

summarized the main topics of the symposium. In her address she emphasized that ethics and ethical choices in oral history study are strongly contextual. They are part of the communication processes and dialogue between both researcher and informant and individual scholar and research community. Besides, they depend on how a particular study is located in the field of science. In other words, neither ethics in oral history nor related discussion are immune to social, political or ideological influences. Ethics may not be easily defined but connected issues may be found everywhere, and therefore they should be constantly under discussion and evaluation.

#### REFERENCES

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### “I’M WRITING ABOUT YOU. PLEASE ACCEPT MY APOLOGIES”

• EKATERINA MELNIKOVA •

Regardless of the extent to which social scientists expect the results of their work to bring about any public effect, their dissemination is obligatory. All ethical issues related to fieldwork are to a greater or lesser extent conditioned by the fact of potential publication. Though publication itself is not declared to be the major reason for ethical debates, it performs this role simply because none of the infringements of rights which may be caused by researchers to those being researched would ever be evident without publication. Since nobody, in fact, can control the conduct of the scholar ‘in the field’, ethical guides in oral history research are primarily aimed at negotiating the consequences of the publications, at the least because such consequences may be traced. Here I would like to discuss how the awareness of the aftermath of publication affects the behavior of the scholar in the field and the very route of the field research.

My initial experience in field work is based on folklore expeditions aimed at collecting various tales and descriptions of old traditional practices regardless of their actual survival till the present. This is a more or less traditional practice in Russia, which follows the direction of Soviet and pre-revolutionary folklore scholarship. I worked mostly in rural areas of Russia, in the region of the Russian Northwest (not further than 500 km from St.

Petersburg). Materials from these projects were often published and only recently the rule of hiding the original names of the informants became generally accepted. The issue of possible reaction by the interviewees to publication of their words, as well as to interpretations made by the researcher in the resultant article, had been never discussed in regard to those materials. This situation is understandable in light of the distance between the academic sphere where the interviews were published, interpreted and discussed, and the informants' circles: their spheres of reading and interest. It was always evident (or seemed to be evident) that the scholarly treatments would never reach those who were the subjects of interpretations.

In recent years I have participated in rather different projects focused on representations of histories of various events or epochs (a project on the history of childhood in the 1920s–1930s and the history of adaptation by Russian migrants in the former Finnish Karelia, among others). In projects of this type the issue of publication and discussion of interviews becomes rather topical as it is the individual who is the focus of investigation and all personal details are vitally important. On the other hand, the topics of oral history research and the very link between the interview and 'history', which is usually evident for informants, conditions their significance for interviewees. Consequently, scholarly works become potentially interesting and available to informants.

My current project is focused on the Soviet migrants who came to Ladoga Karelia in the 1940s–1950s after this territory had been ceded by Finland to the USSR. My primary issue is the accommodation of the pre-war past of Karelia within the historical discourse and personal memories of the newcomers. The fieldwork took place in August, 2008. Previously I already had access to a significant number of interviews with 'ordinary people' who came to Karelia within the migration flows of 1940s–1950s. These were the materials collected within the international project "Building New Russia? Something Old, Something New and Something Borrowed" which was a part of the Academy of Finland project "The Conditions for Constructing New Russia" (2001–2003). I participated in the project and collaborated with other members of the team in preparation of interviews' transcription, their analysis and presentation.

One of the major results of the project was the volume *The Border and People: the Recollections of the Soviet Migrants of Ladoga Karelia and Karelian Isthmus*. The book was prepared by the team of researchers who took part in the project, and was structured as a publication of interview excerpts that were grouped in thematic blocks. The way of grouping was the result of general analysis of all the narratives recorded. This was not the classification of texts, but rather representation of the major topics especially popular among the local dwellers, which were discussed by them even without the intervention of the interviewer. The way of grouping was not only descriptive; it presented the very principles of the organization of the local views toward social reality—past, present and in some cases even future. These principles were also laid out and emphasized in brief introductions preceding thematic blocks. Thus the book was not so much the publication of narratives, but of the results of their scholarly interpretation.

When departing to the field in 2008 I took several copies of our book along as I planned to present them to people like leaders of the administration and heads of the museums. Although published interviews were recorded in many places, a large number of them came from Lahdenpohja, where my colleague and I worked that time. It is also important

to stress that Lahdenpohja in fact is a very small provincial town where almost everyone knows each other. I did not plan to give the book to 'ordinary people' who were cited in it and nor did I plan to present the book to those with whom I was going to speak during the current project, and I was slightly afraid to find out that somebody from our earlier interviews might have already read it. I had two principal doubts in regard to presenting our book. Firstly, I was aware that locals did not perfectly realize the aims of our work (regardless of my explanations); inasmuch as they could not understand how 'History' and elders' 'idle talk' could be connected, and how their own stories could be inscribed into Big History, so they had not fully believed us and had retained some suspicions concerning the use of the material for secret goals. So I was apprehensive that our book would only confirm their suspicions because they would see that we did not write 'History' in actual fact but rather picked up the most absurd rumors and gossip and published them for public observation. Secondly, I was afraid of their reaction to interpretation of the gossip and rumors and even to the very fact of the interpretations having been made. This would again support their suspicions because they might reach the conclusion that we actually had some secret goal: research of people rather than history or something else. Nonetheless, at the end of my visit to Lahdenpohja I still presented the book to people who had kindly hosted me and my colleague. Some time later I found out that they enjoyed it very much and a lot of their neighbors had waited for their turn to read it.

When meeting with 'local experts' such as amateur historians and the leaders of local historical museums I was challenged by the other fears. After the book was presented to the director of the museum at the main city factory I had the opportunity to see her reaction to the publication. She did not like the book very much, mostly because of the publication of all the words in their oral forms, in the way they were pronounced with all the grammatical mistakes, all the confusions and all these "hmms", "a-as" and so on. Her main argument was the following: the readers of the book would think that the locals are like "narrow-minded, Russian Ivan", not able to tie two words together. In several further conversations we tried to explain why it was so important to publish the interviews as they were recorded, and the kinds of information we could receive when analyzing the form of the utterance, not only its content. She finally agreed and found it rather interesting but her main argument remained vital for me. The book represents the people of the region to the general public, and people have the right to be anxious about their image.

When talking to the head of the local historical museum at Kurkijoki, I found that the issue of a 'bad image' of the people is irrelevant to her. The museum was conceived as an academic institution aimed at popularization of historical heritage of the region, and its members presented themselves as partisans of scholarship, as those who did not have any doubts about the significance of academic work. The publication of the oral speech in the way it was recorded was not a problem for the museum members, but another issue arose. Though the director of the museum agreed to the interview she informed me that she found my work entirely impossible as the people who were the core focus of my research were alive and could not be analyzed and publicly discussed.

Once the expedition was over, the issue of further work and publication of the project materials became even more crucial. I became aware of several risks. Firstly: the danger of presenting an image of local people which they might not appreciate and the corresponding issue of whether I should take this argument into account in my follow-on with the project.

Secondly: the risk of coming into conflict with local historians who also might not appreciate the project though for different reasons. Thirdly: the risk of presenting an interpretation of the activity of the very same local historians who are also the subjects of my work. Although oral history analysis certainly does not offer evaluation of the actions of the particular museum or its leader, it does suggest explications of motives for that activity which may not fully coincide with those that are declared by the museum members. Would I have to start my future papers not with acknowledgements but rather with apologies?

And there's one more risk. Once the publication of the materials collected in the previous stages of the project entered the local milieu it is highly probable that the book affected the recollections of local people. Now a fear of encountering citations from our own work will accompany all ensuing researchers in this particular place.

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## THE INTERVIEWER'S DILEMMA

• LEENA ROSSI •

In countless methods books, authors in various fields of oral history research have written a great deal about the attributes of a good interviewer and about personal qualities which could have an effect on the interviewing process, consequently making a difference in the oral material created and to the whole research project; age, sex, ethnicity, social background, appearance, religious or political ideology and way of speaking are examples of such qualities. Some of these can be hidden but others cannot and need not. The possible impacts of the interviewer's characteristics have also been frequently reflected upon in research publications, which have, of course, come out after the interviews have been conducted.

In addition to many other ethical problems, all oral historians encounter the question I call the 'interviewer's dilemma': "Am I the right person to interview this particular individual?" This leads to another question inseparably connected with it: "Could another interviewer with different qualities be able to conduct a richer and more fruitful conversation with this person?" Answering these questions compels the researcher-interviewer to an honest and careful self-reflection about her qualities, not only after the interviews, as has mostly been done, but also at all phases of the research. And the reflexivity should not be restricted to individual textual approaches in publications but it should be extended to a collective process within academia. Moreover, the researcher should be prepared to withdraw from interviewing certain persons if she is worried about the impact of her special qualities on the interviewees-to-be and simultaneously on the whole research process. So far, I have found hardly any author questioning her suitability as an interviewer, let alone revealing that she has given up interviewing certain narrators and invited a colleague to replace her.