



## FORUM 36: FROM FIELDWORK TO WRITTEN TEXT

**Abstract:** This edition of 'Forum' addresses the issues raised when anthropologists attempt to transform their preliminary records into an academic analysis. Participants of the 'Forum' answer the following questions: What are the processes by which encounters and conversations with informants become a 'written source'? Is it acceptable to edit field notes? By what processes do these 'written sources' turn into the text of a publication? Is it acceptable to share one's own field notes and interviews with other researchers? Is it possible to carry out productive research using field data gathered by others?

**Key words:** fieldwork, field notes, interview, written text.

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## Participants in Forum 36: From Fieldwork to Written Text:

**Andrei Andreev** (independent scholar, Moscow, Russia)

**Leonid Barandov** (Herzen State Pedagogical University,  
St Petersburg, Russia)

**Tatyana Barandova** (National Research University Higher School  
of Economics, St Petersburg, Russia)

**Olga Boitsova** (Peter the Great Museum of Anthropology and  
Ethnography (Kunstkamera), Russian Academy of Sciences,  
St Petersburg, Russia)

**Ugo Corte** (University of Stavanger, Stavanger, Norway)

**Stephan Dudeck** (University of Lapland, Rovaniemi, Finland /  
European University at St Petersburg, St Petersburg, Russia)

**Marina Hakkarainen** (University of Eastern Finland, Joensuu, Finland /  
European University at St Petersburg, St Petersburg, Russia)

**Alexandra Kasatkina** (Peter the Great Museum of Anthropology  
and Ethnography (Kunstkamera), Russian Academy of Sciences,  
St Petersburg, Russia)

**Ekaterina Khonineva** (European University at St Petersburg /  
Peter the Great Museum of Anthropology and Ethnography  
(Kunstkamera), Russian Academy of Sciences, St Petersburg,  
Russia)

**Olga Khristoforova** (Russian Presidential Academy of the National  
Economy and Public Administration / Russian State Humanities  
University / Moscow School of Social and Economic Sciences,  
Moscow, Russia)

**Mikhail Lurye** (European University at St Petersburg, St Petersburg,  
Russia)

**Olga Sasunkevich** (University of Gothenburg, Gothenburg, Sweden)

**Inessa Tarusina** (North-Western Institute of Management, Russian  
Presidential Academy of the National Economy and Public  
Administration, St Petersburg, Russia)

**Andrei Tiukhtiaev** (European University at St Petersburg / Peter the  
Great Museum of Anthropology and Ethnography (Kunstkamera),  
Russian Academy of Sciences, St Petersburg, Russia)

**Nikolai Vakhtin** (European University at St Petersburg, St Petersburg,  
Russia)

**Aimar Ventsel** (University of Tartu, Tartu, Estonia)

## Forum 36: From Fieldwork to Written Text

This edition of 'Forum' addresses the issues raised when anthropologists attempt to transform their preliminary records into an academic analysis. Participants of the 'Forum' answer the following questions: What are the processes by which encounters and conversations with informants become a 'written source'? Is it acceptable to edit field notes? By what processes do these 'written sources' turn into the text of a publication? Is it acceptable to share one's own field notes and interviews with other researchers? Is it possible to carry out productive research using field data gathered by others?

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### EDITORS' QUESTIONS

When we go into the field, we look, we listen, we smell, we touch, we absorb the atmosphere, and then we transform our impressions into a research text that describes, analyses and explains. On the face of it, the ways in which sensory experience gets changed into a written text are familiar and obvious: the recording and transcription of interviews, the composition of a field diary (or field notes or other such records). And it is these in turn which underlie any publication. Nearly thirty years ago, US anthropologists already noted the crucial role of these field notes to the career path of the anthropologist and to the construction of anthropology as a discipline [Sanjek 1990].

But how exactly do these processes work? Can we describe them, formalise them, predetermine the manner of the transformation, the reduction of the diversity and multidimensionality of the fieldwork situation to verbal form? Is that reduction once and for all, or can we move back to complexity again? As the authors collected in [Sanjek 1990] made clear, there is no once-and-for-all answer to such questions, but we have at least to grapple with them, constantly returning in our professional community to discussions about how we work with fieldwork material, about the transformations it goes through and about how our analytical text comes into being.

This 'Forum' is devoted to these processes. We presented the participants with the following questions as rough landmarks.

- 1 *How do you set down what happens in the field? If you keep a diary, what becomes of your field notes afterwards? Does your diary become a stable document, a 'written source', or is it legitimate to edit it?*
- 2 *How are informants' oral communications, with all the typical features of spoken language (fragmentary, incomplete, contradictory, illogical) turned into a 'written source'? What changes, is added or distorted in the course of this process? May the transcription of a field interview be regarded as a completely adequate replacement for the audio recording, a definitive stable document that is not to be altered? Have you had to introduce changes or corrections into the text of a transcription after the event, if so, on whose initiative or for what reason, and what is your attitude to this? Do you consult the audio recording after the transcription has been made, and if so, what for?*
- 3 *How are the 'written sources' created by you in the field subsequently converted into the text of a publication? Do you develop the text directly from your diary, or do you include quotations, with reference to your field notes, in the article? Do you use quotations from interviews to illustrate your own ideas, or do you let the informants 'speak for themselves' by introducing their points of view into the text? Which alternative do you regard as correct, and why? Should an anthropologist make use of techniques for working with spoken language developed in other disciplines — conversation analysis, discourse analysis, and so on?*
- 4 *Discussions about whether it is permissible to use other peoples' field materials have become very lively in the international social science community (mostly among qualitative sociologists, with little input from anthropologists so far) over the last twenty years, as a result of funding bodies' requirements that research data should be deposited in archives. In particular, a number of questions were raised about the status of the experience of personal presence in the field and its role in research [Forum... 2005; Moore 2007; Mauthner, Parry 2009; Hammersley 2010]. Do you think it possible to make your field notes and interviews available to other researchers? To what extent do you consider the experience of personal presence in the field important? Can research based on field data collected by someone else be productive?*

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## ANDREI ANDREEV

1

The hero of Wim Wenders' film *Alice in the Cities* is compelled to travel around the cities of Germany in search of Alice's house. He is employed at a publisher's, where he receives a commission for printed material from the editor in chief. In the course of his travels he photographs various subjects with a Polaroid camera, and from time to time he lays out his photographs with the aim of writing the reportage that has been commissioned from him. Two aspects of this (one of the subjects of the film) are significant for us. The first lies in the laying out of the photographs in a particular order. The order of the snaps, and their reordering, stimulates the thinking of the man who writes. Researchers call this sort of use of photographs the evocation of the 'extrasomatic memory' (i.e. the memory of the researcher's previous state), and the photographs themselves 'a visual record'. A participant in research may be asked to comment on a photograph that the researcher has taken. A photograph very often serves as a stimulus in the course of research, and the method that uses this technique has been called 'photo elicitation'.

The second aspect which we may conceptualise out of the film's subject is connected with one of the basic characteristics of a photograph: it converts the three-dimensional space visible to the eye into a framed image in a single plane

which is material in nature. In this way the preliminary selection from the material world by photography in the field is effected, and the objects and their arrangement are grouped. Thus photography creates the preconditions for classification. The surface of the photograph has the distinguishing features of Polaroid emulsion and the format of the image. The colours of a photographic image are significantly different from those perceived by the human eye; the classic standard frame of a Polaroid picture is a square (78×79 mm) within a white rectangular frame. The photographic image is a construct of the researcher's, and sets of such photographic images are known as 'visual diaries' [Prosser, Schwartz 2005].

While the 'visual record' is a means of fixing the field that does not form part of the research report or journal publication, the 'visual diary' is both the means and the goal of the research that is being conducted. Consequently, the methods used in the practice of photography which compose the 'visual diary' should be taken into account when the data (the photographic images) are analysed. The two concepts, 'visual record' and 'visual diary', function as ideal, typical constructs. One could give a number of examples of their combination.

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Let us consider the 'visual diary' in greater detail and enumerate some peculiarities of how it is compiled. A 'visual diary' consists of photographic images of different kinds which are supposed to fulfil different tasks.

A. We shall group under the first type of photographic practice the photographing of notices, posters, slogans at public demonstrations or any text which in order to save time (or for lack of it) is not transcribed in the field (although it could have been) but photographed. The photography of various details such as clothing or everyday objects is assigned to the same type of photography. This sort of photography gives additional weight to research, as indisputably based on presence in the field. In this case photographs are used as illustrative material, occupy a secondary place in any publications, and are often consigned to the appendix. Or else, like the 'visual record', they are not published. In the course of our research on the legitimisation of the activities of NGOs in Moscow dealing with the problem of HIV / AIDS, we photographed the exteriors of the buildings where the organisations were housed. Large-scale photographs of the entrances to the buildings where the organisations were housed were taken, thus showing that at the entrance there were no plates bearing the names of organisations connected with HIV / AIDS. However, this statement — in our case, the lack of any plates with the organisations' names in the public space — could have been formulated in a sentence without the use of illustrative material. In other research, into garage-building cooperatives, we used

photography as a means of determining the territory on which various buildings are situated in proximity to each other. For this we used a wide-angle lens (21–28 mm), sometimes using a high viewpoint in order to create a distinct linear perspective, by means of which it is easy to describe objects across the fore-, middle and background. But to photograph details (including notices) we used low-sensitivity photographic material to give a good resolution of the material [Andreev 2016]. Photographs taken using this sort of practice were thus designed primarily to answer the question ‘What does it look like?’

B. Here we examine the basic practice of photography in the field, using a camera with a short exposure time and moving objects. In itself the concept of a short exposure time with regard to motion is a reason not to trust the human eye. Motion, in that instant when it is caught on camera, is imperceptible to a human being, who sees motion as a whole, mostly apprehending its beginning and end. It is people who are the fundamental data here: their emotional states (kinesics) and movements and the relative positions of the participants in the research (proxemics). It is essential to collect the data while taking the time factor into account, for example for the entire duration of Hallowe’*en* celebrations at a particular place by a particular number of people. For photographing such events (which may be public or which may be celebrations by closed communities or small groups) a lens with a focal length close to the norm (30–35 mm) is best, which neither etiolates nor inflates the human figure, and does not create unnecessary distortions in the backgrounds, which can be used as the basis for studying the material context of social interaction.

The researcher’s influence on how social interaction takes place is largely determined by his / her role in the field. Thus the public nature of an event creates to a sufficient extent a spatio-temporal continuum in which the practice of photography is not noticeable. If a researcher has drawn attention to himself / herself with his / her camera, this does not mean that (s)he might be an uninvited guest or an obtrusive photographer; it is also a reason for thinking that the participants in the interaction are only absorbed by the process of interaction to an insignificant extent. The practice of photography in a closed community and the open use of a camera are more likely to mean that the researcher is a member of the community that (s)he is studying.

C. Disruption of the existing order of social interaction — when people are being photographed — is one variant of the practice of photography. Erving Goffman, the student of people’s everyday interaction, had a Leica camera, but we do not find any photographs that he had taken himself in his publications. Nor do we know what photographs he might have had lying on his desk. One possible reason why Goffman attached little importance to his own photographs is

implicit in his advice to fieldworkers: ‘be a horse’s ass’. In this metaphor Goffman shows that the researcher is not involved in *what is being observed*, but obediently follows *what is being studied*. And although a Leica camera has a quiet shutter and no winking mirror, the horse’s head might turn round. Without his / her camera the researcher has a better chance of remaining unnoticed. The field can be minuted in writing when one has stepped outside the actual field, but this is impossible for photography by reason of its materiality.

In our research into manual labour (when we were members of a closed community) we photographed a workman cutting up and polishing metal pipes. When the workman noticed the camera, he asked us to take some ‘more heroic images’ which he could give to his wife in order to show how busy he was and what difficult work he was doing. The camera creates the conditions for visible observation, potentially producing the Hawthorne effect. In researching into urban space, an inevitable feature of which is the presence within it of people in uniform, we photographed policemen. At the moment when the photograph was taken one policeman was involved with his smartphone, staring into it and moving his finger across the screen. When he noticed us we were taken to the police station where our film was exposed to the light. And, as we found out, the main reason for destroying our film was that the policeman might be discredited if the photograph were published on social media (‘a policeman on duty shouldn’t be using his smartphone’).

When a researcher reveals himself / herself to be engaged in the practice of photography, (s)he determines the rules of behaviour for the participant in the research, who in turn supposes that the photograph will reveal something about him / her. This means that the practice of photography is a sort of instrument of provocation, forming a descriptive textual element of the research outside Goffman’s paradigm. However, a man with a camera may simply be an object of interest to the people whom (s)he wants to study. Thus during a field expedition to Belgorod Oblast, where we were studying the professional life of farmers, the practice of photography in itself engaged the attention of some proprietors to such an extent that we spent a whole day with them making a detailed photographic report [Andreev 2014a; 2014b].

The means of making photographic images listed were prepared in advance by the aims of the research. The context for creating an ‘A-B-C visual diary’ is formed during the practice of photography. On the whole, those photographs are selected from the ‘visual diary’ which, in their details and in the significance of the subjects (things) photographed, fit into the analytical conception of the research being conducted. The correlation of context, significant research results and the photograph itself leads the researcher to discover the

‘photographic event’ [Caldarola 1985]. In this way the variants of the practice of photography listed under ‘A-B-C’, which form the *context* for taking photographs, must be described in connection with the *results* of the research and with a particular *photograph* (or series of photographs). For example, the context in photographic practice of type B is the interaction between people and speech that may be recorded on a dictaphone, and the social positions of the participants in the interaction (including the role of the researcher) who are being described. The selected photographs are provided with annotations in which the researcher introduces into the integral image the individualising properties of text by a full description of the objects that can be seen and their condition.

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The cliché ‘shoot like Bresson’, which is common in the photographic community, has acquired a particular resonance, frequently one of objectivity, in texts describing research methods using photography. Researchers (Michael Ball, Gregory Smith) note that sociology has yet to see its own Henri Cartier-Bresson. Douglas Harper displays his own photography, accompanying it — with a certain lack of confidence — with words about its stylistic resemblance to Bresson’s. Harper is probably largely concentrating on description, which provokes that sort of thinking in a researcher who comes to the field in search of a text which in turn is underpinned more by a wide range of *reading* of the analytical literature than of *looking* at the photographic heritage, Cartier-Bresson’s in particular. A researcher’s attitude to his / her field of study may be so imbued with text that after the fact, when the field is no longer accessible, (s)he might send out a call for a photograph from the field to put on the cover of his / her report, book or presentation. Overall it may be said that preparing a photograph requires particular concentration on the researcher’s part, and sufficient immersion in the fieldwork, perhaps even more than when collecting textual data.

We have already mentioned ‘photo elicitation’ when the researcher himself / herself prepares the photograph, but it may also be borrowed from the field itself. For example, the study of an amateur photograph, when the researcher looks at family albums together with the participants in the research. Another example may be the archives of industrial enterprises, when the researcher examines the photographs together with employees. In the latter case photographs of actual work activities are commented on in relation to the aim of the research: the determination of power relationships within the enterprise. In this way the research technique of ‘photo elicitation’ works as a means of activating the memory, applied in the search for the socio-economic positions of the participants in the research, which are not immediately visible on the photographs. In these examples the appropriation of photographs does not mean that the researcher is excluded from the field or not participating in it.

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**TATYANA BARANDOVA, LEONID BARANDOV,  
INESSA TARUSINA**

**Tatyana Barandova**

National Research  
University Higher School  
of Economics  
St Petersburg, Russia  
tbarandova@yandex.ru

**Leonid Barandov**

Herzen State  
Pedagogical University  
St Petersburg, Russia  
faustarx@yandex.ru

**Inessa Tarusina**

North-Western Institute of  
Management, Russian Presidential  
Academy of the National Economy  
and Public Administration  
St Petersburg, Russia  
frolya68@mail.ru

**Multimodality in Research:****The Way from the Field to the Text****1**

In our opinion the questions raised by the editorial committee concerning the problems that arise in the transition from fieldwork to the composition of a scholarly text are important to be reflected upon not only by the community of professional anthropologists, but also for other social sciences (and even, more widely, for the humanities), and likewise for interdisciplinary discussion, and so we have risked entering the debate from a position of representatives of political sociology and ethnoculturology who use ethnographical methods in their research practice.

We take into account, firstly, that the contents of the research equipment of these disciplines have much in common (considering also the direct borrowing of research methods within those social sciences which use a qualitative methodology), secondly, that all researchers, though to differing degrees of involvement, are orientated towards work in 'field' conditions, the collection of material derived from (participant) observation and the conducting of various types of interview, thirdly, that — besides verbal communication with people and personal 'experience' of the fact of such communication within the symbolic space of the (sub)culture being studied, within the framework of the social behaviour considered acceptable within it — the researcher's sensory experience does in all qualitative strategies become in one way or another a (supplementary) source of information in the sphere of objects of the material and 'lived' world being studied and is subject to subsequent interpretation and inclusion in the final description, and fourthly, that the outcome of the research takes the form of a final (written) text that is offered to the community for discussion.

At the same time it is immediately necessary to qualify our understanding of the 'text' in a number of ways, inasmuch as in conditions where the processes (political, social and cultural) and socio-cultural realities of modern society with its complex structure are mediatised and digitalised, as is the incipient research practice itself, it seems essential to examine the text in an extended sense, including its visual and audio (and perhaps synthetic) forms. It is also important to be aware of its essentially 'plural' ('stratified') nature, insofar as the researcher's final analytically constructed narrative (article, monograph), is based on a complex of diverse primary 'raw' texts, either created independently by the researcher or borrowed from the material received from informants during the research, which is an intermediate stage of the transformation of the information on the way from the field to publication, sometimes even using elements created by other researchers or by the subjects of communal life themselves. Incidentally, the texts' creators are not always professional researchers — in the conditions of the development of actionist (participatory) approaches involving representatives of the group being studied in the actual process of their study, or the independent production of artefacts which are to be the object of further study, they may be the persons immediately being studied or their representatives. An example of this is some research done by our Norwegian colleagues, which we witnessed. In 2007 they gave mobile telephones with videocameras to homeless people in Oslo, taught them how to use them, and asked them to record what they considered to be the important moments of their everyday life; afterwards the recordings were analysed by social anthropologists.

In answering certain questions we would like to focus our reflections about work with field materials on these two directions, based on our personal experience and specific examples of complex research, also taking into account those theoretical frameworks which predominantly determine not only the research questions and the aims of the research that we undertake, but also the methods of collecting data (both before and during the process of work in the field) and also the subsequent steps and procedures for their analysis, the very prospects of those field data which are examined as significant, and the assumptions and impressions during fieldwork, which will receive the researcher's attention. Essentially, the theoretical foundations in the anthropology textbooks which we have been able to study over recent years are noted as basic in the construction of practical work in the field and the subsequent processing of the material collected [Eriksen 2004: 42–84; Wulf 2008: 69–78]. We note that the interpretation of the questions asked takes place within the framework of an interpretative paradigm to the extent that, in our opinion, the social practices and their meanings which are liable to be studied by us are relevant to it.

In respect of the first aspect of our 'qualification', we would especially emphasise that here we examine as field notes not only verbal records on paper, but also the use of photo and video recording during fieldwork, which, frankly, came into being in anthropological / ethnographic research practice ages ago and have been actively used in it ever since the technical means of making them were invented (starting, probably, with Mead and Bateson). Moreover, visual records were being made even before they appeared, for example, by missionaries and commercial travellers, by means of sketching details of clothing, equipment, domestic furnishings, etc., and afterwards this was analysed by desk-bound ethnographers, when and where they could get access to the material. This mediated supply of data is probably as significant for recording sense impressions during the subsequent interpretation as the written part of independently compiled diaries or the transcripts of interviews. As equally valid field data, drawings, photographs and video recordings not only assist in remembering the details which will afterwards be translated in a reduced form into words, but also serve as illustrations to the observations which are described verbally, expanding their effect on the reader and guiding, even 'correcting' the imagination, and sometimes they directly provide analytical material for the study of 'another' culture without physical immersion in its milieu. (The obvious examples are James Frazer's *The Golden Bough*, which was written without visiting the 'field' in the flesh, and Ruth Benedict's *The Chrysanthemum and the Sword: Patterns of Japanese Culture*, based *inter alia* on remote study of the 'field', including cinematic material.)

Roland Barthes, listing various types of narrative (among which we also include the examples of original ‘written’ narratives in the form of the field notes or diaries of observations mentioned in the question), and pondering the possibility of structural analysis, emphasises that ‘le récit peut être supporté par le langage articulé, oral ou écrit, par l’image, fixe ou mobile, par le geste et par le mélange ordonné de toutes ces substances’ [Barthes 1996: 1]. In the introduction to the 2007 round table published in *Antropologičeskij forum*, no. 7, Viktor Krutkin notes that ‘[i]n books anthropological knowledge is expressed in words and sentences, often with drawings and photographs. Visual anthropology begins with the proposition that the film and the screen may be not only an illustrative elucidation of the language of words, not only an ancillary service, but an independent means of anthropological writing’ [‘Forum...’ 2007: 113]. The ‘iconic (visual) turn’ in the social sciences (the philosophical basis of which is examined in [Inishev 2012: 184–211]), which emphasises an understanding of means of constructing and perceiving subjectivity by means of the visual image, has influenced both research and / or cultural practices and the expansion of the field of social studies, allowing in particular the productive use of digital equipment in the field and / or the necessity of developing new methodological foundations and ways of describing ‘digital field data’ when research aims are being achieved in the ever more popular sphere of internet ethnography (‘netnography’), i.e. data made up of fragmented and decontextualised material (on which see: [Eriksen 2004: 50]). We follow W. J. T. Mitchell [Mitchell 1994: 11–34] in our description of the essence of the expansion of frameworks that is taking place within the academic community, agreeing that a society in which the word used to be the key means of acquiring knowledge is being replaced by one in which ideas about the world are presented as (video / photo) images. Therefore, when the aim of research is a complete and comprehensive description of what is happening in the field, we use every available means of collecting and recording it: interviews (with the informant’s consent) are recorded on a dictaphone, and for preference the most important notes of a ‘protocol’ type are made on paper as the conversation progresses; the performative aspects and the spatial surroundings are recorded by photography, or else a video recording is made (if the conditions and agreements permit); when the conversation is finished, in the absence of the informant, but still with pictures being taken, significant personal feelings, first personal impressions, suppositions and features of the situation where the photographs were taken are recorded on the dictaphone, and when a video recording is made, where necessary, a running commentary is given, aloud, on the objects being recorded; any accessible material artefacts (newspapers, booklets, posters, etc.) which may be taken away without damaging the environment in which they are produced are taken and studied afterwards.

As a rule, when observation is planned in advance, a diary is kept. Sometimes the need for it arises spontaneously (depending on the situation), and then the observations are recorded taking into account the basic parameters: time, place, action, actors, processes, utterances, one's own attitude. We regard the first variant as a stable document and fully valid source: it is compiled in the process of working the 'field', and is not edited after leaving the field, since the whole point of keeping a diary, in our view, is to record, immediately and deliberately, what happens at the point of a professional intervention in the milieu, of which, as a rule, all the participants in the interaction are aware.

It is important to have a reflexive attitude to the fact that formal interaction of this sort (for example, participant observation in the office of the ombudsman or in a local authority) does not always allow the recording of certain informal practices, since the researcher is not usually felt to be 'one of us', and either (s)he is not invited to take part, or the interaction takes place in the awareness that an outsider, the researcher, is present. Still, the aspect of 'distancing' is classical in anthropological discourse and practice. In the second case, when observation usually takes place spontaneously, as soon as the observer is back in the appropriate conditions, his / her rough notes are worked up into something more substantial (and this is precisely where one can make use of the aural method rather than recording things on paper, 'speaking' into the dictaphone what has been observed during the process, for example, of the meeting or performance, and sometimes, when photographs or videos have been made, they can be used as ancillary materials during the editing process). We are nevertheless still inclined to regard records obtained in such a way as stable documents, although when they are processed and subsequently reproduced it is particularly important to take account of the many 'external' parameters that are hard to translate into verbal descriptions, from one's own psychological and emotional condition at being suddenly (and sometimes extremely painfully) involved in the events to the ethical aspects of the use of results obtained 'incognito'.

2

It is logical that the transformation of the material collected, including oral accounts by informants, into a written text will be accompanied by a series of difficulties and potentially serious losses of meaning — and not only those of the subjective character noted above, and resulting from following the canons of a theoretical framework. These last, in our opinion, more often occur when no use was made of ancillary instruments for the collection of data during the study of synthetic phenomena or (cultural) actions, speeches and behaviour of a demonstrative nature which make up elements of contextualised social / political discourse. 'An anthropologist is like a literary critic who deals with social discourses, which are sometimes

uttered in verbal language, and sometimes using non-verbal (visual, behavioural) constructions. His / her aim is to articulate what was said in the utterance, write down his / her interpretations, and interpret his / her interpretations, as (s)he approaches a thick description' ['Forum...' 2007: 112]. Ethnoculturologists and political sociologists do the same when they have to apply an anthropological apparatus to their own research aims, probably because an introduction to Geertz's method of 'thick description' [Geertz 1973: 3–30] is included in the programme of their professional training. The theoretical basis of the 'interpretative paradigm' and the cultural sociology approach (as developed by Geertz's American colleague J. Alexander [Alexander 2003]) are being ever more frequently used to study, for example, such phenomena of social life as manifestations of political consumerism and actionist (performative) protest, which recruit into politics from the sphere of culture activists who are able to bring a profound symbolic level to political action / utterance. In order to extract the meanings of these utterances and produce a 'thick description' of them, it is desirable to have a video recording of what happened, which apart from individual study may be used for collective (interdisciplinary) expert discussions. The new forms of mass protest which were actively manifested during the last electoral cycle in the form of the 'protestors' creative self-expression reflected in the large number of memes and home-made placards, also require the political anthropologist at least to make a photographic record of these synthetic (or, as they are called in political linguistics, creolised) texts, so as to carry out the field study as fully as possible and, if not to develop a conceptual theoretical model, then at least to classify them and actively describe them.

Anticipating somewhat, we should note that we allow the use of field data collected by other people, forasmuch as something recorded is not a speech act, but a noeme of speech which is constitutive for the whole discourse, i.e. the fixed content / semantic meaning of a speech event which is open in the text for multiple interpretations by readers and other experts (here we take for granted the possibility of a polylogical reading of a visual text). On this level the 'ABC of Protest' project, edited by Vadim Lurye [Lurye 2012], is illustrative. A high-quality selection of photographs from several protest actions is structured according to generalised topics of expression without additional hermeneutic verbal texts. It became a good source for analysis by other researchers of the semantic discursive field of the protestors. What changed or was distorted when this happened? A difficult question. Perhaps one should take into account that the socio-cultural and political contexts in which the interpretations take place change, as do the interpreters themselves, who are inclined to discover different / other semantic connotations, starting likewise

from their theoretical or disciplinary training and their aims in studying the sources. Was what the photographer had succeeded in recording as a primary source distorted by this process? No, but the transformation of understanding and interpretation of field materials takes place at the level of theorising prompted by them.

The study of different forms of performativity has been the subject of a separate book by C. Wulf [Wulf 2005] (translated in Russian as [Wulf 2008]), which means that they can be regarded as significant topics for anthropological studies. Indeed, even for the study of ‘traditional’ folklore, such as fairy tales, rules were developed for recording them taking into account not only the contents or structure of the text (though one of the most popular is the approach based on V. Ya. Propp’s structuralist principles, which ignores performance, reducing the phenomenon to identifiable functions as elements of the structure), but also the manner (genre) of performance, and also its social function, which has allowed the inclusion in descriptions of informants’ oral tales elements of the actions they performed. It enriches both the field material itself, and the final product (we rely here on A. I. Nikiforov’s elaborations [Nikiforov 2008: 75–7]). We suppose that Wulf is right to note that when actions of a theatrical or performative nature are studied, this opens a new perspective, which requires hermeneutic ability in analysing such ‘texts of the body’, while for the development of the concept of performativity all three aspects are important, whereas only the first of them is created within the framework of cultural anthropology (that is, the forms of cultural performance), while the other two stem from the philosophy of language (performative utterances) and aesthetics (the art of performance) [Wulf 2008: 147–53].

Various rituals of non-political interaction also have a performative character, including those that are newly invented or (re)constructed, between people in a particular setting, between people and objects, and people and cultural artefacts, which are the ethnoculturologists’ sphere of investigation. In our case the object of our ‘field observations’ were the interactions of tourists and tourist sites, monuments of material culture, the labyrinths on the Solovetsky Archipelago. The interactive ‘participation’ of the tourists in various activities with them was recorded on the researcher’s own mobile phone and on those of the tourists who were engaged in it (who could afterwards share their recordings with the researcher both directly and via social networks). When the data thus received were being transcribed, a distinct problem manifested itself: it was practically impossible to understand what the tourists / informants / performers were saying without observing their actions either directly or on video recordings, because their fragmentary nature and other characteristics, without the context of performance and bodily movement, appeared illogical and absurd. Short lightning

interviews also refer to what was actually happening and how the informant 'felt' at a particular moment of his / her passage through the labyrinth. (It should be noted that it was the simulated labyrinth on Bolshoy Solovetsky Island, which it is permitted to enter, that was used for the interaction.)

It is evident that whenever it is a research aim to reveal (and construct) the meanings of social actions by the informants and to study them, the research should make use of transcription and audiovisual recording at the same time, since the informant's emotional states must be included in the analysis. But how can one express the experience undergone by someone else's body — the experience of living through events by means of someone else's body — in words? Such questions may also be asked with regard to the practices of carrying out rituals in (ethno)cultural milieux which also bring physicality into play (initiations by means of painful practices, for example). It is possible to interpret what is seen and imagine what is felt using words only to express one's own perceptions, or else record the experience in the informant's words — but the informant is not always capable of a verbal expression of the experience undergone by his / her body. '[Die] Inszenierung <...> das zur Erscheinung bringt, was seiner Natur nach nicht gegenständlich zu werden vermag,' Wulf stresses, quoting W. Iser [Wulf 2008: 151], and therefore he notes that after the linguistic and visual turns, there comes the performative turn, and all three of them are turns 'towards an anthropological way of examining', and therefore 'texts must be studied from a performative perspective,' including not just photo / audio / video recordings, but also group interviews, on the basis of phenomenological research describing the process of the performative as 'an oscillation between the perceiver and the perceived' [Ibid.: 152–3].

The illustrative examples given are intended perhaps to affirm approaches to their work with the transcription of oral verbal texts of those social researchers who consider it necessary to retain all their 'roughness' when transferring them into written form, for example in applying the methodology of objective hermeneutics (the basic methods are quite well described in [Titscher et al. 2000]). Nevertheless it should be understood that there is not always any necessity of this, and we allow variation in transcribing method (for example, a rigidly structured formalised interview with the head of a city council, which is supposed to be processed according to the methodology of 'condensation of meaning', does not require the preservation of every element of speech, including interjections). At the same time, the question of introducing changes into the transcription after the event is by no means a simple one for political anthropologists and sociologists. In the first place it is determined by the status of the person who was the informant, since people

belonging to the administrative elite hardly ever agree to be interviewed without being given the opportunity to look over and edit the resulting text afterwards. Obviously, when it is a choice between the possibility of having ‘access to the field’ and not being allowed to, agreeing to the editing of materials after the event is sometimes ‘the key’, and regarded as a necessity imposed upon us. The need to have access to audio / video recordings after they have been transcribed is similarly determined by the situation, and this is particularly important when carrying out collective research, when not all the interviews have been made independently, and the transcription may have been done by other people. In the latter case we listen to the originals and make notes, if possible, of hesitations in intonation and pauses (if they are not indicated in the transcription), and check the written text; however, this process may be regarded as a partial answer to the fourth question, meaning that we do consider it possible to make use of material collected by other colleagues, both when they are processed analytically by the group, and for secondary analysis by individuals.

3

That which has been said above shows that we regard the development of multimodal methods of research and the application by anthropologists of techniques from other disciplines (and *vice versa*) not only as a useful research practice, but as a vital necessity. And this is so not only when working with the spoken word, but with other forms of communicative action, collected materials and artefacts (for in ethnic culture even elements of clothing such as a belt or an embroidered edging are texts). Perhaps the hardest thing is to give a straightforward answer to the question of how the mass of sources is turned into a publication. The process has not only an individual creative character, but is determined by a multitude of circumstances: the researcher’s educational, professional and practical experience, how literary his / her personal style of writing is, how many kinds of source, and which of them, are used, their overall volume, who collected the data and how, whether they are subject to provisional ‘censorship’ by the informants themselves or by the people who ‘control access to the field’, the methodology used by the researcher (for example, in the model of developing a text on the basis of ‘grounded theory’ [Strauss, Corbin 1998] all the processes and stages of encoding are described in some detail, and the result of following them will be that there will be hardly any ‘corroborative quotations’ from the interviews in the final text: they will be systematised and categorically processed within the analysis itself), and others, down to the technical possibilities of including visual ‘quotations’ in the draft of the publication (easier for a monograph than a journal article).

It is the process of encoding for analysis, ‘reading’ and description of the visual text that creates the greatest difficulties: here too we

cannot do without the formulations of other disciplines, such as semiotics, psychoanalysis, art history or sociology. (We use Piotr Sztompka's formulations as basic [Sztompka 2005], but with serious additions of our own.) This stage of work with data should probably be considered in a separate discussion. Still, if the research that has been carried out is relatively actionist or involving in character, we suggest that it is appropriate to include direct quotations from informants speaking for themselves in the final product, and to sum up our theorising in the introductory and concluding statements of the article.

The most common variant is still, perhaps, to illustrate one's own ideas with quotations citing field notes, since in the professional milieu in this country there are still certain standardised conventions in force which dominate this sort of presentation of the results of social research — or else these formats are dictated by the scholarly journals themselves (which at present are in a state of active transition to following Western models), which forces one to 'format' one's analytical thought processes to make them fit the available conditions and pass peer review. We think that this perspective deserves a separate wide discussion, since there is no point in 'writing for one's desk drawer', and one can only bypass the rigid demands of the format by publishing a monograph (which, besides the greater expenditure of resources, is often rated lower in a bibliometric evaluation than an article in a Scopus journal).

4

In our view, the question about conforming to the requirements of funding bodies, who are another significant 'player' in the field of producing social knowledge, belongs to the same sphere. We have already given a positive answer to the basic questions asked in this section. Starting from the fact that 'an interpretation of interpretations' can only be made with access to data collected by other researchers, and likewise considering that it is important for data to be open to society, we support that side in the discussion that defends availability for use and the accessibility of one's own field data for analysis by other people.

However, the difficulty of the dilemma concerning 'the status of the personal experience of presence in the field' is obvious. For example, if a performative action is not recorded in photographs or video, even if there is a sound recording of the impressions of the researcher who was 'seeing' it, any adequate interpretation by anyone who had not seen it is problematic. Here we return to the first question, about what is reduced or 'slips away' when talking replaces watching (or is not combined with it). At least, without a photograph of the informant (whoever he or she was) and the space and atmosphere in which the interview was conducted, it is hard to imagine the possible effects on the process of communication that was taking place, which could

have had a great influence on certain answers. Without a sound recording of an oral communication or the performance of a narrative, it is hard to discern or understand, for example, its suggestive properties and the effect when it is perceived. Without a protocol (if it records the gestures or emotional condition of the informant) a transcript may not be of very much use. Of course, only someone who has personally conducted wide-format observation and who has collected a mass of material can use it to the full for analytical processing and attain a high level of theorising. At the same time, though, anyone can see and hear that towards which a research question is primarily directed (though a degree of professionalism will allow them to record wider contexts too), and besides, anyone can be mistaken in their interpretation (based only on personal insights). The productivity of any research, be it conducted on one's own data or data collected by others, depends not only on the researchers' professionalism, but also on the circumstances and canons for evaluating that productivity, and on belonging to a particular community...

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## OLGA BOITSOVA

1

There is another way of keeping a field diary that is not mentioned in the question: recording one's observations on a dictaphone, dictating them immediately, as soon as you are by yourself, 'while the iron's hot.' As a swift means of recording this is very convenient. When I do this, obviously, I edit my spoken language when I transcribe it, getting rid of 'so' and 'sort of', and turning my fragmentary phrases into proper sentences with a subject and a predicate, so that afterwards my description of what I have observed can be cited in a publication as if it were the written diary that everyone is accustomed to, and not a hurried oral account. There is no need for the reader to have such an intimate view of my proceedings: it is enough for him or her to know that I keep a diary. I quote the diary in my publications, for example, when I try to explain what an 'iconographical canon' is, and there is no avoiding a demonstration of how visual literacy operates in my own case, but it is no business of the reader's what colour my notebook was, or how thick it was, or whether it existed at all.

Furthermore, all the years when I was working on my dissertation project I noted down short dialogues relevant to my topic, which I happened to take part in, as they happened, on cards: it is easier to sort through them, like cards with words of a foreign language on them, than it is to scroll through a long Word file in search of the place you need.

2

Leaving aside the fact that I transcribe my interviews in accordance with the norms of the Russian language, since phonetics are not part of my research interests, I have had experience

### Olga Boitsova

Peter the Great Museum  
of Anthropology  
and Ethnography  
(Kunstkamera),  
Russian Academy of Sciences  
St Petersburg, Russia  
boitsova@gmail.com

of editing my informants' answers. One instance of this seems 'harmless': my informant had (and, I think, still has) the habit of repeating his words several times. Here I shall allow myself to cite a fragment of his interview uncut, taking advantage of the absence of any keys that would allow him to be recognised: 'What is important is not, not, not some temporary appearance, yes, as it were, of a person, yes, which comes into being there, that's what, yes, that's what takes place there, all those stages, yes, of infancy there, maturity, well there, infancy there, youth there, maturity there, old age, there, then, well, so to speak, he dies, that's what.' When I cited passages from this interview I removed the repetitions and replaced them with dots in square brackets, guided principally by the reader's convenience. From my previous conversation with the informant I knew that these repetitions were a peculiarity of his speech, and not provoked by the situation of the interview, and this peculiarity did not seem to me to have any importance for the subject of the research. In my eyes this justified editing.

The other case seems more debatable. I recorded a memory of a supernatural experience from an informant who had been educated at the same place as I had, and with whom I shared a common professional terminology. In her story she used a particular term: 'In short, the bewitchment was inflicted through this contagious magic.' The person who told this story was hesitant whether she should believe in any connection between the events she related. In my material on this subject, this was the only oral memory in the first person: I had collected all my other evidence about supernatural events similar to this one from newspapers or the internet. The result was that in the paper I read on the topic, I had to use the interview as the only example known to me of the oral transmission of the subject being studied. Furthermore, after the interview I had managed to find a document which had a direct relation to the memory that I had recorded. Finally, it was an interesting and picturesque story, full of details and circumstances that were lacking in the written sources. I included it in my paper, but when I cited it I said nothing about my informant's educational background, saying only that she had a degree, and omitted only the word 'contagious'. Quotation without omission would have required the explanation that my informant had studied in the same place as I had, and could have led to her being recognised. I supposed that letting slip information about her identity would not have been good for her, especially since in the story she was the person who was bewitched. What is more, her doubts about whether to believe in any connection between the events could, in the years that had passed since the interview was conducted, have resolved themselves in one direction or the other. But I had a more selfish motive besides my efforts to preserve my informant's anonymity: we look far better in our colleagues' eyes when the people

we interview are ‘just people’, and not each other. I realise that it was not entirely fair to my hearers to make a cut in the quotation, and I have still not made up my mind what to do when I write up my paper into an article on the subject.

4

Speaking of including other people’s field materials in one’s own work, I should like to mention photographs. Using other people’s field pictures in one’s own project is extraordinarily difficult, because usually they were taken as a result of a research question of the field photographer’s own, as a means of committing something to memory which I cannot possibly remember, not having been there. But I had the good fortune of encountering a remarkable exception. In 2016 Ekaterina Tolmacheva, the leader of the Museum of Anthropology and Ethnography, Russian Academy of Sciences expedition to Vologda Oblast, brought some professional photographers (Elena Dyakova, Olga Zubova, and others) into the field and set them to recording the material. The numerous pictures taken by professional photographers from various angles allow a person who was not a member of the expedition to ‘look into’ houses and ‘look around’ on all sides, ‘raise’ and ‘lower’ his / her gaze and ‘look’ at the walls, floor and ceiling. I took advantage of this with gratitude when in 2016 I had to write about the ‘grammar of things’ — the positioning of photographs in a village dwelling in relation to the corner with the icons, the mirror and other places and objects in the home, that is, to answer a question which the members of the expedition had not addressed at all. These same photographs, which, taken together, create a three-dimensional effect, could probably be used by someone else for other work. It is a wonderful thing that this project of field photography was accomplished, and a pity that it remains unique!

## UGO CORTE

### **Fieldwork as Writing: Overlapping Stages and Interviewing by Comment**

Ethnography can be many things, and entail different processes and tasks towards different goals [Corte, Irwin 2017] just like cooking, dancing, or cleaning. Or probably anything else you can think of. Yet at its most essential and general form it is a type of work like many others [Fine, Hancock 2017], but not like all others. Perhaps it is a job most akin to those in which gratification is largely not immediate and whose progression is uncertain. In this sense it is like any other scientific practice or artistic endeavour,

**Ugo Corte**  
University of Stavanger  
Stavanger, Norway  
ugo.corte@uis.no

but different than for example play, eating or sleeping. While not as essential as plumbing, nursing or firefighting, it is similarly physical and dangerous, both in the field and at the desk. I once received a call from a colleague admitting having dislocated his shoulder while reading his notes in bed. And judging by the amount of standing desks recently being ordered, I suspect that back pain may be quite common among my fellows. Most distinctively, ethnography is an occupation, rather than say, a pastime, or a calling regardless of whether someone would like to see it as such. Can you think of anyone (sane), and who is a professional — or aspiring to be — who would *truly* do ethnography during his / her free time? Hanging out, sure, can be fun (sometimes), and especially so when done with groups whose vision we may share. But novelty and excitement inevitably turn mundane and become tiring when participating in group activities is also required at times when one does not feel like it. And think about recording ‘data’ while observing and trying not to look too out of place. In these instances building a thick skin is a necessity [Goffman 1989].

Much ink and thinking have been applied to describe the emotional toll that this exerts, and rightly so (e.g. [Kleinman, Copp 1993; Irwin 2006]). Which is not to say that there are no joys in it: after all it is a voluntary activity which, like any other creative work, should optimally be grounded in the ability to learn how to enjoy the process so that it can be sustained despite its challenges and delayed gratification, making it ‘autotelic’ — rewarding in itself, rather than instrumental [Csikszentmihalyi 1996]. And while ethnographers may hopefully savour some aspects of their work, there are certainly others that they simply have to learn to deal with. Live with. Else, they may eventually apt for another method which is not uncommon, nor deployable. Paraphrasing sociologist Robert E. Park: for how long are you willing to get the seat of your pants dirty while scribbling notes? Every method has its own merits and defaults, despite the fantasised romantic images portrayed in ‘tales from the field’. Sometimes outsiders belittle the craft for being ‘fun’ — perhaps really meaning unserious and unstructured, while not understanding that there is seriousness in fun, not least in its implications [Ibid.; Fine, Corte 2017], that there is method to this madness. The belittlers either have never done it themselves, or never published anything based on this approach. Or they may simply have been overly excited, having removed the pains they necessarily have endured. Or have used the term improperly, by for example, doing a strictly interview-based study instead. Contrary to what some may think this is not deplorable, or inherently of a lower quality (or pain free), but certainly different.

In this essay I use the terms fieldwork and ethnography interchangeably. While having become increasingly widespread and respected in and outside of academia, and not just in anthropology, they have also

come to mean different things to different people. Just like other subcultural practices, diffusion translates both into dilution, fragmentation, confusion, and the delineation of boundaries. But *ceteris paribus* it also may potentially contribute to richness [Crichter 1980; Corte, Edwards 2008; Hannerz 2015], and also reflection. A clever doctoral student once asked: ‘But how ethnographic does a study need to be to qualify as such?’

Discussing the process ‘backstage’ is indeed useful. It should make the practice more creative, and perhaps more enjoyable because more intelligible and manageable — and therefore, somewhat less anxiety producing too. By explicating what some of us do, others can arguably better appreciate our efforts. Then maybe try to emulate some of our distinctive styles, before hopefully forging their own. And it is in this spirit that I engage in this conversation.

Which leads me to the question of the title of this occasion: From Fieldwork to Written Text. The topic of this discussion is both deceptive and of paramount importance. Its importance lays in the ‘black-box’ — what goes on backstage before a piece is published, while its *apparent* deception is grounded in a misunderstanding of its practice (or in my overly pedantic reading of the call which I opportunistically use as a springboard for my contribution).

The least discussed aspect of fieldwork — and qualitative work in general, both in published work and in teaching, lies in the mystery of how to get from raw data to a product that is shared, displayed, perhaps sold, viewed, and potentially used by others. Some might say that every project is different and thus entails different procedures — and to some extent, I would agree. In a period when North American sociology was being largely and quickly professionalised, C. Wright Mills wrote a classic piece on intellectual craftsmanship. As he famously put it: ‘Let every man be his own methodologist; let every man be his own theorist; let theory and method again become part of the practice of a craft’ [Mills 1959: 245]. And while there are idiosyncratic patterns of workmanship that every researcher more or less explicitly develops and eventually perfects, I think that those should not be entirely created willy-nilly, or from trial and error and personal ingenuity, but should be built, made our own, also by being exposed to others’ exemplary work, to their analysis and critique, and researchers’ ways of working [Corte, Irwin 2017]. And I applaud this occasion for urging us to collectively think about this matter by trying to verbalise some of our work habits.

I understand qualitative research, and ethnography specifically, as particularly iterative and entailing a series of overlapping phases that are often repeated. And just as there is no pure distinction between induction or deduction — the two ‘models of research cannot be disentangled’ and require a ‘recursive process’ of analysis [Fine 2004:

81], there is no perfect way of doing things, but different styles, which if learned could become part of our own toolkit. And in order to be appreciated, emulated, demystified, and eventually challenged, those need first to be understood, and common mistakes talked about. And there is certainly something to be said for improvising too, yet I tend to believe that it is easier to do so when one has first learned from identified models and practised the craft. Novelty of the kind that any research to a greater or smaller extent should hopefully strive for, tends to be partially based on the unique recombination of different ideas. Building on Merton and Barber [Merton, Barber 2004], researchers like Malin Åkerström [Åkerström 2013], talk about serendipity. In a similar vein, I talk about ‘pursued luck’ — meaning maximising the chances that something interesting, and not anticipated, and potentially counterintuitive, might happen. Building on the work by Derek Price [Price 1975] on intellectual creativity, Randall Collins argues that one way to assess novelty and relevance is grounded in looking at how ‘ideas make it possible for other people to make their own statements’ [Collins 2009: 31]. Put differently, how many more questions do you, and the scientific community, derive from a specific piece of work?

But just as one informant in my current ethnographic project put it: ‘luck is when opportunity meets preparation.’ Preparation in fieldwork, according to scholars like John Lofland [Lofland 2006], and earlier Bronislaw Malinowski [Malinowski 1922], entails producing ‘foreshadowed problems’ — a list of themes, questions or hypotheses to test in the field. Those are derived from previous reading, individual study, experience and theorising [Swedberg 2014]. Others, like Patrik Aspers [Aspers 2009], detail the importance of ‘pre studies’. All thinkers argue for the imperative of recording early on our evolving thoughts and observations and their relationships, not postponing analysis till you get back to your stuffy office. Most recently, Aspers and Corte have provocatively argued that the very notion of what is qualitative in qualitative research (thus, including fieldwork) — and its definition, are unclear, hindering development of the craft. These scholars advance that qualitative research ‘involves making significant new distinctions in the process of getting closer to the phenomenon studied, which results in an improved understanding to the scientific community.’ They also contend that fieldwork is particularly well suited to its pursuit, both because it is longitudinal by definition, and because it allows researchers to get especially close to the data and consequently have many opportunities to test whether their findings resonate or not against ‘reality’. The empirical material is given the opportunity to kick back resisting attempts to apply theoretical straightjackets [Becker 1970: 43]. It is, in contrast, not only testing a hypothesis that is either retained or rejected. By spending considerable amounts of time working with

the material, the researcher develops theoretical ideas that in subsequent stages guide him / her and the research project forward. In this process coding and analysis are interwoven, and thus are often important steps for getting closer to the phenomenon and deciding on what to focus next [Aspers, Corte s.d.].

Part of my argument thus tends against the proposition that our impressions and experiences get turned into an academic analysis ‘subsequently’. I contend rather that this practice should take place explicitly *earlier on*, and continually, until work is published, and beyond. But ‘can we formalise this process?’ In the spirit of this discussion, my offer is in the form of providing some examples from my experience.

In the field I make sure to record field notes, interviews, photographs that I take or that others have taken, and video recordings [Lee 2018]. Lately, I have started to experiment with sounds. I think it is acceptable to edit my notes in two ways. First, by granting anonymity to my informants, especially when matters that are sensitive are discussed, and even if I was given permission to use their real names. Second, by invoking ‘plausibility’. I will not say that something has happened while in fact I am not aware if it ever did, or clearly know that it didn’t, or couldn’t have. But I will use one instance I have recorded to describe a broader pattern if the observation is strongly consistent with other data I have collected, and if it is part of a secondary argument in my work — not the main one. If I sense that such episode could potentially be generative, then I include it in my instrument and test it.

Do I ‘go back to the same recording?’ Yes, multiple times and to the point where I often learn quotations almost verbatim. I revisit them for various reasons, not least to understand how such utterances were spoken, both to ascertain their accuracy and that of my interpretation, and possibly also to pay notice to intonation, pauses, and diction. I also revisit them to understand how to edit them without changing their primary meaning. Citing an overly long quote for example, could read as flat and disrupt the narrative. Sentences from a long interview can thus be broken into smaller units and interspersed with some of my own commentary on informants’ expressions or movements to enliven the rhythms of the writing and bring the conversation to life. As a poet has recently told me: ‘quoted words (from an informant) could be more like short drum solos in the music of the piece.’

Besides jotting down notes, if I sense that the situation could be particularly important, and if it seems ethically appropriate, I audio-record conversations in the field which are not interviews. In regard to the iteration between data recording, analysis, and writing I keep a diary of my evolving ideas. Also, and perhaps less commonly,

I interview *by comment* [Snow et al. 1982], and in particular *by quotation* [Corte 2013]. This technique is a distinguishing feature of my own style that I employ through various phases both while in the field, and between fieldwork visits. It entails reading selected excerpts from other interviews (while usually keeping the speaker anonymous), or from other sources, to my informants to elicit specific responses on topics and questions — that as the research progresses I come to value as particularly promising. The practice can be used to achieve three outcomes: first, to trigger longer accounts and reactions to what it is described in the excerpt; second, to triangulate the validity of their contents; and third, to learn about sensitive matters by giving a pretext to approach the topic and ultimately providing a way of inducing extensive and hopefully sincere answers. Further, I am currently using it by quoting excerpts from my own analysis to understand whether it resonates with my informants. And while I believe that informants should be able to ascertain whether I correctly quoted them or not, like other ethnographers, I also don't think that they necessarily need to agree with my own conclusions, and for at least two reasons. First, if I pursued my job correctly, then I will have identified different viewpoints and positions within the group and setting within which my study is based. This means that my view towards the end of the project should be more extensive than any single informant's or faction's within the community I may be focusing on. Second, if I employed a comparative analysis by considering findings across cases within the literature I consulted (including my own previous work), then my ultimate theoretical gain should be both more general and more accurate than that of someone in the field who is mostly familiar with their own situation (or that of their closer associates). I rule out the possibility of comparing cases within different phenomena — not settings, or groups within the same phenomenon, because it is generally too complicated; especially if pursued alone. But it is a possibility, and one that can also be employed sequentially — meaning as each project is completed similar questions are addressed in the following study (see: [Fine 2014]).

I see the sharing of ethnographic data as viable only as if the researcher who has collected the data collaborates with someone else. For example, in the case of two ethnographers sharing their own work to the writing of an article. This position is grounded in the idea that while much of the data is recorded in idiosyncratic ways, the data itself is inexorably related to how it was gathered and by whom. This means that by becoming an 'instrument' the researcher should be the closest to the data, and thus best positioned to fully appreciate its nuances. Plus, data elicit memory which is inherently connected with who collected it. However, this does not preclude the proposition that a researcher could collaborate with another researcher to balance and improve their work — data collection, analysis, and writing (as, for

example, in [Snow, Anderson 1993]). Or by testing whether their interpretation of the data is correct and objective in the full sense.

Dennis, one of my informants who had studied psychology in college, once told me that interviewing is like dating. Maybe he was referring to the fact that it takes much emotional energy, and that it entails entering a rather stressful situation which is uncertain and potentially quite consequential. While his analogy is correct, I think that fieldwork should also be referred to as speed dating. Ethnography is generally known as a slow practice typically requiring much time, yet field encounters require quickness. Much of the data ethnographers collect, and often some of the most interesting and methodologically unique, is acquired through ‘perspectives in action’ — informal conversations in the field, rather than by ‘perspectives of action’, which are most typical in interview settings [Gould et al. 1974]. On this reasoning, I advise field researchers not only to strive to be good conversationalists (which while true, sounds overly abstract), but to develop a number of go-to questions that they can easily think of if they stumble upon an informant before or after they have conducted a formal interview. Such fleeting encounters could spark interesting ideas, and potentially develop into deeper relationships. And those experiences are typically one of the predictors of whether the study will be innovative. And not lastly, one of the joys of fieldwork and life.

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STEPHAN DUDECK

**Two Ways of Understanding ‘Translations from the Field to the Text’**

The metaphor of translation describing the process of anthropological knowledge production between fieldwork and the academic text can be understood in two quite different ways. In what follows, I will try to describe these two ways of reading the act of translation and juxtapose them. The first way is what I would call translation as interpreting — similar to the way in which we translate from one language to another, trying to deliver the content of words in a different language. The alternative understanding of translation I would call bridging. Here I see translation as work to build a connection — a construction that stands with one bridgehead on each side of a gap or rift — making the division between both sides palpable, yet allowing for the transport of items in both directions and fostering change on both sides.

*The work of the translator*

Anthropological research starts in a place called the field where knowledge is gained. It is usually a social and cultural context significantly different from the one in which the researcher herself or himself was primarily socialised. The gathered knowledge is refined in a process called writing up an academic text. Classical fieldwork, consisting of a broad mix of data collecting practices, is followed by an analysis of these data, which, ideally, provides material suitable to solve a theoretical problem and to be compared with other ‘data’ from other ‘fields’. One could look at this process as moving along a chain on which translation or transformation happens and along which anthropological knowledge is refined and produced.

The main problem to solve relates to the adequacy of representation of content along this chain of translations. Knowledge becomes something that might be detached from a particular practice, stored and transferred, like the content

**Stephan Dudeck**

University of Lapland  
Rovaniemi, Finland /  
European University  
at St Petersburg  
St Petersburg, Russia  
stephan.dudeck@ulapland.fi

of containers — boxes to be exchanged for each other. The main problem is, if the knowledge content has to be fitted into different containers, what does not fit, and how either the content or the container have to be modified in order to make them fit. According to this view, anthropology can be compared with an interpreter in the courtroom trying to be honest to the object of investigation and representing his / her words for judges, prosecutors and defence lawyers.

There is no doubt what should be the final and only product of the anthropological endeavour according to this view. It is the published academic text. Such view of translations presuppose that the loci of knowledge production are in a particular relationship — that of a place where the knowledge lives a full life and another, where the same knowledge is represented in textual form as a particular form of representation. This model of translation relegates the existence of both worlds — the world of the field and the world of academy to a time pre- and post- proper anthropological work. The anthropological analysis should have an independent existence in between a place of collection and a place of dissemination of knowledge. The field exists unchanged and independent before and after the research. Academia receives the readymade gift in form of the scientific text.

In the writing-up process, one imagines the researcher home at a desk surrounded by books relying on the materials collected in form of a diary, notes, personal memory, pictures and films. But often transcription of interviews becomes the dominant form of research material. The process that follows can be described as filtering away all the forms of knowledge that do not serve the ultimate goal of making an argument and contributing to solve a theoretical problem, often enough predefined before the fieldwork. This filtering transform all kind of knowing, experiencing, reflecting, interacting and exchanging into a written text detachable from the context of its origin. The hierarchy of knowledge starts with a rather messy admixture of all kinds of information and experiences. In a next step, ambivalences, doubt, uncertainties and blurredness have to be eliminated. Everything should end with a stringent narrative of only one author under which all other voices are only allowed to support and confirm or to illustrate the main voice. The conventions of scientific rationality require concentration on the typical, argument in cause-consequence relations, avoidance of contradictions, thought in structural-functionalist, processual, or systemic terms. The model is deeply rooted in the history of anthropology, being part of a panoptical colonial project where researchers rushed like bees to bring back what they collected in order to store it in their honeycombs and feed the other members of the hive.

The problem of the pyramid with a broad variety of fieldwork materials at its base, and at the top the distilled academic text, begins

already at its foundation. I would claim it starts with the common misconception about fieldwork, that it is the place of action, not of reflection. In today's fieldwork, often the interview dominates over participant observation and observation upon participation in the latter. We consider the observer to be an instrument of knowledge collection detached from the circumstances of the place and time knowledge comes into being. Reflection upon the collected materials represents a higher type of knowledge production and should start after the 'dirty' phase of getting into the field. Handbooks of anthropological fieldwork suggest almost as a rule a hierarchical relationship of participation and observation [Dewalt et al. 1998; Bernard 2002; DeWalt 2002; Delamont 2004]. Already Malinowski describes the participation as part of the method preceding observation and collection of information as the proper parts of research [Malinowski 1922: 6, 8]. The scientist considers himself / herself as the one generalising and the others as the ones generalised about: 'We (anthropologists) have "method"; they (those whom we purport to observe) have culture' [Cohen 2007: 111]. It is a question of 'rapport', of finding oneself a place in everyday life and integrating into the social group whose practices one wants to document and to understand. The 'problem' of rapport should be solved at the entrance phase of fieldwork. Participation should soon make place for proper research imagined as observation, documentation and interviewing. Often researchers believe that participation and observation are mutually exclusive practices (e.g. [Hauser-Schäublin 2003; O'Reilly 2005: 101, 106]). They consider it impossible to do reflection simultaneously with participation. They behave as if observation would not always be part of social practice itself in the society under study. The reason is the belief that observation would demand distance in an intellectual and even physical way. The inability to perceive participation and observation, practice and reflection as simultaneous and intertwined with each other, has its roots in the European scientific traditions originating in the conceptual division between *vita contemplativa* and *vita activa* in European monastic practice. To prolong this separation into anthropological fieldwork requires ignoring the knowledge and agency of the others, who do not stop interacting with the researcher when (s)he wants to refrain in order to observe from a distance and who never stop being aware of the presence of the outsider when (s)he tries to become a 'fly on the wall'. The social roles taken on by the researcher depend not so much on his / her own wish or skills but on the will and the interpretations of the communities (s)he is working in [Berreman 2007; Berger 2009]. This might seem self-evident, nevertheless handbooks are full of recommendations about what to do in order to integrate into the alien society and how to manipulate the situation in order to become a trusted collector of information (see the critiques of: [Lecompte 1999; Tedlock, Denzin 2005]).

According to the above-described pyramid, the academic text originates in detached contemplations back home at the writing desk following the monastic ideal of insight in a state of seclusion from the world. This knowledge is clearly superior to the one born in the battle of everyday life. Such everyday life often disturbs the aim: to produce the authoritative and generalising voice that fits academic expectations. Aspects that contradict clear and evident typologies or understandable functions have to be cleared away. The problem remains that often the insights resulting from bodily participation are less easily to forget and are more emotionally charged than some theoretical speculations found at the working desk. They might sink into the researchers' unconscious or live in the professional folklore of so-called corridor talk. I recommend the appeal to acknowledge their importance in Peter Berger's concept of 'key emotional episodes' [Berger 2009]. Using Rosaldo's experience of bereavement and rage or Briggs' experience of refusal, he narrates how insight is borne in tense and crucial situations that not only change the social status of the anthropologist in the field, but inscribe local values into the body. Another seminal example is the idea of entering the realm of cultural intimacy [Herzfeld 1997] and states of complicity [Marcus 1997] as for sure described in the Geertzian cockfight [Geertz 1972]. Key emotional episodes mark not only moments of crucial insights into local world but also important steps in the researcher's socialisation into these worlds that are often not easily revocable. Caroline Humphrey describes the complicated but fruitful consequences the status as a partial insider bears for a researcher who is also always partially outsider [Humphrey 2007]. In a less dramatic way, Hans Steinmüller refers to the feelings of illegitimate intrusion into intimate spheres and how the reflection on legitimising practices of the sharing of intimate knowledge can provide for an understanding of local socio-cultural conditions. He calls it the 'reflexive peephole method' [Steinmüller 2011].

The conceptual separation of practice and reflection affects not only the relationship of participation and observation: it repeats itself in the separation of fieldwork and writing up process. The ideal seems to be a researcher who returns from a place where the aim was full immersion into a social world in order to extract knowledge, which is then, after return into the safe haven of academe, refined into a scientific text. This renders invisible all the other impact that anthropological work has — publish or perish is the motto of this race for publication, and subsequently measurable attention by the scholarly community in form of citations. This picture builds a strong hierarchy between different forms of knowledge, which build on each other, become more and more scientific in order to be crowned at the end in form of an article or a monograph. Non-verbal expressions and experiences lay at the fundament, followed by non-structured

verbal conversations. The more formal interviews and the field diary — communication clearly addressed to one person, the researcher himself / herself — sits on the next level. Conference lectures and workshop presentations build the next level in order to refine the findings, discuss and test theoretical propositions, which finally are ending up in the one and only thing that counts — the scholarly publication. This seems the only place where the anthropologist exists — everything else is preparation, transitory, ephemeral forms of existence — the backstage realm that provides unacknowledged support to the star's appearance on stage.

### *The bridge*

As one of the possible alternatives to the 'problem of translation' I would like to suggest another metaphor that seems to be similar, but is able to pay attention to some aspects which inevitably are ignored by the metaphor of translation. It is the metaphor of a bridge linking two separated social spaces like two banks of a river. In this metaphor there is no hierarchy between both places, the two ends of the translation — and what is more, this metaphor also refuses to separate and build a hierarchy between the different social practices involved, such as intersubjective interaction and intellectual reflection. It allows us to see how the field gets into the anthropologist and not only how the anthropologist goes into the field. The picture of a bridge, a precarious construction between separated places, makes the separation, the rift between the places palpable and knowledgeable but also allows for exchange and flow in both directions. The bridge is a construction, which fosters mixture and amalgamation of knowledge — sometimes invasion and occupation of foreign loci but sometimes also hidden counter flows, which become known much time later. This model makes clear that the language of anthropology is not that of containers that have to fit the knowledge put in. The best metaphor for anthropological work is that of paths, tubes through which exchange might flow — with different outcome on both sides the bridges might connect. It makes also clear that anthropology is a split between different social positions, and rooted in worlds not equal to each other in terms of their relationship to political power and economic resources. Without an understanding of these power differentials, an understanding of the split stand of anthropological research might be impossible.

Another moment made visible by the metaphor of bridging is that of the flow — the mobility of the person of the anthropologist and of anthropological knowledge in both directions. The flow which goes in both directions follows different demands. One can define the one — the discourse of the discipline of anthropology as the one that follows the ups and downs of theoretical concepts and research

agendas. The other is formed by the agendas of social groups in the field interested in constructing their collective social face, their own histories, often as contributions to the world's cultural heritage.

I would like to introduce the term *epistemic movement* for the mobility between different epistemic standpoints that get into flux together with the social roles and identities of the researcher. I acknowledge the important contribution that feminist standpoint theory has made to research on identity formation and the way social interests and political agendas influence research. In seriously undertaken anthropological fieldwork, the aspects of identity of the researcher, hexis and habitus in Bourdieu's sense, have to be challenged during the socialisation of ethnographic fieldwork. The person of the anthropologist becomes something like a palimpsest on which different socialisations are inscribed, where diverse models for and of the world compete with each other for acknowledgement and legitimisation. The quality of anthropological production rests, I believe, not so much on the hierarchical order described above, but on the ability to move between perspectives, standpoints, forms of experiencing and understanding the world. It is clear that the suggested perspective demands quite a high level of understanding of the researcher's own locatedness. It requires a level of self-reflection that does not make the person of the anthropologist the focus of research but that uses an understanding of his / her role and position as a tool to understand both — the field and the social position of the academy — and to grasp the difference that keeps them apart. What I understand here as self-reflection does not aim so much at looking at the construction of the suggested bridge, but looking at its foundations on two sides of the river — its two bridge-heads and where they rest.

To discard the hierarchical view of fieldwork — the academic text relation — helps to overcome another problem, that of the dominance of written text over other, first of all visual, forms of representation that are usually relegated to illustrations. Visual or audio-visual forms of representation are able to be multilayered and polyphonic, to retain complexity and even enable interpretations running counter to the author's position. The interplay of artwork, exhibitions and film with the written text enriches the flow of anthropological knowledge over the bridge, and not only for the general public or the local communities as the 'bridgehead' located in the 'field'. These non-textual results of anthropological work might turn out as a more valuable output on the long run, when the theoretical deliberations of the texts become outdated, interesting only for historians of science.

What could be strategies to preserve the multidimensionality and polyphony of voices in the field? There are literary techniques like montage, commentaries and hypertexts, providing counterarguments

and the inclusion of materials that contradict the author's opinion, which are not very common for scientific texts. Peer reviewers might not welcome the fostering of doubt and suspicion about the singular truth of the interpretation. However, I believe it is possible to include for instance humour and irony in the text — not to distance the voice of the author from the reality in the process of being understood, but to allow for the potential of undermining the authority of the author's voice and his / her ultimate judgement. It is clear that this will be an almost impossible task in a scholarly article. It might be difficult in a monograph, but it is for sure possible in ethnographic film or exhibition work. The common demand to 'provide people with an authentic voice' in the anthropological text is often only a mask for the authoritative voice of the author. Verbatim quotes from interviews are often merely used as illustrations and to produce the impression of valuing the subjectivity of research participants.

### *When the 'field' comes to visit*

The field itself started a rebellion already long ago. It is emancipating from the role of the mine that research extracts data from. People that anthropologists work with display a growing knowledge about the potential social impact of anthropological research. They demand their interests are recognised, want to negotiate the direction of research and its possible use and misuse. This discussion of the means and end of anthropology might start already in the field even if they are not part of the pre-fieldwork requirements of research, for instance in indigenous communities. It becomes impossible to relegate reflection and theorising for the time and place when the anthropologist has left the field. And the field does not allow ties to be cut after fieldwork is completed. If in former times anthropologists could clearly know when they left the field, this moment, the ideal of separation of the field of research and the academic field becomes more and more blurred when 'the field comes to visit'. Modern information technology and transportation infrastructure makes it possible for the field to move into the academic world in electronic form in social networks but also physically, as these days 'informants' may visit conferences and take part in the discussion of scientific findings. The media, politicians, and business demand the expertise of anthropologists, and the field becomes increasingly politicised in areas with growing social inequality and economic and political injustice.

The anthropological fieldwork is caught in different nets of reciprocity following different rules of adequacy, legitimacy, and justice. The longer and deeper social contact develops during fieldwork, the more vulnerable the researcher becomes to the demands of reciprocity from the side of the field. Often the researcher is expected to contribute

to positive image production, the representation of ideal representations of identities. Problems might arise when different factions compete for the allegiance of the researcher and the recognition they aspire to through his / her work. This loyalty might get in conflict with the demand of academia for its 'gift' in the form of the published scholarly text according to the maxim 'publish or perish'. For a long time it was easy to hide behind the authority of scientific discourse as representing and serving universal values of progress and development. The field has not only realised this broken promise, but is physically and virtually moving into the space of anthropological knowledge production. The field comes to visit — in the university classroom and in the private life of anthropologists. Social networks in the internet and other communication technologies make it impossible to cut the social ties with the field and enable the continuation of fieldwork from the computer desktop. The part-time socialisation of classical anthropological fieldwork might become a simultaneous subculture in the life of the anthropologist. I remember quite well the warning story of an anthropology professor during my university studies about field researchers that 'went native' and got lost for academic research. Nowadays it happens more often that the 'natives go anthropologists'. They remain not visitors but start to live what Abu-Lughod [Abu-Lughod 2006] calls a 'halfie' life in academia. Anthropology today requires us to create an identity as 'halfie' or as the hyphen between insider-outsider [Humphrey 2007], an insight that is productive not only for the native anthropologist, but for every serious researcher, I would conclude.

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## MARINA HAKKARAINEN

### Marina Hakkarainen

University of Eastern Finland  
Joensuu, Finland /  
European University  
at St Petersburg  
St Petersburg, Russia  
marina.hakkarainen@gmail.com

I should like to thank *Antropologicheskij forum* for inviting me to discuss the topic of the correlation between field experience and the ethnographic text. This topic is extremely relevant to my everyday research. Not a day passes without my having to answer questions about my field and its place in my research and other people's: the documenting of observations and interaction with people, the processing of sound recordings and other documents,

the legitimacy of the material, its inclusion in the final analytical text of the article, etc. At the same time, being within the field of social sciences, I want to reflect my own subjective observations and experiences as ‘honestly’ and ‘objectively’ as I can, that is, in a way that is useful for a collegial understanding of my research results, and to present them to other people in a form that allows my experience to be distinguished and a certain consensus about its interpretation achieved, and, finally, to arrive at a sense of commonality within the scholarly community and others that are important to me. However, even in a sphere of research and writing with such a plurality of genres as ethnography, the languages of subjective experience and objectivity are very different. Therefore I have, as it were, to keep changing direction in my search for compromises between the accepted scholarly practices for writing papers, reports and articles and ‘honesty’ in presenting my field. After many years working as an ethnographer I no longer feel myself to be a spiteful trickster who is always deceiving the reader, always concealing something or describing it inaccurately, as I did when I was first getting to grips with the profession. Now I have learnt to accept the conventions for writing scholarly texts and to draw a distinction between different ways of knowing. But there remains a tension in my work in the relationship between the realities that I have ‘on entering and leaving’.

Judging by how often and how much time we spend discussing this problem with colleagues, they too are concerned about the transition from the personal to the social, from the subjective to the objective (or rather, intersubjective), from the oral to the written. And this problem, it seems to me, is to do not only with ethnographers’ methodology and the way they construct their discipline, but also with the professional community and how one finds one’s place in it. All this makes the topic of the relationship between the field as experienced reality and the final text as a result of ethnographical thinking extremely relevant, and it fits into the general context of the wider discussion about various modes of knowledge and its rapidly changing place in modern life. And although a good deal has already been said about the methodology of creating field documents and constructing the field, about the anthropologist as a research body within the field, as the creator of the field, and about the anthropologist as field, the topic still spurs us to new deliberations. These I would like to share. First I shall answer *AF*’s basic questions, and then consider certain questions which are connected with them and topics that concern me, among which the role of the emotions and intuition and the means whereby the ethnographer can include himself / herself in the research appear particularly important.

1

How is what happens in the field recorded? This of course depends on the task in hand: what I want to find out and how much time

I have to study it. This is what the construction of my field depends on. One can 'make a field' quickly, for example, in commissioned research one may collect a certain number of interviews, and then I or someone else will work on them. One can do nothing but observe. One can combine various degrees of inclusion and various means of observation and recording. It is not a question of there being correct methods of collecting material, but of the need to find the optimal correlation between the various techniques for solving the problems that have been set. The most important thing is to understand the research possibilities offered by the chosen techniques and the material collected by means of them, and the limitations imposed by them on the research.

I started in ethnography with two 'fields'. One was the field of old ethnography — pre-revolutionary published texts. I am still very fond of reading them, though this field has undergone major changes in my eyes: I no longer view it as a locus of objectivity and have come to regard it as a zone of politics. My second field consisted of ethnographical interviews which I conducted far from my home. This was something intermediate, as it seems to me, between the qualitative sociology of today and old-fashioned folklore studies, in which almost all attention was concentrated on the informant's words. Just so. And it was quite possible to notice nothing apart from them, neither oneself nor other people. At first I paid very little attention to describing the context in which the words were pronounced.

I think that words are very rich material. Therefore it is very difficult to be distracted by something else. I must admit that I usually begin the field with my diary. But if I have to conduct many interviews, the diary tends to be abandoned. I still have this tendency in my work. Besides, certain social realities of our life only exist in words, such as biographies. But recently I have preferred more and more not just to ask about, but to participate in. As if I am moving from virtuality into multidimensional physical space. And, obviously, to observe. This is probably connected with my recent work in areas of silence, for example with children, who are capable of not answering my questions at all, or of answering an hour later and in such a way as cannot be translated into my terms. The work of trying to interview children in the family is very useful for an ethnographer focused on interviewing. Only after the visiting the family, when you have already 'left the field' and, perhaps, sat down to write your article, do you suddenly realise that you came with questions for the children, and left with nothing but answers from the parents. This makes an impression that overturns all your former conceptions of a social order that you thought you had known since childhood. In connection with the study of transnational mobility you may also find yourself observing situations in which it is often inappropriate to ask questions

and engage in conversation, for example, while crossing international borders. At that time the regimented and disciplined character of the routine conduct of border formalities and their almost sacred solemnity give neither the occasion nor the possibility of upsetting the established order with questions, conversations, or experiments of various kinds. In these cases I try to remember the situation, the sight, understanding and experience of it, and to record it; so I make entries in my diary.

My diary entries have a specific character connected with my field. They are thematic entries. Recently I have been conducting research in the big city where I live, that is, a city in which I am an ordinary resident. When I go 'on an expedition' to a distant place, it is separated from my 'ordinary life' by distance, by travelling, and by the notion that I am 'somewhere else' and should not be living a 'normal' life. In that case the field becomes 'dense' with information. I make it so by my heightened attention. If the field is at home, 'round the corner', one needs to define that corner clearly: on this side it is not the field, and on that side it is. Otherwise it can create great inconveniences for life outside the profession, which does exist even if we don't keep office hours. It seems to me that it is not merely impossible, it is wrong to turn one's whole life into one uninterrupted field record. It is unethical in respect of oneself and others. Therefore, when I am at home, I make, as it were, little islands of the field by means of thematic entries in my diary. Thus I define the limits of my field at home.

What happens to my field materials? I start from the fact that field material belongs to a certain social whole which has temporal limits; therefore, having written down my observations, I try not to change them once a certain period of time has elapsed. I might add something that I had forgotten, at the time when I remember it, to the diary with a reference to the date of when it happened. But an addition will be a new record defined by a different date. So for me field records made by me at a particular time and in particular circumstances are a document that I try not to change. And I appear in it not only as a 'character', but also as the one who wrote it down, and thus position myself as the author of the document, a position which is determined historically. So this is my way of objectivising the field and including myself in it.

For me, an interview and its transcript are stable documents. How one translates the spoken word into writing and whether that should be regarded as a valid replacement for the sound recording depends on the tasks in hand. When I write down an interview for my usual purposes — for example, when the explanations given by the person I was talking to are important — I try to strike a balance between closeness to the original and the readability of the text; that is, I try

to reflect in writing the mood of the conversation, using punctuation and a small number of notes. While doing this I rely on my experience of reading fiction or plays. But still, I dislike overloading the text with additional notes, since this breaks the flow of the conversation and will make it hard to read. Besides, when I read a transcript of one of my interviews I seem to hear the voices of the people in it again. And I remember my interviews quite well, at least the general mood and the situation. If I suddenly forget the general tenor and intonation of the conversation (which seldom happens), I go back to the sound recording.

Sometimes my research aims change. For example, the tempo of the speech becomes more important than the meaning of the words. Or I might become interested in the detailed circumstances of the recording. Then I always go back to the primary document, the sound recording, and I am glad if there are any visual materials. Visual materials frequently reveal matters which I had failed to notice when I was present at the scene. Given the 'analogue' nature of the work, a transcript in which the material is processed with the inclusion of objectivised parameters, such as giving the length of a pause between words in seconds, is of no help to me in interpreting the material. Such notes formalise certain phenomena (which might be very important for particular research purposes), but they are not very relevant for me, since they give the impression of the tenor of the conversation at a considerable remove. In this way, the sound file activates my inner vision of the situation of the conversation.

The text file also activates it; but a written text nevertheless conceals a good deal by creating distance. In this sense, speaking of other people's interviews, of course I sometimes make use of them. It seems to me in general that one can work with one's own or other people's material, so long as it is not embargoed. But they are 'dull' to me, that is they lack the extra information that remains outside the record. Not to mention the fact that another researcher collects his / her material for his / her own purposes (which are often different from yours), which has a major effect on the quality of the material. So processing audio materials does not really present any sort of problem for me: you take the recording and you write down what you hear in the manner you find most convenient. If you have some difficulty in understanding it, you listen to it again. If that doesn't help, well, we don't hear everything that's said to us in everyday life, either.

Still, this whole situation becomes problematic when the voices in the interview emerge from the researcher's kitchen and take their places in the reception rooms of a publication. On the one hand, words removed from their field context need to be arranged to meet the requirements of various categories of reader, who as a rule are quite unacquainted with the specifics of our work. In addition, ethical

problems arise on various levels. That is, the creation of an ethnographical work obeys certain conventions which imply that when we present our field we should change what we have found, sometimes to the point where it is unrecognisable. Not only that, if the reader speaks a different language, and the interview has to be translated, the mutability of the text included in the publication as premiss, illustration or evidence, seems almost infinite. Is there anything left in it of that document and its original 'objectivity' and impartiality? This must depend on the ability of the researcher who has to engage in editing and retelling.

On the other hand, much has been said recently about how not all the groups of people who have become objects of research interest have yet been given the opportunity to speak about their lives for themselves. In publications about some communities we seem to take it upon ourselves to organise their lives — or at least, we transform them into our own terms. Many people are thus left without a voice. But the point of our research, it seems to me, is the dialogue which we conduct with our informants, and with our colleagues, and with the wider public. I, for example, have always found it almost magical, the way serious social problems discovered in the field, such as poverty, are transformed into such beautiful analytical texts. In this way we take part in aestheticising social inequality and reduce the extent of our sympathy and involvement. But in any case, ethnography has built up vast experience in representing other people's lives and other people's experience.

Still, I would not say that we know how to represent our own experience and ourselves 'honestly'. We still do not pay very much attention to ourselves. It would probably be a commonplace to say that we have a multichannel construction of reality; but it has somehow come about that we always forget this. We talk about what we have seen and heard, and represent it in words. We think that we are creating / perceiving the world (our system of receiving and transmitting reality) verbally, rationally, in a detached manner distanced from our emotions, our unconscious, or experience, or social position, gender identification, intuition, etc. We seemingly peel off everything 'superfluous', and lock up these peelings in our professionalism, which repeats willy-nilly the model of business and economic rationality — detached and pretending to objectivity. And we lock them up in words — and end up with a text. It is worth recalling that a text, in the form of some physically existing document, cut off from its context, does not change. That is how the convention suggests that we should regard our scholarly publications. But a text, even in its physical form, does change, both in itself (it accumulates commentaries in the margins, the paper turns yellow and wears, and so on) and in relation to its context, and in relation to the person who reads it. Now this has become even more complicated because

texts are reproduced not only in physical space but in electronic space too. What is happening is what Benjamin wrote about photography: contexts are multiplied, and we can have no control over them.

In addition, we are somehow covering up the relationship between research intuition in the field and the supposed objectivity of published research. What do I start my research with? An impression. My impression is intuitive. It is a dreadful thing to say, but before a research question is posed, it always 'seems' to me that it must be in this very place. An impression consists of a flow of many small details: sensations, visual images, fragmentary thoughts. The riches of the field might be expressed in the multiplicity of interpretations. It is they that create the lost volume out of which I draw a thread of research on the basis of my personal experience — what I have done before and what I shall be able to do. The material which I have just begun to collect gives the impression a beginning; the material which I carry on collecting gives it confirmation. As if I don't trust myself and am beginning to check up on myself. And I really do think that somewhere inside me there may be some distortion or partiality. That is, I am still aiming for 'objectivity', whatever that means. Because 'objectivity' is what I share with other people. And I want to share my impressions with other people. Of course, there are also my professional ambitions, and my obligations to the projects, and my fear of being insufficiently professional, and my obligations towards the particular people with whom I have been working. Therefore writing the final text is a compromise between all these factors. Thus creating an ethnographic text is not just setting down your result on paper. When I am working on a text, I try to pick up the field material which mobilises my 'sense of the field'. And in this sense of the field I, as it were, clear a path, build a bridge between my impressions and the external observer to whom I wish to convey these impressions. Of course, creating a text is a process of reflection at the same time. And of course, this reflection need not be verbal.

Because perception is multichannel, the impression created is rationalised in the process of writing the final research text: we create explanatory models in accordance with the rules of the scholarly construction of arguments, etc., so that texts are as a rule the result of rationalisation. But it is to be understood that to rationalise is not to create 'objectivity'. Rationality is also subjective. During the process of rationalisation our perception (which, according to the constructivist anthropological notions which I profess, is also conditioned by our position in an intersectional cell) undergoes a process of normalisation into a text which is then accepted, or not, by the community. I do not think that everyone must perceive this normalised text in the same way. But there must be something in it that stimulates the interest of the people around, so that even normalised texts are not objective, but socially and culturally

conditioned. But it does seem to me that it is precisely the element of the author's subjective experience that makes for a richer text than simply producing a rational general model. And that, it seems to me, is what has changed in ethnography over the last thirty or forty years. Not so long ago we found it important to produce schemes, models and structures. Then it became important to understand how these structures change. What sets them in motion. Now I am interested in how the individual acts within the framework of the structures that (s)he imagines.

One further important category of anthropological field experience is what I would call surprise. My new research subjects usually arise as a result of surprise. Moreover, many of my colleagues also talk about their surprise when they recount their discoveries in the field in informal conversations. At the same time I find surprise a profoundly personal feeling, founded primarily on life experience and only secondarily on professional knowledge. This is probably why it is hard to introduce into a research text. When I try to talk honestly about surprise in describing my methodology — 'at the beginning I was surprised', or 'this seemed unusual to me' — my referees usually call me to account. Sometimes expressively: 'What does "seemed" mean, what does "surprised" mean?' Or more formally: 'One must not base one's research on personal impressions.' Indeed one must not. But the initial surprise, which is afterwards absorbed by research procedure, is always, in my case, an indicator that there are questions here, that it is necessary to stop and do some research, or at least give it some thought.

Why have I touched on the problem of personal surprise? Because it is followed by the problem of the inclusion of the researcher's personality in the framework of the research. In many disciplines the researcher's personal presence is ignored in reports and articles. In this respect anthropology has been much more fortunate. Anthropology is an individualist discipline, and anthropological research, once completed, is hard to repeat. Anthropological research requires the presence of the researcher and of 'somebody else' with particular physical, social, cultural and biographical characteristics. But still, in the end, we are again faced with something 'objective', a result that is separated from the researcher and set down on paper. The question arises, how does one include oneself in the research? How can one identify one's own significant features that have influenced the research? There are many ways. One can turn to history. There are the notes of travellers who do not conceal their own presence and position. There is the position that claims to look at things from outside or from above — the divine position. There is the inclusion of one's own characteristics in the text, when the researcher tells us about his / her own partialities and tries to define his / her priorities, and, through them, his / her vision. Personal

characteristics, experience and partialities are important, since they determine not only the material, but also the choice of the professional predecessors who are significant for the researcher.

Your own position. It is always with you. But to reflect on it in terms requires experience. It is hard to separate yourself from the field. I have my own ideas about the field and its limits and documents. My colleagues have their ideas about how it should be written. I have recently started to include myself among my informants. I cannot exclude myself from the list, since I too am an involved participant in events. The only quality that distinguishes me from other people (and not from all of them) is the ability (most of the time) to see the situation in terms of my discipline, which is what I then do.

### ALEXANDRA KASATKINA

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I have over the years been struck by the anthropological expression ‘to write up’ — to convert one’s field notes into a text for publication — and the practice behind it (see, for example: [Sanjek 1990: 38]). That was when I first started to think about how the reality of the field is transformed into text, and how this process may be, firstly, organised, that is, evidently at least partly managed, and, secondly, organised in various ways. And I discovered that in my own case a complex and wasteful means of translating the field into text had spontaneously arisen, in which field notes occupy a strange position. I try to write down as much as possible in the field. I set aside time specially for this in the evening, and during the day I scribble in my pocket notebook, or more recently in my smartphone (though smartphone notes are the most difficult of all, as they are easily lost or forgotten). I wrote especially large amounts in my first visits to the field, and for various reasons.

My first field, when I was writing my dissertation, was an allotment association, where I spent my holidays every year. At that time I described my everyday life at the dacha in the greatest possible detail, in the hope that the alienating effect of writing would help me to step back from my routine and see research material in it. I will remark that this is indeed what happened, but,

#### Alexandra Kasatkina

Peter the Great Museum  
of Anthropology  
and Ethnography (Kunstkamera),  
Russian Academy of Sciences  
St Petersburg, Russia  
kasatkina@kunstkamera.ru

alas, much later, when my research outlook had been formed in other fields and in conversation with colleagues. My second field, was, by contrast, extremely far away, it was a village in the Malaysian part of Borneo. There I tried to write everything down, because absolutely everything seemed unusual and worthy of record, and I could not rely on my memory. And I was right to do so, because after only a week in this field I ceased to see everything as unusual, and notes like this from my first days are invaluable, because they preserve my first impressions. But never to this day have I elaborated my research texts from my diary entries, and indeed, often enough I have not drawn on my diaries at all, or else I have only remembered about them towards the end of my work, leafed through them and hardly used them.

Is there any need now to question these first clumsy steps of a novice? It would seem that my practice, which developed spontaneously, reflects certain important aspects of field notes as documents and their relationship to the texts which come afterwards, and to field research as a whole. Therefore in what follows I intend to reflect on why my field notes are so strongly distanced from my research texts.

In the first place, in the one instance I came to the field with a research question that was too broadly formulated, and in the other, with one that was too narrow. My interest in allotment associations was everyday life at the dacha (and what could be broader?), and my dissertation still lacked any clear research focus. One can write field notes like that, and they will be 'about everything' (and they were). But one cannot write an article like that; in an article one has to answer a specific narrow question. Questions of this sort began to be formulated after the field, often in response to external requirements from colleagues or at conferences on the topic: attitudes towards power, towards private property, or towards public speaking in the allotment society. On the one hand, there were plenty of indirect data on these subjects scattered about my first diaries, mostly details of everyday life, overheard stories and conversations, and such like. By no means always does one succeed in collecting such details during a focused sortie into the field, when one's attention is concentrated on a particular topic and everything which is not at first sight relevant to it is discarded. On the other hand, I still had to go back to the field and make observations at general meetings and conduct expert interviews.

In Malaysia I had the narrow task of collecting as much information as possible about photographs and objects in the museum that had been brought back from those parts a century ago. Moreover, I was staying at my friends' house in the village and only received the information I needed during meetings with local people knowledgeable about the past, which these friends arranged for me. There were

special pages in my diary set aside for these records. But village life was in full swing all around me, and I could not help keeping a detailed diary, imagining myself to be a pupil of Malinowski's abandoned on a far-flung island for a whole year, although in reality I only had two weeks at my disposal. The result was two thick books of notes 'about everything' (we only had one power cut, but I still hand-wrote my diary for some reason), which may probably be of some use in understanding the contemporary culture of the Mountain Dusun. But I have still hardly ever returned to them, whereas I often cite the records made in conversation with the villagers concerning the objects and old photographs in my articles about this collection in the museum. That is what I do — I cite them.

Why has it never occurred to me that field notes might be the raw material for scholarly articles? And here begins 'in the second place'. The field chronotope creates distance. After the field, my field notes seem like a finished text which documents a particular time and place. This text was written by someone else, who was formed by the situation in the field, and not the person who is sitting at her desk at home writing an article. For this reason we have distance and inverted commas. The effect is multiplied when the notes are handwritten in a notebook. If they are even going to be cited, they have to be typed up first. (How did anthropologists in the age before computers write up their texts from manuscript and typescript?..) With notes written on the computer one finds oneself being hyper-correct: electronic text is so susceptible to change, its status as a document is so fragile, and the temptation to write over and between the lines written in the field is so great that my attitude to such files is one of exaggerated caution.

And finally, in the third place, the very style of what is written in the field plays an important part. In my field notes I give pride of place to details, not to analysis, and put great effort into describing little singularities, fearing to generalise prematurely. I try to reduce the selection of the details to be described to a minimum. In the field I frequently do not have the energy for on-the-spot analysis on the pages of the diary, since the evening hours of exhausting days in the field are taken up with documenting the details. And sometimes I do not have the courage. A good diary should no doubt include both details (you never know what will come in useful one day) and preliminary analysis, particularly considering that a generalisation that has been made may be verified in the field. And of course it would be much more economical to begin to write one's books and articles in the field. But still, how could one do that and still preserve the inductive principle of work, the progression from the material to the theory? How could one avoid imposing upon the field generalisations that were far too hasty? One possible answer would be a long period of fieldwork, not a fortnight,

not a month, but a year or more, continuously or with frequent returns in order to check one's ideas. Alas, the conditions under which scholarship is financed these days do not always allow such a thing.

When I was working on a collective project in Obninsk a different situation arose. There were several researchers who were in the field at different times, and to coordinate their activities they put their field notes on the BaseCamp digital platform. This is a net service for digital work which looks like an ordinary internet forum, where topics can be introduced and one can leave comments on one's own topics and other people's, resulting in topical threads of discussion. Each fieldworker started his / her own topic, where (s)he set out his / her notes from the field: descriptions of meetings with informants, summaries of interviews, impressions from walking about the town or visiting its museums, thoughts about what (s)he had seen, heard or read in books and documents. It was remarkable to see the metamorphoses in one's own manner of keeping field records once the diary had become public: the selection of details was now directed towards colourful reportage, personal deliberations receded, generalisations became bolder, and comments directed towards colleagues (questions, suggestions, doubts) appeared. At first I kept a separate diary of my own and wrote my public electronic records separately. Later I no longer had the energy for this, and I transferred my diary onto BaseCamp almost entirely. Now I sometimes regret this, because certain aspects that I preferred not to write about publicly, or details that seemed superfluous in a short pithy note, thus remained unrecorded and are fading from my memory, and they are important for new interpretations of what happened in the field. On the other hand the public records made then are much more suitable for turning into scholarly texts, and I occasionally use bits of them (and, incidentally, without surrounding them with inverted commas) in my articles and reports. Perhaps the boundary between a diary which is a document and a diary which is the rough draft of an article is not far from the boundary between a dialogue with oneself and a public pronouncement. The forum of a collective project forced me to confer public status on my notes even before I had actually published any research texts.

Since BaseCamp offers the possibility of making commentaries and remarks, sometimes discussions developed around diary entries that resulted in the addition of new details from the field or the formulation of hypotheses. The digital format thus makes it possible to create special field documents — collective, shared, polyphonic field diaries. The possibility of their use for publications raises the question of authorship, and at the very least requires both citation and the author's agreement to the use of particular words (since BaseCamp is a closed platform).

4

The question of the personal nature of the field diary is closely linked with the question of whether it is to be archived. Some textbooks of fieldwork recommend keeping separate personal and working diaries. However, the researcher's personal emotions and reflections are now recognised as an important part of the field experience, influencing the material collected and its interpretation. Therefore separating the description of facts and personal effusions cannot and probably should not always be done. I usually do not, at least while I am in the field. But if I deposit my diary in an archive, I redact it: I remove certain personal elements and also some details about the informants. At the Museum of Anthropology and Ethnography there is no obligation to deposit one's diaries in the archive (only field reports), but nevertheless, if the diary has been brought home from a distant field, I regard it as important for future generations of researchers studying that region, and therefore I usually do deposit my diaries from Malaysia. But the question of the status of the document which I deposit in the archive remains. The diary, after all, has been edited, but no traces of any revision are visible on the printout from the text editor. It will still be useful to our successors, only they will never know what I added or subtracted, or when, or why. Perhaps the revisions should be documented — some indication left where something has been removed? I do not know...

There also remains the question of whether an archived field diary is a stable document, not liable to be altered. As research progresses, data are always refined and errors corrected. An archived diary records a particular stage of the research. It is easy to add or correct something in an electronic diary, but these changes will not be made in the archived copy, and the researcher himself / herself can easily lose track of his / her corrections and of the trajectory of the development of his / her knowledge and his / her understanding of the field. Perhaps someone should invent a format that would allow the various layers that comprise the research process to be recorded. But how could archives work with such unstable documents as electronic research diaries?

2

We encountered the problem of the instability of another kind of field document, the interview, in the Obninsk project, when we started to transcribe the interviews we had conducted in order to put them in an open on-line archive. (For more detail on the project see: [Orlova 2016a].) We had to authorise the texts of our transcriptions — agree them with the informants — before we could archive them. I have written in more detail about the complications of authorisation and its potential for cooperation in my reply to the Forum no. 30 ['Forum...' 2016] (for even more detail see: [Orlova 2016b, Kasatkina, Vasilyeva, Khandozhko 2018]), and on the same subject in the context of the problems of making qualitative data public in my article [Kasatkina 2016]. Here I would like to consider a particular subject

connected with the properties of transcribing an interview as a document: quite often, when our informants received their transcript, they wanted to introduce changes, ranging from minor stylistic corrections to serious rewriting.

The article by Galina Orlova, one of the leaders of the Obninsk project [Orlova 2016b], shows that amongst sociologists engaged in qualitative research at the international level the idea of demonstrating transcriptions of research interviews to the informants is met with rejection, and the attempts that have been made to do so are generally regarded as highly unsuccessful. In the first place, the informant is placed in a position to supervise and evaluate the quality of the researcher's work, which is not exactly pleasant. In the second place — and this is what is most often written about — the informant will want to change the transcription, and this is inadmissible, since only what is said spontaneously is regarded as containing authentic, 'real' information. In our interdisciplinary project, which included historians, anthropologists, literary scholars, psychologists and philosophers, there was also considerable discussion about authorisation. There were objections to showing the informants unedited transcripts, and to stylistic editing which distorted the meaning of words, and to changing the transcripts at the informants' insistence... And when I discussed our problems concerning authorisation with other colleagues from Russia, ethnographers, anthropologists and sociologists, this was the first thing that I heard: 'They will want the text changed and do whatever they want with it! And we'll lose all our most interesting data!'

For some reason whatever is uttered in haste, practically snatched surreptitiously without the informant's knowledge, is the 'most interesting' and closest to the truth. Bourdieu once compared a sociologist conducting an interview and trying to get the most spontaneous, i.e. authentic answer to a psychoanalyst looking for structure and logic in a patient's incoherent stories [Bourdieu 1996: 30]. However, one might think that in an interview situation 'the free flow of associations' that we value so highly in spontaneous conversations with our informants might be controlled, and very much so. After all, our informants do understand that they are not on the analyst's couch, and can structure what they say perfectly consciously on the basis of one consideration or another. After the interview, on mature reflection, their positions might change, and other instances of control might be introduced into the file. After thinking things over in peace and quiet, or consulting other people, or examining literature or photographs, a person might put together a no less truthful, and indeed more accurate version of past events, if it is a question of an oral history interview, or else formulate a more balanced, complex and interesting opinion, or else provide more details about the topic that the researcher was interested in. This is

what often happened in the Obninsk project. In that case, do not the informant's additions to the interview add value to it, rather than spoiling it? Perhaps (s)he should be allowed to do what (s)he likes with it? How has it come to pass that when we analyse an interview we are less interested in what the person says than in what (s)he lets slip? I think that the fault lies with the binary view, long since assimilated by anthropologists, which always identifies a dichotomy such as public / private or official / unofficial and unerringly directs the researcher's attention towards the second element of the opposition, where, so it is thought, lurks (only to be revealed by accident) that which is usually hidden from the eyes of outsiders and is therefore the most interesting and informative. At the same time, certain authors are critical of the use of such oppositions, say, for research into Soviet society, considering that they oversimplify the situation and help to describe a view that is only characteristic of a narrow group of dissidents and their circles (e.g. [Galtz 2004]).

What happens when an informant is actively involved in the work of preparing the transcript of an interview for the archive? Firstly, his / her position relative to the interview changes, becoming active and genuinely informed. Secondly, the researcher gains access to a new type of material — corrections and variants to the same story. This means the possibility of asking new questions not only about the content of what has been said, but also about the circumstances in which it was said and the subsequent modifications. This must be taken into account when the material is archived, and the traces of changes introduced by the informants in the electronic archive of the Obninsk interviews must be visible. Thirdly, a biographical account with subsequent accumulations of changes becomes a dynamic process, which, one might think, corresponds better to the properties of the stories that people tell. Fourthly (and, I am sure, by no means lastly) if an informant undertakes a radical rewriting of the text, the transcript becomes completely detached from the audio recording of the interview, and evidently becomes an independent document which is considerably more flexible and open to new layers of change.

It is only one step, technically and conceptually, from the transcript of an interview with changes made by the informant indicated in it to a new genre of research text and a new trajectory of movement between the field and the text — the transcript of the interview with the researcher's commentary, about which J. Fabian, inspired by the possibilities of digital media for anthropologists, has already written some time ago [Fabian 2002]. In the interactive milieu of the modern internet, if the informants are active users of the net, a publication of this sort could be a space for communication between researchers, informants, other inhabitants of the same field, and any other interested parties, and then there would be a very blurred line between a field document and a publication, the field and the study.

3

In my opinion every anthropologist needs a toolkit for analysing the spoken word and conversation: even if (s)he does not conduct interviews and engages only in observation, (s)he still engages in everyday conversation with the people whom (s)he is observing. Many tools of this sort have been proposed by linguistic anthropology (see, for example, the numerous works by M. and C. Goodwin). It is, however, a pity that the achievements of linguistic anthropology rarely extend beyond its own specific circle of topics. Surprising it may be, but over the past thirty years *Learning How to Ask*, by the anthropologist C. L. Briggs [Briggs 1986], which addresses the questions of the specifics of the interview as a source from the point of view of an ethnographer of speech and provides a working toolkit, has, judging by the citations on Google Scholar, been mainly in demand by practitioners of qualitative sociological research, and not among anthropologists.

In consequence it is possible to see anthropological texts based on interviews and consisting of collections of quotations illustrating the author's ideas which lack any sort of commentary regarding the circumstances in which the words were uttered and even with what sort of intonation, ironically or whatever. At the same time, that sort of accompaniment can alter and even reverse the meaning of the words, and so can the speech situation or dialogue in the course of which the words were pronounced. An interview is also a social situation, an encounter during which an anthropologist can engage in participant observation and analyse the interactions during which the words are pronounced alongside the content of what is said (cf. the recent collection of articles in which anthropologists point out that interviewing is a valid anthropological method and, being a social situation, can and should be combined with participant observation [Skinner 2012]).

The transcription of an interview is the first step towards transferring the oral to the written and its interpretation (and the interpretative power of transcription is well illustrated in [Bucholtz 2000]). But a transcription is not yet a text, but a technical recording of an oral utterance constructed according to accepted rules which are different from the rules for writing a written text [Bourdieu 1996: 32]. Approaching it as a text would be to ignore its richness as a source. To analyse field interviews and other oral genres collected in the field by means of the special tools for the spoken word (whatever their origin — ethnography of speech, discourse analysis, conversation analysis, pragmatics of communication...) is to acknowledge their non-textual nature and to work with them as social events, as a result of which not only their content, but also behaviour and situation may be interrogated. This means a smooth and attentive transition from what has been seen and heard to what is written and published.

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## EKATERINA KHONINEVA

## Ekaterina Khonineva

European University  
at St Petersburg /  
Peter the Great Museum  
of Anthropology  
and Ethnography (Kunstkamera),  
Russian Academy of Sciences  
St Petersburg, Russia  
ekhonineva@eu.spb.ru

**Ethnography as the Reduction of Data:  
Towards an Anthropology of the Production  
of Ethnographic Description**

When I first started working in the field, what worried me most was the potential distortion of the social reality observed as a result of my inexperience, lack of observation, intellectual timidity or idleness (underline an appropriate).

My informants shared my apprehension that I might reduce complexity not only to simplicity, but to an explanation which was irrelevant to any reality. Indeed, to many representatives of various religious communities an anthropologist's attempts to 'pigeonhole' mystical experience which is *a priori* incapable of being systematised or generalised seem naïve, if not downright harmful. I study the practices of self-transformation in Russian Catholicism, and in my case my research focus on the narratives about vocations that circulate in Russian Catholic parishes and religious houses, provoked mistrust for the following obvious reason. The fact that I plan to examine each experience of a Christian's calling by God to one or another way of life on the same level as other similar experiences regularly led to my being suspected of unthinkingly reducing it to a simple set of repeating stereotypes. That is to say, entirely neglecting the uniqueness and singularity of this mystical experience. According to this logic, analysis of the multidimensional space of religious experience is inevitably accompanied by a distortion of its description. At the same time I was predominantly worried about my essential accountability to the academic community and an adequate reencoding as a research text of the fragments of observed reality recorded in my field diary and on the dictaphone — adequate in the sense that, when one analyses what one has noticed and written down, one can indeed draw represented conclusions about an imagined complete picture. In other words, my informants suspected me of a lack of attention to detail and thus of oversimplifying in aiming to describe the whole, whereas I suspected myself of oversimplifying the whole in my work on the concrete details that I had recorded. An awareness of this concern on both sides about an analytical mechanism that constantly eliminates the unnecessary, and, perhaps, that which might be necessary as well, in the process of producing an ethnographic description, strange as it might seem, brought me to a certain indifference towards this property of work with field material.

Some reflection on the limits (or limitlessness) of the reduction of data is of course necessary. This reflection is connected with an understanding that reduction is not only inevitable, but cannot even be interpreted in terms of reversibility. To what condition, supposedly, could it or should it be reversed or returned? As such a provisionally ideal state one might take a completed field diary which recreated the chronological order of events over the period that the anthropologist had spent with the community being studied: in such a case one really could 'wind it back' by returning to this source document with new questions or new answers to old questions. But even the document itself is nothing other than the result of a consequential reduction of data. This process was set in motion even before the researcher arrived in the field: at the very least, the

formulation of the research aims and the questions to guide the interviews already created certain limitations. True, I imagine that anthropologists who unwaveringly follow a course set before they entered the field are in the minority. Most often the things one sees and hears in the field either widen or narrow the research focus. And, continuing the optical metaphor, one may notice that things that are not focused on are partly blurred.

Likewise fieldwork itself represents one of the stages in the reduction of data: the anthropologist chooses whom to talk to, whom to listen to, where to go first and where not to bother with, according to his / her basic research tasks. When working in the field I have done my best to combat this limitation by writing down in my diary everything that I could remember, even if it bore no relation to the problem as initially formulated — it might come in handy. Also, in writing I have tried to be as objective as possible (leaving aside the discussion about whether one can talk about an anthropologist's objectivity at all), not to apply ready-made and expected etic categories to the fragments that I had recorded, in other words, I tried as far as possible to abstract myself from what I expected to see in the field and how I had been prepared to describe it. Incidentally, this led me as a result to a completely different object of study.

Therefore I am closest to the image of the field diary as a document that does not contain any anthropological explanations and is ready for 'secondary use'. At the same time I see returning to the field diary from time to time and adding new details as a serious problem. Turning to the discussion about the problem of the authorship of field notes and responsibility for them, personally I have a simple answer to that question: I am the author. More than that, I am a fieldnote [Jackson 1990]. Filtering of data and focusing are mediated by the anthropologist's consciousness and physicality: this is a fairly obvious thing, but this aspect of the mediation of significance must be borne in mind. Even though I have admitted to aiming for objectivity, even this objectivity is in many ways determined by my cognitive and sensory possibilities. The simplest example, ironically, is human memory: it is natural to us to forget and to reminisce. It is hardly possible to pretend to a description of the most detailed picture possible of what is happening, even if an anthropologist sometimes has the chance of recording things almost simultaneously with the unfolding of events: something will still be forgotten or omitted. But, equally, some details may be reconstructed in the memory some time afterwards, and then one can return to the diary and to the events described. I see nothing prejudicial in this, though I do understand that people have a certain mistrust of their own memories, particularly when it comes to remembering events that are a long way off the present moment.

*'Contextualisation'*

But, if we are talking of the reduction of data, a large part of such research operations of course occurs as part of the prolonged process of reading and rereading, categorisation and reordering of the material collected. Even though within the academic milieu skills and work experience in the field are considered to be the anthropologist's basic identity, his / her main sphere of occupation was and still is work with texts — field notes and transcriptions of conversations. For myself I formulate what happens in the course of this process to a certain extent provisionally, using the concept of context taken from pragmatics in linguistics. Let us suppose that the fieldwork has been completed and the diary takes the form of a more or less representative account of the sequence of events which the researcher had the pleasure of observing or even participating in. Let us admit that this is when the process of categorisation — a scrupulous search for connections and contradictions, persistent templates of behaviour and speech genres — begins. Every anthropologist has his / her own habits of analysis, but in its general form this work is a constant manipulation or game of contexts. When (s)he starts to analyse his / her material, the researcher disrupts the linear structure of the narrative, and breaks it up into quotations, subjects and dialogues. These elements are removed from the context of the temporal sequence recorded in the diary and recombined in accordance with the categories that have been identified. Every subject, taken separately, is placed in a new context of similar things — a set of elements that are characteristic of more or less constant regularities. And it is by creating this new context of patterns that the researcher discovers their connection or dependency on each other. Moreover, these contexts are liable, if one may put it this way, to regular redistribution: one and the same subject may be related to one category after another. And then contexts of a different kind become relevant to this subject. The anthropologist places it amongst similar examples from research on other cases, and now analyses the new picture that has emerged for connections and contradictions. In my opinion, it is thanks to such a manipulation of contexts that anthropological explanations come into being. It is another matter that with every iteration of this process the same fragments of recorded reality may be represented very differently, in the smallest details or abstracted. And the closer one gets to the writing of the research text, the greater the anthropologist's reflection on what can and should be ignored.

*'Aestheticisation'*

Here I should like to draw attention to one of the peculiarities of reduction that most often remains unmentioned amongst the others

and which has a direct bearing on the look of the final products of ethnographical activity — of articles and papers. I have in mind the aestheticisation of the material and of ethnographical writing. I shall give one example. I began my discussion with a story about how at the very beginning of my work in the field both I and my informants felt concern at the possible simplification of the reality that I observed in the process of its description. That is, moreover, how it was; I reached ‘a certain indifference’, as I put it then, not only in connection with the circumstances indicated, but that has nothing to do with the discussion in the *Forum*. The function of the story that I told is not only to state the problem, but also to make an elegant beginning for my deliberations (whether it did so successfully is another matter). An elegant illustration is supposed to be brief and clear, and so subsidiary details are eliminated for the sake of a cleverly constructed one-paragraph story. These are often stories of how the researcher’s interest in such-and-such a problem arose. But in reality, does a research interest so often arise in the conditions of the events described? When working in the field we sometimes catch picturesque formulations in what our informants say and write them down to be used afterwards in the heading of a paper. We also find ourselves looking for anecdotes in the field for our future articles. These stylistic highlights may be detached from their fundamental context and acquire in the author’s text a somewhat different meaning for the sake of producing an aesthetic effect.

The discussion about whether anthropological writing is ‘literary’ is not new, or not entirely [Clifford 1986]. Personally I often notice my colleagues’ remarks: ‘what a beautiful text’ (about some fragment of an interview), ‘a beautiful interpretation’, ‘a beautiful case’, ‘a beautiful topic’. The use of the word ‘beautiful’ looks to me more like a mystification than an explanation. And when I hear such judgments applied to my own texts, be they written or oral, it always baffles me. I do indeed regard the production of texts, among other things, as an aesthetic exercise. But aestheticisation may take place not only at the level of stylistics, but also at that of the choice of subjects, quotations and materials as representative examples. Thus talented and witty narrators, suppliers of ‘beautiful texts’, become our ‘favourite informants’. Anthropological articles and monographs are full of striking examples of informants’ clashes of perspectives, engaging descriptions of events, and eloquent quotations. Duller subjects and texts, from an aesthetic point of view, are mere material for consideration. Ethnographical description looks like a concentrate of all that is most interesting and analytically attractive, and in a certain sense the anthropologist does what Roquentin discusses in Sartre’s novel: in recounting what is at first sight a most ordinary event, (s)he creates ‘an adventure’. But this aestheticisation is always exclusive. The hours and even the days on which one feels like writing

in one's field diary 'nothing happened today' remain outside ethnographical description, but, I hope, not outside anthropological interpretation.

*The field diary: A document by an anthropologist about an anthropologist*

À propos of the *Forum's* last question about other researchers' use of the field diary I recalled a story told to me by a colleague a few years ago. He had been doing fieldwork for several weeks as an individual researcher in a certain village, when another, independent expedition arrived. Obviously, the researchers did not ignore each other's presence in the same field, far from it, they decided to combine their efforts as far as possible. One step towards this joint effort was the new arrivals' request that their colleague should share with them the field diary in which he had been able to record his observations before they came. This suggestion caught the hero of this story off his guard. It was by no means acute questions of the boundaries of representation or the subjectivity of anthropological field notes that sprang to mind at this point; not even questions of the ethics of distributing such data. His chief worry was the possible evaluation of the adequacy of his exposition in his field notes, or, to put it simply, the evaluation of his professionalism. The strategy of looking for a contradiction between practices and representations, of which anthropologists are so fond, was here directed straight at the anthropologist himself. What was the correlation between what was recorded (or not recorded) by the researcher in the course of his fieldwork, the reality he had observed, and those elegant, witty articles that he wrote on the results of his activity? Would not a contradiction be found here too between words and actions?

This young anthropologist's anxiety (though I do not think that this could not concern more experienced researchers too) is understandable, although the practice of making one's field records available to interested colleagues and readers is one that I would rather regard in a positive light, and first and foremost for the authors themselves. Access to the field diary, incidentally, like the answers to the questions in this *Forum*, exposes the mechanisms of research, and makes the means of production of ethnographic knowledge more transparent and accountable. As a consequence it motivates the anthropologist to reflect on the process more. But when one considers the prospects for the readers and potential users of these diaries, one must bear in mind certain circumstances or, if you prefer, limits. The first regards the relationship which the researcher has built up with his / her informants, and the absence of any such relationship between anyone who is using a diary written by someone else and the heroes of the story. Participant observation presupposes sometimes

exhausting work winning and maintaining the trust of the community under study, and the agreement that the data obtained will be anonymous. In the best case the informants begin to share their personal experience and current concerns with the researcher. They may not have given special permission to record particular fragments of their everyday life, but when the researcher describes these subjects in his / her diary, (s)he always understands that it is in his / her own interest not to abuse their trust. These fragments become valuable material for reflection, and not picturesque vignettes for research texts. When working with other people's field diaries, we shall most probably be guided by a no more than approximate idea of the attitude of the community described towards the limits of the privacy of the data recorded and their blurring or infringement. The second circumstance regards the obvious fact that a field diary is not raw data awaiting sophisticated analytical procedures. However hard (s)he aims for objectivity, the researcher interprets the material from participant observation even as (s)he describes it — and all the more so if (s)he is not even aiming for objectivity. Therefore the most important thing that anyone working with someone else's field notes should do is to discover the author's perspective, and then either try to get rid of it (whether this is possible is another matter) or else make it yet another object of the research.

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### OLGA KHRISTOFOROVA

1

When talking with informants I make a sound recording (if the informant does not object) and make notes on paper (if this is possible). After the interview I write everything down in detail in a paper diary. (When I first started to work in the field there were no such things as laptops, and besides, it is not always convenient to use a computer in the field, for various reasons, so I cannot imagine fieldwork without a thick notebook and a pen.) I record the whole course of the conversation in detail in my manuscript diary, its context, my observations and my

#### Olga Khristoforova

Russian Presidential Academy  
of National Economy  
and Public Administration /  
Russian State Humanities  
University / Moscow School  
of Social and Economic Sciences  
Moscow, Russia  
okhrist@yandex.ru

impressions. I write some phrases — exact quotations — in inverted commas, copying them from the notes that I made during the conversation, and I note down extralinguistic factors (mimicry, gestures, pauses, emotional reactions; I add them to the transcript of the sound recording in the form of notes). I make notes in the margins of the diary about the sound, photographic and video recordings, if they were being made during the conversation. After the expedition I list the contents of the diary and indicate the headings and certain other positions with a marker pen, after which the diary becomes a document and does not undergo any further editing. In parallel I transcribe the sound recordings (in electronic form), which then also become a document.

It seems to me that a field diary ought to be individual even when the fieldwork is done by a group: it matters who exactly saw the situation in just such a way; a collective field diary (and I believe certain colleagues sometimes produce such things) seems like a sort of ‘letter from Prostokvashino’.<sup>1</sup>

2

In consequence, it is, firstly, the transcripts of the interviews with annotations describing their non-verbal elements and context, and, secondly, the manuscript diary, which contains fewer of the informants’ exact words, but pays more attention to context and observations, which become ‘written sources’. A source understood in this way is a ‘cast’ of an oral utterance — not a complete copy, of course, but, it would seem, a perfectly adequate ‘transfer’, which contains in addition the researcher’s reflections on what has been happening and what meanings — not necessarily altogether evident at first sight — were contained in the communicative situation. In the transcripts these reflections are given in separate annotations, so as not to confuse them with the record of the actual conversation, and also, in greater detail, in the manuscript diary.

The transcripts are edited until there is no more unclear text in them, and then they become a stable document. I have never had to change any transcripts after that, but I can imagine a situation when, for example, the transcript has been made by a student, and then his / her supervisor listens to the recording and corrects the transcript.

If one is confident in the quality of the transcript and there is no need to verify it against the sound recording, then there might be other motives for returning to the recording: it might be a mnemonic technique (to remember the informant’s face, how his / her house was furnished, or some circumstances connected with the interview), and sometimes it helps one to question the material in a new way, see it differently, and produces new ideas.

<sup>1</sup> A reference to the stories of the Russian children’s author Eduard Uspensky. The letter in question was written jointly by a boy, a dog, and a cat [Trans.].

3

There may be different strategies for working with field materials and different principles for their publication: this depends on the scholarly workshop, the paradigms and methodologies of research, and also the purpose and character of the publication. The usual way is to use quotations from interviews within an analytical article, giving preference to fragments of the transcripts of the recordings, but also allowing quotations from the field diary (with the appropriate qualifications) if there is no recording or if it is a matter of something that could not be reflected in it. In folklore studies it is usual to publish fragments of the transcripts which are understood as discrete folkloric texts (chastushki,<sup>1</sup> songs, ghost stories, legends, jokes, etc.), while it is the author who decides where the fragment (s)he publishes begins and ends. In anthropological and sociological works it is frequent for extracts from interviews to be cited as examples that illustrate the author's position. It is possible for large extracts and even complete transcripts to be published (though I have not often come across that), so that the reader can grasp the informant's point of view and his / her and the interviewer's communicative strategies, see how meanings come into being and what initiates the putting together of 'a folkloric text', and, ultimately, feel the atmosphere of the conversation.

As it seems, scholars in this country rarely use the strategy of creating an analytical text directly out of the diary, and if extracts from this are published, it is 'in brackets', as a sort of 'lyrical digression', and not as part of the analytical text. Moreover, such lyrical digressions are felt to be part of the 'back kitchen' of the research, and we still do not feel that it is proper to put it on show, or so it seems to me. Very recently I was present at the discussion of the manuscript of a monograph at one of our institutionalised anthropological communities, where there were scholars present who were highly respected and enjoyed high status, with various higher degrees. One of the comments made to the author — and almost everybody there agreed with it — was that all the 'diary descriptions' must be deleted from the book, as 'too personal' on the one hand, and as manifestations of 'that postmodernism, which is now, fortunately, over', on the other.

As for me personally, I use all these strategies, often all in the same publication (if it is a book or a long article in which it is possible to 'play' with the forms of presenting the material). And, as experience shows, the results cannot be considered unambiguous. On the one hand, making extracts from the field material to illustrate the author's theses is in a way doing violence to the source, a reduction of the meanings contained in it. On the other, that sort of text is more easily

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<sup>1</sup> Simple songs consisting of short rhyming lines, frequently on humorous or indecent subjects [Trans.].

understood by the reader. If one gives large segments of the transcripts, allowing the informants ‘to speak for themselves’ and hoping that the reader will see the richness of meaning in what they say, ‘will feel their culture’, then everything depends on the reader and his / her purposes: some people will find such a method valuable, because it helps them to come into contact with ‘local knowledge’ with a minimum of mediation, or else the materials published might be useful for their own research, while other people might find it a bore to have to read through long extracts of interviews, and they will hurry through the pages to find out what the researcher, not the informant, has to say, and how (s)he will treat the interview analytically.

A toolkit for working with the spoken word (conversation analysis, discourse analysis, etc.) may be extremely useful, but here too everything depends on the character of the material and the aims of the researcher.

4

Field materials, even those kept in someone’s personal archive, must be accessible to other researchers. (Those which for ethical reasons cannot be opened for a particular period of time should be marked as such.) Of course one can and should work with other researchers’ data — that is what we do when we study archives. Personal presence in the field is a most important thing, but it is not always possible, for example when for one reason or another the field has ‘gone’, but there remain sound and video recordings, transcriptions of them, and field diaries.

## MIKHAIL LURYE

### On Field Notes and Other People’s Archives

1

3

I think that the status we give to our field notes and the later forms of their use in academic texts is determined not only by how much of them is due to the outside world observed by the researcher in the field, and how much to the researcher’s own inner world, but to a large extent by the pragmatics and form of the notes themselves. When I first began to go out into the field (at the end of the 1980s in literary folklore expeditions), we did not keep individual field diaries as they are understood today at all. Of course, we did write things down, and quite a lot of things, but our notebooks could rather be called journals: this was where we wrote the names of the villages, recommendations

Mikhail Lurye  
European University  
at St Petersburg  
St Petersburg, Russia  
mlurie@inbox.ru

of whom to visit, our informants' 'passports' (i.e. their names and biographical data), and, without fail, summaries of the interviews, which were made in parallel with tape recordings of them, or, if for some reason no sound recording was made, we wrote down the 'texts' themselves (as we defined and labelled our material in those days).

There were several reasons why the format of a manuscript diary was absent from our field practice of that time, among them the fact that the field was chiefly interesting for its literary folklore, which was supposed to be written down, not described, was the main one, but not the only one. I shall give four such reasons.

Firstly, there was no institutional tradition of this skill. Literary folklorists were taught how to write down folklore material and provide it with a 'passport' and an attribution, and also how to describe ritual or other ethnographic contexts, but were not given any special schemes or models for recording current observations. In the field, therefore, apart from the folklore, everyone was free to record whatever and however the promptings of his / her own experience of keeping notes, his / her scholarly or aesthetic preferences, and practical considerations might suggest. At the same time we had in front of us other quite distinct and authoritative models for narrating immediately observed ethnographic realities — the numerous remarks of those ethnographers of the second half of the nineteenth century and beginning of the twentieth who were fortunate enough to encounter this traditional culture while it was 'still alive'. These were the 'stable documents' and 'written sources' which enchanted us with their completeness and expressiveness, while the process of their genesis was not particularly called into question. (These same texts, incidentally, served us as excellent textbooks of how to objectivise observed reality while placing the observer outside it, a practice which anthropologists nowadays warn each other against or reproach each other for.)

Secondly, we saw it as our aim to capture that reality which was not observed — the traditional peasant culture which, as we then thought (with certain reservations) belonged on the whole to the past and was accessible largely through fragmentary stories and reminiscences. Therefore observation (the recording of which is the prime function of a field diary) was for us not so much our 'field method' as an unavoidable (but in fact extremely important) side effect of our field, requiring no special instruments to work with it.

Thirdly, although working with cassette recorders in rural conditions was often problematic and the ability to 'take everything down by hand' that the informant said or sang was still an important professional competence, recording folklore in writing without a sound recording was already regarded as not entirely valid. As

a means of a more exact, reliable and permanent recording of verbal and musical fragments, the tape recording was regarded as the basic and in a sense sufficient form of 'receiving' the material, and as material in itself.

Fourthly, we always worked in groups of two, three or four people, and all the impressions, observations and ideas about the 'texts' that we heard, the locals and our dealings with them, the life of the village and the situations we found ourselves in were perceived as common to the whole group. They were shared at once, and thus lost their individual character, as soon as we had a chance to exchange a word our two without our informants' being present — during a break for a smoke, travelling from one village to the next, or during an overnight stay, if our accommodation was sufficiently independent, and so on.

The need for keeping a running record appeared at the point when it became clear that the cassettes and files of transcriptions of 'texts' that remained after every trip were by no means everything that you had seen, heard or noticed while on the expedition. As a result, people's research interests changed as well, and observation became an important part of fieldwork. Nevertheless, I think that the way I take my field notes is partly connected with my early experience and with certain of the circumstances described above.

In the field I record separate observations, and set down spontaneous dialogues with my informants or 'overheard' conversations, trying to do so as soon as possible afterwards (sometimes speaking into a dictaphone in the manner of a report from the scene of the event), and I also make short notes 'to help me remember' of the ideas that occur to me while I am working. In addition to this, I still find discussions with colleagues in the field no less an important and convenient means of externalising my field impressions and ideas. Last year I had the interesting and, I think, productive experience of setting up a closed community on Facebook within a particular collective project during the period of work in the field, so that groups working in different places could regularly upload their field notes, which were immediately commented on by other groups and sometimes developed into discussions.

I do not, however, keep a 'classical' field diary in the form of regular narrative sketches. The systematic narration of field observations (and material in general) suggests to me, to use an old metaphor, their transformation from 'ore' not even into 'metal', but into finished 'parts' — a final, integral, literary text, and therefore it is admissible only in the process of work on the ultimate research outcome. In my case, therefore, turning field notes into a stable document which can be quoted and referred to, or working them up directly into the text of an article, are both impossible.

4

The researcher's personal engagement in fieldwork is a sort of gold standard, an essential attribute of model anthropological research. Why this is really important is hardly worth discussing. But in the end it all depends on the famous 'subjects, objects and methods', and in certain cases the real significance of the factor of 'one's own field' may approach zero. Research conducted on the basis of interviews wholly or partly conducted via 'outsourcing' is not so rare (particularly projects of a retrospective, 'historical' character), and its relative value, *ceteris paribus*, depends far more on the interviewers' professionalism than on whether the author was one of them. Other projects are only possible with material collected by the researcher's predecessors, and sometimes quite remote predecessors. To put it schematically, one might say that the coefficient of the necessity of the researcher's presence in the field increases according to its position on the scale of primary methods (from the structured interview to participant observation), on the scale of objects (from the study of texts to the study of practices), on the scale of time (from the past to the present), and on the scale of features of the socium studied (from amorphous commonalities and 'imaginary communities' to compact groups).

As for whether it is permissible to make researchers' materials available for use by others (or for general use), there simply cannot be an unambiguous answer to this question, because in every case there will be a different configuration of individual ideas, ethical conventions and corporate or legal requirements. Much depends of the character of the material: field notes, it seems, are more individualised and even intimate than interviews. I am convinced that archives ought to be open to all researchers, but I am not convinced that everything ought to be placed in the archive, or straightaway. Let us say that I always share some of my material with a close circle of interested colleagues, but I would not be prepared to deposit all of it in an archive or make it universally accessible. However, there is some point in making available, after a certain period of time, material of potential interest to others generated by large collective projects involving fieldwork, once they are completed.

#### OLGA SASUNKEVICH

My experience of ethnographical research is perhaps not the most informative. I did not have an anthropological education. I am engaged in feminist and gender studies and the study of borderlands, and my undergraduate education was in areas such as communication theory and sociology. My PhD was taken in a department

**Olga Sasunkevich**  
University of Gothenburg  
Gothenburg, Sweden  
olga.sasunkevich@gu.se

of Eastern European history in Germany, but my work is somewhat unconventional by the standards of German historiography, being concerned with the female experience of cross-border trade on the frontier between Belarus and Lithuania, beginning in the 1990s, and based on ethnography to a much greater extent than on a historical methodology. In this way my relationship with field methods originates in practice rather than theory, although, being engaged in gender and feminist studies, I do keep a careful eye on the methodological discussions regarding the ethical questions of interrelations with informants male and female, inequality in interaction between male / female researchers and male / female informants, exploitation, power, own / alien fields, and so on. My experience of field research includes two completed projects (on female cross-border trade and the ethnicity and self-identification of Belarusian Poles) and my current research project on feminist and LGBT activism in Russia (undertaken under the direction of my Swedish colleague Mia Liinason and financed by the Knut and Alice Wallenberg Foundation). That is, what I say below reflects my own experience of ethnographic work and my own vision, and may not be a perfect fit with the current discussions inside anthropology and ethnography.

I always keep a diary when I am working in the field. That is how my colleague Elena Minchenia taught me on her course on qualitative methods at the European Humanities University at Vilnius, that is how I teach my students, and that is how I do it myself. I became convinced of the necessity of keeping a detailed diary in practice, when I was writing my dissertation. Some of the records in my diary were at first sight odd. They described absolutely insignificant interactions with people in the streets, in the library, on public transport, and had no direct relevance to the subject of my dissertation. Since my 'field' was a small town and there were not many people at first who were willing to share their experience of informal cross-border trade with me, I had to find something to do. Therefore as far as I could, and as the occasion arose, I entered into all kinds of insignificant interactions and then described in detail who had said what. Afterwards, when I was writing up my dissertation and trying to show how everyday life in a small provincial town on the border between Belarus and the EU was organised, these notes proved invaluable. Furthermore, I saw that many of them were, in any case, constructed around the interpretation of informal economic practices (including, for example, work on the allotment, which had no less significance in this little town than trading in cigarettes or foodstuffs brought across the border). But I had had no inkling of this when I was collecting the information, and much valuable data might simply have been lost.

During the same project I realised that my way of keeping diaries was not very convenient for subsequent analysis. I had, in fact, two

diaries. One of these consisted of handwritten notebooks in which I made notes during my trips across the border with the women who were transporting goods, or else wrote in detail of my impressions while waiting for the bus. (I never took a laptop with me.) The second consisted of Word files, which I wrote in the evenings, describing in detail what had happened during the day, and also reporting on my interviews (where and when they had taken place, what had been said that had not been recorded, etc.). Overall, when I was making my analysis, working with these two forms of diary was not very convenient. Now, as I am trying to learn NVivo for another project and make analyses on it, I understand that there will be real difficulties with handwritten diaries, since they will never be part of the mass of digitised data. During my second project I wrote nothing by hand and did everything in an electronic format. But now, working in Russia, I have returned to the format of 'the handwritten diary'. During three fairly short field trips I have filled three thick notepads. At the same time I continue to keep detailed diaries in an electronic format. On the whole I do not know how I shall analyse all these data, but my handwritten diaries are important to me, because I take part in various activist events during which I try to document what takes place in detail. Typing, that is, recording everything immediately in electronic format, would not be possible.

Still I have no practice at editing the diaries. I carefully set down all the information that I have collected and leave it for better times. I do not know whether this is the right thing to do, but unfortunately I have neither the time nor the energy to return to my diaries while the project is still at the stage of the active collection of data. It would be truer to say that I do return to them, but only to those aspects of them which I need to move the project forward (various notes about how the project might develop, what interesting directions are emerging, and so on). I must say that keeping diaries takes up a great deal of time, particularly those written after the occasion (after meetings or events). Sometimes I run out of patience and want to write very briefly, but I force myself to remember everything in detail and record it, because I do not trust my memory, and, moreover, I know that the theoretical ideas developed in the course of carrying out the project may have a strong influence on my 'recollections'. No, I don't want to take the risk, I have to write it all down at once.

As for oral information, I make a distinction between random field interactions written down afterwards and interviews with those informants, male and female, in whom I am interested and which I regard as a separate source of data. I try to record chance encounters and conversation in the field at the first opportunity (I try to avoid doing so during the actual conversation, as a rule, so as not to be distracted and not to lose contact with the man or woman I am talking to). But I use them as a source of supplementary information,

and not as fully-fledged data, in the sense that I have a different approach towards the analysis of interviews, being as a rule interested not only in the content but also in the form. This is particularly important for my project on ethnicity, since I am trying to demonstrate the performativity of ethnic identification, its non-predetermined character, and how people question themselves during the interviews, trying to understand who they are and which ethnic group they belong to. In order to record this a sound recording and a detailed transcript are very important; there is no other way that I could undertake such an analysis. Here too I regard the aforementioned application of the tools for analysing communicative interaction as very important. Sometimes I myself try to work with narrative and discourse analysis. But many elements become comprehensible not out of the contents of the interview but out of its tonality, its mood, the choice of words, the pauses. This is sometimes hard to grasp, but it is very important. My interdisciplinary background is obviously making itself felt here. This sort of combination is normal for me, so the question of whether anthropologists should make use of such analytical methods is not one that I entirely understand.

This is the point where I am faced with the problem of whether it is possible to use data collected by other researchers. There is a certain poetry in fieldwork, when not all meanings are perceived rationally. Something arises during the process of communication, and it is hard to convey this 'something' in words without having experienced the situation alongside the other participants. At this level I suppose I am a conservative ethnographer. I am not against data being made available and being used by colleagues, and moreover when I discuss the process of transcription with my students I always ask them to transcribe in the greatest possible detail, so that the same data might be used by another researcher. This is why the sound recording of an in-depth interview is so important. Not even the most detailed transcript could convey everything, and someone who was not present during the interview can be helped to feel the situation by the actual process of the conversation, the voice of the informant and his / her emotions and pronunciation. But I am not sure that even this is sufficient. Personally, I have many insights while I am in the field. It is hard to understand how I begin to be aware of what is happening and sort it out into a more or less comprehensible picture, but I find that there is a certain magic whereby everything suddenly falls into place. It seems to me that when a researcher is working with other people's material and has no experience of being in the field, this magic is lost. Not that the research itself might suffer from this; participant observation, keeping diaries and analysis are, after all, the techniques of the scholar, and not of the shaman, but it is important to me. I have the feeling that for the time being I myself am not likely to cross this border and start working with other people's data. I am

not even entirely comfortable in situations where I hand the interviews that I have conducted over to someone else to transcribe. It seems that I shall inevitably miss something in the analysis. But unfortunately constraints of time and the amount of work leave me no choice.

## ANDREI TIUKHTIAEV

1

It seems to me that the stability of a diary as a document depends on the type of diary that it is. When I go out into the field, my work schedule takes up almost the whole day. There is not much time left for keeping a diary. There is always something happening around me, and one of my informants will be distracting me from making notes by talking to me or suggesting that we should visit some interesting event. Besides, on one trip I was living in a tent village for pilgrims, and the only private place I had was my living space, which was only waist-high. In a situation like this it was tempting not to write extensive field notes, but to tag what was going on so as to write it up extensively after I had gone home. However, since I could spend up to two months at a time in the field, relying on my own memory was not the best solution. It should be pointed out that my role as a person who regularly wrote things in a public place (to be precise, in a certain café) made me quite conspicuous. Sometimes this was a cause of suspicion on certain people's part, but more often it made it easier to get to know people and explain the purpose of my visit. The New Age pilgrims whom I study are for the most part townfolk, temporary visitors to a holiday resort on the shores of the Black Sea. They are easy to make contact with, and the whole pilgrim area is a place of heightened social involvement in comparison with, say, a city street. Taking these circumstances into account, I tried to record as many observations as possible in my diary. In so doing, my aim was to write down the situations that I had observed and the conversations that I had had with various people, but not to include my own analysis or any subjective impressions connected with what I thought about the material that I was collecting.

### Andrei Tiukhtiaev

European University  
at St Petersburg /  
Peter the Great Museum  
of Anthropology  
and Ethnography (Kunstkamera),  
Russian Academy of Sciences  
St Petersburg, Russia  
atiukhtiaev@eu.spb.ru

On my first trip, which was also two months long, I preferred the sort of diary that assumes a mixture of observations and initial analysis. The description of events and recounting of conversations was combined with this categorisation and attempts to begin conceptualising what I had seen and heard. Now I find the technique based on combining of these operations, but keeping them in separate spaces, more effective. For example, my notes may be accompanied by analytical commentaries in the margins, or these latter may be transferred to a different notebook or file. The advantage of this second technique, in my view, is precisely that it allows the creation of a more stable document. Etic categories may start to subordinate the material to situational research tasks, depriving the anthropologist of the possibility of drawing on it in future and looking at the data collected from a different viewpoint. In other words, as Margery Wolf remarks, as one collects material it is worth remembering that it might be brought into play in future for new purposes [Wolf 1990: 350].

3

Although the field diary is often regarded as the anthropologist's prime source (and this is true in the case of long periods spent in the field), we are more frequently seeing quotations from interviews and other sound recordings used as material in articles and monographs. It is far from every anthropologist who studies a community, and by no means every one spends a year living among his / her informants. The interview is becoming an ever more important source, thanks to which many details of the lives of the people being studied may be elucidated. In the case of my own research project I see my informants in a context which is quite specific even for many of them, and know practically nothing about their everyday life in the town. Although my work is focused on pilgrim practices, comparing them with ordinary urban life may lead to the discovery of interesting facts. In fact the only thing I can do is ask them questions about what they usually do in the town and how they do it. By asking the question in the right way one can get a more or less objective answer, which makes up for the lack of participant observation.

Besides, since I am interested in traditionalist narratives, and also in the discursive and rhetorical devices used to legitimise them, the texts generated by my informants come to the fore as a type of source. For the most part I obtain such texts by means of sound recordings of various lectures, seminars, concerts and interviews. As a result quotations from sound recordings are inserted into the article or dissertation more often than anything. In this case the field diary is used as a means of fixing the context in which the recordings were made and the interviews conducted, and also information about the informants. The diary also includes accounts of conversations and curious instances of interaction between the pilgrims, but all this functions essentially as supplementary to the narratives recorded on my dictaphone. This does not, however, exclude the possibility of

there being records in the diary which it will later be necessary to include in the text of an article, but usually not as direct quotations, but translated into the language of academic writing. Since I bestow more attention on the principles of the construction of narratives and their legitimising, I use quotations as illustrations to my own generalisations. Extracts from interviews serve as the most typical instances, the patterns which I have been able to identify. However, the priority of this means of including material in the text, in my view, is determined by the specifics of the subject of research and the chosen analytical viewpoint.

I see the question of how the anthropologist's research practices are organised as no less important than the choice of a strategy for presenting the sources in the text. It is through these practices that the process of turning sources into text takes place. It may be a young researcher's misapprehension on my part, but I am forming the impression that there has not been enough discussion about methods of working with material in the Russian-speaking anthropological community. On those courses which I attended there might have been something said about the techniques of interview or anthropological theory, but the intermediate stage between the collection of material and the published text was often ignored. As a result everyone starts acting on the basis of intuition, and it is often impossible to verify from the outside whether a particular extract from an interview in an anthropologist's article does represent a pattern that (s)he has identified, or whether it is just a striking quotation. I find methods of reducing data such as indexing and coding useful when working with material such as field notes and transcriptions of interviews, and also books and brochures published by my informants. They are described in particular by Kathleen and Billie DeWalt [DeWalt, DeWalt 2011]. According to these authors, indexing represents the reduction of data using etic categories, whereas coding takes place on the basis of emic categories. The combination of the two analytical operations is useful, insofar as it avoids the extremes both of excessive immersion in the informants' context and of oversimplifying it by using the material only in accordance with research aims that are determined situationally. Besides, the actual process of developing categories allows the widest possible view of the material. Working with etic categories helps in posing the problem of research questions set in advance, insofar as in the process of checking the correspondence between these categories and the data, it becomes more evident which questions are capable of being answered, and which are not.

4

The answers to the questions about allowing other researchers access to one's material and about the factor of personal presence in the field depend entirely on the aims of the particular research. Whatever a diary is like, it will in any case be interesting to historians of science

in  $n$  years' time, but other people's data will also be useful to colleagues working on similar topics. I shall not here address the problems connected with the ownership by particular communities of the data collected, or with making personal sources generally accessible. One way or another, we make use of the interviews we have conducted and the photographs of our informants, and the question of making them generally accessible is often solved without particular difficulty when it turns out that people do not see any ethical problems where anthropologists see them. I shall touch exclusively on the problem of the analysis of material. An anthropologist often finds it useful to compare his / her own observations with other researchers' material collected in similar contexts. However, the immediate text that we check against will be somebody's article or monograph, i.e. the results of specific research with its own specific aims and theories. Reading texts about the New Age and pilgrim practices, I often find myself in the situation where the work seems insufficiently convincing from an analytical point of view, but may at the same time contain information that interests me. Perhaps if I had read about this information in a diary or discovered it in an interview, I would have had more detailed data, and comparing them with my own material might open new analytical prospects.

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## AIMAR VENTSEL

### Field Notes and the Hidden Story

*While their historical precedent is uncertain, anthropologists can readily be identified on the reservations. Go into any crowd of people. Pick out a tall gaunt white man wearing Bermuda shorts, a World War II Army Air Force flying jacket, an Australian bush hat, tennis shoes, and packing a large knapsack incorrectly strapped on his back. He will invariably have a thin sexy wife with stringy hair, an IQ of 191, and a vocabulary in which even the prepositions have eleven syllables.*

**Aimar Ventsel**  
University of Tartu  
Tartu, Estonia  
aimar.ventsel@ut.ee

*He usually has a camera, tape recorder, telescope, hoola hoop, and life jacket all hanging from his elongated frame. He rarely has a pen, pencil, chisel, stylus, stick, paint brush, or instrument to record his observations.*

*This creature is an anthropologist.*

<...>

*You may be curious as to why the anthropologist never carries a writing instrument. He never makes a mark because he ALREADY KNOWS what he is going to find. He need not record anything except his daily expenses for the audit, for the anthro found his answer in the books he read the winter before. No, the anthropologist is only out on the reservations to VERIFY what he has suspected all along — Indians are a very quaint people who bear watching [Deloria 1989: 79–80].*

These lines were written in 1969 by Native American lawyer, activist, and professor of political science Vine Deloria Jr. It was the 1960s, a period when the civil rights movement swapped over to the reservations of Native Americans. As a result, a native rights movement emerged, led by a few university educated indigenous intellectuals. Vine Deloria Jr. was very critically disposed, not to say hostile, towards anthropologists. Probably with good reason. To be precise, he accused anthropologists of exploiting Native Americans, and their knowledge, in building their own academic careers. Deloria dismissed anthropological research and publications as useless knowledge that had no benefit for the indigenous population. The disappointment had also a concrete foundation — namely, according to Deloria, anthropologists did not show any support for the indigenous rights movement.

What is striking here is how the essence of anthropological research and knowledge is reduced to the notebook. This is what seemingly defines the essence of discipline, and the lack of it demonstrates the total uselessness of anthropology and anthropologists.

Field notes are, indeed, very important. I would like to cite my former boss in the Estonian Literary Museum, Mare Kõiva. In one of our endless discussions she said: ‘When you go to the field, you have plenty of expensive equipment to keep an eye on. When you return from the field, the most valuable item for you is the dodgy diary with your field notes.’ Which is very true. This worn out diary with its hectic content is the most precious item, the thing I care about when I collect my belongings and head home after the research trip. I keep it always with me and store it in the bag I always carry with me. On my way back, I have the habit of reading my field notes when spending time in buses, trains, or planes, I add short comments or underline names or quotes.

I must admit that my fieldwork diaries are a total mess, understandable only to me. Not only that my handwriting is unreadable to anybody else, and the notes are written with any pen I had to hand. When recently going through my field notes from my one year PhD research period, I discovered that I had made them in at least five languages — Estonian, English, German, Sakha, and Russian. It proves that a very big part of my documentation was conducted spontaneously, I just had time to quickly write in the moment, whether a short discussion with a taxi driver, or a short summary of the last day. Reading my field notes was an act of time travel. I not only remembered situations, I reenacted them in my head. I was in the middle of events again. I felt the sour smell of freshly processed furs laying in my bed, the cold wind of the tundra when riding reindeer during the weekly migration; I had the taste of a strong black tea and red caviar on my tongue. I remembered all the people I lived with, what were their daily routines, and how our relationship evolved over the months. I also remembered that a big part of my fieldwork was ‘doing nothing’ [Corrigan 1986], days filled with repetitive routine — tea in the morning, catching reindeer, harnessing them, chopping wood, waiting for another tea, lying on my sleeping bag reading some book, sitting with reindeer herders on a log and smoking in total silence. There was nothing new, I collected very little new facts, and even fewer were worth putting down on the pages of my diary. Or this was what I thought in that moment. In order to justify this evident lack of progress, I adopted the belief that every day that I wrote at least a sentence in my diary should be regarded as a successful research day.

My fieldwork notes are not meticulously written reports where everything is analysed in detail. This is the big difference between them and the field diaries of some of my past and present colleagues, who manage to produce well ordered and coherent texts. My field notes are a total mess, as I mentioned already. There are shorter or longer texts mixed with phone numbers, quick drawings of details of a reindeer harness or Arctic fox traps. Pages are decorated with oil, fat, or dirt stains, words are shortened in a way that only I can understand (and if not I must reproduce the whole situation in my head in order to decipher what I wanted to say, because that was apparently important), the language shifts within a sentence — I can start in English, and after a few words in Sakha, end with Estonian. This way, the diaries remain a chronological report of my fieldwork, which helps to restore the pecking order of events. The problem was (and still is) that things happen unexpectedly, and by no means always could I take my pen and write everything down. It is impossible to document a spontaneous discussion when you are riding reindeer in a windy tundra, your voice recorder lies — packed in a rucksack — on the sledges, and you are focused on keeping your balance so as not to fall off the reindeer saddle. Or when you go to visit someone in

the middle of the night, and when opening the door you see a mass fight in the living room. When grown aggressive men are pummelling their fists into each other face, you just don't sit down in the corner and start describing the situation. And of course, there were moments when I was too exhausted to fulfil my obligations of keeping a diary regularly. In a situation when you have to drive snowmobile eight hours in a snowstorm on a polar night, you only have enough energy to empty your mug of tea, fall down on reindeer hides, and sleep. In such situations, I noted down a few keywords I thought should be fixed in a diary, and wrote it up during the first moment next day. As the fieldwork progressed, my brain turned to a recording machine, and I could glance at the keywords, and restore the key points of the discussion or an event, to document it all in the diary.

When reviewing my field notes I noticed that I have changed my style of documenting over time. My first notes are very short ('I met Andrey, the brigadier, and he took me to the herd where we spent three hours sitting, discussing and watching animals') and I discovered in the middle of that year that this was not enough. Sometimes things happened very quickly and I had problems remembering what we discussed, whether it was of any interest at all, what people had taught me, and how that particular part of the tundra looked. So I applied a more detailed way of describing the situation. I wrote about my emotions and thoughts, whether I felt comfortable in the situation, what voices and mimicry were used by all sides. I also started to describe material objects in the way used by the classic Soviet ethnographers of material culture — I recorded the size, colour, and shape of axes, rifles, sledges, dwellings, roads or buildings. Over time I developed the habit of having a 'reflective day' on those days when apparently nothing happened that I counted worthy of writing down. On those days I spent an hour or so with my diary putting down my thoughts, interpretations of events, analysis of body language, and so on. My diaries became more analytical and more personal. This writing was necessary to organise my thoughts and feelings, to put things in a certain logical framework. As time passed, I learned to use these moments to develop concepts I would use in future publications. So, my diaries became like the rehearsal tapes of a musician, where they record different melodies and experiment on guitar in order to leave this behind, and maybe use these melodies many years later.<sup>1</sup> On the other hand, this writing was and is important in order to analyse details as long as I remember them. I mentioned that my head worked like a recording machine, but I have noticed that when the fieldwork is over, the adrenaline has settled down, the information — just as the recording machine dies

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<sup>1</sup> Look U2's guitarist's interview in a documentary 'It Might Get Loud': <<https://www.youtube.com/watch?v=CT2MuizGQ5I>>.

when the hard drive is full — is erased. I never know when I might need these small bits and pieces, and therefore it is important to save these moments. I have learned this when I tried to recall events that happened several years ago, and which were documented only in few sentences, as mentioned above.

In 2000–3 I was a PhD-student in the newly established Max Planck Institute for Social Anthropology. During its first years, the institute was in the constant process of evolving, finding ways how to become more efficient in the field of academic research and publishing, testing different forms of organisation. In 2003, the institute's directors decided that because our research was financed by the institute, they should also have a copy of our field materials. Technically, they proposed that all our field diaries should be scanned and preserved electronically. I remember the protests it caused among my colleagues, especially the Americans. They treated their field diaries as top classified information that could be used as... I never understood the hysteria. I remained pretty calm, because I knew that no one can use my diaries unless I helped to decode the text. So, without any hesitation, I let the technical staff take my notes and do the boring work of scanning. Later, I must admit, I profited from their work because I was allowed to keep the PDFs, so I am able to check my notes without carrying the physical copies.

My experience with sharing fieldwork diaries is very limited. I cannot recall reading any other published field diaries, only the world famous ones of Malinowski [Malinowski 1989]. Mostly I was struck by the foreword by Raymond Firth where he tries to apologise on behalf of the founding father of British anthropology for his prejudices against indigenous Trobrianders. Frazer argued that before throwing a stone, we should look in the mirror and think maybe we also are not always adequate to our emotional judgements. The famous scandal around the published field materials of Malinowski demonstrates that uncensored or unedited field notes are not for the use of the wider public.

Years ago, I was involved in a research project funded by the British Arts and Humanities Research Council and based in the University of Warwick. We studied punk in post-Socialist countries, all of us were 'punk scholars' [Furness 2012], having being involved in punk subculture in various periods of our life. We had a sense of brotherhood, outside of, and during our academic discussions, we talked about music and extensively exchanged it. From the first meeting onwards we felt very connected. The downside of the story was that according to the project rules, we had to share our field diaries. This put me in an inconvenient situation. First of all, I had to write my field notes on computer, and solely in English, to make them accessible to my Croatian, English, Norwegian, and Russian

coresearchers. That put me in an awkward position in many respects. My documenting was now related to certain places and situations. I kept my diary mainly in my office. In a few cases, I had to carry my laptop to some café, which required forward planning. I just do not carry a laptop with me all the time: a laptop is heavier and more fragile than a paper notebook. The fixation on language, place, and method of documentation killed any spontaneity that keeping a diary means for me. I could not just write what I had on my mind, but I had to keep in mind the formulation of my thoughts in grammatically correct English, so as to be understood by the others. This, to be honest, made the whole process, for me, very boring. I think the text I produced was too dry and extremely *sachlich*. I did not enjoy it at all!

The diaries were uploaded to a password-protected folder in the project website which was part of the host university's official internet site. I read my colleagues' diaries and enjoyed them very much. I learned a lot about punk in St Petersburg, Vorkuta, Krasnodar, and Pula. I also learned about my colleagues. I learned what was important to them, and what was worthy of documentation.

This was fine with me. Where I had a problem was when I had to deliver anonymous copies of field notes that were available for a wider public. As mentioned above, my field notes became personal, and I was not ready to share them with people I did not know. For the anonymised version I edited my field notes heavily. I deleted sections I thought were private, or I thought that could be misunderstood. For instance, in my punk research field notes I deleted many episodes related to drinking or violence.

Looking back at the editing of my diaries, I would argue that the process was very similar to how I used fieldwork material in my academic publications. I choose what fits into the 'straitjacket of an analytical framework' [Crandall 2008: 41]. In writing a paper I develop a concept first, and then look for facts that illuminate what I want to say. Doing this, I very often face the dilemma of how I portray my informants and their life. On the one hand, the purpose of a quotation in field material is to create a 'local context' [Okely 2012: 61] for the reader. On the other hand, I do care what picture I deliver. When one has conducted fieldwork in one region for years, then one has built up a very personal relationship with the people and their culture. When 'the act of writing notes is a mnemonic device, making an act of remembering' [Ibid.: 55], then publishing field notes is making sense of the 'contradiction or cultural puzzle' [Crandall 2008: 47], and the researcher has to make them public in a simplified form. One of the most favoured phrases of anthropologists is 'This is so complex!' or 'This is so complicated!' The researcher takes one or many aspects of this complexity and discusses them in the academic publication. The field material is used in this context

as fact, cementing the argument of the publication. Therefore, the simplification of the situation is unavoidable. For me, there is little difference whether I quote one sentence or use a section long description of some event. Both ways of using field material have a similar purpose — to make my argument stronger.

There is, however, a significant nuance for me as a writer. This is the responsibility for how I portray ‘my’ people. In theory, a researcher must be neutral and deliver an objective analysis. In practice, this assumption is not working. By choosing or ignoring certain field material, the writer is engaged in self-censorship. For example, when I wrote my thesis and converted it to a book [Ventsel 2005], I noticed that I avoided stressing too much the heavy alcohol consumption that is part of everyday life in Arctic tundra communities. It was not only an academic text that I was producing, but also a picture of a very concrete group of people that I spent a year living with. For me the reindeer herder A was a very concrete person — the brigadier of the 3rd reindeer herding brigade Andrey. His wife was from the neighbouring region and spoke a very different dialect of Dolgan language. Andrey had two sons, I slept in a bed where the eldest son usually slept. Andrey was a shy quiet man but he loved drinking vodka. Once during a huge celebration on the Day of Reindeer Herders in the village of Saaskylakh, he came to me and said: ‘When you are taking pictures, do not photograph drunken people!’ When I wrote about the reindeer herder A, for me I reconstructed all these moments I spent with Andrey. The depersonalisation of informants can lead to massive generalisations and deprivation of the informants’ agency, as is correctly assumed by Buckler [Buckler 2007]. On the other hand, my field diaries are a place where the personalities of the people I met are documented in their complexity and controversies. They stay with me in detailed, hectic, and sometimes unreadable field notes. Opening the diary, I restore contact with these people, although they are several thousand kilometres away. The uneasiness I experienced when I had to make my diaries public for the sociologists in the University of Warwick, signaled to me that I do not relate neutrally to my ‘field’ and am not ready to share everything with unknown people. Therefore, I fully agree with Judith Okely, who distinguishes the two processes — ‘knowingly writing down’ and ‘writing up’. Field notes are written down, this is a rich ‘thick description’ that serves the purpose of maintaining or reestablishing a personal ‘local context’. Using field notes as quotes in an academic publication is writing up, where the depiction of a full personality is not needed, but is also not my aim.

Vine Deloria Jr. was right — field diaries define what anthropologists are about. The paradox is that field diaries define the anthropologists but not the discipline. Anthropology is how field notes are used. Most of the text produced during the fieldwork remains knowingly hidden

for other people, and is needed to construct the larger picture we will discuss in our academic publications. I personally believe that approximately 80% of data I have collected remains unpublished, but I need it to make sense of the 20% I deliver for a wider public. In retrospective, I do not understand Malinowski, and why he decided to make his own personal journey public.

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## NIKOLAI VAKHTIN

### How Meanings Are Created (An Afterword to the Discussion)<sup>1</sup>

[I]t is no business of the reader's what colour my notebook was, or how thick it was, or whether it existed at all.

*Olga Boitsova*

#### Nikolai Vakhtin

European University  
at St Petersburg  
St Petersburg, Russia  
nvakhtin@gmail.com

This *Forum* asked questions about how exactly a researcher's field experience is transformed into the text of a scholarly publication, or,

<sup>1</sup> Certain ideas which Alexandra Kasatkina sent to the author, who is grateful to her for the possibility of taking her opinion into account, have been used in this afterword.

in Ugo Corte's words, 'the mystery of how to get from raw data to a product that is shared, displayed, perhaps sold, viewed, and potentially used by others.' As usual, some authors preferred to answer the editorial board's questions one after another ('How do you set down what happens in the field? How are informants' oral communications turned into a "written source"? How are the "written sources" converted into the text of a publication? How individual is this process, i.e. can one use other people's field material in writing one's own text?' and so on). Others wrote short (or not so short) essays on the topic that had been proposed. Thank you to everyone who responded: the discussion turned out an interesting one.

Ugo Corte began his essay by expressing doubts about the way the editorial board had formulated the question of how sensory experience is transformed into text, out of which publications 'subsequently' emerge. In his opinion, these are not two consecutive phases: the process of transformation should begin as soon as possible and not stop until the work is published, and indeed continue after publication. In this he is at one with Stephan Dudeck. Dudeck, however, remarks with chagrin that in reality the anthropologist's various avatars — the field, conferences, lectures, discussions, publications — are not of equal worth: 'This seems the only place where the anthropologist exists — everything else is preparation, transitory, ephemeral forms of existence — the backstage realm that provides unacknowledged support to the star's appearance on stage.'

Several people (Aimar Ventsel, Alexandra Kasatkina, Stephan Dudeck) recalled the English expression 'to write up'. Thus Aimar Ventsel (following Judith Okely) distinguishes the two processes of 'writing down' and 'writing up'. The difference between the two is that the first is a personalised dense description intended to preserve and if necessary recreate the local context, while for the second no such personalisation is necessary. In Stephan Dudeck's opinion, there is nothing to be gained from opposing 'work in the study' to 'work in the field': this opposition is embedded in the European ('monastic', writes Dudeck) tradition and needs deconstructing. It is the model of a hive, where the anthropologists 'rushed like bees to bring back what they collected in order to store it in their honeycombs and feed the other members of the hive.'

This model does not work. The unpleasantness begins at the very beginning, with the vicious notion that 'the field is the place of action, not of reflection'. Other people agree with this: '[A] field diary is not raw data awaiting sophisticated analytical procedures. However hard (s)he aims for objectivity, the researcher interprets the material from participant observation even as (s)he describes it' (Ekaterina Khonineva). In other words, '[a]ll thinkers argue for the imperative of recording early on our evolving thoughts and observations and their

relationships, not postponing analysis till you get back to your stuffy office' (Ugo Corte). It is of course possible to combine collecting data and analysis, observation and participation, but it must be remembered that by no means everything here depends on the anthropologist: 'The social roles taken on by the researcher depend not so much on his / her own wish or skills but on the will and the interpretations of the communities (s)he is working in' (Stephan Dudeck).

Some of our authors constructed their texts around the metaphor of translation or that of the bridge, but it is a bridge between different things for each of them. For Stephan Dudeck it is either a bridge between the field and the text, or between two different social spaces. 'I <...> build a bridge between my impressions and the external observer,' writes Marina Hakkarainen, adding that '[i]t is hard to separate yourself from the field,' as if she is continuing Dudeck's idea: the metaphor of the bridge shows not only how the anthropologist enters the field, but how the field enters the anthropologist.

However, it is by no means always that one sees such unanimity: many of the authors are diametrically opposed in their answers. It goes from 'I always keep a detailed diary' to 'I never keep a diary', and from 'field materials must always be accessible to other people' to 'in no case must one use other people's material'. '[U]ncensored or unedited field notes are not for the use of the wider public' (Aimar Ventsel) — when I read that, I thought 'It's all right for Aimar, he writes his diary in five languages (Estonian, English, Russian, German and Yakut), so that in any case nobody else can understand what is written there.' Still, it seems a reasonable idea that there is a point in sharing your field material, but only if you are collaborating with someone on the same topic (or writing the same article), as Ugo Corte argues. Ekaterina Khonineva too points out how important it is when working with someone else's diary to pay attention to the author's position as a 'subjective filter'. Furthermore, 'data elicit memory which is inherently connected with who collected it' (Ugo Corte); compare: 'I force myself to remember everything in detail and record it, because I do not trust my memory, and, moreover, I know that the theoretical ideas developed in the course of carrying out the project may have a strong influence on my "recollections"' (Olga Sasunkevich).

In general, 'In the field <...> everyone was free to record whatever and however the promptings of his / her own experience of keeping notes, his / her scholarly or aesthetic preferences, and practical considerations might suggest' (Mikhail Lurye), since, as Ugo Corte contends, there is no one ideal way of doing such research, but a variety of different styles that, once mastered, may become part of our working repertoire. Or, as Marina Hakkarainen writes, one needs 'to find the optimal correlation between the various techniques for solving the problems that have been set.'

Is it acceptable to edit an interview? Opinions vary on this too. Sometimes it is essential: there are certain ‘high-status’ informants who will not talk to an anthropologist at all if they have not agreed to their right to edit what has been said afterwards, and given the choice between having and not having ‘access to the field’, one is sometimes forced to give permission to edit material afterwards (Tatyana Barandova et al.). ‘[I]f I deposit my diary in an archive, I redact it,’ writes Alexandra Kasatkina, though some people might think that a redacted diary is an oxymoron. One might agree with Mikhail Lurye when he writes that ‘I am not convinced that everything ought to be placed in the archive, or straightaway.’ The keyword here, evidently, is ‘archive’, *a repository for documents from the past*: so long as the data collected are still being used, or may still be used by the collector, it is too early to deposit them in the archive. One has to allow the material that one has collected to rest, to ‘cool off’, to recede from the immediate present; lying in the archive, the material collected will begin to acquire all the wonderful qualities of ‘a document from the past’, and the longer it lies there, the more valuable it becomes.

There is another thought connected with this, which pursues anyone who reads all the replies one after the other, and that is the thought of the radical changes that the development of audio and computer technology has brought to the field. It seems that all the authors of the replies record their field data the way they learnt (or were taught) to do it in their first field, but that first field (the field of their youth) came at a different time for each of them. I myself well remember how I went on my first expedition to Chukotka, carrying my things — mostly clothing — in a rucksack and a case containing a tape recorder (reel-to-reel, this was before cassettes) weighing about eight kilograms. The tape recorder only worked off the mains, it had no batteries, so that it could only be used in the village, and only when the electricity was on. Naturally, such cumbersome equipment had a direct effect on what, when, and where could be recorded on tape. Afterwards, less cumbersome cassette recorders appeared, but ‘working with cassette recorders in rural conditions was often problematic’ (Mikhail Lurye), mostly because it was impossible to buy batteries for them. The same applies to field diaries: a researcher who is used to paper will continue to use paper, even if (s)he invents pragmatic explanations for this (‘I do not usually carry a laptop with me, it is much heavier and more fragile than a paper notebook’ (Aimar Ventsel)). Olga Khristoforova writes everything down on paper, because at the beginning of her fieldwork ‘there were no such things as laptops.’ And when I began to work in the field there were no computers at all, so we wrote everything down by hand, on paper, and the people who began like that still do so today, constantly encountering the amazement of young people: ‘How did anthro-

pologists in the age before computers write up their texts from manuscript and typescript?..' (Alexandra Kasatkina).

Therefore for many of us the question 'Is an archived field diary a stable document to which no changes can be made?' or 'How should archives work with such unstable documents as electronic research diaries?' simply does not arise: a field diary on paper is always stable (and so is a photographic one (Andrei Andreev)). Naturally, anything can be added to it after the fieldwork, but all these additions are clearly visible, and it is not possible to conceal them (nor is it necessary). (I always used to leave the verso in my notebook blank for such additions, later commentaries, cross-references to earlier or later pages of the diary, and things that I had heard or read later, and wrote my transcripts of interviews or observations only on the recto.)

Still, the fact that we still use the old terminology in our present digital age speaks volumes. It seems that in the field and after the field we still do everything the way we used to, only on the computer. We 'keep a diary', we 'write texts', we deposit the texts 'in the archive'... As if we were still writing everything by hand, as if we were still using paper.

It may be that we have not yet learnt to make use of the vast possibilities offered by the computer age. It is like the first years after the invention of printing: books were already being printed on the press, but the illuminated initials were still being painted by hand in every copy.

Are we not now in just such an intermediate period 'between the letter and the byte'? Is this not the reason why 'electronic text is so susceptible to change, its status as a document is so fragile, and the temptation to write over and between the lines written in the field is so great that my attitude to such files is one of exaggerated caution' (Alexandra Kasatkina)? And why 'there will be real difficulties with handwritten diaries, since they will never be part of the mass of digitised data' (Olga Sasunkevich), and, one might add, not being part of this mass, will in time become no less valuable than manuscripts are today?

There are a few more subjects that emerge from the replies.

Many of the authors write of their desire 'to take the field away with them', to which end they often attempt to make a complete video or audio record of what happens and take large numbers of photographs. If the action is not recorded on video or in photographs, but only described in words by the researcher who was present at it, there is no way of 'sharing' this material: no one who has not seen the action can hope to interpret it adequately (Tatyana Barandova et al.).

Multimodality turns out to have various applications: conveying the many voices and many dimensions of the field in publications (Stephan Dudeck), or ensuring 'a complete and comprehensive description of

what is happening in the field' (Tatyana Barandova et al.). It also helps to make multidimensional field experience available for use by others (Tatyana Barandova et al.). See, however, the experiment in [Burrell 2016], which its author considers a failure: having access to the field only via her student's multimodal notes (textual diaries, photographs, maps, video and sound recordings), she constantly felt that she was not fully present and lacked information.

Olga Boitsova writes about the same thing: 'Using other people's field pictures in one's own project is extraordinarily difficult,' because they were usually taken for the purposes of the photographer's own research interests and serve, among other things, as a reminder of things which someone who was not there cannot remember. And in any case one cannot succeed in 'taking the field away': when the material is taken 'home from the field' the context changes, and, therefore, so does the material: 'the voices in the interview emerge from the researcher's kitchen and take their places in the reception rooms of a publication' (Marina Hakkarainen) and come to sound different. When analysing field data, 'the researcher disrupts the linear structure of the narrative, and breaks it up into quotations, subjects and dialogues. These elements are removed from the context of the temporal sequence recorded in the diary and recombined in accordance with the categories that have been identified. Every subject, taken separately, is placed in a new context of similar things — a set of elements that are characteristic of more or less constant regularities. And it is by creating this new context of patterns that the researcher discovers their connection or dependency on each other. <...> it is thanks to such a manipulation of contexts that anthropological explanations come into being' (Ekaterina Khonineva).

The ideas about the spatial juxtaposition of 'the field' and 'not the field' are also interesting. As Marina Hakkarainen writes, 'When I go "on an expedition" to a distant place, it is separated from my "ordinary life" by distance, by travelling, and by the notion that I am "somewhere else" and should not be living a "normal" life. In that case the field becomes "dense" with information. I make it so by my heightened attention. If the field is at home, "round the corner", one needs to define that corner clearly: on this side it is not the field, and on that side it is.'

It seems that Olga Khristoforova is the only one to note the differences in the question under discussion between anthropological traditions in this country and in the West. In the Western tradition they try to reduce the distance between the field material, the voices of the informants, and the final text ('Approximately 80% of data I have collected remains unpublished, but I need it to make sense of the 20% I deliver for a wider public' (Aimar Ventsel)); at home they are still at war with 'this postmodernism', and letting people into 'the

back kitchen' is not recognised as a tactic for verifying the research, but regarded as inappropriate...

The question was raised of how to convey 'the many dimensions and many voices of the field' in a text, and the usefulness of such literary techniques as montage, commentaries and hypertexts, and material that contradicts the author's point of view (see the replies by Stephan Dudeck, Tatyana Barandova et al.).

Marina Hakkarainen also notes the pressure exerted by the academic representation of results: 'the languages of subjective experience and objectivity are very different. Therefore I have, as it were, to keep changing direction in my search for compromises between the accepted scholarly practices for writing papers, reports and articles and "honesty" in presenting my field.'

And finally, participants note the harm done by concepts of beauty. The text is liable to aestheticise the problem: 'I, for example, have always found it almost magical, the way serious social problems discovered in the field, such as poverty, are transformed into such beautiful analytical texts. In this way we take part in aestheticising social inequality and reduce the extent of our sympathy and involvement' (Marina Hakkarainen). Aestheticisation is yet another filter through which reality passes. '[W]e sometimes catch picturesque formulations in what our informants say and write them down to be used afterwards in the heading of a paper. <...> These stylistic highlights may be detached from their fundamental context and acquire in the author's text a somewhat different meaning for the sake of producing an aesthetic effect.' And further on: 'talented and witty narrators, suppliers of "beautiful texts", become our "favourite informants"' (Ekaterina Khonineva). Aiming for beauty and elegance in the final academic text naturally gets in the way of accuracy in conveying the original material, since 'it is often impossible to verify from the outside whether a particular extract from an interview in an anthropologist's article does represent a pattern that (s)he has identified, or whether it is just a striking quotation' (Andrei Tiukhtiaev).

In conclusion, I should like once again to thank all the participants for an interesting and productive discussion.

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*The answers originally written in Russian  
were translated by Ralph Cleminson*