarded as one of the most fundamental conditions of anthropological fieldwork, one of the hidden shortcomings of anthropologists is precisely the fact that often they barely have a basic knowledge of the language of the community they are researching (Burling 2000:v). The total or partial lack of local language skills may often even question the value and credibility of the collected data. Perhaps the best known example of this is Margaret Mead’s fieldwork in Samoa, which did not only provoke criticism from her contemporary Paul Radin, but also became (at least partly) discredited after the fieldwork of Derek Freeman in Samoa (Freeman 1999).

39.

Depending on the collector's language skills, any collecting activity in a foreign language can be implemented in two ways: with or without the help of an interpreter. Both methods raise different problems. First, interpreters affect the quality of the collected material: '[...] ethnographers who knew little of the source language (hence relying on interpreters) either downplayed the importance of or missed certain themes entirely' (Winchatz 2006:84). The presence of an interpreter changes the communication situation, it '[...] will systematically affect and distort the respondent's behavior in often obscure ways' (Levy–Hollan 1998:337). Consequently, it is precisely the study of the everyday life and use of texts that fieldwork performed with the help of an interpreter seems to be unsuitable for, since it changes the pragmatic context fundamentally, and the text produced by translation is not suitable to observe the language of local communication. Furthermore, the indirect nature of the interpreter also makes reactions during the interview and changing the subject quite difficult. That is the reason why in the course of collecting activities performed without the assistance of an interpreter, based solely on the language skills of the anthropologist on the field, it is possible to observe and record more informative texts and speech situations. This method, however, may pose other types of difficulties:

40.

1. The anthropologist cannot understandably formulate specific questions, i.e., the respondent does not understand the question or misunderstands it.
2. The anthropologist does not understand the lexical choices the respondent makes, and as a result, fails to get the point, to understand the response (based on Winchatz 2006:84).
3. The anthropologist is unable to participate in a communication situation (e.g., conversation, card game, gossiping in front of the store), and the participants of the communication situation turn away from him.

41.

From these three pitfalls associated with the collecting activity, we should first examine the inability to formulate the question in an adequate manner. It was especially during my very first fieldwork, starting in December 2002, that I struggled with the problem of *how to ask* proper and valid questions. I came up with the following solution: before the fieldwork, I discussed with my local mentor and host N'ukkolaj what I would like to focus upon in my collecting activity (at that time, it was kinship relations and their age-related changes), and we compiled a short questionnaire together that I had with me during each interview. The questions were asked in Sakha language, although my interviewees were often unsure if I understood what they were saying. A good example of this was my interview with Lyuba, who gave birth to three children and raised three girls. But she offered her youngest child for adoption and adopted the child of a deceased relative instead, and her two other children were from different fathers at that. All through the interview, I could not understand how it was possible that she offered her own child for adoption and adopted someone else's daughter, still I could not find a proper way to ask about it, only later, in the summer. During interviews, I often had a feeling that I did not receive the type of answers that I expected, but I am not sure if the reason for that was the inadequate formulation of the question or that the answer was in fact, unusual. Let me quote an excerpt from an interview I made in 2004 with Vienna, who grew up in her native village but worked for many years in

Yakutia afterwards, and only just returned to her native village after seventeen years. Initially, she even had difficulties with the Sakha language, she spoke primarily Russian instead. In the interview, I wanted to find out how her return and settling in went.

42.

– So you were a bit of a stranger, first in your village?

– A stranger? No, of course not. I was not a stranger. All my relatives were her, and everyone knew my father, and – as I have already told you – I grew up here.

– Certainly, I meant that although you had relatives here, with whom could you meet and chat?

– Well, I worked here in the school, and I had a good relationship with my colleagues. As for chatting. One could have chat with everyone in here.

43.

Apparently, I formulated the question inadequately since I did not get an accurate answer. But in hindsight, I now believe that the response to my original answer was very much adequate in its own particular way, since how else could we grasp strangeness but by the values it raises. I think in cases like these, I have failed to create an interview situation that is based on the common understanding of the interviewer and respondent. In connection with interview excerpts where I could not feel the safety of understanding on the spot or in hindsight, during re-listening to the recording, it is easy to make the mistake of generalisation or simplification. Therefore upon noting down and processing interviews, we should always be aware of how successfully the questions were formulated. Where they may have been inadequate, we must refrain from jumping to seemingly straightforward conclusions (e.g. the term does not exist in the respective community or the interviewee refuses to answer, so the evasive answer is adequate, etc.).

44.

As opposed to the problem with understanding the question, at misinterpreting answers the most common reason was that I could not understand a particular word in the text. The following interview with Ujban excerpt illustrates this well:

45.

– After all, people rumoured that Konoohoj was a fortune-teller [körböchchü in Sakha]. Therefore he was often visited from remote villages – even from Kur-buhakh.

– Only because he was blind? [Not knowing the Sakha term körböchchü, I thought the uttered word derives from the negative form of the verb “see”, which sounds similar.]

– Blind? In what way blind? No, he had perfect eyesight, and he was an excellent hunter and a real marksman!

– Didn't you say that he did not see?

– No, he had good eyesight, but he was a shaman [and now, he said the word in Russian, to make sure I understand it].

46.

Then the interview went on, and Ujban even explained to me the meaning of the word. Finally, he told me that Konoohoj was not actually a shaman, only a 'seer' (körböchchü). During fieldwork, it usually turned out already during the interview that I did not understand a particular word, but at other times (as it became evident upon checking the sound recordings), not. Understanding the names of places (that I often took down incorrectly in the first place) was more difficult. We reviewed and adjusted these later with the person responsible for the allocation of

hayfields using the administrative and grassland allocation maps available in the villages.

47. The use of the various linguistic codes and styles in Sakha also created some difficulties during my fieldwork. This difficulty was especially pronounced in a peripheral East-Yakutian bilingual village, where local Evenkis were inclined to express themselves using a lot of words of Russian origin and where the use and vocabulary of the Sakha language were significantly different from the language I learned in Yakutia and used in Central Yakutia. My interviewees initially even felt some shame for not being able to speak Sakha with appropriate fluency, so at first, I often recorded explanatory parts that had major significance from a linguistic aspect. Recordings made later illustrated well how the residents of this village gradually unbent in their use of the language parallelly with my own progress in mastering the local registers thereof. Finally, I also need to note that in the case of practically every interview, upon taking the respective notes, I found words that I could not comprehend by hearing.

48. The third type of language problems resulted from being left out from the respective communication situations. Initially, it often meant that I was simply sent home, or it was made clear to me that unless we could continue the conversation, it would be better for everybody involved if I left. It often happened, for example, in connection with the 'campings' at the shower following the training sessions of volleyball team. After dressing and showering in the radiator water they always saw me home to keep me from harm at night (like attacks by a dog or a drunkard). I only realised later that these campings represented the most important and most intimate occasions for the local male society to engage in open conversations— which I later also gained admission to because they were interested in my opinion about the issues discussed. So after a while, they considered it interesting and desirable to have me with them to chat a bit. My attendance of the teachers' staff room in another village was a similar phenomenon. Initially, it was limited to my bare presence, but later my colleagues tried to involve me in the discussion of current matters at the school.

49. Language-related difficulties should, therefore, not be regarded as problems that are easy to tackle (especially not with the help of Russian language skills or a suitable interpreter) in Yakutia or anywhere else in the field. In addition to the requirement of reciprocity between the community and the collector, the adequate knowledge of the local language is a fundamental condition for the local community to grant collectors admission to the collecting game, assuming that based on their language skills, they are capable of participating in it, and that it will create an interesting and valuable communication situation for the community, too.

## Conclusion

50. Many studies and books have been written on the importance and methodology of fieldwork already; therefore, the repeated discussion of the issue may seem nothing more than the navel-gazing of anthropologists. Still, it is much more than that. Research areas that work with soft and qualitative data, such as anthropology, may face difficulties upon legitimising their results from the aspect of scientific methodology. Therefore anthropological research is regarded by some

anthropologists not simply as scientific work but also as art, as fiction writing (Leach 1996:46). Rather than focusing on scientific conclusions, i.e., observation, other anthropologists prefer to put more emphasis on interference, that is to say, on participation (Greenwood–Levin 2007; de Laine 2000:2). Still, the establishment of scientific legitimacy is a fundamental requirement for anthropology since it is an essential precondition of discourse with other social sciences that work with harder data.

51.

Anthropological case studies, i.e., fieldworks and their respective descriptions, therefore, require particular attention with regard to their methodological foundations. In connection with the majority of publications, these foundations are not only of epistemological but also of rhetorical nature. That is to say, the demonstration of participant observation (the description of the events of inclusion), instead of explaining or interpreting the authenticity or validity of the fieldwork process, rather has the function of a tool in the anthropologist's self-representation. It is a rhetorical tool, the use of which has become hard to avoid and which (even unspoken) already represents an accepted and expected element of the argumentation of anthropologists, as well as of anthropological studies and books by now. As opposed to this, the issues of language skills that also give rise to a number of problems are far less frequently subject to open discussions by anthropologists, typically only when discussing epistemological problems (or in works dealing with linguistic anthropology). While the tragic picture that Robbins Burling painted of the lack of linguistic competence when it comes to anthropologists may seem exaggerated, it is beyond doubt that there are relatively few methodological studies available on this otherwise important epistemological subject. The separation of the *self-representation* of researchers and the *epistemological principle* is not only difficult within anthropology that works with soft data but also in connection with other social sciences. This study intended to point out that in the case of anthropology, precisely because of the methodological peculiarities of the data collection process, this separation is a particularly important task and a major challenge.

## NOTES

1 Fieldwork is an equally substantial research method for ethnographic research, anthropology and ethnology; therefore, I shall not cover the methodological differences between the various approaches in this present study, but treat the three terms as synonyms instead.

2 However, according to another opinion, there is a substantial epistemological difference between anthropological research (anthropology) and ethnographic description (ethnography) (Ingold 2007).

3 Julie Cruickshank, for example, during her over ten years of fieldwork along the Yukon river has also performed various other tasks (Cruikshank 1988).

4 Waldemar Jochelson, for example, performed collecting activities for several museums during his subsequent fieldworks (Mészáros 2001).

5 The interpretation of accepted separation as participant observation is common in anthropology. Wayne Fife has observed education in Papua New Guinea sitting in the back bench of the local school, taking notes. In his work on methodology, the author presented this collecting method (that can certainly be regarded as one of the types of accepted separation) as participant observation (Fife 2005:72–74).



6 Upon describing a rite that included dance, Powdermaker expresses her doubts to what extent her presence and her inclusion in the dance makes her an actual part of this shared dance experience as compared to the locals (Powdermaker 1966:115–116).

7 For a detailed critique of contrasting the social and the subjective see Reyna 1997.

8 'Dialectic of objective structures and incorporated (subjective) structures which operates in every practical action' (Bourdieu 1990:41).

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