

Fieldwork Ethics

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The Russian version of the Forum also included comments from Maria Akhmetova, Dmitry Arzyutov, Aleksandra Brytsina, Elena Chikadze, Natalia Galetkina, Natalia Drannikova, Mikhail Lurye, Irina Razumova, and Nikita Ushakov.

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QUESTIONS FOR DISCUSSION

In Russia at present, whether to introduce codes of conduct for fieldwork is currently the subject of intensive discussion. In the Soviet era, such codes did not exist in written form, and issues such as the rights of informants to personal dignity, or the need for transparency of information in research, were of little or no concern to most researchers in anthropology, as in the human and medical sciences generally. In many other places where anthropology is practised, however, such codes have existed for some time, both at the level of national organisations, and of individual universities and research institutes. We felt it would be useful to canvas the views of colleagues who have had experience of using professional codes of practice, both in terms of learning the benefits that can come through using such codes, and in terms of learning what should be avoided. Given that freedom of information is still a vexed issue, it struck us as especially important to consider whether restrictions on fieldwork practice might sometimes act as a form of censorship on scholarly activity. We accordingly invited both scholars

with fieldwork experience from inside ‘post-Soviet space’, and those with such experience, and also direct experience of the functioning of codes, from other places, to take part in our discussion, drafting slightly different questionnaires for each group.

Questionnaire for participants from Russia and the former Soviet Union:¹

- 1 What kind of problems have you and/or your colleagues encountered when carrying out fieldwork and preparing publications based on this? Have there been cases where ethical considerations have caused you to alter your publication plans?
- 2 Have anthropologists (sociologists, folklorists, oral historians, etc.) the right to conceal the true purpose of their investigation from people they are working with? To what extent is it proper to resort to fiction (the so-called ‘fieldwork legend’²) when introducing oneself to informants? Have we the right to collect information without the explicit sanction of the person providing it, for example by using technological means such as hidden microphones?
- 3 Is any researcher able to be a genuinely impartial observer when working in the field? Can his/her very presence and intention to

¹ The questionnaire was circulated with the following preamble: Recently, the complex relationships between the scholarly and ethical position of the fieldworker have become the subject of more and more attention. Every academic who works with human subjects inevitably gets caught up in ethical questions. A good many of these questions do not have straightforward and universally satisfactory answers. At the same time, a fair number of countries already have their own professional agreements on ethical standards. For instance, the AAA code [*Code of Ethics of the American Anthropological Association*, 1998] makes the following stipulations: 1. **Research must observe transparency:** in particular, it is essential to be open about the sources of funding and the purpose of research with informants, and honest about the aims and principles of the research to be carried out with funding bodies (this also holds for applied anthropologists); 2. **Research must observe respect for the human dignity and integrity of informants**, as expressed e.g. in the need to determine in advance whether the informants wish to remain anonymous or not, yet also inform them that practical circumstances may mean their choice cannot be honoured, and the need to acquire informed consent, though not necessarily in written form; 3. **Research must facilitate collaboration:** anthropologists must facilitate the research of others working in the same area, make their research materials available for use by others, and preserve their research materials for posterity. There is nothing peculiar in any of this, and most fieldworkers already observe principles of this kind (though some of the stipulations are less customary here in Russia, e.g. the suggestion that written consent should be obtained before interviews take place: on this see <<http://www.web-miner.com/anthroethics.htm>>) And of course, no code of practice can foresee the subtler difficulties that may come up in the course of fieldwork. The purpose of the present discussion is to consider concrete difficulties and problems that researchers may encounter when they carry out fieldwork and publish its results.

² The ‘fieldwork legend’ is the more or less simplified explanation given to informants about what the purpose of the research is, and what the information collected will be used for. [Editor].

publish material about a given society be a form of interference into people's lives, which might have very direct, and possibly negative, impact upon them? May we sometimes become, willingly or not, the participants in unethical acts? Can our presence provoke extreme reactions, and if so, how can we prevent this?

4

Is it permissible to publish material relating to people's private lives and analyses of this even in circumstances where this might cause an informant harm or present him or her in an unfavourable light? Is it enough to use code for names and places if one is to avoid ethical and legal problems? Should obtaining an informant's formal consent to the collection and publication of data be obligatory? To what extent should informants be made aware of the results of an investigation?

5

Where do you think the causes lie of the rise in interest in professional ethics over the last few years? Can we talk about a specifically 'Russian' tradition of professional ethics? Do you regard the composition of professional codes of practice like those existing abroad as an essential step? Could such a document function effectively in Russia? If not, why not?

Questionnaire for participants from elsewhere:

1

Does your institution have a code of ethics, and if so, what, briefly, does it specify? How helpful (or obstructive) do you find the code in your research? To what extent is it possible to maintain an appropriate balance between freedom of information/the imperative of making information available to the scholarly community and respect for informants' privacy?

2

Obviously, no single code can embrace all the different ethical problems that may come up in fieldwork, particularly bearing in mind the possibility of variation in perceptions of personal dignity across cultures. Have you experience of problems that fall into 'grey areas' from the point of view of ethical codes, and if so, how did you seek to resolve them?

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1

I'd like to take this question in a slightly different direction. Russian ethnographers, like ethnographers everywhere, are working with living human beings and so they need to be very aware of the ethical side of their activities. Yet this obvious fact has not become the subject of particular interest or discussion. Why is that?

One possible answer is that Russian ethnography is still dominated by the neo-Romantic view according to which science and scholarship represent an absolute good, bringing people and society only positive results — enlightening them and showing them the way to a rational future. This attitude naturally extends to ethnography, which, in imparting information about other cultures and revealing the rules and patterns by which they live, is assumed to have an exclusively humanitarian mission. The link of scholarly knowledge with repression, as agonised over in many works by European and American anthropologists, strikes most Russian anthropologists as either distorted and inflated out of all proportion, or simply a fiction to be

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gin with. The result is that Russian scholars are accustomed not to worry about the morality of their subject, or if they do come across problems of this kind, to keep them to themselves.

There's also another possible explanation, though: that the lack of reflection on ethnographical ethics is a form of self-defence. After all, fieldwork is an abnormal activity to begin with. People who are constantly trying to find things out, who do the rounds of everyone in the community, who foist themselves on other people, insist on making friends with them, aren't 'behaving properly' — whether in terms of the life they ordinarily lead, or of the life they've pitched themselves into. If one subjected everything a fieldworker did to ethical scrutiny, then the inevitable conclusion would be that anthropology is an innately asocial and amoral profession. In order that this does not happen, in order that the illusion of 'science' is maintained (without which fieldwork would lose its point), ethnographers have simply to ignore the awkward questions and nagging doubts.

Which explanation is closer to the truth, I simply have no idea.

2

The question about concealing the aims of your research isn't as simple as it seems. To begin with, the people one's working with may well not understand or trust the explanation that you give them, even if it's completely honest (this happens quite often). In one village where I'd been working for months and where I'd built up strong relationships of trust, one person I knew quite well said to me, '*But all the same, you **are** working for the KGB, aren't you?*' Trying to persuade him otherwise was pointless: all my letters of introduction from scholarly institutes, my talk of scholarship and even invitations to participate in the research process struck him as so much hogwash. I didn't need to make up a 'legend' — the people I was working with had done it for me.

The second point is that the techniques of ethnographical research (I hope this doesn't sound sentimental) assume that the distance between informant and researcher will be reduced as much as possible, that the highest possible degree of trust and informality will be reached. This means, for instance, that some facts and views will only be accessible if the person researching them affects uninterest. At some level, this is clearly an abuse of trust, an act of deception. But if working like this is forbidden, how is one to satisfy one's need to get quickly to the heart of things in a given society, including the various rumours, conflicts, drifts of ideas and so on bubbling under the surface? How is one to cope with studying topics that are obviously taboo — crime, prostitution, illegal migration, different kinds of closed societies and so on, which you pretty well can't investigate while waving your Academy of Sciences identity card around?

In many situations, someone doing fieldwork has to choose between moral obstacles on the one hand and truth on the other, and may decide in favour of the latter, however chimerical and illusory it may seem as a goal.

Of course, the boundary between commitment to scholarship and moral cynicism is fragile. I personally would regard as totally unacceptable the strategy adopted by a graduate student in the US who infiltrated an Islamic radical group by pretending to be a believer and then wrote up his experiences (as a Ph.D, if I'm not mistaken). In this case, the deception was flagrant and the exploitation of the informants that it involved frankly disgusting. (These days, it's all too likely that the results of research like this would end up being used by the security services to hound down the members of the group in question). But should outrageous cases like this lead one to formalise the relations of researcher and informant so thoroughly that one ends up with something like a manual for sales staff?

Here I'd like to emphasise that I'm not advocating rejection of the principles of ethics in an abstract sense. The researcher should, without doubt, adhere to these punctiliously. But it would be wrong to turn self-imposed restrictions into a fetish and to ignore all the concrete circumstances that ethnographers may come up against. I think the way out of this conflict may lie not so much in strengthening control over the researcher's activities (after all, control of this kind is hardly going to be effective) as in making the techniques of information gathering the subject of reflection and analysis in themselves. It is important to discuss this side of our work openly and frankly, and to make issues of this kind central to training in fieldwork. We already have a good many resources for scrutinising, if not regulating in a total sense, the cases where ethnographers have 'exceeded their brief' in a professional sense.

3

In the mid 1990s, I was doing research in a small village in Tadzhikistan and ended up in a rather unpleasant situation. This came about just as I was starting to 'get assimilated' in the local community.

This was a bad time generally. The standard of living had collapsed, and the Tadzhik government had introduced a new currency that local people regarded with distrust. A good many conflicts were provoked by the economic hardships of the time. For instance, someone in the village where I was living — I'll call him T — wanted to get some meat at the market, but the trader wouldn't accept Tadzhik money, he insisted on roubles only. The buyer was furious about this. He stormed into the village soviet and said to the chairman, this, that, the next thing, if he doesn't give me that meat I'll kill him. The chairman promised to sort things out. T went off back home, but on the way he met up with some other men who were sitting in the local tea-house drinking spirits. T sat down and 'had

a few' himself, then set off to the butcher's house (they were neighbours, as a matter of fact), called him out on the street and stabbed him. The threatened murder had taken place.

I was one of the men in the tea-house — I'd gone there to try and 'get closer' to the local population. Need I add that I was taking part in the drinking as well? I think I'd actually left by the time T turned up, but all the same the facts are indisputable: I'd got mixed up, consciously or not, in a chain of events that had led to a person getting killed. This didn't escape people's notice locally and I had to think very seriously about whether to leave and start work somewhere else.

What do I mean by citing this story? It's clear that an ethnographer can get mixed up in all kinds of social processes that he or she hardly understands, but whose consequences he or she can't possibly avert or ignore. Once they enter the 'field' (and especially if they are there for any length of time), researchers start to influence those around themselves and to be influenced by them — no matter whether they want this to happen or not. Ethnographers themselves get made use of (sometimes in underhand ways), they get manipulated, quarrels and conflicts blow up round them. Even the innocent (at first sight) desire of informants to get one over other informants, their jealousy of one another, can generate conflicts or exacerbate ones that already existed. And there are no means of avoiding all this.

The situation where researchers themselves adopt the role of lobbyists on behalf of their informants and turn from external observers into active participants and even catalysts of events is very different. There are plenty of examples of this happening, and frankly I find it difficult to be cut-and-dried about the morality of it. There may well be cases where ethnographers 'fan the flames' of conflicts, provoke people into undesirable forms of behaviour and even actual crimes. There are other cases where the oppressed and maltreated of this world — and it's often people like this who are an ethnographer's main informants — actually need support and the ethnographer is the only person who can give it to them. The rights and wrongs of situations like this are hard to fathom.

There is no one recipe for dealing with these situations. All our professional community can do is to call for humanism in the broadest sense, the sort that's supposed to characterise our work anyway, and for a sense of personal responsibility. Choosing which way to handle things has to be left up to individuals themselves.

4

It might seem that the answer to these questions is perfectly obvious. Of course you mustn't bring about harm to informants, of course you should conceal real names and place names, of course

you should get informants' consent to collecting and publishing material about them. But that's in the ideal world. In practice, though? How do you actually decide what information may be harmful, and what not? How do you work out procedures for agreeing the content of publications with informants (or with people who may be affected by those publications)? What will be left of scholarly freedom when you've catered for all those different pressing requirements?

Concealing real names and place names (using codes, substitute names etc.) seems like the best way out of the problems. This is a widely-used method and probably the most effective one around. But it too has flaws. For one thing, the precise location of a given place may actually be of central relevance to one's discussion, as may the exact dates, especially if one is citing archival documents and other written materials. For another, concealing place names and the names of your sources makes it much harder for other scholars to check your materials and your conclusions. Making sources anonymous is a way of ensuring that a researcher's mistakes get accepted as unchallengeable truths.

In any case, these ways of avoiding responsibility for the information we pass on do no more than create an illusion of fair dealing. It is a good deal more honest to reveal to readers the way that fieldwork has proceeded in its entirety, complete with mistakes and blind alleys, to analyse the effects of one's own presence in a given society. Even better is giving space to the living language of one's informants in unedited form, to give them the chance of criticising one's ethnographical observations and conclusions, to write academic work that is meant to inspire discussion, not simply to provide material for other people's footnotes, to allow for conflicts of opinion and vehement discussion of the pluses and minuses of what one has written. Perhaps then people will stop seeing the fact that academic work is politically engaged as a disadvantage...

5

I don't think that Russian ethnography is yet ready for a code of professional ethics. Let me list a few reasons for my doubts.

First, any code will simply not answer many of the questions that come up during fieldwork. I've discussed this point in detail above.

Second (I've also said something about this in my answer to question 1): far from everyone in the ethnographical field here in Russia is bothered about the content of a hypothetical code of this kind. If there isn't real interest at the grassroots, any code will just turn into empty phrases, and the pointlessness of the document will still further discredit the principles enshrined in it.

Third, Russian ethnographers still have a very weak sense of corporate identity. The vertical ties in our profession are much strong-

er than the horizontal ties. Almost nothing gets done without an order being passed down ‘from above’, but orders of this kind are generally made by people who have little concern for the aims of science and scholarship as such.

Fourth, it isn’t even the anthropological community that has to be persuaded by ethical codes. Documents of this kind must be the result of a meaningful dialogue between ethnographers and ‘civil society’ (human rights organisations, different types of public association, journalists, legal professionals, and so on). But I can’t at the moment see any great public interest in research ethics, nor any social institutions that might generate it — more’s the pity.

MARCUS BANKS

Introduction

British anthropology has shown a concern with ethical and moral issues since well before the Second World War — the 1929 edition of *Notes and Queries* asks investigators to respect the beliefs of research subjects, for example — but it is only since the 1960s that serious discussion on ethical matters has taken place in the discipline. The American Anthropological Association (AAA) issued a ‘Statement on Problems of Anthropological Research and Ethics’ in 1967, followed by a statement on ‘Principles of Professional Responsibility’ in 1971;¹ the UK and Commonwealth Association of Social Anthropologists took almost two decades to follow suit, with its 1987 ‘Ethical Guidelines’.² However, these were modelled on revisions of the AAA statements and it is assumed that prior to 1987 British anthropologists were referring to the AAA statements.

Two issues stand out in recent decades. First, that discussion of ethics within the discipline tends to be reactive rather than proactive. Second, that the jural nature of ethical codes sit uneasily with the ethos of the discipline itself. With regard to the first, there are period epi-

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¹ See <www.aaanet.org/committees/ethics/ethics.htm>.

² See <www.theasa.org/ethics.htm>.

sodes in which an anthropologist or group of anthropologists is accused of unethical behaviour; in response to this, existing ethical codes are revisited and rewritten.¹ More recently, however, the second issue has become prominent and could ultimately prove to be more productive than previous attempts at fire-fighting.² As the discipline is inherently concerned with 'the social', it is or should be difficult to discern a difference between a social anthropologist's professional and personal ethical responsibilities; thus abiding by a code in one's professional life — particularly one primarily designed to forestall legal action — serves to dehumanise not only the anthropologist but also her research subjects. Of course, much the same would be true for many other disciplines, but the nature of ethnographic research means that it is far more difficult for a social anthropologist to identify precisely which activities in the field count as 'research' (and are therefore liable to the strictures of an ethics code) and which do not. For this reason, many anthropologists have difficulties with ethics codes which are based upon those drawn up primarily for medical researchers. In medical contexts, the research activity is clearly defined, and this is also potentially most damaging part of the process (when it comes to, for example, unforeseen side-effects in a drug trial). Once the research is over, there is very little risk to the participants:³ data derived from them are anonymised and ameliorated and even if the participants could be subsequently identified, there is very little harm that could come to them in the vast majority of cases. Social research, and anthropological research in particular, operates in quite the contrary fashion. In the vast majority of cases the research 'activity' offers little or no risk at all (e.g. holding a conversation). It is the subsequent publication and dissemination of this material that carries moral and ethical implications. Publications (including films and photographs — see below) may bring hurt or distress to individuals, but more generally an entire corpus of work, such as anthropological writings on 'primitive society' in the late nineteenth century, may bring hurt and distress to untold millions as a result of stereotypes that become established.

It is likely that the rapid rise of research ethics committees, the medical research model, and the threat of litigation, mean that an embedded or contextual approach to anthropological ethics will

¹ See [Fluehr-Lobban 2004] for a comment on the latest 'scandal'.

² Important contributions include the call for an 'embedded ethics' [Meskell and Pels 2005] and the call for 'interactive' ethics [Harper and Corsín Jiménez 2005], both of which stress gaining an ethnographic understanding of the context in which the ethically-inflected encounter takes place.

³ One significant exception to this is the 'retained organs' scandals of the 1990s in which organs of deceased children (as well as foetal tissue) was found to have been retained at various hospitals in the UK for research purposes, without the knowledge of the parents.

never become established in a formal sense, that is, in a way that is acceptable to university authorities and grant-giving bodies. Consequently, social anthropologists will have to continue to set aside their qualms when it comes to ‘one size fits all’ questions concerning, for example, ‘informed consent’ (how much information is enough?) or ‘psychological damage’ (how can I judge another’s mental health?). It is with these thoughts in mind that my responses below to the two sets of questions should be read.

1

The University of Oxford does not have a code of ethics as such; rather, the Committee that oversees the administration of research ethics clearance has a brief ‘policy statement’ that runs in part: *‘The University of Oxford is committed to ensuring that its **research** activities involving **human participants** are conducted in a way which respects the dignity, rights, and welfare of participants, which minimises risk to participants, **researchers**, and **third parties**, and the object of which is **public benefit**’* (the underlined terms, along with many others subsequently used in the documentation, are defined in a glossary). One reason for this brevity is that researchers are expected to abide by the far more detailed ethics codes issued by their professional bodies or funding councils.

There is, however, an implied ‘code’ in the questions asked on the preliminary form (called CUREC/1) that all researchers in the University (staff and students) must complete before undertaking research. Several of these questions relate to bio-medical issues (for example, *‘Does the research involve any alteration of participants’ normal patterns of sleeping, eating, or drinking?’*) which would not normally be relevant to social anthropological research; of those that are more relevant, *‘Have you made arrangements to obtain written informed consent from participants?’* and *‘Does the research involve as participants people whose ability to give free and informed consent is in question?’* are the ones most likely to cause problems for social anthropologists, for the reasons described in the introduction. Answering ‘no’ and ‘yes’ to these two questions respectively would normally require a researcher to complete a far longer and much more detailed form (CUREC/2). For example, on the matter of written informed consent, the sequence of questions on CUREC/2 runs: *‘(a) Will you obtain written consent? Yes — please attach consent form. No — explain how consent will be obtained and recorded and why this method is used. (b) If participants are unable to give valid consent, how and from whom will you obtain consent? (c) List those researchers who will, with the authorisation of the principal researcher, secure the consent of participants.’*

To lighten the load on both researchers and the assessment committee, the University allows for protocols to be submitted by departments and disciplines, that explain to researchers what action they

should take if implied norms — such as obtaining written informed consent — are unlikely to be achieved. The protocols may also include sample documents, such as form letters to be given to participants explaining the nature and process of the research, that can be adapted to suit individual project needs. Once a protocol has been accepted by the assessment committee it can be invoked by a researcher completing the short CUREC/1 form and thus obviate the need to complete the much more detailed CUREC/2 form. Once the form(s) have been completed, and the relevant protocol invoked, the assessment committee considers the application and grants approval, or returns the application for further consideration.

The process of ethical scrutiny is relatively new at the University of Oxford, and therefore it is too early to assess its reception and use. Anecdotal evidence from other anthropologists in the UK would seem to indicate that the Oxford process is relatively light. However, the same anecdotal evidence provides two further insights. First, if it is apparent that groups deemed vulnerable by Euro-American standards — typically children or the mentally-impaired — are not the main focus of research and, moreover, if the research subjects live a long way away from Euro-America, then the committees seem to take less interest than they would if domestic research subjects were involved. This seems an extremely cynical assessment, but when I have asked colleagues at other institutions which applications have been returned for further consideration, the majority seem to be those concerning research on subjects in the UK. The second insight is that suggestions of ethical scrutiny are extremely poorly-received by some social anthropologists. The discipline, especially in early decades, was characterised by its ‘lone wolf’ approach to research, with each anthropologist being fiercely independent. To some anthropologists, even today, the idea that a committee of (typically) non-anthropologists could pass judgement on the ethical dimensions of research concerning people of whom they have almost certainly no knowledge is a direct challenge to one’s intellectual autonomy.

However, other social anthropologists — and particularly students — have said that they find the process useful. As mentioned in the introduction, while there are rarely any clear-cut answers to questions posed by code-based ethics systems, considering the questions allows researchers to focus their attention on ethical matters and to consider in advance the kinds of situations they might encounter.

When considering the balance between sharing information and respecting informants’ privacy, much depends on the subject of the research. Contrary to the normal practice of anonymisation, some anthropologists report that research subjects can be quite flattered

at the attention they are receiving and actively demand that their names, or the name of their town or village are reported directly. Anonymity is also difficult to maintain if one works with a population that has a unique or distinctive attribute. For example, my own first ethnographic research was partially concerned with the politics of a faith-based community centre in the UK. As this was the only such centre in the country, I could do little to conceal its location, and although I used pseudonyms for individuals they were certainly recognisable to each other if not to outsiders. On the other hand, nothing I have put into print has — as far as I am aware — caused any problems to individuals or to the group as a whole.

2

Visual anthropologists, and other anthropologists who make photographic, video or filmic representations they intend to disseminate, have long been concerned about the particular ethical issues involved. With still photography in particular, these often centre on the danger of images becoming divorced from their anthropological context — by being circulated on the web for example — and inserted into new contexts by those who consume them. People whose lifestyles seem perfectly ordinary to themselves — and with the passage of time to the anthropologist who works with them and takes photographs of them — may be seen by non-anthropologists as the epitome of primitive savages: hunting with bows and arrows, wearing little or no clothing, and such like.

However, it is the impossibility of anonymising that causes greatest conflict with the codified approach to research ethics. Two examples will show the contrast between a codified and a contextual approach to ethics.¹ The first case concerns a documentary filmmaker colleague who some years ago shot a film in a young offenders' institution in the UK. All the boys were asked if they wished to participate, and to sign a release form if so. With one or two exceptions all the boys agreed and indeed seemed to enjoy the attention paid to them. Some years after the film was completed my colleague was approached by a television company that wanted to screen the film. Although the consent forms the participants had signed were still valid, my colleague nonetheless tracked down the principal characters and told them about the possibility of television transmission. All agreed except one; since leaving the institution he had made a new life for himself and was now married with a young child. His wife and work colleagues were ignorant of his criminal past and he wanted them to remain this way. Although my colleague had followed the ethical codes of the day strictly, he accepted that they did not cover a change in context and so turned down the offer of the television screening.

¹ More detail and a broader context for the first case can be found in [Banks 2001].

The second case concerns an incident that took place when I was making an ethnographic film in India. At one point during the filming, the central character described a (platonic) relationship he had previously had with a woman belonging to a different faith community. Although they had wished to marry, both families refused to countenance the idea, and the woman was quickly married to a man from her own community. As he told the story on camera, my informant — who I had known for many years and who knew that the results of my research were read or seen by others — realised that he was risking revealing the woman's identity and potentially causing trouble for her. After we discussed the issue together, we realised that simply removing the mention of her name on the soundtrack would resolve the matter: my informant said that he had nothing to lose in terms of his own reputation locally, while beyond their immediate social circle no viewer of the film would be able to guess who was being discussed. Although certainly not planned, the incident retrospectively revealed to me the value of context-specific ethical judgement, but hinged upon the fact that the context includes the ability of the participants to fully understand the medium (film, published text or whatever) and to anticipate future consequences.

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Ethnographical methods of collecting data themselves leave a good deal of leeway for breaches of ethics on the part of those practising them. It doesn't take much. After all, none of your informants is going to check up in your research institute or university what you really are working on. No-one will rush to the dissertations section of the Russian State Library,

or to academic journals, to find out what you were actually saying. I've often argued with colleagues about ethical problems, and I've often had people tell me that the only reason I don't tape people without their knowledge is that my informants are from a big city and so they're a bit more sussed: they know what a tape-recorder is and they've got better eyesight. As a matter of fact, you're wrong there: a modern digital recorder such as an Olympus DS-10 looks exactly like a mobile telephone, and, were I simply to sit there with it hanging round my neck, none of my informants would have the slightest idea what it was. Fieldwork in cities is just as much an ethical minefield as fieldwork in a traditional village.

Here I would like to make a small digression on taping informants without their knowledge. On the one hand lots of people will tell you that this is completely wrong, but on the other hand, many of the very same people actually go ahead and do it themselves. There's a kind of rift between the ethical norms that are officially accepted and the compromises that researchers make with their own consciences. It's generally considered more honest to have your tape recorder in front of you on the table than it is to have it in your pocket: if the informant notices, fine, if they don't, it's their look-out. The slippery slope works in easy stages; anyone going down it is able to say, 'But at least I would never do *that*...'

Breach of ethical norms is usually impossible to check up on and (in Russia at any rate), there are no sanctions against researchers who do break rules. The ease of working is seductive: it's so simple to press 'REC' without warning an informant that you are taping them. If you did tell them, you'd need to engage in all sorts of explanations, and village informants, many researchers firmly believe, just don't understand what a tape recorder is anyway. Or, as another widespread belief has it, they think that tape recorders have devils sitting inside them. These two different versions (which of course are mutually exclusive) have been cited to me by many colleagues when we've been arguing about using hidden tape recorders. But no-one bothers any more to ask informants themselves what they actually know about the process of tape recording.

The problem is that anthropological investigations are unethical from the very outset. The ethnographical method itself constitutes a breach of moral norms. After all, can one call it 'ethical' to spend eight hours a day participating in the life of a community and then spend two hours writing up every word and every event in one's field diary, the very existence of which stays hidden from other members of the community? This question may well provoke a lot more indignation than what I have just said about concealed recordings not being acceptable. No, I'm not in fact intending to attack the central method of ethnography. Arguably, without participant

observation, ethnography would simply not exist as a discipline — why should I cut my own throat? I keep a field diary myself, and I don't tell my informants that I'm keeping it. All I want to say is that ethical problems are bound up with social sciences from their very origins.

Researchers make compromises with their consciences through assumptions about the value of academic knowledge. It is taken for granted that it's all right to lead informants up the garden path if that benefits an investigation. The so-called 'fieldwork legend', and the observation of informants without their knowledge, are meant to keep the object of study 'pure', to create conditions similar to those created in scientific experiments, where everything is subject to control. But is this ideal of 'purity' worth breaching ethics for? After all, it is essentially unrealisable in social sciences, given that the researcher's personality, even if s/he conceals what s/he is up to, will always be present in the field of view.

Once we allow the argument that a line of behaviour 'benefits the investigation', we can use it to justify anything; indeed, we'll find we're hardly able to stop. A thoroughly misleading 'field legend'? Advances the pursuit of knowledge. Hidden observation? Advances the pursuit of knowledge. So why shouldn't we just steal an informant's manuscript prayer-book or some attractive craft object when his or her back is turned? It'll just end up getting lost anyway, and we need it for good academic reasons.

But who are we to decide what's better? Our work gets written, on the one hand, or our informants don't have their rights assailed, on the other — which is morally more important?

Fair enough, all ethnographers have to sacrifice moral feeling (human relationships, a sense of fellow-feeling) in pursuit of the truth, no matter what topic they are studying. But maybe they should make an effort not to pay out too much when they strike their bargain with conscience.

4

It's just as easy to fake the process of making the results of an investigation clear to informants as it is the process of observing ethical norms in the field; hence, this issue is also one that is up to the researcher's conscience to settle. If you don't send your informant a copy of your article or ask him or her to come along to your talk, they'll never know what happened to the texts you recorded when you were analysing them, or how their interview was worked over for academic purposes. I myself have tended to ask informants along to my talks on principle (I didn't have to; it was the same kind of negotiation with my conscience as refusing to tape anyone unawares). Almost always, this turned out fairly badly. My informants didn't agree with what they'd heard me say, and they

took umbrage at the generalisations I was making. In their lives (their own lives, their personal lives, lives unlike any others!), I was seeing overall social phenomena, and I was seeking for something of general significance in what they considered unique. For my part, I found what they had to say irritating, and the fact that my conscience was clear was only a small consolation. In fact, it was never completely clear anyway, since I never asked all my informants along — only a select handful. And it was also never completely clear because, after I'd heard my informants out and listened to all their objections, I would nod politely — and leave my analysis just as it was in the first place.

5

Unfortunately, I think that a completely clear conscience is an impossibility for an anthropologist: none of us can ever be totally honest. But I would like to move the goalposts a little. I think there are currently many cases where fieldwork ethics are breached when this would have been avoidable. There are a lot of topics where collecting material in formal interviews would be just as easy, but in fact researchers work differently, using what is often termed 'the informal conversation method', which in practice means, 'now we'll have a friendly conversation, then I'll write up what I can remember once you've gone'. I would advocate always using interviews in such situations, and always making clear that observation is in progress when one has the chance to do this, and also always stating the topic of investigation where this is possible. Information should never be forced out of people, or tricked out of them, if one can get it voluntarily. What informants tell you when they don't know about your field diary or your tape recorder, they might quite possibly be prepared to confide when you were recording openly. Or perhaps they wouldn't be — but in any case, why not ask them?

Regrettably, all I can appeal to here is the conscience of researchers themselves. In Russia, there are no bodies that supervise research ethics in the social sciences, or written conventions, and even if there were, it would probably not help a lot, given the ease with which one can disrupt ethical norms in the field and the difficulty of checking up on anything. I can only attempt to underwrite my appeal to conscience by referring to the categorical imperative. None of us would like to be in the position of an informant whose rights were disrupted. When I obtained a digital recorder that allowed me to make hidden recordings, my colleagues loudly expressed their indignation that now I had the wherewithal to record them (yes, them! other anthropologists) without their knowing about it. I had to reassure them that I'd never make use of my new technology that way. My colleagues also objected to my showing photographs from the family archives of my friends, assuming they knew these friends themselves (even if these informants had expressly given permission for me to use the photographs in my talks). They were

putting themselves in the position of my informants and they definitely did not want their own photographs up on the big screen. Maybe if we suggested that researchers might 'do as they would be done by' (or *not* do as they would not be done by), we'd actually get somewhere?

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1 So far as my own experience is concerned, ethical issues are now without doubt making themselves more and more evident in the course of my fieldwork.

For instance, it is more and more common for informants to want to withhold their names because they are afraid to pass these on. It is not at all rare for people to refuse to speak if they are being taped. There are clear feelings of unease about records of any kind being made of discussion (this was extremely clear, for instance, when we were researching recently in the Chernobyl area: settlers were delighted to present household items for the museum we were in the process of setting up, but retracted their offers when it turned out that this meant signing a form, so concerned did they feel about having to do this.

I have myself felt disquiet over work done by some of my colleagues with children and young people. My knowledge of the fact that in Western countries special permission would be required to do such research nagged at my conscience. However, I couldn't get these colleagues to see what I meant.

Ethical problems made themselves clear in an unexpected way in December 2004, when the presidential elections were taking place in Ukraine. During the heady days of the 'Orange Revolution' folklorists were able to record some texts of a sexually explicit kind, full of swear words and slang — as recorded in graffiti and slogans scrawled on walls, fences, telegraph poles, road signs, or as then being passed round by word of mouth. The problem was that all of

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these texts vilified just one of the candidates. So far as I know, my colleagues never managed to find anywhere they were able to publish these instances of contemporary folklore. Attempts to do it are restricted above all by ethical considerations (not to speak of the fact that the candidate secured in 2006 a majority vote in the Ukrainian parliament).

Fieldwork with members of youth subcultures requires special tact, but so far as I know there are no dedicated codes of practice for this area of work so far.

2

So far as I am aware, the main criterion in organising any kind of research, including of course, fieldwork, is defence of the *personality* [lichnost'] as the subject of study (the English expression 'human subjects' is a good deal more expressive). The recent history of human rights begins with the Nuremburg Trials, when the facts about the inhuman and cruel experiments first became widely known, and when these had to be addressed as part of the judicial process.

A few principles were taken as fundamental in the further development of codes of ethical practice governing work with 'human subjects'. The first and absolute condition of such work was held to be the fact of freely-given consent to become the subject of a given study, carrying with it a concomitant ban on compulsion, and the recognition of the informant's right to cease co-operating should they so wish. Among other conditions for work are the recognition of categories of risk and of the question of who benefits from a project, and the affirmation of the need for due professional competence on the part of the researcher. Recommendations of this kind were made, for instance, by the World Medical Association in the declaration later known as *The Declaration of Helsinki: Recommendations Guiding Medical Doctors in Biomedical Research Involving Human Subjects* (Helsinki 1964, Tokyo 1975, Venice 1983, Hong Kong 1989). Note that the Declaration in fact set out principles applying to non-medical research, as well as to medical research.

Thus, over the course of time, the three governing principles of ethical research were established: respect for the human subject, protection of him or her from possible harm, and the minimisation of the potential damage done by the researcher's intervention (the balance between good and harm must lie in favour of the former). The last requirement means that the researcher must call on as few informants as possible, and subject each of these to as little scrutiny as possible — no more than is consonant with guaranteeing the scientific viability of the results collected. The idea of respect for human subjects assumed that their *personal* dignity and autonomy be recognised, alongside their inviolable status. By extension,

the researcher was supposed to obtain the free, pre-meditated, and conscious consent of the informant or respondent to the work that he or she was planning to do. Other requirements included the need for the informant to understand the purposes of the study and the research questions being asked, and the possible consequences of the research; this meant that the informant or respondent had the right to raise questions at any stage of the research. An essential condition was specified as being the right to *secrecy* (English: *privacy*), or in other words, *'one's right to determine autonomously what becomes known about oneself'*. The right to confidentiality is considered inviolable. The standards of secrecy (and anonymity) require control over the way that personal information is obtained, and also over the way that this is circulated.

I have enumerated at length the basic principles of research ethics, such as now represent the norm in large parts of the scholarly world, because it seems to me that these principles, long ago developed, as have been the mechanisms for their regulation, could usefully form the basis of an international Code of Research Ethics for Fieldwork (on the lines of the Code of Ethics for Archival Research ratified by the Thirteen Session of the General Assembly of the International Council of Archivists on 6 September 1996).

All of this means that I can answer all the questions below unambiguously and without hesitation.

3

For reasons of history, ethnographical research in Ukraine has generally (a short period in the 1920s and early 1930s aside) been oriented to the study of Ukrainian culture. As a result, there is a widespread view that researchers have a harmonious, 'natural', and in every way adequate response to this, 'their own', culture.

In the 1920s, when professional ethnologists were moving into 'the field' in large numbers, 'participant observation' was the preferred method of working (among the advocates of the method were such well-known figures as Nina Zagada and Lyudmila Shevchenko). The method assumes that a researcher would settle in 'the field' and take part in all the events going on locally; he or she would live among 'the aborigines' and observe them by taking part in their life and noting down the details. The experience of the researcher therefore constituted the prism through which information about a given culture was filtered. Experience in turn became the fulcrum of authority, the central argument for and proof of the fact that the researcher's observations and understanding of the culture in question were accurate. With time, the contradictory nature of the term 'participant observation' came to be understood, and the method concerned was renamed as 'experience and interpretation'. In later years, it was 'interpretation' that began to carry increasing weight.

The issue of the impartiality of fieldwork provoked serious reflection among earlier generations of Ukrainian ethnographers, and particularly those who fervently believed in ‘participant observation’ (and practised this themselves). In her field diary, Lyudmila Shevchenko wrote, *‘I regretted along the way that I had not taken up geology as my speciality; it’s much easier dealing with dead matter than living creatures. You can collect all the stones you want, dig about in the earth as much as you wish, and go on your way without a word, but digging up other people’s souls isn’t so easy. Do we really have the right to peek into these, to behave as though we owned them?’*¹ In 1926 she remarked, *‘Always wearing a mask and being a spy, as I have to, weighs you down...’*² I think that long exposure of any scholar to someone who is at once the subject of his or her research, and the bearer of information about this, brings about a mutual process of transformation of informant and interviewer — a process that is extremely complex in its depth and subtlety. And its final result depends more and more on subjective factors — the scholar’s capacity for self-discipline and inner restraint, his or her sense of proportion and tact. This also relates to a researcher’s engagement or non-engagement in immoral acts (whether knowingly or unknowingly — the difference is, in this particular case, immaterial). A careful process of professional selection of researchers is essential if bad consequences are to be avoided; it is crucial that they have a deep and conscious understanding of their own mission. Their activities have to be regulated by clear normative principles that are verified against local experience, but also correlated with those accepted by the international scholarly community.

4

In answering the questions raised here, I also proceed from the fundamental principle of the human subject’s need to be protected. I would assume that if the publication (and, it stands to reason, interpretation) of private information might cause harm, even minimal harm, to the ‘living source’, then such information must not be disseminated. In my view, possible moral conflicts could be avoided if a Code of Research Ethics stipulated the conditions under which publication would be allowable — for example, if information is published anonymously, and/or after a certain period of time (e.g. 20 years), or after the informant’s death (as an alternative: three years after the informant’s death, should his or her relatives consent?). I don’t think that providing pseudonyms or codes for the informants really alters the situation much. Research ethics is supposed to be consequential in all its parts, to require conformity to all the different principles involved — protection of human subjects. Hence, the scientific and social importance of the information

¹ Central State Archive of Literature and Arts, Ukraine, f. 439 op. 1 d. 21, l. 19.

² *Ibid.*, d. 16, l. 173.

adduced should not be considered to outrank general ethical principles. I recognise that this thesis is open to challenge, but I would like to emphasise that the boundary between ‘must’ and ‘may’ in most cases is hard to spot, and hence, hard to observe.

In an ideal situation, I think that all informants should be informed of the results of a particular study, that they should sign special forms in which the issues of copyright, archiving, and dissemination of their information are addressed. I can see that at the moment all this would seriously complicate ethnological fieldwork (and also research in folklore and sociology). However, this would be a means of raising such work to a civilised level (at the moment it is all too often semi-legal and amateurish), and of putting it beyond challenge in research ethics terms. It is worth mentioning that archaeologists carrying out digs have to go through some form of obtaining official permission for their work (I think it is called the ‘Carte blanche application’ [Otkrytyi list]).¹ I think that work with living human beings ought to demand an equally serious approach.

5

Ethical problems, as I have mentioned, are becoming more and more obvious. I see this as deriving from the fact that the traditional ‘field’ for ethnographers, in the sense of the character of respondents themselves, has undergone serious changes. Society is more open now, information circulates more freely, knowledge is more accessible and ‘niche audiences’ have proliferated. The potential respondent is therefore more thoughtful, more demanding, more attentive. Researchers have also changed; their mentality is quite different these days. Concepts such as *lyudnost* (as used by Fedor Vovk),² the ‘people’, the ‘masses’, the ‘population’, all referring to an undifferentiated object of research (cf. the questionnaire that Lyudmila Shevchenko used in the 1950s) have now vanished for good and all into the past. A certain ‘lack of seriousness’ in the approach to ethical issues was for a long time supported by objective factors: in the recent past, ordinary people were quite ‘malleable’; they had become prepared, over the years, to answer any ‘incomer’s’ questions about whatever subject, wherever and whenever it was asked. This sense of defencelessness, complete openness, the absence of any barriers (ethical above all) on the way to acquiring desired information generated a situation where the informant’s ‘privacy’ was a non-issue. I would hypothesise that the results of

¹ This term is also used for a document giving customs clearance to foreigners; in the context of archaeology, it signifies a document issued by a central authority such as the Academy of Sciences to permit excavations. [Editor].

² Fedir Vovk (1847–1918), known in Russia as Fedor Volkov, was an important Ukrainian anthropologist of the early twentieth century and a follower of the evolutionary school. In the Soviet period his works were branded ‘bourgeois nationalism’ (see e.g. the Great Soviet Encyclopedia). By *lyudnost* (literally ‘peopleness’) is meant a very broad and undifferentiated notion of ‘the people’. [Editor].

this were a kind of ‘consumerist’ attitude to the informant, so that ethnologists were most of all concerned with ‘getting the experiment right’, i.e. obtaining information by all or any means. The principle of defending the informant, of avoiding placing him or her in a degrading situation, or still worse, a dependent one, simply did not get raised. In connection with this, I vividly recall villages where the inhabitants ran and hid the minute they heard the word ‘folklore expedition’ — as it turned out, not long previously some students from the local philology faculty had been ‘doing their fieldwork practice’ in the locality. I can cite cases where the informant’s request (or indeed demands) that interviews not be taped were simply ignored. I know that by no means all researchers necessarily overcome the temptation to conceal a microphone in their bags or under their clothes. And I can also name examples (considerably more of them, it should be said) where a researcher’s behaviour, his or her considered, respectful attitude to the informant, their thoroughly professional behaviour and observation of the unwritten but essential rules of proper conduct, caused all possible difficulties and problems of the kind that we usually class as ‘fieldwork ethics’ simply to disappear.

I think that the development of a corporative agreement such as I have tentatively named ‘Code of Research Ethics for Fieldwork’ is a pressing question not just in Russia, but also in Ukraine. Its effective functioning might well be the subject of further discussion, but I don’t think this should inhibit the start of work on drawing it up. New generations of young scholars are now entering the profession, for whom obedience to the law, the principle of legal regulation, and the idea of civilised behaviour are not just acceptable, but essential, conditions of their work. And older researchers with years of experience would receive an essential support for their work, and one whose emergence is long overdue. The effective absorption of such a code, and by extension its effective functioning, would not come about easily, but the obstacles could in time be overcome.

BRUCE GRANT

When I teach large lecture courses to anthropology undergraduates, I like to begin by telling them that ethnography is one of the highest forms of human art, but also the easiest to do badly. The simple question of fieldwork ethics reminds us why.

Virtually every research and teaching institution in the United States receives some degree of

government financial support, requiring all researchers to subscribe to the 'Common Rule' — a set of legal tenets protecting human research subjects. In theory this is all to the good. Recalling the dark days of human experiments in Germany under the Third Reich, the legislation mandates informed consent for all research participants, and seeks to make the circulation of research results as fair and free as possible. In practice, the review process can be burdensome. Institutional Review Boards or 'IRBs' invented the Common Rule to control biomedical and psychological experiments, and often have little understanding of the complicated boundaries involved in more qualitative, less data-driven forms of anthropological fieldwork, where the fieldworker is herself the unit of measurement, and where the lines between the formal and the informal are continually crossed.

Limited relief for ethnographers with historical inclinations came in 2003 when the American Department of Health and Human Services declared the work of 'oral history' to be exempt from institutional review. But this by no means frees all.

One of the most complicated cases in my own department recently involved a young doctoral researcher working on the emerging private art market in China. Despite her having won several competitive fellowships to interview Chinese art dealers, the university committee was not convinced. In an art world where many financial transactions are not always fully legal, would she not be putting her informants at risk to even discuss their transactions? The young ethnographer rightly argued — and eventually prevailed — with the logic that no one in Beijing would speak to her if she required them to sign legal forms prior to every single conversation.

For all of the frustrations and extra paperwork involved in the dreaded IRB, their existence reminds all of us that even the most innocent seeming projects can have extended consequences. Few forget the fatal paradox in the work of American anthropologist Napoleon Chagnon, whose characterisation of Yanomami in Venezuela and Brazil as 'the fierce people' made for popular reading in the 1970s and went on to become one of the most widely read English language ethnographies of all time. Yet during the military dictatorship in Brazil, government officials relied on Chagnon's work to ratify the 'savagery' of Yanomami Indians and the need for their brutal pacification in a series of village bombings to open new corridors for resource exploitation. It is for good reason that Yanomami indigenous councils have effectively declared a ban today on new ethnographic research. Across the Canadian North, many indigenous groups have followed suit by closing their doors to researchers who never sent copies of their published work, or who earned money in their spare time by acting as consultants to

oil companies looking to build pipelines across Native territories.

Much of my own ethnographic work is historically informed, which means that I have pleaded the ‘oral history exemption’ in grant applications, and have had a more fortunate time than many with review boards. Still, when I formally interview with a notebook and a tape recorder in hand, I ask first if my interlocutor would like to think of a character-style pseudonym for themselves — preserving gender and approximate age — in order to remind that some can be recognised by future readers. Moreover, when I worked with elder Nivkh women during fieldwork on Sakhalin in 1995, to invite discussions on the posthumous English language reprinting of the work of Lev Shternberg, I quickly learned how much more positively people respond to ethnographic themes when they are genuinely collaborators rather than ‘respondents’. Because it became so rapidly clear that I was working ‘with’, rather than ‘on’, them, friends and colleagues whom I had known for five years began to offer substantially more engaging commentary.

Less formal settings are more complicated. In 2002 I spent several months working the family of the leader of a famous rebellion that took place in 1930 in the small city known today as Sheki, in north-west Azerbaijan. Few failed to observe contemporary parallels in an angry time of rioting against the local administration, whose inability to provide for electricity, gas, or farm subsidies during a period of great privation had led to violent demonstrations only months previous to my fieldwork. To draw these parallels in my own writing would have made the scholarly arguments stronger, but it also would have betrayed an agreement with all I met — that my focus would be on better times past, rather than somewhat grim times present.

The principles behind such fieldwork ethics are simple and familiar to all: be clear with intentions; be collaborative in research design in ways that not only invite a heightened editorial voice of respondents but that promote their own access to publication; write about your subjects as you would like to be written about yourself; and be generous in the sharing of results. When ethnography is done well, there can be no greater satisfaction than in the deep humanity of the exchanges, and in the finely tuned writing that makes those exchanges knowable to readers. But how many researchers go astray! Institutional review committees may be a burden to some, but their virtues far outweigh the vices, as the saying goes, vices that many resort to when we momentarily forget that we live in a world where all can read what we write.

Recommended Reading

Anthropology Newsletter, online at Anthrosource via <www.aaanet.org> — February 2004 and May 2006 discussions.

ELZA GUCHINOVA

The Ethical Problems of Fieldwork

1

I have indeed had experience of having to alter research plans for ethical reasons, and of finding it difficult to reconcile my desire not to cause offence and my own scholarly interests. In 2004, I went to Elista to record people's stories about the deportation of the Kalmyks. Understanding what a sensitive business talking frankly about the subject would be, given that it would mean people's recalling their experience of social exclusion, and also (if I'm honest), hoping to make life easier for myself, I turned in the first instance to people that I knew well: neighbours, parents of my schoolfriends, friends and other people my parents knew. Most people were understanding about my interest in their life stories — there have been quite a lot of publications about the deportation in Kalmyk newspapers over the past few years. But almost all of them were also convinced that they had to keep up the image of a 'victim population' and dwell on the injustice of the mass repressions (this has been the standard line among Kalmyk politicians and the press). However, when actually talking about their lives, many of my informants began to feel that their experience didn't actually fit the classic scheme too well, and this would prompt them to ask me to suppress this or that topic from publication. Once, in the middle of an interview, the person I was talking to suddenly said, '*You know, you're not talking to the right person: my Siberian experience was unusual, quite good in some ways*' — she was convinced that her own positive experience wasn't what I expected, after all I was a Kalmyk myself, so I could only write about the deportation in the language of high tragedy.

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Almost everyone I spoke to considered themselves part of the ‘collective body’ of the ‘Kalmyk nation’ (‘the Kalmyk people’), they thought it was essential to stress their loyalty to their community, and they assumed that their stories must avoid casting any shadow on the image of that community. All the same, two of my informants, as they reached down into their memories, confessed to me with great embarrassment that, as children, they’d actually believed that the Kalmyks really were exiled for treachery. *Their own* family was the only exception: *their* father was serving at the Front, but all the other Kalmyks actually were traitors. Accordingly, they’d done their best to avoid socialising with other Kalmyk children living in their Siberian village and had kept their distance from all things Kalmyk.

When I showed my informants the transcripts of their oral narratives, edited only so as to correct grammatical slips, almost all of them felt let down. Most of them had higher education, but around a quarter had left school at fourteen or fifteen. If they’d all been from villages and practically uneducated, they might not have been so worried: either they’d have been flattered about their stories getting into print, or they’d soon have forgotten about my visit. But the people whose stories I had collected were from the city, and I knew them all well. I didn’t want to disappoint them. They’d got so used to reading interviews in the press (which are always heavily style-edited) that they were shocked to read the unvarnished versions of what they’d said — though these, while often a bit fragmentary, were also very lively, colourful, and rich. Thus, one of the issues in my work turned out to be, alongside collecting new information about people’s survival strategies in difficult conditions, illustrating how the ‘language of trauma’ functions in terms of its lexis and pragmatics. The mistakes and slips of the tongue that my informants were so bothered by were of particular importance to my informants. As one of them, a woman from the political elite, once put it: ‘*But that’s not how I talk when I’m addressing a public meeting*’. She didn’t think that a straightforward, stripped-down style measured up to the linguistic standards you’d expect from public discourse, and she was concerned that it played down her own intellectual level and was certain to disappoint the future reader.

Almost half the people I showed transcripts to expressed dissatisfaction, and one of them even tore up the twenty-page print-out I gave him in disgust. This particular person had held a leading position in the political administration of the Kalmyk republic during the War: he hence had access to privileged information and felt directly responsible for the fate of the nation. As he put it, ‘*If there were traitors and deserters, then we’re to blame for it*’. He told me this directly and without giving any sense that what he was saying was confidential — the tape-recorder was running at the time. In

general, what he said amounted to a monologue in the classic style of an official from the nomenklatura, someone who weighs up every word he says and doesn't put a foot wrong. But when he saw the transcript, the verbatim record of what he'd said (I hadn't changed a thing and was sure he'd be delighted I'd been so careful to record everything exactly as he'd uttered it), he reacted as though he'd been insulted or provoked. Maybe the problem was the phrase '*to blame*', which sounded like an admission that accusations of treachery might have some foundation, and hence like a justification for the deportation. I suppose one can't rule out that a point of view that he couldn't endorse as a Party member had crept into his thoroughly honest narration and that the destruction of the transcript was a kind of faint echo of the deep internal conflict in him between his two roles as repressed Kalmyk and as believing Communist and Komsomol leader.

When I'd realised that some of my informants were unhappy with the transcripts, I also grasped that any discussion of the format for publication would have to take place face-to-face. Electronic contact would only make things worse. I accordingly postponed working on the book any further till I was able to visit Elista again.

2

I've never allowed myself to play tricks on informants, still less technological ones, though I must say that I have sometimes regretted the information that has been lost because of this. For instance, when I was doing research on the Kalmyk community in the US during 1997-1998, I from time to time encountered an outright refusal to allow permission for interviews to be recorded. This mistrust had been provoked by the bad behaviour of some Kalmyk journalists, who'd often got the names of people they were speaking to wrong, not to speak of facts and interpretations. This history made me feel prejudiced against other people working in the area, not just journalists, but anthropologists too. Anyway, the upshot was that I had to go back after every interview and write down what I'd heard from memory, more or less as Lidiya Chukovskaya after visiting the poet Anna Akmatova. But this kind of record alters one's source material: it gets turned into observation, a record from a field diary, and not a story as such. The content of what people say gets preserved, more or less, but the words get lost — and they are vital to analysis as well. If I was working in public spaces such as big dinners and so on, people were bolder (or maybe the reverse — more hesitant about making objections to being recorded), and I'd put my tape recorder right down on the table. A few minutes of uncomfortable jokes about spies and the KGB once over, people would forget the machine and start to talk in a relaxed and open way again.

4

It helps if one asks formal permission from informants to collecting and publishing information about them. In the West, the usual rule is to change people's names, which is an obstacle to possible reproaches and litigation. It's far from unknown for informants to change their minds. One day they'll tell you x, a day later, in a different mood, it'll be y. This practice of anonymising transcripts is useful in that it separates professional and human relations. Once the project is over, the scholar won't have contact with his or her informants any more, and it's unlikely they'll bother to pass on the results of their research.

In Russian anthropology, traditions are different. It's common for anthropologists to concentrate all or most of their work in one geographical area. This is the place where fieldwork visits are regularly made, where friendly contacts with colleagues happen, where a pool of favourite informants that are always visited and given presents is built up. If researchers do something out of the ordinary, they risk 'spoiling the field', and getting the reputation of someone who breaks promises about what they're going to do with the material collected once they get back home. The result will be that informants will stop talking to them frankly, will start holding back some of what they know and think.

In situations where you're working on your own culture, things are even more difficult. Then there are thousands of threads linking you and the subject of study. Often this is a help, but far from always. In the work that I've been doing on the Kalmyk deportation, I just felt I couldn't suggest changing people's names — after all, this suggestion was quite likely to offend elderly people who felt a sense of pride in their lives and thought of themselves as success stories, given that they'd managed to survive and not 'lost their way' in life. They hoped that their names would remain in the finished narrative when they were talking to me, and that their grandchildren would be able to read about them and feel a sense of pride. Almost the only condition that they all set was that I should send them a copy of the book. Showing that the struggle to overcome terrible difficulties had not been in vain, that the names of those deported had not been lost to history, that they had entered into the rolls of the past despite everything that fate could do to impede this — all this made up one of the main motivations for people's readiness to talk to me in the first place. The suggestion that names should be changed would have raised the idea that they ought to have something to be ashamed of, as though they should be afraid of being denounced for something.

The only case where a name change was at issue related to someone who was the son of a single mother. His mother had lost her children during the occupation, and her husband had died at the

Front. She decided to have a baby so that her life had some meaning. This person referred to his mother and grandmother as 'his parents' in his life story. At the same time, the issue of his biological father was always present, and not asking it, not telling the full story, not mentioning his missing father would have been a kind of censorship, a sort of oblique condemnation of his mother's non-married status. This man, who, in telling his life story, had been forced to face up to questions that he'd hitherto avoided, in fact decided to leave his name in when he read through the interview a year after it happened. He may have felt this in his heart of hearts anyway, but it was talking over his life with an outsider (me) that convinced him for good and all that he could and should take pride in what his mother had done.

Another time, my uncle was telling me about his life, with his wife also present in the room. He'd been exiled when he was ten, and had become the senior male in the family at a very early stage, taking decisions about all the household, and supporting his mother and four brothers and sisters. If you live like that, you grow up fast, but uncle was full of joy of life in any case. He mentioned in passing that he'd been friendly, when he was a teenager, with someone who ran a shop, so that he'd been able to buy a box of butter once a year from her and use it to pay the milk tax instead of actual milk, which meant that he and his family had enough milk to go in their Kalmyk tea all year round 'Friendly' was a bit of a euphemism — there was more to this relationship than that. My uncle said so directly: *'she was very affectionate towards me, and I'd help out with running the house — feed the pigs, say, when she was out at work'*. I could see the importance of the episode: the Kalmyks hadn't always been outcasts, sometimes the lads had proved attractive to local women, and the exchange of favours for kisses had taken place on the part of both genders. But I'm worried about showing the text to my uncle, because he'll almost certainly ask me to cut that bit out.

Of course, it's hard to be objective about someone's hard life and his or her comments on the fate of a given people if you happen to be a member of it yourself. It's accordingly very important for me to avoid making direct comments about Stalinist politics or indeed contemporary politics, both to avoid getting into arguments with my informants and to avoid giving them the sense that I necessarily agree about everything. This is where I often remember the phrase 'ethnographic seduction', used in Antonius Robben's article about his contacts with people who'd murdered innocent victims. Years after all this had happened (the crimes were suspected, rather than proved), they politely asked in their foreign visitor, made him some excellent coffee, and, having made their overtures, impressed their version of events on him (Robben Antonius C.G.M.

Ethnographic Seduction, Transference and Resistance in Dialogues about Terror and Violence in Argentina // *Ethos*, 1996 — Vol. 24 (#1)). I myself have interviewed former members of the KGB who had information about the Kalmyk Corps. They also seemed to take an interest in how the Kalmyk people would be presented in publications dealing with collaborationism. One former general put it like this, *'I'm telling you this, but promise me you won't say a word about it.'* *'But why are you telling me, if I can't use what you see?'* *'So you know who you're dealing with.'* And he told me of various atrocities members of the Kalmyk Corps had committed. If I'd written about them, I could have continued the discussion about forms of violence and the representation of this in historical practice that was begun by V. A. Tishkov with reference to the Osh conflict [Tishkov 1997]. But I'd promised not to write about what was said, and while my informant is still living, I don't think I can bring myself to do it without his permission.

An embarrassing element in fieldwork is when one has to ask questions relating to the private lives of elderly people and stick to half-truths, hints, avoid being explicit about what one means. For example, when I asked an eighty-year-old woman, *'How did you avoid getting pregnant back in the 1940s?'* she said, *'Using the rhythm method'*. I wasn't entirely convinced, but I couldn't bring myself to enquire further. It's unsettling when people don't tell you the truth, but I'm not a policeman and I don't want to insist on 'full confessions'.

Not long ago I was doing interviews with former Japanese POWs who'd been held in the Soviet Gulag. It turned out that actually asking all the questions I'd feverishly typed into my computer wasn't all that easy. People did confirm, by and large, that there had been homosexual relationships in the camps, but only from hearsay. Asking about their own experience — whether they'd felt sexual frustration, had wet dreams, been afraid they'd be impotent when they came out and so on — wasn't always possible. But sometimes, being bold brings rewards. One man was telling me about his love for a Russian woman he'd met, the scent of bird cherry, her soft white skin, and I took the plunge and asked: *'So did you only kiss her, or...?'* And my informant, an elderly professor of eighty-five, said: *'What do you mean? I only felt a real man after I'd got back to Japan, it took about a year.'*

Should we really let questions like this embarrass us? We need to ask them, or else we're failing to make use of the advantages of oral history as a method, agreeing to an incomplete version of history, an 'airbrushed', schematic version, in which there are events, but no feelings, there are party meetings, armies, collective farms, but where the 'little people' who belonged to the Party, worked in

the collective farms, with their ‘microscopic’, but eternal, problems — how to live? — vanish from view. We have to ask the complicated questions, but we need to be careful about doing it.

5

I agree that the problems of research ethics really have become more sharply felt in the years since the Soviet Union collapsed. Now that those doing research, informants, and readers have gained ‘equal rights to truth’, and different competing discourses have replaced the hard-line monolithic conclusions of an all-powerful author, the form that is used to set out material has become markedly more important. Now, anthropologists’ texts are written in ways that are more literary (cf. Clifford Geertz), and the styles of writing of different anthropologists have diversified and become recognisable, just as is true with great writers. The free manner of discussion has liberated writers, even if it does also mean that clichés and infelicities sometimes slip through into published texts, that stereotypes of a racist, colonial, and patriarchal kind sometimes survive even in scholarly texts. ‘Political correctness’ may be absurd in excess, but small doses of it can be useful, since it makes writers take more care about what they say.

Putting together a code of research ethics for Russian anthropologists would be a valuable task, even if it were not to prove very effective (as is in fact likely, given the weak institutionalisation of our community). None the less, even if such agreements only have symbolic value, they do provide points of orientation for young researchers and define the boundaries of what is permissible in ethical terms; they are a step on the way to the formation of an intellectual community of anthropologists in the Russian Federation.

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The Golden Rule of Fieldwork Ethics

There is a basic principle of human morality, often termed the ‘golden rule’: ‘do as you would be done by’. It has been adopted by and adapted in a huge number of individual ‘golden rules’ specific to certain social spheres, eras, and cultures. One such sphere is fieldwork in anthropology and sociology, which cannot stand apart

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from general moral perceptions. The ‘golden rule’ here can be formulated very simply: ‘*Do no harm to your informant!*’

As with any other sphere of human existence, moral norms and practical aims — such as attract funding and make up the purpose of professional activity — can come into conflict. The attempt to solve this conflict can bring the central golden rule ‘down to earth’, produce various forms of compromise in professional reality. Researchers are stuck between two extremes, between the moral imperative and the logic of their own intellectual processes.

One possible reaction is to stick rigidly to morality at all times, to insist that you can only work properly when there is no risk that you will break or bend the rules. Given the prevalence of such risks, however, the end result of such an attitude is often that a person espousing it will end up collecting kind of ‘press releases’ (or to put it more bluntly, hogwash) from the informants — slavishly replicating their own view of the world, that is, while presenting this home-spun propaganda as ‘scholarly research’. In other words, he or she will have fallen into a moral trap — passing off fiction as reality, and thus, while avoiding deception of informants, foisting deception on his or her readers.

The other end of the scale is total moral cynicism of the kind that regards any moral restrictions as mere obstacles to professional success. In practice, everyone has to decide on their own position between these two extremes, though their decision will of course be influenced by the incentives and disincentives that happen to operate in a given scholarly community. There can be perfectly pragmatic reasons for a step in the direction of moral behaviour. Cynicism is often driven by the idea that you won’t ever meet these particular people again. But suppose you do, or suppose the people that you’ve mistreated start talking about it, and you end up bringing not just yourself, but your entire profession, into disrepute? This is the phenomenon that is often referred to as ‘wrecking the field’. In reality, any sane person doing field work, anyone who is endowed not only with a sense of morality, but also with professional self-respect, will try and mediate between the two extremes and reach a sensible compromise when it comes to making a decision about the various issues of strategy and technique that he or she encounters.

Arriving in the field begins by the researcher introducing him- or herself to likely informants. Sticking to the letter of ethical principles means that you have not only to say vaguely what you’re about, but to be frank in every detail, leaving no agendas hidden, no purposes unexposed. However, this worthy aim is fraught with snags. If your informants are really supposed to grasp what you’re doing in the full sense, then you’ll have to give them a crash course in

anthropology just to start off with. So some kind of compromise is essential: you have to introduce yourself and your project with a few well-chosen sentences, making use of non-technical language. For example, I spent a couple of stints of fieldwork in Germany working on a research project that was actually called 'The Social Construction of Identity among German Settlers from the Former USSR'. Could I really sensibly have repeated that to my informants verbatim, given that they'd never have heard at least half the words I was using? Clearly, I couldn't, so I told them I was interested in how settlers adapted to life in a new place. In essence, this is a different topic. So, I'd committed a fieldwork crime by lying to my informants — though the central principle, 'Do no harm' had not been undermined. My small fib did not risk anything at this level.

When I was doing fieldwork on religious communities in the US and Germany, I came across a different version of the same dilemma (strict honesty versus the likelihood of actually getting anywhere with your informants). It goes without saying that my informants regarded me as a lost sheep: I needed to have my eyes opened to the path of the one true faith and to be received into it. The professional cynic in me was nudging me: '*Pretend! You'll start being one of them and make your informants very happy.*' But another inward voice, this time on the side of morality, rose up from somewhere deep inside my consciousness: '*You can't exploit people who believe sincerely like that.*' I therefore decided to be completely honest, though this was probably not the best starting point for participant observation: I told them that I had not yet come to Christ. There's no doubt that my informants would have been perfectly justified in wondering whether I was a bit wanting, mentally speaking: after all, every week of the nine months I was in America I heard sermons about how there was only one true view of the world. I heard all the arguments dozens of times from dozens of different people. But there was a grain of professional good sense here too: as a non-believer, I was able to ask questions of a kind that would have been quite out of order from an actual member of the faith.

An in-depth interview is a kind of double-act in which the interviewer has to play the role of a loyal and slightly naive listener, albeit perhaps not one who agrees with the interviewee in absolutely every particular. At the same time, in my experience, one often ends up talking to people with whom one has nothing in common. Sometimes I find their views completely inimical. For instance, I've talked to murderers, drug dealers, fascists, even to a former concentration camp guard who had taken part in executions. But if I don't nod along with the rubbish that I hear poured out in the form of moralising commentaries about the given person's biographical choices, the whole interview will turn into a squabble — and to be honest, I can't see the point in that case. So I do appear

to go along with what I'm told, adopting an expression of understanding (or maybe one of strategic agreement?). It's another compromise with strict morality.

Technology raises another set of moral problems. Everyone knows that using a recording machine tends to be an impediment to natural discussion, though it doesn't always work in the same way. Some people freeze up completely, others (the majority) are prepared to speak, but do so more cautiously than usual, weighing every word. As a result, conversations tend to turn into performances on the part of the informant. No wonder that people doing fieldwork are tempted to save themselves trouble by keeping the tape recorder in their pocket or briefcase. Particularly since modern machines produce excellent recordings even if you have to camouflage them thoroughly.

A journalist I knew used to take a very brisk view of moral issues — he thought it was simply a load of prejudices that got in the way of his work. He didn't see the need for compromises and simply walked into people's offices with his tape already running. Then he published the verbatim quotes, and when the outraged high-ups tried to deny they'd ever said that, he simply switched on the tape. The result was that people not only stopped arguing, but stopped agreeing to interviews at all, or if they did agree, they stuck to safe banalities. The field was wrecked for good and all. He had to move to another city. The journalists he left behind now have a much more difficult job than they used to, because people tar everyone with the same brush: *'we've met your kind before!'*

As a rule, I myself have avoided making secret recordings, though I must say that I have succumbed to the temptation to keep the tape running in my rucksack every now and then. The professional in me won out over the moralist. For instance, once during an evening's drinking, a very interesting conversation was going on. If I'd whacked out my tape recorder and asked people whether I could switch it on, the life would have gone out of the party. From the point of view of strict morality, what I did was definitely wrong. All the same, I console myself that I've still not broken the 'golden rule' about harm to informants. I shall never publish the texts that I recorded with the informants' real surnames to identify them.

Professional ethics, then, are a special case. This isn't just a question of sticking to ordinary politeness, which would in any case forbid asking personal questions, particularly about topics that might be sensitive, cross-questioning people about how much they earn and what they buy, foisting your company on them, and so on. If you insist on following all the ethical principles that are current in everyday life, you'll probably find yourself needing to change your job. Anthropologists and sociologists have to retrieve information that is

often inaccessible. No-one is likely to ring you up and spontaneously offer you an interview. It is quite common for interviewees to agree to an interview rather unenthusiastically. You've invited yourself round to visit and you're sticking your nose in other people's affairs. You're foisting your company on them and trying to open doors that people don't want to open. You've got no shame — you ask about everything you feel like. In sum, you're nosy, persistent, and even a bit rude. But if you behave 'properly', in the ordinary sense of the word, all you'll get to hear is how it's turned chilly lately and maybe it's coming on to rain, or you'll end up only discussing serious things with lonely people who've got no-one else to talk to. So, in order to carry out your professional functions, you have to discard ordinary perceptions about polite behaviour.

When doing a study using **visual media**, the issues relating to technology take on special significance. The moral norms and behaviour rules that are propounded by social figures of every kind in masses of books and articles all point to the fact that you mustn't take pictures of people without getting their permission (preferably in written form). But once again, if you took that completely seriously, you'd end up junking your camera and not bothering with visual recordings at all. After all, what would happen if you really did ask people's permission before every photograph you took? You'd end up with a heap of tortured-looking smiles in the 'cheese' style, every hair in place, every button done up. Like in a photo album or a waxworks display.

I've also come to a compromise about this issue. For instance, when I was doing my research on church communities, I asked permission at an early stage to take still photographs and make videos. Having secured this on one occasion, I took it as *carte blanche* to film and photograph at will — though I obviously avoided close-ups during services and prayers, and so on. I had my camera with me all the time and I took pictures and made videos of all kinds of different scenes. To begin with, the church leaders had agreed to my presence and role as a kind of 'reporter', and in any case, no-one would agree to posing every minute of the day. Of course, this wasn't strictly 'moral' either. But if you don't want to compromise, then you shouldn't be doing fieldwork in the first place — you should be writing books on research ethics.

Taking photographs or making videos in public places also presents problems of this kind. The reason that person over there is walking along the street most certainly isn't because they want to be photographed by me and appear somewhere in print with God knows what commentary attached. It's common for journalists to use a simple trick. They show some totally banal images taken somewhere or other and use a voice-over that refers to something quite differ-

ent. For instance, a documentary about prostitutes or drug addicts may be illustrated by footage of ordinary people a million miles away from these social groups. It would hardly be surprising if a girl who'd once seen herself on television as an illustration to an item about rocketing prostitution statistics tried to smash the camera of anyone who showed signs of filming her without permission. But at the same time, if you really had to ask literally everyone who strayed into your viewfinder while you were filming in public place for permission, you might as well give up. My advice is to be completely open about using your camera, and then anyone who doesn't like what you're doing can step out of the picture or walk in the opposite direction.

On the other hand, that strategy isn't always ideal either, since it leads to a distortion of reality as well. So once again, there is an irresolvable conflict between morality and professional duty. And once again, the '*do no harm!*' principle is the key. If I can't see that harm is likely to be done, I'll cheerfully violate ethical norms that don't have much to do with morality in a real sense anyway — for instance, I'll keep the camera down by my belt or use a hidden camera. Sure, there's no guarantee of avoiding harm totally — I can't rule out the fact that the picture I'm taking may include someone whose boss or whose family doesn't think they should be where they are at a given time.

In North Korea once, our group of four people was being followed around by three... (I'm not sure of the right way to describe them). Our hosts did everything they could to control the way their country was represented. They only let us take pictures of monuments and so on. Eventually I lost patience with this and said ironically, '*So, imagine the reaction when I get back to Russia and I show these films — everyone's bound to ask, "Are there actually any Koreans in Korea, or just statues?"*' The people taking us round had good reason to be so punctilious about regulating what we saw and photographed. As a Western writer has commented, if anyone visits North Korea and then publishes negative comments about the country, and particularly ones illustrated with photographs of ordinary people, then the guides who took him or her round will get into serious trouble. In this particular case, my own sense of morality made me feel I should only take pictures of the great leader, while professional cynicism tempted me to go round with my camera switched on all the time, but hanging down by my belt, looking pointedly in the other direction and feeling like a filthy spy. I got back from Korea with a full notebook, several videos and whole gigabytes of photos. And now I'm tormented by indecision: should I publish it somewhere — which might cause trouble for people who'd done everything to ensure I liked their country — or just leave it sitting in my home archive?

In fact, **publishing** is where the main moral dilemmas lie. People have talked to you on a private basis, but fine words butter no parsnips, and at some point you have to earn your keep by circulating your results to a broader public. At this point, really serious conflicts between moral principles and your professional duty set in. But if you bear in mind the ‘golden rule’, the conflict can often be solved relatively simply — by making your informants anonymous. A private individual confided his thoughts to me, but I’m citing ‘informants’ in general. This means I can’t do him any harm. If one’s working with so-called ‘ordinary people’, many will be so like dozens of other people that even a private detective would have trouble working out who said what.

It’s a lot more difficult when you’re working with ‘big name’ informants. Whatever you do to conceal their names, incident details that you can’t cover up will easily reveal who they are, unless your research report is so bland it becomes totally meaningless. One way out is to agree with informants which information can be published with a direct attribution, and which is strictly confidential. At the same time, often information in the second category is especially valuable. You can try citing it anonymously, but that puts you in an awkward position: if people accuse you of making things up, you can’t very well cite the tapes to prove them wrong. So, if you cover up your sources, you put yourself in the line of fire. Well, every profession has its drawbacks, and there’s no doubt that breaking an agreement about what’s private really is an assault on the ‘golden rule’. When information of this kind comes up, it’s sometimes the most important stuff you have, but my strategy in that case is to file it for use later on. That way, you’ve got a clear conscience, and it’s likely the information won’t seem so problematic in the future anyway; ‘big name’ informants have a way of disappearing into the ‘dustbins of history’. Even a major official will, in time, be just another face from the crowd.

Another problem relates to the assumption about the interviewer endorsing what informants say that I mentioned earlier. It may well come as a shock, given that I’ve been nodding away, when the informant sees what I’ve made of what he or she told me. The result could be that they start seeing me as a kind of ‘enemy agent’. When you’re studying conflict situations, you have to talk to all the different sides, and so whatever you write is bound to annoy someone. They may well start shouting: ‘*The bastard! I told him all I knew! Treated him to tea and cakes! And then...*’

Something like this once happened to me when I was working on my book *Vlast i ugol* [Coal and Power], about the miners’ movement in Vorkuta. I went and talked to different groups that weren’t on speaking terms with each other, and the people I talked to told

me the most awful things about each other. But they were all very friendly to me and spoke more or less completely frankly. To be honest, I was dreading what would happen when the book came out and the informants got hold of it. I could just never go to Vorkuta again, I suppose. However, I did go, and I took the book along. And to my astonishment, the representatives of the warring groups all said to me, ‘*OK, I don’t agree with everything you’ve said. You shouldn’t have paid so much attention to them as you did in some bits of the book. But by and large I think you’ve got the story right.*’ I found a way out of what I’d thought was a dead end by honestly recording what people’s different positions were and pointing to their weaknesses by citing what their opponents had to say. I’d layered one set of interpretations on top of another. As a result, things that one side had ignored were recorded in the bits of the book dealing with the other side, or in citations from an archive that had long ago been forgotten. The picture was like — to put it metaphorically — a many-petalled flower. The centre of the discussion was a ‘zone of intersubjectivity’ — somewhere where the opinions overlapped. The ‘petals’ were the sections dealing with different opinions, all of which were carefully annotated. Of course, I wouldn’t want to overstate how tactful my book was. In a whole range of places, I had to state my opinion directly and make clear that I didn’t agree with someone I’d spoken to. But at least my work passed the test of whether my informants would be prepared to work with me again, having seen the book. Not one of them refused.

The issue of **agreeing the final record** with informants is a separate matter. If you do give the informants the right of veto, often the whole purpose of the study will be jeopardised. You’ll be letting dilettantes correct a discussion that they don’t have any means of understanding. This kind of situation sometimes comes up when one’s working with a company or government agency. They want to know the truth, but only if it’s flattering. Sometimes, direct censorship can occur — a ban on publishing your results. Often, this destroys the entire point of your work, from an academic point of view, and it turns into a document only of use to the given company or agency. At the same time, it’s sometimes worth putting up with the aggravation, given that you may have no other way of looking at the organisation’s internal mechanisms close up. You can then publish your general findings later on, without reference to the specific case-study. True, it’s likely to look rather bald and abstract, and it could be that only people who know the background will trust what you say. Whichever way, you have to decide before you begin whether you’re happy with the rules of the game as suggested to you or not. Breach of contract is no more acceptable in the academic world than it would be in any other sphere of life.

There are cases where letting informants see (but not censor!) your draft text can be useful. They may, after all, pick up mistakes and misunderstandings. But this is a practical and not a moral consideration.

It's become quite common to discuss the idea of **codifying moral norms**; this has happened in the press for some time, and to a certain extent it's now enshrined in legislation. Now, anthropologists and sociologists are following where others lead. But there is a serious risk here too. Official contracts, codes, and so on don't allow for moral compromises. They always assume that the researcher should be a *'very parfait knight'*, someone without any flaws. But does one actually want to be such a bloodless person? After all, any profession has its drawbacks and problems, and only people who don't know things from the inside would argue otherwise. Can doctors swear that they'll never kill a patient? Can teachers guarantee that no pupil given a fail will ever try to commit suicide? Someone doing fieldwork is like a surgeon opening up the body cavity — investigating areas that many would like to see protected from view. If their desire to do this is not heeded, if investigations are so hedged about with moral strictures they can't be done, then the stink of rotting flesh won't be noticed till it becomes overwhelming. Surgery may not be too pleasant at times, but serious disease that isn't diagnosed is far more dangerous. And the 'patient' that anthropologists and sociologists are working with is society itself.

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1

My university has only recently imposed a code of ethics¹ for research involving human subjects on those carrying out fieldwork in areas such as anthropology, oral history, and linguistics. The starting point for this code is the established system of controls in medical research — the clearance forms for researchers require one to state, for instance, whether one's work will involve the 'use of organs or other bodily material' from 'present or past patients'. This highlights the fact that, in Britain, the area of science that has attracted most controversy — apart from experiments on live animals — is research using stored human materials, above all in cases where it emerges that a particular hospi-

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¹ To be more accurate, an implied code of ethics. See Marcus Banks's contribution above. [Editor].

tal has taken tissue samples without the consent of a given patient or his or her next of kin.

The abstract principles expressed in codes are uncontroversial enough. It is perfectly proper to require special clearance for work that involves ‘deception of participants’, and sensible to ask that researchers meditate on ‘potential risks or actual ill effects’. But a much more slippery issue is how to prevent harm, as opposed to expressing a pious statement that injury to informants should be avoided. Can the use of codes go beyond a purely symbolic, pro forma regulation of intellectual activity, according to which the research director ticks ‘No’ or ‘Yes’ in the right places and everyone gets on with business as usual? And if it can, might it act not just as an extra bureaucratic annoyance at the start of a project, but as an impediment to investigation while this is running?

The issue of control over research certainly inspires reflection. The current political climate in Britain is one in which the receipt of public funding is tied to public accessibility. Subsidised museums and artistic venues are required to monitor visitor numbers, universities to increase participation among socially disadvantaged groups. The view that researchers have a duty to treat their human subjects with sensitivity, and that it is unacceptable to carry out research without the expressly phrased consent of those affected by it, is part of this overall ideal of democratic participation. However, defining who is and who is not ‘affected’ by research is potentially a serious problem. Should it mean that public money cannot be used to fund research that the general public (or some section of it) happens to find offensive, whatever its possible efficacy in terms of scientific insights or scholarly advances? Obviously, it is important that issues of freedom of information, and the role of the academic community in maintaining this, are asserted alongside the right to privacy. But there is little sign of this happening so far, at any rate in Britain. While journalists regularly invoke ‘public interest’ arguments in order to justify actions that are contested as invasion of privacy, such arguments are only gradually coming to be raised in the academic world. The sole existing Research Defence Society in Britain, so far as I know, is one set up by biomedical researchers <<http://www.rds-online.org.uk/>>, and this came into existence only because of extreme pressures from the supporters of animal rights, up to the level of physical threats and actual violence. It is healthy for academics to be aware of the historical link between knowledge and power, but the assumption that the knowledge-power nexus is still located in academia might be misplaced. It seems more likely that, as Regis Debray suggested nearly thirty years ago, cultural hegemony has slipped from academic institutions, and that scholars will have to defend their rights to self-regulation and to a leading role in the so-called ‘knowledge economy’, or lose them.

One's more paranoid reflections might anyway suggest that the institutionalisation of codes at the level of academic institutions has less to do with genuine concern for informants' welfare than with a concern to protect universities against litigation. Intellectual property is an ever-expanding concept, recently extended to areas such as the subject's own claims with reference to a photographic portrait; clearly, citations of someone's words or even descriptions of their behaviour might in principle be actionable too.¹ A second paranoid thought is that the proliferation of ethics committees represents the currently all too familiar assertion of authority by administrators over those actually engaged in research. It is one thing for a professional body to evolve, though wide-ranging consultation, a code that is binding on its own membership, and another for such a code to be imposed top-down by the management of a university, especially if the basic rules are set by people who have not done research in a particular area themselves.²

Yet to blame codes themselves for increased control would, I think, be getting things the wrong way round. In certain contexts, codes can be the instruments of annoying micromanagement (what used to be called in the Soviet Union *melkaya opeka*), but this type of approach finds other weapons if it has to. The benefits of codes can be sensed if one imagines oneself at the other end of them — not hard to do, since in a modern urban society, any 'researcher' also has an alternative life as somebody's, most likely a doctor's, 'human subject'. I have a vivid and unpleasant memory of sitting, twenty years ago, in an Oxford college senior common room and falling into conversation with a clinical medical researcher who was visiting from another university. In response to the usual polite question about 'how was your day', he launched into a long complaint about how he had wasted the entire morning because 'some stupid woman' in the hospital had not been able to decide whether she wanted an abortion or not, thus delaying the arrival of the foetal material on which he had planned to carry out an experiment. Clearly, this incident represented a particularly grotesque failure of intelligence and taste, but it does highlight the dangers inherent in a monomaniacal devotion to professional goals as a self-justifying good.

The starting point for arguments about constraints on anthropological fieldwork or oral history would, though, be slightly different. The medical researcher I was talking to was extremely eminent,

¹ For interesting reflections about the commodification of personal images and the resulting shifts in their legal status, see [Frow 1998].

² Oxford tries to get round this by inviting different disciplinary bodies to submit their own 'protocols' for problematic areas, where codes set up by professional associations may be cited, thus recognising the possibility of diverse appreciations of needs and constraints according to the 'human subjects' in view. Since compulsory ethics-committee clearance was introduced only in 2005, it is still too soon to tell how well this will work.

and securely located in the power structure of the hospital where he was working: his impatience could (though I sincerely hope did not) have added to the pressure on someone making a difficult life decision. The status of fieldworkers relative to the cultures in which they work is usually marginal, and harm, where it occurs, will often happen by accident. It is notoriously difficult to legislate against accidents, hence the built-in limitations on the usefulness of codes. Much also depends on local conditions: an abstract instruction to treat informants properly doesn't help one resolve the difficult issue of whether or not informants ought to be paid.¹ In terms of 'how to get it right' (or 'how not to get it wrong'), advice produced by professional associations,² discussions such as this Forum or the blogging site recently set up by the ASA, or informal exchanges with colleagues, are more useful than regulation *ex cathedra*, particularly in the audit-obsessed culture of British universities, where form-filling often takes the place of reflection on methodologies and procedures. Equally, it is clear that the issues of ethical research in anthropology and oral history go far beyond the basic principles that are articulated in codes. For example, the current scholarly interest in 'collaborative anthropology' is not framed in these at all. At best (perhaps not a bad best) codes may express a set of working principles that are useful to the conceptualisation of future projects, and to the initiation of debates about professional ethics, but they cannot be understood as legislation for all occasions.

2

As abstract statements of principle, codes necessarily simplify reality, and hence grey areas are inevitable. For instance, the principles expressed in codes reflect (and one could say, thereby perpetuate) a traditional view of research as the encounter of the educated member of a social and cultural elite with cultural 'otherness'. But is a company director or politician the same type of 'human subject' as a migrant worker, road-mender, or prostitute? In practice, researchers seem to accept that the principles relating to protection of informants may not always operate in quite the same way. Cer-

¹ The obvious solution for a foreigner is to ask local advice, though one can perfectly well hypothesise a situation where an educated member of culture X would feel it was inappropriate to pay informants, while informants from an economically marginal community could not understand why they were not being rewarded for their time. Or conversely, where local academics who had worked with commercial companies doing market research would advise paying informants on a commercial scale (I have heard sums of fifty dollars per interview mentioned in Russia), when payments of this kind would strike some other academics, not to speak of grant-awarding committees, as highly dubious (there might be the associated danger of informants finding something to say in order to earn money, and so on). There is currently no consensus in the Russian academic community about whether it is best not to donate anything to informants at all, or to give informants small token gifts, or to involve them in the work of the project by suggesting they comment on texts for publication, presenting them with offprints and books, etc., or, as a fourth alternative, offer them a payment (and if so, how much it should be).

² For example, the Oral History Society in Britain has a useful website with guidance on how to talk to informants, copyright, information about training courses in oral history run by the Society, links to other oral history websites, etc.

tainly, a recent anthropological study of European fascists [Holmes 2000] refers to individuals by name, and expresses acerbic views about the levels of political maturity and intellectual sophistication attained by some of them. Codes as such can give little help with deciding when 'naming and shaming' informants is ethically acceptable or indeed essential, and when the expression of such attitudes represents an authoritarian gesture on the part of the researcher, or may even 'do harm'.

I haven't myself felt tempted to 'expose' or 'denounce' my informants when writing about them, but I must admit to having come across other 'grey areas' in work with human subjects.¹

When you are essentially asking people to talk about themselves, the process of data gathering is relatively straightforward. Certainly, there are informants in whom personal recollection inspires embarrassment or resistance, particularly men — women are more heavily represented among informants not just because they live longer, but also because they are happier to talk about the past. But by and large, if informants agreed to give an interview about their childhood at all (and I certainly never pressed anyone who appeared to have serious doubts²), they talked freely. In many cases, the impression was that informants were inwardly heaving a sigh of relief at being able to talk about the past without inspiring the impatience, critical reactions, and indeed boredom that would be provoked in family or friends by the same material. It was rare for anyone to misunderstand, or react with hostility to, a question, though, as it turns out, it is essential to phrase discussion in ways that are appropriate to the phase of life that is recalled (not a point that is mentioned in ethical discussions, but an important one).³ Interest-

¹ This was an oral history project funded by the Leverhulme Trust, grant no. F/08736/A, 'Childhood in Russia: A Social and Cultural History'. I did not do all or indeed most of the interviewing myself (out of roughly 150 informants interviewed by the time of writing, around 30 were interviewed by me), but the comments made here are based on my first-hand experience, which I do not think differed greatly from that of other people involved with the project. Indeed, the semi-structured interviews that resulted were strongly similar in terms of the methodological principles used and the level of the rapport with the informants that interviewers managed to establish.

² By 'serious doubts' I don't mean the kind of modesty topos that is quite frequent with potential informants — how could my very ordinary childhood be of any interest? (all that is usually needed here is reassurance) — but hesitation on the grounds, say, that childhood experiences were painful and that remembering them would be an ordeal.

³ Most of the interviewers had a background in ethnography and cultural history, disciplines that are strongly concerned with questions of rhetoric and style. The one case of serious difficulties with the style of questioning came about when an interviewer from another discipline, psychology, ignored a standard question asking informants about their experience of 'love', and bluntly asked an elderly man whether he had masturbated as an adolescent. Inevitably, the raising of a 'forbidden topic' (*zapretnaya tema*) of this kind (i.e. one about which children would not talk to outsiders, and one that was unmentionable in the public culture of the time the informant was recalling, the late 1940s) created considerable embarrassment, and the informant abruptly terminated the interview not long afterwards, claiming he felt very tired. We stopped working with the interviewer at this point.

ingly, the expression of serious anxiety about the consequences of giving an interview was less characteristic of informants generally (despite the marginal social status of some) than it was of former child-care professionals. No doubt the latter group's long and painful experience of distortion by the press (in one direction before glasnost, in another direction during the glasnost years, and in a third direction at the moment) was a causative factor; I suspect also that people in these professions also held confidentiality essential to professionalism, as might well be the case in Western countries as well.

This far, work with informants does not bear out the dark anxieties about possible intrusiveness and harmfulness that are voiced in ethical codes. At the same time, even when dealing with an apparently innocuous subject, such as a person's childhood, points of conflict and difficulty may arise. Recalling childhood involves a kind of retreat into the self, and at one point or another, informants may spontaneously recall forgotten experiences, feeling a surge of emotional pain as they call these to mind. I came across cases where people broke down in tears of grief for lost parents, or for the 'childhood' (i.e. idyllic childhood) they never had; there were other cases where people felt a surge of acute anxiety or shame over misdemeanours committed years ago (the theft of apples from someone's orchard, say), or where they related, with visible distress, the unhappiness that had been caused by an alcoholic or sadistic parent, or conversely, the misery provoked when other family members ostracised the children of a parent who had been declared an 'enemy of the people'. I was at times left uneasy by the fact that an interviewer can prompt a flood of recollections, then walk away, leaving the informant to deal with the distress that revived memories inspire alone. Psychotherapy is sometimes described as 'the talking cure', but it is not clear that talking is always helpful, particularly in circumstances where people have hitherto coped by keeping pain silent, or where they live in cultures that expect them to do this. Abstract statements about not interfering with informants' lives do little to help decide how one, in a practical sense, minimises damage at such moments. And I am not at all sure where the ethical 'balance sheet' in such interviewing stands — nor, quite likely, are our various informants themselves.

The possibility of mismatch, miscommunication, or affront (which may be read as, or indeed may be, a form of exploitation) increases at the moment one starts trying to publish the collected materials. For anyone using oral history as part of a general historical study, anonymity is not a problem, in the sense that, given history's emphasis on typicality, outline biographical details such as age, gender, and social background are of much greater significance than precise origin. It is sufficient to say 'village, Novgorod province';

one does not, as someone carrying out a case study might, have to name a specific place, meaning that informants will be readily identified by anyone from the locality (and disguise won't help, because local gossip will immediately, if not necessarily accurately, decode the pseudonyms). The difficulty in writing a general study is, rather, the opposite — depersonalisation. An individual's treasured memories will necessarily need to be placed in a general analytical framework, or, to put it more simply, taken out of context. Someone's key life experience of narrowly survived starvation during the War becomes 'war survival story no. 49'. As anyone who has ever talked to a journalist will know, seeing one's own words placed in someone else's controlling narrative, given an alien 'spin', invariably creates dissatisfaction in those whose words are cited. Sometimes, just seeing off-the-cuff comments reproduced in print is enough to produce a sense of alienation. Also, informants may not necessarily like the company they are forced to keep: I am reminded of a story told by Boris Sokolov about folklore collecting in the 1920s, where a woman who had refused to admit to knowing any formal laments was provoked into communication by a version from another informant: '*Da kakaya dura Vam tak skazala?*' (What silly cow told you that?) In the case Sokolov described, irritation with other informants stung someone into acting as an informant herself, but the same irritation may take less creative forms after a text gets published.

Producing versions of interviews for publication in direct collaboration with informants, and without an interpretative framework, is one possible way of working. However, this simply defers the process of generalisation to the point when another historian uses the life histories as primary data. And the end result tends to be a bit like 'ghosted autobiography'; the characteristic pungency of speech is lost, and so too, sometimes, are interesting details of motivation, characteristic hesitations and fluffs [cf. the discussion in Guchinova 2006]. Certainly, looking to the future, it is desirable (with informants' consent, which I have never known withheld)¹ to find some way of preserving verbatim transcripts alongside edited versions, since these represent different kinds of authenticity; and also because attitudes to what it is decent to say, and to be seen to say, have a habit of shifting over time. One may owe it to one's informants to 'improve' their statements for publication (reading verbatim

¹ One interviewee asked me only to archive material outside Russia, and provided me with a pseudonym rather than a real name (the person concerned was homosexual, and was worried about possible harassment, should their anonymity be sacrificed); another interviewee, this time brought up in an observant Jewish family under Soviet power (a highly unusual situation) asked me not to reveal any names in print, but made no restrictions on archiving. All the project interviews will in any case be available for future study only in anonymised form, and access will be restricted to bona fide researchers.

transcripts of one's own oral maunderings is indeed a shocking experience); one surely owes it to posterity to preserve their unre-touched self somewhere too.

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I am going to address three subjects in my replies that may at first seem marginal to the general discussion, since, instead of talking about the power of scholars over informants and the need to restrict this, I'm going to speak about how power is exercised over scholars themselves. The first part of my text touches on the moral dilemmas that crop up when the identity of the researcher as the guest of his/her informants and his or her identity as a scholar come into conflict. The main person to suffer as a result of this conflict is the researcher him- or herself. The second part is about the dangers of being bewitched by tradition. The 'Castaneda syndrome' is the result of the power that research material exercises over researchers. The third part is devoted to the question of the topicality, or otherwise, of ethical issues among Russian researchers in the field.

The price of hospitality

I have a vivid memory of the first ethical dilemma to crop up when I was doing fieldwork. It was my first ever expedition, and I, then a second-year graduate student of the ethnology faculty at the European University, St Petersburg, had ended up — to my own surprise — as one of a team of four that was otherwise made up entirely of folklore specialists from St Petersburg State University. I was assigned three students from the first and second years of the

undergraduate course, told where we were going (it was a place many fieldworkers had visited before) and the name of the person we were going to stay with. As usually happens, this person was the director of the local club, who was ‘adopting’ us on behalf of the village, so to speak. She fixed us up in the more or less empty premises of the local library. The no-man’s land of kindergartens, schools, and libraries that have been shut down for the summer, or indeed for good, exactly reflects the character of our place as outsiders in the social landscape of the village.

One of the main reasons why fieldworkers feel uneasy about ethical issues is the sheer impossibility of reciprocity, of repaying hospitality in kind. By no means everyone is prepared to find somewhere to stay for their informants or to spend time with them if they happen to turn up in the city (though I do know of cases where this has happened). The British anthropologist Morris Bloch has written in his work on the symbolism of money among the Merin, an ethnic group living in Central Madagascar, about these sharp feelings of unease on account of not being able to reciprocate. When he started to get ready to go back home to England, the head of the family he’d been living with presented him with some money to help pay for the journey. The sum was, by local standards, very substantial. Bloch was extremely embarrassed — refusing the money would have been an insult — and so he accepted the gift, later analysing the reasons for his feelings in a brilliant essay. He identified two main reasons in particular: different attitudes to money (in urban Europe, lending money or borrowing money might well be a quick way to falling out with a friend, while just the opposite is true of the Merin), and the asymmetry of social status between the informant and the researcher, meaning that the latter feels indebted twice over [Bloch 1989].

The club director, on the other hand, unlike Bloch’s benefactor, did allow us a chance to pay her back. One evening she turned up and asked us to take part in the concert being held in the club to celebrate the local saint’s day. We demurred as much as we could, but refusing outright was obviously out of the question (a nice example of the moral obligations that flow from gifts!) — so we felt we had to agree. Our doubts (I should say my doubts, given that I was really the one to feel them, basically because of my position as group leader) about whether it had been right to say yes mainly derived from the fact that ‘participant observation’ is a bit of a tightrope walk — how do you manage to keep the balance between ‘participating’ and ‘observing’? When a local festival is going all out, I’d rather have a chance to watch what’s going on than to be up there on the stage of the village club. But we were expected to play along, and so we did, after some hesitation. At least now I

know for myself that these festivals haven't died out (on their character in Soviet days, see [Vadeisha 2001]).

After a week during which every evening was spent rehearsing with the accordion player and the other musicians, which meant our time for writing up field diaries and indeed eating supper was severely curtailed, the holiday itself arrived (I think it was St Peter's Day). The biggest hit at the concert was a local couple well past their first youth who surged through a repertoire of *chastushki*, most of them unprintable; the accordion completely drowned out the sound of the woman's voice, but the spectators (the hall was packed) weren't going to let that bother them. They obviously knew the songs anyway, at least they treated the performance to plenty of warm and friendly laughter, which appeared to come in all the right places. I'll cut short the details of the fun and games (all the *artistes* were made to down a few before they went on stage and the spectators weren't stinting themselves either). But let me just mention one episode that has its charms from an ethnographical point of view, though few from a general human one. This had to do with a fight — what village holiday would take place without one, after all! A young man just out of the army had suddenly taken a strong dislike to a lad from our own group, supposing him to be a rival in love. This invented conflict happened to fit exactly with the plot of a hugely popular song of the day, 'The Student', about how a lad from the university comes to a village for the summer and steals a local lad's girlfriend. The upshot was that a bout of fisticuffs was only narrowly averted. In the course of avoiding it, the lad from our group lost the keys to the library, and so the entire village (or what seemed like it to us) had the chance to watch us breaking into our lodgings in the pouring rain. The next day passed with a guilty sense of having thoroughly mis-spent the day of the concert. I can't remember who mended the lock — either it was the club director's husband or it was the jealous boyfriend himself. Everything ended OK, but I'm still sure we'd have seen more if we'd been watching and not performing.

I've probably remembered this incident because I've never been asked to do anything like that again, and nor has anyone else in my party. Never again have I had the role of the 'granddaughter back for the summer' thrust on me. That's one reason. The other reason why this was memorable was the sense of being forced into something you absolutely didn't want to do. This is a story about pressure from informants — gentle pressure, yes, but pressure all the same, made legitimate by the rules of reciprocity and by the moral obligations to informants that they express.

Castaneda's Syndrome

It was also during my very first expedition that I encountered what someone else was later to describe when referring to a different expedition as 'Castaneda's syndrome'. As I now realise, this syndrome is quite common among ethnographers working in the field. The reasons for it lie in the same sense of an unsettled balance between participation in a given situation and analytical distance from this. Such nexuses of esoteric 'local knowledge' as folk medicine, 'sorcery', and above all shamanism represent particular threats where this is concerned. Carlos Castaneda himself, in his 1968 bestseller *The Teachings of Don Juan*, describes how he went to collect information about healing herbs used by American Indians and met the eponymous Don Juan, a member of the Yaki tribe who later became his key informant. The book is set out in the form of a field diary tracing the development of an anthropology student into a practising mystic. Castaneda decided to follow the path of the Yaki, but in so doing, he stopped being an anthropologist (assuming he ever really was one in the first place). 'Castaneda's syndrome' was what my friends and colleagues dubbed the loss of professional distance and the ability to separate oneself and one's research object.

The more exotic the tradition encountered, the greater the danger that this syndrome will take hold. No institutional affiliation of any kind acts as an obstacle. Not long ago, the site of the Moscow Institute of Ethnology and Anthropology was graced by a notice about a seminar taking place somewhere out of town about shamanism. Sessions on 'how to be a shaman' were promised. I have checked up with eyewitnesses: they really did take place. I know one professional anthropologist, also a researcher in an Academy of Sciences institute, who believes himself to have magic powers and who has become a practising sorcerer. If gossip is to be believed, he's a big hit with the general public — after all, the man has letters after his name.

But to return from one branch of academe to another: Our university fieldwork was over, and we'd arrived at the station in the unfriendly town of Cherepovets to make our way back home. When we were on the train, we heard that a girl in another group had gone down sick: she'd developed a high temperature and then furunculosis. The general view of the expedition was that she was suffering from *kila*, not a condition that responds to conventional medical treatment. So far as I remember, she did get given antibiotics of some kind, but the senior members of her group were seriously concerned that she hadn't been taken to a local healer who might have been able to put her out of her misery. There was a good deal of speculation about who might have wished the *kila* on

her. *Kila* is the name given in local tradition¹ to a condition caused by the evil eye that presents as a tumour or septic eruption. The sorcerer sends it by wind, so that it can end up landing on someone completely by chance — and this, so the general view had it, was what had happened with this sick girl. As a matter of fact, I think she'd had this problem before: its causes were perfectly explicable in terms of conventional medicine, and intermittent flare-ups were part of the general pathology. But the students had got into a mystical frame of mind and written down their notes about passing on knowledge and started dreaming about passing things on themselves, so they got it into their heads this was *kila*. I think the girl herself believed it too. It was like the logic of the Azande as described by Evans-Pritchard — the barn fell down because termites gnawed through the struts holding it up, but the fact it fell at *that* particular moment and on *that* particular person was the result of sorcery [Evans-Pritchard 1937]. 'Castaneda's syndrome' can thus be the result of an incomplete or unsuccessful professional initiation. It's the kind of immersion in another culture you never re-emerge from. Or not as a professional anthropologist, at any rate.

Are we to be ethical?

The discussions about ethics in Western anthropology began after the collapse of the colonial system, since anthropology had once worked to further the ends of this (indeed, the discipline was invented for that reason). The whole issue over how the situation of participant observation, interviewing, and above all writing up may put the anthropologist in a position of domination with reference to the informant and his or her entire culture has emerged as a result of the objective processes of globalisation. These turned the former colonies (which previously required rational administration) into economic partners and independent (give or take various factors) political players, which had to be engaged with from a new position of equality. Thus, one can see a transition, at the global level, from unqualified domination to partnership. From the mid twentieth century, a whole tradition of 'public service' began to emerge in anthropology: much has been written about the need for anthropology to be socially useful and accessible (see e.g. [Eriksen 2006] on 'engaged anthropology'); another term that has come into widespread use is 'collaborative anthropology' (see e.g. [Lassiter 2005]), i.e. various forms of partnership between informant and researcher in the creation of new knowledge. An entire discipline of 'applied anthropology' with its own journals, conferences, and

¹ The word is in fact of Greek origin. (My thanks to Dr Will Ryan for pointing this out to me). [Editor].

active network has come into being. This rapprochement between the discipline of anthropology and society involves many different elements: the search for job appointments, to begin with, not to speak of Western anthropology's traditional left-political leanings, and a good deal else besides. Whichever way, discussions of anthropology in Western academia derive directly from the socio-political context in which the discipline itself is located. But Russian realities are very different from Western ones; we have no tradition of anthropology as such — (with apologies to those working in institutes using the name!) — because there was no 'social demand' for the subject in the USSR (on the demise of anthropology as such, its conversion into a branch of history, see [Slezkine 1993]). At the moment we are in a complicated phase of transition: either we have to try and seek out some authentic core in Soviet anthropology and build on the basis of that, or to draw a line under the past and start reading Western anthropology like mad and adopting Western theory and methodology for the analysis of Russian materials (the strategy adopted by the most forward-looking Russian sociologists). The choice is being enacted as one watches: there are 'Slavophiles' and 'Westernisers', but there is no disciplinary unity, or at least not at the moment. That will only come when university departments and faculties have started to develop, and that is all in the future.

In the article they published a decade ago dealing with the way the field is constructed in anthropology, Gupta and Fergusson [1997] write about the process by which 'the field' has come closer and closer to the researcher and the problems this raises, conceptually speaking. The issue of whether 'the field' can be somewhere you can reach on the underground tends to be answered in the affirmative (though not by everyone — on this, see e.g. [Passaro 1997]). But it is still an important question, and it has been noted that you have to become all the more reflective the nearer you work to home, so as to avoid exoticisation on the one hand and banalisation on the other. Russian anthropology, as has been pointed out often before, has never been through a process of decolonisation, accompanied by recognition that making use of someone else's culture requires a sense of responsibility. . For us, the 'field' never seemed any distance away — and this in turn is an important reason why ethical issues don't bother us too much. After all, we think, if Armenian ethnographers are studying Armenians, or Ossetian ethnographers are studying Ossetians, their behaviour can't be classified either as 'ethical' or as 'non-ethical'. Efforts by ethnographers as members of national elites to study their own culture are seen in purely positive terms, as service to society at large. This leads to a kind of assimilation into the field and identification of oneself with the culture under study, and in turn to the danger of 'Castaneda

syndrome' developing, as described earlier. We start from an assumption that our informants — whether these are members of religious groups, shock troops, city-dwelling sorcerers, or villagers — are 'ours', because they went through the same school programme as us, celebrated New Year along with everyone else, bought food with ration cards in the early 1990s, and so on. After all, they have to bribe the traffic police along with the rest of us when they drive past a 'No Entry' sign¹ that's been put there deliberately, to raise funds, don't they? In sum, we simply assume that we have a full right to the cultural knowledge of our informants since this is part of 'national heritage', and the fact that they know more about x, y and z than we do is simply an accident. So far as 'ethics' are recognised at all, it's in the terms of everyday morality: we've all known since we were small children how you were supposed to behave out visiting. That's usually seen as enough.

Of course, we don't want to look like provincial hicks and ignoramuses in front of our Western colleagues, so, like them, we avoid using the names of real people and places in our articles and academic papers. But no real discussion of ethics such as might end up codified in rules for everyone in our profession has yet taken place. Maybe this Forum will initiate it.

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¹ Literally, 'a brick' [*kirpich*], a slang term for such a sign. [Editor].

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Make Your Choice, Anthropologist

If one rolls up all the questions relating to ethics in this Forum, then it turns out the central idea is extremely simple and as old as the hills — 'does the end justify the means'? It's a question of philosophy, and all researchers will answer it in their own way depending on world-view and attitude, and the attitudes to morality that flow from these. In turn the problem of whether truth in science or scholarship is more important, or, on the other hand, the methods that have been used to get at it, is one that faces everyone in the academic world, especially social anthropologists, whose discipline deals with human activities. In this sense one must be grateful to the editors of *Forum for Anthropology and Culture* for organising this discussion.

Immediately the question arises — is there actually a specific ethics for the conduct of fieldwork? Certainly, since contact with the culture of another ethnic group requires a researcher to adopt the forms of behaviour that are characteristic of that culture. For instance, a woman visiting a traditional tent in Siberia would be extremely ill-advised to sit down on the right side of the hearth which, according to the local perceptions of the cosmos, is the place meant for the male sector of society. However, occasional breaches of local custom — an issue that has a direct connection to fieldwork ethics — need not lead to deep conflicts between the researcher and the surrounding society, especially if he or she is able to deal tactfully with slips and — assuming local convention allows — treat them humorously.

But the questionnaire is primarily concerned with the culture of behaviour at a deeper level,

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and the extent to which this relates to ethics as such. In this regard, I recall my first stint of fieldwork in Sakhalin as a member of a small group of ethnographers. One day the woman we were staying with prepared, in honour of the expedition leader and of the other members of the expedition, the traditional ritual dish of *mos* (fish in jelly with lingonberries). The ceremonial meal was supposed to take place that evening, and the leader of the expedition was supposed to cut up the jelly and give a piece to everyone present. However, as it happened one of us had to leave the settlement in the afternoon and missed the chance of taking part in a central ritual of traditional society. Frustrated by this, he put huge pressure on our hostess to cut him a small slice of *mos* so that he could try it. Our attempts to persuade him that this was completely inappropriate fell on deaf ears. He kept insisting that his whole purpose in visiting had been to study the culture of the given people and that he couldn't possibly leave without a grasp of this central element in the culinary code. Is this relevant to the issue of fieldwork ethics? Of course. But now let's imagine something else. You're visiting a neighbour and there is a festive pie on the table. Would it be ethical to ask for a slice on the grounds that you've no time to wait around for other guests to arrive and that you've heard so much about the amazing culinary skills of your hostess?

Further questions of this kind are raised by all the other suppositions behind the questionnaire here. Does someone have the right to '*publish private information and analysis based on this*' which he or she has gathered in the course of ordinary conversations with colleagues, friends, neighbours, chance acquaintances if they might '*cause damage to the informant or represent him or her in an unfavourable light*'? Is it on to record conversations with such people without asking permission first? I imagine that most people, including ethnographers, sociologists, and folklorists, would answer 'no' to these questions. After all, even the police have to warn suspects that what they say is being recorded.

It follows, I think, that the basis for fieldwork ethics has to be respect for informants, more broadly, for other peoples and other cultures, indeed for other people generally. Yes, the norms of behaviour in other societies have their specificities, which the ethnographer, the sociologist, and the folklorist ought to observe, and world civilisation has evolved various general concepts of behaviour ethics which have already become the norm in interethnic contacts. Scholars ought in many situations to behave 'in the field' and when it comes to information gathering as they would wish to be treated themselves. As a famous sage once said, 'respecting others means respecting yourself, and vice versa'. And this perception also underpins one's answers to the other questions raised here: for instance, may you extract information by whatever means happen to suit you,

may you conceal the purpose of your investigation, may you make use of a 'field legend'? One should observe here that a lie is always a lie and that there is no people on earth who regard lies with approbation. Of course you can obtain information by deception, but how are you going to feel about that afterwards in a professional sense, when you've forced people to pass on material because you weren't able to persuade them to tell you things of their own free will? And what about the next time you need something out of people that you've tricked — will you be able to rely on getting full and frank information out of them then?

I think the central issue with reference to fieldwork ethics lies in investigators' attitude to the purpose of their work — getting hold of material at all costs, or learning about a given people and its culture. This in turn generates opposing attitudes to informants — do you see people just as repositories of information, or are you prepared actually to live among them, win their trust, and get to know their culture from the inside?

The second way of working is more difficult, and definitely more time-consuming, but it protects one from many of the problems raised here. After all, if you really respect people and value them, you'd never dream of concealing the purpose of your research, using a hidden tape-recorder and so on, because doing that would indicate that you had no self-respect. But it's a free world, and people must make their own choices.

DONALD RALEIGH

The Office of Human Research Ethics (OHRE) at my home institution, the University of North Carolina at Chapel Hill (UNC), does require mandatory code of ethics training (an essential part of its 'review' process) for all research that involves human subjects, irrespective of the discipline or methodology. In agreeing to share my experiences with you in regard to how this review has facilitated or obstructed my research, I intended to take this opportunity to grumble about what, upon first reflection, struck me as undue interference on OHRE's part. But after weighing the alternatives and familiarising myself more deeply with the larger national debate that provides the context for my own experience, I must conclude that, until something

better is in place, mandatory codes of ethics training/review strike me as necessary.

My current book project traces the transformative developments of the second half of the twentieth century that brought down the Soviet empire through the life stories of the country's first post-World-War-II generation. The seventy or so individuals I study graduated in 1967 from Moscow's School No. 20 or from the provincial city of Saratov's School No. 42, newly opened 'special' schools that offered intensive instruction in English. When I launched this project in 2001, a newcomer to oral history, I was fortunate to have specialists at my home institution with whom to consult, especially colleagues affiliated with UNC's Southern Oral History Program (<http://sohp.org/>), founded in 1973 'to foster a critical yet democratic understanding of the [American] South — its history, culture, problems, and prospects'. Since then, scholars and students affiliated with the programme have recorded more than 2,900 interviews with individuals from all walks of life.

The question of research ethics arose already in the early stages of defining my project as I familiarised myself with the methodologies of oral history. At this point, I benefited enormously from making full use of a pamphlet prepared by the Southern Oral History Program, *Oral History: A Practical Guide* (March 2000), and of a booklet put out by the United States Holocaust Memorial Museum, *Oral History Interview Guidelines*, printed in 1998. In addition to all kinds of valuable information, both resources contain bibliographies of works on oral history methodology, ethics, management, and preservation, such as Donald Ritchie's classic, *Doing Oral History* (New York, 1995). I also joined the Oral History Association (OHA), founded in 1967, a professional organisation for all those interested in oral history (see <<http://www.dickinson.edu/oha/>>). The OHA website posts evaluation guidelines — in essence, a code of ethics — available as well in pamphlet form. Membership in the OHA includes a subscription to the organisation's *Newsletter* issued three times yearly, and to a professional journal, the *Oral History Review*, published by the University of California Press. The former provided me with helpful practical information, while the latter introduced me to new scholarship and to the major theoretical debates within the field of oral history. In addition, the association sponsors an on-line discussion group, H-Oralhist, a network for scholars and professionals active in studies related to oral history.

After a semester's preparatory work, I applied for, and received, a small research grant from my home university to launch my project during the summer of 2002. But the funds came with strings attached: they would not be released to me until I provided documentation certifying that my project had been approved by one of

my home institution's five Institutional Review Boards (IRBs), sponsored by UNC's Office of Human Research Ethics (<<http://research.unc.edu/ohre/>>), responsible for *'ethical and regulatory oversight of research at the University of North Carolina at Chapel Hill that involves human subjects'*. Four of the five IRBs deal with biomedical, nursing, and public health issues. I needed the consent of the Behavioral IRB, *'focused on research in behavioral and social sciences and the humanities'*.

In order to have my research project approved, I had to complete (with a passing grade) an on-line tutorial on research ethics offered by the National Institute of Health. I had to submit a project description that discussed the purpose of the project; my procedures (detailed enough so that the reviewers could evaluate the risk to the participants); who my participants were; my methods of recruiting them; and what, if any, inducements I offered for participation. I had to explain why I believed my informants were not at risk, what steps I would take to minimise any risk that might arise in the course of conducting my work, what security procedures I would take for privacy and confidentiality, and how prior consent would be obtained by attaching consent forms for interviewees, and an interviewee tape disposal agreement. The consent form spelled out the interviewees' rights (such as the right not to answer questions, the right to ask me to switch off my tape recorder, the right to conclude the interview at any time, the right to remain anonymous, etc.). The interviewee tape disposal agreement indicated what restrictions, if any, the informant wished to place on my use of the transcribed material and on what I might do with the audiotape.

At UNC there are three levels of IRB review (full board review, expedited review, and determining if a project is exempt from continuing review). The IRB makes a decision on what level of review it deems necessary based on the nature of the project, the level of potential risk to human subjects, and the subject population. Like the vast majority of oral history projects conducted at my home university, mine underwent 'expedited' review. Only research involving no more than minimal risk to subjects may be considered for expedited review, which is carried out by an individual reviewer or a few reviewers, rather than by the full board.

Even though I made no substantive changes to my project, I was required to seek IRB renewal each year. Failure to do so would have made it impossible for me to accept institutional and extramural grants and fellowships to continue work on the study. Renewal involves submitting a renewal form and resubmitting my consent form for interviewees and interviewee tape disposal agreement. When I sought project renewal in 2005, the IRB informed me that for future investigations, or for further renewal of this study,

I would need to complete a new Collaborative IRB Training Initiative (CITI), a web-based training package on issues relating to human subjects research developed by a national consortium and maintained by the University of Miami. UNC became one of over 400 institutions in the United States using CITI for their mandatory training in research ethics. The Initiative contains modules on topics such as informed consent, vulnerable populations, ethical principles, and IRB regulations, with a quiz at the end to assess one's grasp of the material. I found the training initiative redundant. However, I had no choice but to comply with my IRB's dictates: I completed CITI training before submitting my application to have my ongoing project renewed by the IRB for the 2006–07 academic year.

Although I understood the vital importance of obtaining the 'informed consent' of the people I interviewed, I was somewhat anxious over how they might react to my presenting them with forms to sign insofar as they had spent most of their lives under the Soviet system, and were likely therefore to regard any *anketa* with suspicion. But for the most part my informants responded with understanding, and even with sympathy, when I explained that the purpose of the documentation was to protect them. No one who agreed to meet with me declined to be interviewed after I presented them with the agreement forms. A small minority asked me to use a pseudonym in excerpting material from the interview, and a handful requested that I destroy the audiotapes after completing my project or return these to them. Now that I have begun writing my book, I see how respecting their restrictions determines how I use their interviews. Be that as it may, concern for my informants' privacy does not in any way encroach upon freedom of information issues. I may not be able to ascribe 'ownership' to a particular view or opinion, but I nonetheless can get the point across.

I believe my colleagues outside the US would benefit from learning about the larger debate in the United States that provides the backdrop against which my home institution's IRB operates. To begin, I wish to comment on the activities of the Oral History Association. From its founding in 1967, the OHA has given top priority to developing professional standards for oral historians, issuing its first 'goals and guidelines' in 1968, and establishing a Committee on Ethical/Legal Guidelines. Since then the OHA has issued a set of principles and standards that consider oral historians' responsibility to interviewees, responsibility to the public and to the profession, and responsibility to sponsoring and archival institutions. The OHA's Oral History Evaluation Guidelines have been adopted by the National Endowment for the Humanities as the standard for conducting oral history and are available on line from the OHA's web site <http://www.dickinson.edu/oha/pub_eg.html>. Through

its promotion of oral history, publications, and annual conferences, OHA-facilitated discussions of research ethics have demonstrated greater awareness of how race, class, gender, ethnicity, and culture can impact interviewing, and appreciation for the impact oral history projects can have on the communities in which they were carried out.

Given OHA guidelines, why do university review boards, initially set up to monitor biomedical research, 'interfere' with the work of oral historians? After all, unlike most biomedical research involving standard questionnaires administered to a large sample of individuals who mostly remain anonymous, oral historians tend to conduct open-ended interviews with identifiable individuals who give their informed consent. At many American universities, IRB oversight dates to 1998, when the U.S. Office for Human Research Protection (OHRP), part of the U.S. Department of Health and Human Services, included oral history into federal regulations for review boards. This gave rise to conflicts at many institutions whose review boards frequently tried to apply standards drawn up for biomedical research to a qualitatively different type of research. Both the Oral History Association and the American Historical Association joined the fray to represent their members' interests. In conjunction with these two professional associations, in 2003 the OHRP retreated from its earlier position, determining that oral history projects should be excluded from IRB oversight. But the imprecise language OHRP used in formulating this message gave rise to multiple interpretations.

According to federal regulations, research subject to IRB review is defined as '*a systematic investigation, including research development, testing and evaluation, designed to develop or contribute to generalisable knowledge*'. In contrast, the OHA website describes oral history as '*a method of gathering and preserving historical information through recorded interviews with participants in past events and ways of life*'.¹ The problem is that both sides elect to define 'generalisable' differently. The OHA insists the term could not possibly mean knowledge that lends itself to generalisation, because this characterises 'every form of scholarly inquiry and human communication'.

Despite the 2003 ruling, review boards continue to demand compliance. A recent study carried out by the American Historical Association's Research Division of the situation at 252 universities and colleges in the United States found that, despite the 2003 agreement that seemingly excluded most oral history projects, institu-

¹ See 'Oral History Excluded from IRB Review', <http://www.dickinson.edu/oha/org_irb.html>.

tional review boards further extended protection of human subjects intended primarily for medical and scientific, not historical, research. In roughly 95 percent of the cases, however, oral history is designated as one of the research methods subject to ‘expedited’ rather than to full review. The AHA study maintains that most IRBs operate under the assumption that *‘even if oral history is excluded from review, only the review boards can decide what is excluded on a case-by-case basis’*. Moreover, in many cases the sparring between IRBs and professional historical associations has elicited *‘a stronger assertion of review board oversight’*.¹ This conclusion prompted the American Historical Association to send a letter to the director of the OHRP, requesting it clarify its position on the matter.² Moreover, the Research Division of the AHA sponsored two panels on oral history at the association’s annual meeting held in Philadelphia in early January 2006, one of which directly addressed the question of review, ‘Oral History and Institutional Review Boards: What Oral Historians Need to Know Before Doing It’ (the other dealt with recent innovations in digital technology).

The situation at my home institution confirms the American Historical Association’s findings. UNC’s Office of Human Research Ethics claims as its turf research involving human subjects that *‘contributes to generalizable knowledge, is designed in advance, and utilizes a systematic approach’*. Dr. Barbara D. Goldman of UNC’s Office of Human Research Ethics explained to me in a personal e-mail communication that there is nothing on the OHRP website to suggest that IRB review is not warranted, and that consequently our IRB is still reviewing oral history, anthropology, case studies with interviews, and so forth. A document Goldman authored but which has not yet been approved by the Office of Human Research Ethics explains that *‘oral history interviews differ from conventional research interviews in ways that can increase the risk to participants, and thus merit additional attention by the IRB’*. Addressing the issue that the decision by the OHRP ‘might appear to deny IRB review to most oral history projects on the grounds that they are not designed to contribute to generalizable knowledge’, she maintains that *‘the decision by OHRP was not that IRBs cannot or should not review oral history projects, but that they did not need to do so; IRBs may choose to perform at a higher standard’* [emphasis mine]. Understanding the reasons behind the challenge oral historians have posed to the IRB’s authority, Goldman acknowledged that IRBs at some institutions have responded *‘stupidly’*, and that she even heard concerns expressed from her own board about the potential risks of

¹ Robert B. Townsend et al., ‘Oral History and Review Boards: Little Gain and More Pain’ // *AHA Perspectives* 44:2 (February 2006), 7–9.

² See ‘Letter to the Director of the OHRP’ // *AHA Perspectives* 44:2 (February 2006), 11–12, 35.

open-ended interviews. *'But we try to keep it reasonable'*, she opined. Based on my own experience, I can confirm that she does. What might happen, however, if she were to be replaced by someone who doesn't share this understanding of things? This is a legitimate concern, for that's what occurred in cases at other institutions in which IRB boards responded *'stupidly'*.

In weighing the plusses and minuses of the review process and research ethics, I believe the guidelines on ethics issued by the Oral History Association prepared me for fieldwork far better than anything I learned from my IRB. Being subjected to IRB oversight has cost me valuable time. I have had to fill out forms, complete online courses that often have little to do with the problems I encounter as a practitioner of oral history, and have not learned anything I didn't already know — or needed to know.

That said, I believe IRB oversight, for now, is a necessary inconvenience. The preliminary findings of an OHA study underwritten by the Mellon Foundation and involving the Oral History Association, the American Folklore Society, and the Society of Ethnomusicologists, confirm the widespread acceptance of oral history, which is 'everywhere' within the academy. But, the OHA admits that *'many faculty members and students are conducting oral history research with little preparation'*, and that *'adherence to the OHA "Evaluation Guidelines" appears to be spotty'*. These findings alone would explain why *'institutional review board issues are alive and well'*. Appealing to association members to share their opinions on the matter, the OHA owns up that it *'may need to imagine creative ways of getting out the need for the "Evaluation Guidelines" and other materials'*.¹ Until it does, and until the federal government issues clearer guidelines, institutional review board oversight guarantees that faculty members and students receive at least some preparation that sensitises them to the risks involved for their informants and to the need for informed consent. Simply put, duplicating code of ethics training is better than none.

None of the guidelines on codes of ethics for oral historians addresses the issue of variations in perceptions of personal dignity across cultures. Not surprisingly, the one 'grey area' that concerned me most had to do with whether I should identify my informants in my publications if they gave me permission to do so (as is standard practice in oral history), or mask their identities as is customary in ethnographical fieldwork. I decided to take the cue from them: most of my informants placed no restrictions on my use of the interviews or on archiving the audiocassettes should a library or other collection express interest in storing them. However, in-

¹ 'Mellon Foundation Underwrites Joint Study', *OHA Newsletter* XL, no. 1 (2006): 1-2.

sofar as I interviewed two cohorts of individuals who attended school together, I saw the need to retain some level of anonymity when my informants said things about others that could be interpreted as critical, judgmental, or hurtful.

Moreover, I came across no literature of a theoretical nature written by oral historians on cross cultural interviewing, undoubtedly because the phenomenon itself is a fairly new one. Unlike anthropologists and ethnographers, oral historians — until recently — tended to carry out studies within their own cultures, and were therefore more concerned with issues of class, ethnicity, and gender, rather than with the discourse generated in representing the ‘other’. Questions of a cross-cultural nature immediately came to mind as I began interviewing my informants. For instance, would their answers to my queries be the same had a Russian interviewer posed them? Were their responses shaped by their attitudes toward Americans and/or toward American foreign policy, controversial as it was/is? Would contemporary developments in Russia or even lingering fears from my informants’ Soviet pasts restrain their replies to my questions? How did issues of gender play themselves out in the interview process across cultures? Ultimately, I found that my many years travelling to the USSR and Russia (since 1971), knowledge of the people and culture, and fluency in Russian provided me with a sensitivity toward these and other issues of informed consent and risk that no guidelines or generic training could offer. I realised that establishing rapport with my informants was up to me, that my seriousness of purpose, openness, ability to relate to them, and overall preparedness greatly affected my informants’ willingness to talk, to share, and to trust. I also became conscious of the fact that how I used their memories was a matter of discernment and judgment on my part. This made me all too aware of my intentions — both professional and personal. And it made me even more appreciative of the gifts they had given me in allowing me to record their memories. The project was about them, but its execution, and the integrity and professionalism of the undertaking, was about me.

BRIAN SCHWEGLER

From the Power of Ethics to an Ethics of Power

I first considered the practical limitations of the current US system for ethical oversight of research while conducting dissertation fieldwork in Slovakia in the late 1990s. Prior to starting the project — an investigation into relationships

among concepts of nationality and state power — I developed a research proposal whose details conformed to the terms of the American Anthropological Association's (AAA) Principles of Professional Responsibility (AAA 1986). I then submitted the proposal to my university's Institutional Review Board (IRB), a committee charged with ensuring the ethical conduct of research involving human participants. Thus, my project, like those of most US-based researchers, was subject to overlapping ethical mandates from the time of its conception: professional (the AAA) and regulatory (the IRB and the federal regulations that structured its practice). The terms of the IRB's approval of the project authorised specific procedures for my research — I was required to acquire the informed consent of my interlocutors, to store data securely, and to use pseudonyms to protect individual's identities. The presumption was that these procedures would minimise harms associated with participation in the project and were accompanied by an unofficial admonition from the committee to avoid endangering my research subjects.

I kept these requirements in mind while in the field. I began interactions with informants with descriptions of my research and my plans for using pseudonyms. The informed consent of these individuals was granted with a clear understanding of the prospectively determined terms of participation. I was therefore quite surprised when one of my interlocutors published a record of our conversations in the local paper. This simple act by an engaged individual caused me significant ethical anxiety: Had I somehow violated my assurances to other informants through his actions? Did this publicity about the research jeopardise the privacy of my other interlocutors? Did it obviate my commitments to this individual's privacy because his participation was a matter of public record? Would I have to inform the IRB about this event? These were not intractable ethical conundrums. Nonetheless, they highlighted the extent to which the ethical codes and regulations according to which the research was developed lacked adequate conceptualisations of research as a social activity subject to the actions and commitments of its participants.

There was nothing wrong with the guidance of the ethics review committee. They urged attention to social relations within the research context and their determinations reflected the terms of their mandate. Since I became a member of an IRB in 2002, I have given similar advice: consider the social ramifications of your actions. Nonetheless, the questions that the newspaper article raised pointed to misconceptualisation of power and agency in the research encounter within current ethical codes and regulations. These ethical guidelines assume that researchers necessarily mobilise greater social power and authority than their interlocutors and that research-

ers are able to control the terms of research relationships. Moreover, they assume processes of informed consent that underscore these assumed inequities of power to be the preferred mechanisms for demonstrating respect for research participants' interests and desires.

These comments call for a refocusing of the ethics of research on the dynamic complexities of research encounters. Contemporary structures of ethics review (both professional codes of ethics and US federal regulations) consider research to be a delimited interpersonal interaction — an activity that poses measurable risks to participants, that requires bureaucratic oversight to minimise these risks, and that functions according to normalising decontextualisations of social interactions that render research legible within institutional 'audit cultures' [Strathern 2000: 1–18]. The power of this conception of ethics derives from the social exceptionalism accorded to research interactions by the bureaucratic and conceptual structures that actively (in the case of the IRB system) or passively (in terms of professional codes of research ethics) exercise oversight over research activities. A reconceptualisation of research ethics is required at this time — one that recognises that research encounters manifest negotiations of power, intent, and expectation among researchers and participants. In short, the ethics of research should be an ethics of power: the power of voice, of intent, and of personhood.

My arguments suggest the need for discussions of research ethics to focus on the social pragmatics of power. They constitute a critique of the contemporary system of research ethics regulation within the US — a system that is in ascendance globally. In this, they join the chorus of criticisms of IRBs and ethics oversight as modes of censorship [Hamburger 2005], as ahistorical moralising [Shweder 2004], as overextended '*gatekeepers for "responsible" research*' suffering from bureaucratic '*mission creep*' [Gunsalus et al. n.d], and as distortions of the philosophical requirements of ethical principles [Simmerling and Schwegler 2005]. These comments neither deny nor revisit these criticisms. Instead, they suggest that the telos of protectionism that underlies current ethics review guidelines and its accompanying bureaucratic infrastructure is based on an improper conceptualisation of the social character of research. US-based anthropologists continue to debate whether or not the discipline's traditional methodologies meet regulatory definitions of research subject to IRB review, despite a 2004 statement by the AAA which noted that ethnography is generally subject to this oversight [AAA 2004]. These important discussions insufficiently pursued the conceptual foundations of the current system of ethical oversight of research. The appropriate question was not whether ethnography meets regulatory definitions of research, but whether these regula-

tions and their philosophical mandates accurately model research encounters.

Without recognition of the social pragmatics of research, the regulatory emphasis on protections for participants, and calls for resistance to oversight, alike extend conceptualisations of social relationships that are antithetical to the anthropological endeavour. Although charged with ensuring that research manifests a respect for persons, IRBs often require procedures intended to minimise risks that disrespect the desires of research participants, as my experience with the newspaper demonstrates. Challenges to the IRB model that emphasise its threat to intellectual freedom or criticise unwarranted bureaucratic creep similarly downplay the extent to which research encounters are interpersonal affairs. In short, if every critical ethical position manifests a social, political, or historical structure of power that threatens the liberty of the researcher (itself a product of a liberal ideology of personhood), then the researcher is the sole, ultimate, and appropriate ethical arbiter and agent. This is, however, precisely the moral hubris that the regulatory enterprise seeks to counteract in its efforts to protect research participants.

Professional and regulatory codes of ethics can play a positive role in research. By offering researchers an opportunity to reflect on the risks and benefits of participation, ethics review processes can strengthen research design and relationships among researchers and research participants. Researchers with whom I work as a member of an IRB consistently remark that the review process encourages critical reflection on their practices. Current US ethical codes and regulations do not, however, adequately consider the negotiations of access, intent, and possibility that constitute relationships among researchers and research participants. Whether within the clinical encounter or ethnographic fieldwork, the presumed differential of power between researchers and research participants within regulatory frameworks for research ethics does not adequately reflect the complex and dynamic social pragmatics of interpersonal relationships. The privileging of informed consent within this system, for example, has the effect of disengaging research from the social contexts in which it is practised. Despite the intentions of professional codes of ethics and IRBs to nurture, limit, or render research benign, some research projects cannot be considered collaborative endeavours with participants; some scarcely benefit participants, and some initiate social actions beyond the consideration of the most prescient review committees. The ethical concerns of research with human participants, then, have less to do with oversight and control (i.e., the regulatory power of ethics) than with raising awareness of the *'moral asymmetry of the fieldwork situation'* [Geertz 2000: 33].

The sociability of fictions

Clifford Geertz has argued that fieldworkers and research participants each exercise power and influence in relationships initiated by the research encounter. Researchers muster scientific legitimacy, money, and connections to institutional power structures; participants offer access to communities, expert knowledge, and social authenticity [Geertz 2000: 33–35]. For Geertz, the calibration of these expectations within fieldwork relationships is a mutually sustaining ‘fiction’ grounded in an ‘ethical ambiguity’ of intent [2000: 3]. Investigator and participant sustain their relationships through the inherent ‘moral tension’ of social encounters built upon divergent expectations. Power in the research relationship is thus diffuse and multidirectional. The research project is not sustained by the acquiescence of power by the subject — a granting of consent for submissive, delimited participation in the study. Rather, it is sustained by the ongoing balancing of power and intent. This balancing maintains the fiction of complementary expectations among researcher and interlocutor.

The regulations that authorise ethics review committees, by contrast, locate research interactions external to interpersonal relationships. Within these regulations and the practices of the committees that implement them, research is an inherently dangerous encounter that requires external oversight according to rules codified in the service of beneficence. Research is cast as a specific encounter subject to its own rules and communicative requirements. The regulatory emphasis on complete disclosure within informed consent processes, for example, endeavours to render the implicit entailments of social relationships transparent through a contractual agreement concerning research participation. There is, however, little about interactions among researchers and participants that is qualitatively different from other social interactions or that justifies the suspension of social norms of interaction. Even disciplinary codes of ethics, like that of the AAA, that recognise the objects of research to be the ‘*manifold products of peoples’ interactions*’ [Strathern 2000b: 294] — and not people themselves — separate research from other social relationships. Although the AAA Code of Ethics positively considers informed consent as an iterative and negotiated process, it underscores informed consent as an explicit recognition of and negotiated acquiescence to the intent of the researcher. Within the AAA Code of Ethics, research interactions are one of a number of personal and professional relationships that generate ethical obligations. Rather than being interwoven with these, however, the Code suggests that research initiates its own obligations, even when a researcher has additional personal obligations to research participants. Within the Code, then, research obligations are distinct and should be managed as if research were separable from other forms of social interaction.

Bureaucratic authority and ethics regulation

Research ethics is increasingly understood as a bureaucratic *'jurisdictional area'* [Weber 1978: 956] for the management of the practice of research. The bureaucratisation of research ethics has sundered ethics from its philosophical origins in a manner similar to its separation of research from interpersonal relationships. Ethics is subordinated to bureaucratic structure: legal definitions distinguish research from other types of social behaviour, the structure and practice of ethics review committees are determined by statute, a cadre of certified professional human subjects research administrators has emerged, and a set of principles has been developed to enable the “*objective*” *discharge of business [determinations of the ethical] [...] according to calculable rules and “without regard for persons”* [Weber 1978: 975, original emphasis]. This bureaucratisation of both research and ethics is described as a legislative response to instances of research abuse in the biomedical and social sciences — from experiments by Nazi doctors to studies of stigmatised social behaviour — and put forth as a means to prospectively curtail studies that would generate similar harms [e.g., National Research Council 2003: 59ff].

This bureaucratic regimentation relies upon artefacts of management to document compliance with static definitions of the ethical, limiting ethical considerations to principled abstractions of the sociability of research and reducing ethics review to the completion of checklists. The most prominent of these artefacts in the US is the 1978 Belmont Report; this report proposes a philosophical foundation for systems of research protections and specifies three principles that must be met for research to be considered ethical: respect for persons, beneficence, and justice. ‘Belmont’, as it is widely known, is the primary mechanism of legitimation for the human subjects protection enterprise in the US. The statutory requirements of the federal regulations governing human subjects research (45CFR46) draw from and underscore Belmont’s principles. Belmont is the only statement of ethical principles recognised as a legitimate foundation for ethical review processes in the contract required by the US government for institutions receiving government funds for research.¹ Consequently, US universities and other research institutes nearly universally hold their research practices

¹ This contract, the Federalwide Assurance (FWA), outlines the terms according to which institutions will apply the federal regulations to research conducted by their affiliates. This assurance is required for the dispersal of any federal research funding and includes stipulations for the establishment of local IRBs, which must be registered with the federal government. Applicants may specify an alternate set of ethical principles to guide their ethics review processes, but little guidance about what, if any, alternatives the regulatory agencies consider equivalent is available. The application form is available at: <<http://www.hhs.gov/ohrp/humansubjects/assurance/filasur.htm>>.

to Belmont's definitions of the ethical. IRB review process and professional codes of conduct tacitly or explicitly incorporate the principles of the Belmont report. IRB reviews document that risks to participants are minimised within a study's design (beneficence), that benefits and burdens of research participation are equitably distributed (justice), and that the informed consent of participants is prospectively acquired and documented (respect for persons). Belmont and similar statements in other countries are the foundation for an *'elaborate system of monitoring and surveillance [of research] which leaves much to the interpretive discretion of ethics review boards'* [Amit 2000: 227]. This interpretive discretion is sustained by the institutional sanctity of the *'calculable rules'* and principles of these technologies of the ethical.

Determinations of the ethical character of research encounters are consequently reduced to the mechanistic fulfilment of these rules. The practice of acquiring the prospective informed consent of research participants is the primary example of a consistent bureaucratic reduction of research practice to a static conception of the ethical. Belmont identifies the practice of informed consent as a manifestation of its principle of respect for persons. It stipulates that informed consent entails that participants are prospectively informed of the purposes and terms of study participation, that they are judged to meaningfully comprehend this information, and that the voluntary nature of participation be conveyed to them. It is only when these requirements are met that research demonstrates respect for persons, according to Belmont. In the practice of ethics review, however, the Belmont conception of informed consent *'has become ensconced as the cornerstone principle of research ethics'* [Rhodes 2005: 6].

Ethics review committees are bureaucratically unable to document that research projects meet definitions of the ethical unless they include provisions for acquiring such consent or meet regulatory provisions for its waiver. Unfortunately, many review committees improperly consider informed consent a manifestation of the inherent ethics of a study — if informed consent will be prospectively acquired, a project is ethical; if it will not, it is not.¹ To support their determinations, ethics review processes consistently focus on the artefacts of informed consent — *'consent forms'* — in their evaluations of the ethics of a research proposal. In accordance with federal regulations, committees require that these documents detail all prospectively determined entailments of the research encounter: study goals, methods, risks, benefits, confi-

¹ This perspective misrecognises the fact that *'informed consent is not a principle, but a practice intended to embody and reflect the ethical principle of respect for persons'* [Simmerling and Schwegler 2005: 44].

dentiality procedures, and reminders about the voluntary nature of participation. In cases where informed consent will be sought verbally, researchers are generally required to submit static scripts that will be performed at the start of the research encounter — these must include the same information as informed consent forms. Regardless of the required format, the prospective determinations of the form and content of these informed consent processes *‘in effect define the anthropologist’s informants not as autonomous persons engaged with the ethnographer in acts of interpretation and narration about the nature of social and cultural life, but as so many individual “human subjects” whose consent can and must be given in an informed way’* [Strathern 2000: 245].

Informed consent has become an enterprise of its own because of this centrality to determinations and documentation of the ethical. Academic papers describing innovative formats for translating consent forms and the benefits of using pictures in informed consent processes are standard in research ethics journals. Companies sponsoring research provide researchers with templates for consent documents that meet regulatory and IRB requirements for the form and content of consent. Many of these efforts are laudable for their attempts to maximise the transfer of information about research projects to participants and, increasingly, to incorporate conceptions of personhood and structures of decision-making in contexts in which research is conducted [e.g. Marshall 2006]. Nonetheless, the regulatory emphasis on informed consent as a process separate from the conduct of the research encounter improperly excludes the social entailments of research as an influence on informed consent processes.

At a 2004 conference on research with Native American tribal groups, for example, a senior official from the federal Office of Human Research Protections (OHRP) asked attendees if they ‘consented’ their subjects. Upon receiving an affirmative response, the official admonished the audience, explaining that informed consent was not an action, but a ‘gift’ granted by research participants. This gift, he explained, was to be safeguarded as a manifestation of the trust of the participants. These comments offered a positive message — that informed consent is not an action to be done to research participants, as it is commonly discussed in bureaucratic newspeak. At the same time, they framed informed consent as a contingent transfer of autonomy. Participants presented their personal sovereignty to researchers as a condition of participation in a research project controlled by the researcher. The gift of consent in these comments did not address or encapsulate relationships outside of the immediate context of the informed consent process. These comments did not describe a gift in the Maussian sense of a *‘total social*

fact'. Rather, social relationships remained external to the process of informed consent, with the exception of a nebulous conception of trust in the research enterprise.

The pragmatics of sociality

The privileging of informed consent underscores the social exceptionalism that current regulatory frameworks and bureaucratic practices ascribe to research. The prospective production and review of informed consent documents and scripts fixes the terms of research encounters without input from research participants or, in many cases, contextually sensitive determinations of the risks of research participation [Sieber 2006]. At best, the motivations, beliefs, and concerns of research participants are represented in the IRB review process by a committee member or external expert with knowledge of the study context or a representative of the study population.¹ As Marilyn Strathern notes, the conception of research that underlies these practices '*pushe[s] the exploratory, indeterminate, and unpredictable nature of social relations [...] back on to a "point of production" with the ethnographer as initiator*' [Strathern 2000: 295]. Geertz's fiction of mutual expectations is replaced by the fallacy of an omnipotent researcher able to prospectively limit interpersonal activity. Prospective determinations of risk, intent, and the terms of interaction are central '*aspect[s] of ethics in advance, of anticipated negotiations, [that] belittle[e] the creative power of social relations*' [2000b: 295]. Moreover, this prospective approach to ethics is not free from its own practical dilemmas. These emerge when participants contest limits imposed on their terms of participation — by writing in newspapers, for example.

This criticism of the practices of the auditable ethics of research does not fail to acknowledge that relationships initiated and sustained through research are free from power disparities. Rather, it advocates a wider conception of respect for persons by problematising negotiations of authority within the research encounter. Anthropology is increasingly engaging institutions of governmental and financial power as objects of study. In these contexts, it would be naive to suggest that research harms are derived purely from the relative authority of the researcher and can be neutralised solely by prospective informed consent. More broadly, ethnographic research consistently demonstrates that structural economic, political, and social authority is consistently challenged and subverted in practice.

¹ Many IRBs require external reviews by local ethics committees for research conducted outside of the US. Because such committees are not constituted in all countries or communities in which research is conducted, such reviews do not necessarily ensure any more knowledge about the social context of a study than the US IRB can marshal internally. These 'local' reviews are, however, generally considered to demonstrate that an IRB has based its determinations on adequate knowledge of a local research context, as stipulated in the US federal regulations.

This observation is applicable to the research encounter — study volunteers can manipulate drug regimes to meet goals external to the research, provide misinformation, and challenge privacy procedures. The model of prospectively approved informed consent does not remove these social processes from the research encounter any more than the discharge of the regulatory requirements for this consent makes a study ethical.

A resocialisation of research and a reconceptualisation of research ethics are required for a true respect for participants in research. This will require recognition of informed consent as an intractable component of the social encounter in which research is encapsulated. Informed consent should be understood as part of an ongoing calibration of expectations among all parties involved in research. If it is to be understood as a gift, it should only be as a manifestation of mutual obligations and an act that generates and extends social relationships. Informed consent needs to be recognised as a process that constitutes social relationships through negotiations of authority. It is part of, and not supplementary to, social relations.

This should not be dismissed as a quibble over semantics. It is a concern rooted in the pragmatics of ethics review and the research encounter. Ethnographers regularly petition IRBs for waivers of informed consent requirements for their research. Their research proposals include remarkable legal arguments demonstrating that the informal nature of ethnographic research makes informed consent *'impracticable'* — impracticability is a requirement for IRB authorisation of a waiver of informed consent. The review of these proposals, however, consistently identifies detailed plans for negotiating permissions for interactions with individuals and gatekeepers (whether municipal officials, community leaders, or institutional administrators). In short, these projects emphasise the spirit of informed consent and display commitments to an *'emergent ethics'* that *'locate[s] ethical discussion in the negotiation of individual or communal interests'* [Pels 2000: 155]. Nonetheless, they are considered incompatible with the bureaucratic manifestation of respect for persons. This discrepancy between institutional requirements for documenting the ethical and the ethical commitments of researchers consistently renders ethnographers as IRB outlaws [Katz n.d.].

Recognition of the social pragmatics of research is needed to minimise tensions between researchers and review committees over the appropriate means for realising ethical commitments in research. Ethics reviews should emphasise that research is inherently social and call attention to its contexts of practice and the negotiations of intent and authority that sustain it. This would require researchers to explicate their often inchoate ethical commitments and ethics review committees to recognise contextually and methodologi-

cally appropriate applications of regulatory standards. As the IRB on which I serve frequently reminds researcher who argue for the impracticability of informed consent in ethnographic research, it is difficult (and ethically problematic) to live with and among a group of people, to insert oneself into their private lives, and to openly record their interactions without some form of permission or discussion of research and its aims. At the same time, ethics review committees must exercise their authority to recognise that the aims and entailments of research can be effectively communicated, understood, and mutually recognised as voluntary in ways that cannot be documented by compliance checklists. Some positive efforts in this direction are underway but additional work is needed to refocus ethics review procedures on substantive ethical issues and not the rote application of regulatory requirements.¹

Toward an ethics of power

At this time, research ethics are ensconced in controversies about academic freedom and the limits of state authority. These are unquestionably important discussions about the power of ethics as a technology of accountability. They are, however, epiphenomena of the broader exclusion of research from the pragmatics of social interaction. The bureaucratic power of auditable ethics emerges from the framing of research as a distinct form of behaviour that can be bureaucratised. In contrast, recognition of the social character of research focuses ethical considerations on dynamic, contextual, interpersonal negotiations of authority born of mutual expectations — in a word, they become an ethics of power. An ethics of power does not deny the cachet or institutional resources of the researcher, but neither does it assume that the researcher's role necessarily holds greater contextual authority than the participant's. Recognition of the social character of research further challenges the bureaucratic rationalities manifest in current ethics review processes. It requires that informed consent, for example, be considered as an act of interpersonal communication subject to negotiation among researchers and participants — and not an interaction prospectively delimited and scripted. Within an ethics of power, the practice of informed consent recognises social personhood and individual agency. In short, it embodies a respect for persons as complex agents able to meaningfully influence social worlds.

An ethics of power does not deny the importance of informing research practices through considerations of ethical principles. Rather, it interrogates these principles by removing the fallacies of the con-

¹ For example, a journal has been established to provide empirical support for researchers' and IRBs' negotiations of regulatory and institutional standards in ethics review processes [Sieber 2006].

trolled research environment and the asocial research participant. When research participants are recognised as having authority within research encounters, ethical considerations demonstrate a respect for persons that empowers individuals. This requires a shift in conceptualisations of the ethical from the dictates of static regulations and reports toward concepts of personhood and autonomy that vary among research contexts, communities, and cultures. Within an ethics of power, respect for persons requires that cultural beliefs, institutions, and mores be considered in the research encounter — and not just that regulatory requirements be translated into local dialects.

My goal in suggesting the resocialisation of research through an ethics of power is to avoid conundrums generated by conceptualisations of research as a distinct form of interpersonal interaction. The dilemma of publicity that opened this essay was generated by compliance with a system that underemphasised the social nature of the research encounter. Although codes of ethics and ethics review processes can be valuable tools for refining research design and practice, they risk reduction to the mechanistic fulfilment of rules and regulations unless they recognise the fundamental sociality of research. Given the structure of the current system for research ethics under which US researchers operate, it is thus perhaps unsurprising that many researchers and research participants feel that their authority over the terms of the research encounter are limited by the bureaucratic regimentation of research and the power of auditable ethics.

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NONA SHAHNAZRIAN

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Local rural communities present those doing fieldwork with specific problems in that the members of such communities not only know each other very well (down to the level of being able to recite genealogical histories back for several generations) but also take a keen interest in every detail of each others' lives. No doubt this is because nothing is supposed to be hidden from the 'objective judge' of the community, its 'all-seeing eye'.

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Case study 1

In October and November 2004 I was staying with a family of Hemshil Turks in the Ashperonsky district of Krasnodar region. I was working there as part of an international project, and the plan was that I would make two visits and stay with a family there, carrying out participant observation and also completing an agreed number of taped interviews, each lasting no less than two hours. The settlement I was living in consisted of twelve houses arranged linear-wise down a highway. Almost all the villagers were related to each other. I had got to know my host family long before the study began, but I'd never stayed there for as long before (a month and a half). As I worked, my relations with the family became closer, but this did not happen straight away. I sometimes sensed a slight chill in the atmosphere when I went off to do interviews with the neighbours (who were also relations of my host family). I got into uneasy situations several times afterwards, since my hostess displayed a keen interest in what had been said. I usually told her that the rules of the project I was working on forbade me from saying anything. There were, however, at least three cases where it did not prove so easy to stave off her questions.

The first time was when I did an interview with a woman who was about to become a family connection — my hostess's daughter was about to marry this informant's son. The wedding had been delayed several times because various close relatives kept dying (the bridegroom's grandfather and the bride's cousin), and in any case, the couple were in the centre of local gossip because the bridegroom was considered to be quite a catch. And now my hostess — let's call her Nurie¹ — was wanting to know exactly what had been said during my conversation, lasting several hours, with her daughter's future mother-in-law, Gulbahar. Nurie was interested not only in what this woman had been saying about her own daughter, but also in why Gulbahar had done 'something odd' a few years before — taking in a foster daughter because she had no daughters of her own, only sons.

The second time, the trigger for the questions was an interview with the younger sister-in-law of Nurie, Madina. Nurie herself was the senior daughter in law in her generation, or the senior one left living in her husband's parents' household (and so she was the chief heir, although by custom it is the younger son who usually goes on living with his parents). Both sisters-in-law reacted to the situation by seeing themselves as chief victim, and spent a lot of time insisting that they'd only moved out/stayed on with the parents because

¹ Some of the names in the case studies have been changed.

they absolutely had to. Nurie was worried that my conversations with Madina might threaten this official version of her own life.

A third case was related to a conversation that I was having with Nurie's older brother Suleiman. Suleiman had always been a successful businessman, but the story going round was that after a series of crackdowns by local officials he was now almost bankrupt. This rumour appeared to be confirmed by the fact that he'd stopped helping his relatives out in a financial sense — Nurie was one. She tried to find out from me what his real financial situation was. Could this be a bit of sharp practice on her brother's part, a way of avoiding his family obligations?

This shows how a researcher can end up possessing detailed and many-sided information that puts him or her right in the middle of the life interests of his or her informants. In this kind of situation, it's easy not just to 'wreck the field', but to destroy your relations with people who've offered you hospitality, patronage and help and support, and to be the focus of misunderstandings, misrepresentations, and angry conflicts between people's relations and other members of society.

It's not that easy to pick the right line of behaviour and to come out of the situation with your dignity intact, and to avoid being used as a means of doing down other people in the community. One particular point of difficulty is that informants persist in using you, the anthropologist, as an arbitrator of family disputes and constantly ask for your opinion on this or that point.

Added to this, the whole interview situation can give rise to judgments that have a direct impact on personal relationships, if not on life in general. For instance, when I was doing a joint interview with a husband and wife, each of them said things that made the other think of their relationship in an unexpected new light.

To quote from my field diary:

29 Nov. 2004. K. settlement. Visit to Hasan and Liutfie Karaibragimov. Hasan is a professional bee-keeper. He lives on the same street as his birth family. He's now married to a relation. Their first child died (Liutfie is sure that the doctors were to blame), and after that medical advice was to avoid pregnancies in future or otherwise their children might be deformed. Liutfie didn't listen and now they have two children, a boy and a girl.

Hasan insisted that he only wanted to be interviewed with his wife present. They kept correcting one another. Their children (a girl of two and a boy of seven) kept butting in too. Also, there was a moment of embarrassment that made me regret Hasan's refusal to talk to me on his own. Hasan was talking about his marriage, and he said, 'It

was my wife who chose me, not the other way round.' His wife looked at him in astonishment and said, obviously upset by this, 'Yes, I loved him and I told my parents I wouldn't get married unless it was to him.' We quickly changed the subject, but Liufie had clearly had her mood spoiled by what had been said.

Social moments of this kind that might well have remained hidden if it weren't for the researcher's presence, and the curious shifts of dynamic it brings about. Obviously, this places a heavy moral load on those doing fieldwork. On the other hand, situations like this are totally unpredictable, and could perfectly well be provoked by something quite different.

Case study 2

In the spring of 2001 I was in a town in Nagornyi Karabakh, where I was collecting material for a study of gender issues. Fairly late into the research, I got to know a women who had taken part in direct military action during the Karabakh War. She'd arrived in Karabakh from Armenia itself in 1992, at the height of the Armenian-Azerbaijani War. I asked her a sympathetic question at a school matinee to mark the anniversary of victory in the War, and that was how our friendship and collaboration started. As everyone knows, personal and indeed even business relations in the southern republics of the former Soviet Union are highly emotionally charged, and that characterised our relations as well. My informant had only two days of leave after every ten days of military service outside the town in an army camp. Never the less, she devoted all her free time to me, despite having to spend all her working days in a camp that had important defence duties to perform. During her limited free time, she told me the story of her life. We were using a variety of different resources — videos, tapes, conversations, written answers to questions. Of course, the amount of information that she gave me about different phases of her life varied quite widely. She told me about army life in huge detail — this was something heroic and worthwhile — but replied briefly and reluctantly to questions about her life before the war. Since I was intending this to be the in-depth case study at the centre of my analysis, I noted down everything I could find out about her — opinions, reactions, views, rumours. I gradually worked out that there'd been some kind of problematic occurrence after which she'd had to leave her two children with her mother-in-law and go to Karabakh (by the time hostilities started, she was already divorced). When you put all the interviews and the other information together, the message was that she'd got involved in the war as a way of solving a personal crisis. The message seemed to be that she wanted to either clear her name or die.

My ethical problem with all this was that she'd been expecting me to write something completely different about her. Or to be more accurate, she was convinced that anything which appeared in print had to be of the 'heroic' kind (she'd shown me various newspaper articles about herself). It was hard for me to explain what I really had in mind for several different reasons. To begin with, I didn't fully realise the issues myself at first; also, there wasn't any time to brood; and finally, writing up a research text (in this case a dissertation) takes huge time and effort, quite out of all proportion to the 'minimalist' version of herself that she had in mind. So I have to consider myself lucky that she simply trusted me. Every time I asked her whether I could cite this or that piece of reminiscence, she would tell me to do what I felt like, as it suited me, she didn't object. But at the same time, her own comments suggested that she was expecting a heroic write-up like the ones in the local newspapers and the newspapers of the Armenian diaspora, whose correspondents were also frequent visitors.

The result is that it is precisely the sections of my text relating to her that provoked, and continue to provoke, most sense of doubt and self-questioning in me. In the other sections of my dissertation, I did my best to address the constructs that people were using, the elements that went to make up a kind of collective image of 'the Armenian woman'. Hundreds of interviews were used to illustrate an idea or different ideas. In the 'case study' section, all I had was the story of one specific person. The situation was even more vexed because she herself had refused, on principle, to have a different name used. More: she positively insisted that I should cite her full name and all the concrete details of her biography. If I didn't do this, it was clear that all the work she'd put in would be wasted, as far as she was concerned. However, I'd heard different stories from other people and at the time, was inclined to believe them. I'd heard that the security services on the enemy side were tracking down people once they left Armenia and Karabakh (and persecuting them?) I shared my fears with my informant, but she wasn't having any. She burst out laughing: *'You think I'm afraid of them? Let them try and get me, if they feel like it!'* All the same, I went against her wishes and meticulously concealed all personal information and any information of potential strategic importance. However, even this didn't solve all the problems: the codes I'd used didn't cover anything up for locals, since she was easy to recognise, given the 'abnormality' of her behaviour.

When I was writing up my text, I was tormented by doubts on a scale that have never gripped me before or since. I'd been forced to do a great deal of speculation about her pre-war life, to construct it in my own way and to bring out the chains of events and con-

sequences that had been expressed in her life strategies later on. I wanted to do a few more interviews to check whether my hunches were right. So I went and visited her again two years later. The situation was stressful. Suppose it was all wrong? Suppose everything I'd written was no more than fantasy? Should I junk the work and start again? Stick to teaching and never do anything connected with real people again? To my huge relief, my speculation turned out to be correct.

I have a clear memory of our meeting. We had been corresponding through an intermediary — she didn't have a computer and her written Russian isn't too good — and so I only had news about the major events in her life: she'd been awarded a bravery medal, she'd had a second grandchild. She wasn't in the evening I arrived — she'd gone to a wedding in the hall of the local school. My companions insisted I go round there straight away; they said she'd be delighted to see me. Indeed, she immediately left the wedding and took me back to her home. The occasion was very emotional, and I find it hard to describe. I felt just as moved as she did. By now we were treating each other like close friends. We exchanged gifts and so on. She fussed about trying to find somewhere for me to sit and food for me to eat. We sat up late — the next morning, she was off to the camp for another ten days. I was feeling quite strung up because I'd decided I had to show her what I'd written. She took my text, leafed through it quickly and said, '*I know you won't have written anything bad, I trust you.*' At that point, I realised she was never going to read the story of her life that she'd been waiting for so long. Whether it was the language barrier, or the fact that academic writing is so tedious, so weighed down with terminology — who knows? At that moment, I sensed even more acutely the extent of my personal responsibility in writing about someone else's life. She'd never even find out that her expectations had been — not deceived, exactly, simply hadn't matched the 'research questions' of my scholarly study. Will she ever find out, I wonder? Maybe her grandchildren will at some stage?

I have to say that the chapter about her presented her generally in a very flattering light, both from the point of view of gender theory and from the point of view of the values of neo-traditional culture. The one problematic point was her personal conflict with her surroundings and possible failure as a mother (this was picked up by both my thesis examiners in their reports). But the truth will out. I still do wish, though, that I knew what she would say if she did read what I wrote.

Case study 3

Not long ago, I was putting together an article about Soviet biography for the Moscow journal *Vestnik Evrazii* [The Eurasian Herald]. The object of study was my own great-grandmother, Satenik, who died from shrapnel wounds during the Karabakh War. I tried to avoid the methodological problems that composing a 'unified' text would have presented by adopting a 'polyphonic' style, that is, a fragmentary, contradictory, heterogeneous manner of writing, in which all my relatives were left to speak with their own voice. The article turned out readable, light on jargon, a kind of *creative non-fiction*, if you like. My relatives took my project pretty seriously, assuming it meant something along the lines of '*commemorating our amazing great-grandmother for posterity*'. Thoughtfully and in detail, they narrated their memories of Satenik into my tape recorder. One of her grandchildren who'd been an eyewitness to a certain event in Satenik's life told me a story about it. It was so emotional and so revealing of certain things about my great-grandmother that I included it without further ado in my text, as follows:

1984. Larisa (granddaughter). *I was working in the Party executive committee then. Amik Avanesian, the secretary of the district committee, he comes from Sos village, he had me in to his office and he tells me this. 'So you're Satenik Sogomonian's granddaughter?' 'Yes,' I said, 'she's my grandmother.' 'So is she still alive?' 'Yes.' 'Could you take me to see her?' Yes, I said. We get into his official Volga and go over to Satenik-tatik. Good day to you! I introduce him to grandmother — he's a member of the bureau of the district committee and so on. And she herself was an honorary member of the bureau, right up till the end of Soviet power. She didn't go to the meetings because of being so old, but she was on the list. That meant that for all the holidays, 8 March, 1 May, 7 November, she used to get invitations to all the dos they put on and presents, they'd ask her to give speeches from the tribune... Well, as a person with an outstanding record of service to the Party. For some reason, she got the idea that he'd come to ask her along to something like that. Well, she was used to it. We got tea together and sat down to have some. And then he — this was a total shock — reaches into his pocket and gets out five Nicholas II coins and puts them down on the table. She asks him, 'How do you know that's my money?' And he said his own grandmother had told him that this women, so and so, had turned up with her baby at the turun (the traditional stove dug down into the earth) when we were baking. She'd given him a coin and taken some bread. One gold coin for every piece. 'We were hard up then, it was hard times, and I took the money. But I saved it up, I didn't spend it, I saved it for a rainy day.' 'Now my grandmother's sick, she's been in bed over a month. The money's nagging at her, she told me to come and return it to her so that she can face God with a clear conscience.' He also asked what*

had happened to her child. Satenik told him she'd done a pharmacy degree in Erevan, she'd got married in Martuni, in Martuni in Armenia. That's my story, we saw it all happen right in front of our eyes.

***Zhenya (Satenik's daughter-in-law):** That's right, Sara gave back the money. People told me Sara felt guilty that she'd sold bread to Mama when she was going hungry, she felt guilty that she'd taken money from ragged, homeless refugees. She wanted to give the money back before she died. Her husband had gone blind... she probably thought God was punishing her. People said she was being haunted — this tall handsome woman standing next to the turun with a babe in arms, then handing her a gold coin, taking the bread and walking away. And so on for days on end... And Salenik tried to buy back her rings, they meant a lot to her, she went hunting for them in the 1950s, once she was doing well, she wanted to buy them back. But they wouldn't let her. They denied all knowledge. No, that never happened, they said. Well, it takes all sorts. They turned her down — no-one ever swapped gold rings for bread, no. But Karabakh's a small place, she could remember it all, remember exactly who'd ended up with what... I think she wanted a memento of her first husband, you see. She didn't say much about all that.*

After I'd written up my biography of Satenik, I was expecting critical reactions from my relations about several details where the testimony didn't add up — there were even direct contradictions. I was worried about possible negative feedback on the section called 'The hidden side of a life'. Nothing shocking was said as such about Satenik's private life, but there was some reflection on various delicate topics. But it's impossible to tell in advance what's going to unsettle people, and in fact what they objected to was something quite different. Satenik's daughter, who'd also given me an interview, read the text and on one of my next visits to Erevan, she started a discussion about it. Her main question was: what happened to those five gold coins and why hadn't she, as Satenik's daughter, her closest relative, been given even one of them? As a memento, she meant, of course. Her own daughter listened to these complaints in disgust, face like thunder, snorting in indignation. The situation was as unpleasant as it was unexpected. The information I'd retrieved had stirred up people's lives and created conflicts that didn't seem likely to simmer down quickly. I had to explain that I'd never seen those coins myself and that I had no idea where they'd got to after being given back, and that finding out about them hadn't been my aim at all.

2

I'm not sure that 'concealment' or 'deception' is the right word here. Someone doing fieldwork isn't usually clear exactly what the interviews will be used for in advance of writing up. Mostly I am completely honest about what I am doing, but it has to be said that

the informants themselves don't find my explanations very convincing. It works better when one has a well-put-together and many-times-rehearsed 'legend'. I have also come across blanket suspicion and mistrust on the part of informants — mostly those from marginalised groups.

I can remember something odd happening relating to technology. I had asked the very elderly leader of the Hemshil Turk community where I was working whether I might tape our conversation. He politely declined the request. After I'd been talking to him for over an hour and wanted to check the time by looking at the screen of my mobile telephone, the leader saw the light flashing as I pressed the button and said reproachfully, *'But, daughter, I did ask you not to tape me...!'*

From the case-notes of an Armenian colleague: It's common for there to be a considerable cultural distance between a researcher and an informant, even if they come from the same culture. Particularly, there are often differences between what is considered 'secret' or 'private' and what isn't. The ethnologist Gayane Shagoyan, from Yerevan, was collecting material about fortune-tellers in the Armenian countryside. One of them had graciously allowed her to tape the entire interview. What was more, she frankly talked about how she tricked the clients, about how she'd stolen things, and so on. Then she suddenly asked Gayane to turn off the tape recorder — she wanted to say something private. Gayane did as she asked. The fortune-teller leaned over and, speaking right into Gayane's ear (despite the fact that no-one else was present) whispered, emphasising every word: *'I'll tell you the truth... my husband is actually my second husband, you know, I was married before...'*

The understanding of the anthropologist as someone who has privileged access to technology has not yet been desacralised in rural communities (as opposed to urban ones). Hence he or she is still the focus of attention as someone who has the means of recording people and situations 'in the here and now'. However, people in local positions of power tend to regard this as anything other than an advantage. The very fact that time, energy, and special resources are available to observe the local community is something that they appear to see as a threat to their own monopoly on panoptic control, leading to an attrition of their privileged knowledge of affairs and a dispersal of this to other people. I have encountered problems at this level, and the reason they weren't worse is that I am a cultural insider and have access to a family support network that is able to act in my defence. All the same, I have at times ended up more or less completely falling out with very close relatives whom I'm very fond of. The tendency is for people doing research to be credited with various different power roles: newspa-

per correspondent, boffin, spy, or all three at once. Gogol's stage comedy *The Government Inspector* acts as a lovely illustration of the general atmosphere of suspicion in operation, the paranoid sense of the 'hidden hand' of Moscow, US imperialism, 'third forces' of whatever kind (interestingly, Azerbaijani connections were never raised as an issue, probably because I was 'local', 'one of us'). Coincidences can sometimes lend unexpected authority to these fantasies. For instance, I happened to be staying in one district centre just at the time that the local police chief was sacked, with all sorts of dramatic consequences, including the man's flight beyond the boundaries of the state. This particular official was married to one of my cousins (on my mother's side), and his mother-in-law (my aunt) had, before any of this happened, gone through all my tapes and videos and cross-questioned my grandmother (her own mother) about what kind of work I was doing, who I was staying with during my eight months of fieldwork, and about my daughter, then seven years old. I'd have been perfectly happy to explain everything myself, but I wasn't asked to and no doubt I wouldn't have been believed if I had been given the chance to answer.

3

If one doesn't formulate one's research strategies thoughtfully, one can end up having a considerable impact on social situations. Care is essential over this.

For instance, I was working on an international project, 'Census 2002', and helping colleagues with interviewing work with Meskheta Turks in Krasnodar region. My mistake lay in carrying over material from one interview to another. One informant had confided in me that he was worried the census was being used to work out how many Turks there were and then resettle them. Not long after this interview, we took a trip down a local highway where the roadside traders included Turks. I asked one of them, was he worried about the likely consequences of the census? Of course I didn't mean that he should have been, simply that some other Turks were. My question provoked distress and bewilderment, and nothing I could do put the situation right. The man I'd spoken to obviously decided the question had some hidden agenda; I had access to privileged information, so I must know more than he did.

I think particular caution is necessary when one is doing gender-based studies in traditional patriarchal societies, all the more so if one happens to be a radical feminist. Anush Taulyan, an American colleague of mine in this situation, got into some dangerously confrontational situations because of this position. She arrived in Karabakh when the romantic-patriotic war was at its height. She took work as a nurse and spent around eight years there. She had numerous women friends and spent a lot of time with them; she tried to teach some of them to drive and to swim, using the local pond.

This was taken as a direct provocation, and the local male support network got going and started to put severe pressure on her. In the end, she had to get out and go home in an extremely dispirited state of mind.

I heard about this case just after I began my fieldwork, and knowing about it helped me to be very careful about my relations with men and women. But the way of life espoused by women anthropologists was seen as an expression of egalitarian politics in itself. At the time, a lot of local women were going through a surge of enthusiasm for feminist ideas, and they wanted to talk the talk, to sound liberal and emancipated. The fieriest of them sometimes got into arguments, into verbal fights and denunciations of patriarchal ways, though none of them had the means to live independently, and divorce, the likely result of their behaviour in the long run, would have been a complete disaster.

In this kind of situation, someone doing research can indeed act as a catalyst for situations and conversations that would not have come about in the ordinary way, and for new interpretations of the status quo. Life in general may not be affected, but human relationships certainly are — and, of course, in closed societies such as these, drawing the line between ‘relationships’ and ‘life’ is almost impossible (it’s never easy in any case). Conversations, judgements, rumours have a direct bearing on relationships and on the quality of life. Factors such as gender, class, symbolic/genealogical/economic/cultural capital, status (within the community at large, generational, etc. etc.), membership of or exclusion from certain groups are all significant.

The very presence of the researcher brings about a new situation in which he or she is cast in the role of spectator, made the target for little vignettes of self-representation that express ideas about status and social roles and niches.

Also, researchers themselves undergo a kind of ‘resocialisation’, shifts in their own world-view and identity. Anyone doing fieldwork is in a very vulnerable position, and extremely dependent on personal relations with the family that he or she is living with. At the same time, he or she has the capacity to write about their encounters, and here lies the way to power. Yet all that comes later on, in one’s own study. There’s no doubt that you do become a different person after fieldwork. In this sense, you become objectified, because you can’t control the process you’re going through. Although the trip itself and the decision to do research are of course a matter of personal choice.

5

I think that a corporative agreement on research ethics is essential for Russian anthropology, even though I don’t believe a document

of this kind would be effective in a practical sense, given the continuing prevalence of authoritarian practices in Russia. How far can one expect regulations on paper (or on virtual paper) to cut any ice when there were cases in the recent past of scholars taking groups of students into the field under police escort, and of the police telling the local population to co-operate (with questionnaire and interview work) or else? One also has to bear in mind the character of the Russian legal consciousness.

I think that the Russian tradition of anthropological work is unusual in international terms because of the lack of self-consciousness with which questions are formulated and asked (I'm criticising myself as much as anyone else in this). The fieldworker descends on a local population in much the same way that a visiting journalist might: both of them arrive out of the blue, collect a load of exotic (on the one hand) or scandalous (on the other) information, shake people up, rattle the even tenor of local life, and then... simply disappear. The mechanisms of protection and sanction are simply not evolved, most particularly if one's talking about the kind of video reporting that gets put out on central television.

The Meskhetian Turks in Sauk-Dere: a case study

I cite here a verbatim transcription of a statement by the leader of the 'Vatan' society, Aladdin Samsonidze (born 1942), recorded on 27 May 2002 in Sauk-Dere, Krasnodar region:

I have been a citizen of Russia since 1992, I got naturalised through the embassy in Uzbekistan. In one of my identity documents, it says I'm a Georgian, in another it says I'm Azerbaijani. I have a sort of little cafe in Bakanka, 'Dary kukhni' [Gifts of the Kitchen] is the name. Not long ago, I expanded the premises, I fixed up a roof over the terrace — and applied for permission, of course. I'd almost got it through, I just needed one more signature. But after I appeared on the 'Chelovek i zakon' [People and the Law] programme on 23 August 2001 — they did an interview with me — they decided to close my cafe down, they gave the order to do it. They suddenly worked out I was the leader of a Turkish group. All the documents are in to town planning, but they won't hand them over. I got this notice:

NOTICE

13 NOVEMBER 2001

YOU ARE REQUIRED TO DEMOLISH THE FOLLOWING CONSTRUCTION WHICH HAS BEEN ERECTED WITHOUT PLANNING PERMISSION: ANNEXE TO TRADING KIOSK. DEMOLITION WORK MUST BE COMPLETED BY: 12 DECEMBER 2002.

THE COMMITTEE ON THE CONTROL OF TOWN PLANNING HAS PASSED THE FOLLOWING DECISION: DEMOLITION OF TERRACE ROOF MEASURING 3.5 BY 4 METRES, SITE: THE 'DARY KUKHNI' PAVILION ON THE NIZHEBALKANSKAYA STATION MARKET PLACE (LICENCE NO. 698, 5 JUNE 2001).

SAMONIDZE, THE OWNER, HAS REFUSED TO DEMOLISH THE ROOF VOLUNTARILY. DEMOLITION WILL ACCORDINGLY BE CARRIED OUT BY THE CITY AUTHORITIES. SIGNED: V. N. RYBIN, MAYOR OF KRYMSK AND KRYMSK DISTRICT.'

On 28 August 2001, the so-called 'meeting of citizens and Cossacks' took place to discuss a recent fight in which someone had been stabbed. I was asked along. It was the Turks who were held to blame (surprise, surprise). Around 150 or 200 Cossacks had gathered, led by Bezgulyi [the Cossack ataman — N. Sh.] There were about 30 police present, as well as the mayor of the Moldovansk district, K. V. Kikhaev, Lieutenant-Colonel Fatikov from the police investigation department (he's a decent person, I must say), and a few other officials. Bezgulyi was saying the Turks were to blame for everything, I was trying to put our point of view. Then the Cossacks attacked me, and I had to run. Five of them came after me. Thank God, I was able to jump into a friend's car, I escaped by the skin of my teeth. I made a formal complaint to the public prosecutor of Krymsk district, N. P.. Selichev. But no witnesses came forward.

ALEXANDRA SIMANOVSKAYA

Not long ago when exploring the boundless resources of the Internet, I came across a page on the Russian Ethnographical Museum's filed trip to the Kola Peninsula in 2003–2004. It was illustrated with the author's own black-and-white photographs showing local scenery and members of the local population. Readers were invited to discuss these in an attached forum.

In some of the photographs, I could recognise people who had acted as informants to me when I was doing fieldwork in Krasnoshchelye (for instance, 'A hunter talking about bears' and 'Grandad', also known as 'The Theoretician').

I think these two individuals have worked as other people's key informants as well — one

could probably even describe them as ‘professional informants’. One worked with academics from the Komi Scientific Centre of the Academy of Sciences as long ago as the 1980s (his family tree was published in the resulting monograph), and the other collaborated with a British anthropologist five or six years ago (in this person’s book, he is described as ‘an eccentric homespun philosopher’). Both these informants mentioned their previous work with ethnographers to me, and the books that had resulted from the collaborations.

In the field diaries of the person from the Russian Ethnographical Museum mentioned above (the group they were with spent a total of four days in the village), the real names of all these people are used, including that of ‘The Wizard’. As it turns out, he is reckoned to be a ‘White Shaman’ or a ‘Healer’ by the local villagers (or that, at any rate, is the story here), while his former wife (a Saami) is known as one of the most dangerous sorceresses in the locality. She ‘puts the evil eye’¹ on people, while he ‘heals’ them from its ill effects. This contrast in roles was what led to their divorce. This information is followed by a collection of observations, stories, and fables gathered during the expedition, and also the contents of telephone conversation with the ‘black witch’. The author expresses surprise that the British anthropologist *failed to recognise her as the local specialist in black magic*, despite having visited the village four times and spent nearly a month living in the ‘healer’s’ house. But did he actually not notice, or did he decide that it would be best not to mention the situation?

To be honest, I’ve no idea how accurate this story about black magic may be. Some of the things set down in the diary are definitely inaccurate, so far as I can tell. But I am not concerned with that here; it is the ethical side of things that interests me.

Is it in order for a researcher to mention the name of an informant and then go on to make comments about his or her personal life, social status, and so on? Does the type of publication make a difference — whether this is an academic book or article, or on the other hand a text published on the internet, a comment in an on-line discussion?

I am convinced that if you don’t have the informant’s explicit permission or indeed request to cite personal details, you have to code all your references to him or her. Anything that might be considered private should be only be referred to if this is absolutely essential (for instance, there is no need to state where someone works or what their job is if this is of no significance to the discussion). And it should only happen if there is no chance that the information will do no damage to the informant.

¹ *portit*: literally, ‘to spoil’, ‘to commit maleficium’. [Editor].

An exception would be cases in which the full text of an interview or biography appeared. This should, of course, happen only with the consent of a given informant, perhaps even only with written permission (cf. the collection *Rasskazhi svoyu istoriyu* [Tell Me Your Life Story], based on a competition organised by the Nochlezhka [Night Shelter] charity and the newspaper *Na dne* [Lower Depths]).

There is no doubt that if someone from a remote village happens to read something in print about the place where he or she lives, they'll have no difficulty at all in recognising neighbours, no matter how their names may be disguised. There is no insurance against this. If a community is small-scale (a village or hamlet), it's standard practice for researchers to show the informants what they have written, to bring or send a copy of the resulting publication (it's up to him or her whether to present this in accessible form for the local public). If you happen to be doing fieldwork in somewhere the size of St Petersburg, the chances someone may be recognised are obviously lower, but the circle of potential readers from a given group is, on the other hand, much bigger, given the vastly greater opportunities to get hold of published research — anyone can visit a bookshop.

When it comes to publishing material in an online journal, or your dissertation abstract or field diary on your own website, the potential audience becomes absolutely vast. Anyone who spends a few seconds typing 'Komi-Izhemtsy' into a search engine will be able to locate your material. Your informants may well be able to access the information you've put out with the help of relations and friends. Will they like what you've said? Will they be prepared to help other researchers in the future?

AMRIT SRINIVASAN

Fieldwork Ethics: A Note from India

In colonial India, anthropologists predominantly studied their own society, making the already threatened, squeaky clean image of the fieldworker as a 'neutral' instrument of research, practically impossible to maintain after the Raj. Elsewhere, in the metropolitan centres of the discipline, the changed political realities of a post-colonial world had turned the 'subjectivity' of social research to ethical advantage. Fieldwork was re-aligned methodologically to permit a critical reflexivity and hermeneutic under-

standing of the Other. In India, on the other hand, a fledgling nationalism and the developmental demands of a poor country now both constrained and rewarded the fieldworker towards a study of the Self alone. Subjectivity as a research ethic was justified on the basis of pragmatic, even activist, not primarily theoretical considerations. The re-naming of Indian social anthropology as 'sociology' was one such disciplinary outcome [Srinivasan 1993]. 'Participation' with the field, though valorised, did not go so far as to include civil society's own intellectual traditions which were kept scrupulously apart as providing a mere 'book view' of Indian society, one which was predominantly metaphysical and scriptural, not scientific.

The broader developmental agenda of a newly independent nation, also worked against indigenous theory-making on at least two fronts. On the one hand, activists and NGO's condemned the academic study of local society as a purely 'ivory tower approach'. And on the other, Indian state officials with their imposed notions of 'progress' and 'modernisation' deemed traditional, rural or 'indigenous' knowledge paradigms as backward, because they involved the skills of the poor and the marginalised and not the highly regarded science and technology expert.

Not every fieldworker was able to withstand the pressure of these powerful universes of discourse and many conveniently confused an ethical commitment to the public, autonomous domain of the university with that of nation-building and the state. Things were complicated because a shared, reformist middle-class morality of change and progress informed both. The adoption of metropolitan theoretical paradigms became a function of the Indian university's pervasive political correctness, which could not bring it to challenge 'universalistic' modern science and the tremendous influence, social respect, and international state funds it garnered in a newly independent state.

Any discussion consequently of fieldwork ethics in India today must be presumed on the radical, democratic interrogation of the fieldworker's middle-class self. The need for transparency and accountability to the profession and not to a lurking nationalism, casteism or regionalism is both ethically and methodologically urgent. At present however there is no formal body regulating an examination of the self or even research conduct in the field. During their coursework, students are of course trained in obtaining voluntary, informed consent when dealing with the people they are studying. Given the stark inequalities of income, status, language and education in India, they are particularly guided on the principle of non-exploitation when dealing with the field. The giving of financial inducements of any kind for instance is prohibited for purposes of getting information of whatever nature.

The Right to Privacy however, the Indian fieldworker needs to be reminded, can never be the only ethical goal for social research in a society where people have been made ‘invisible’ and disempowered by the mainstream. Indeed, the demand from the people often is that ‘their story be told’! Researchers consequently have to deal with the very real issue of people in the field asking for ‘help’. The later publication or circulation of field material does of course have profound and beneficial effects for them. But in India, to be able to document differences without moral or class bias, in a society oppressed by official neglect and iniquity, is itself the single most radical contribution the fieldworker can make to the discipline and to society. The task is not easy, since, in the study of one’s own society, ‘difference’ takes on a political, ethical and ideological burden.

To seek to change oneself and not the people around one is the first step then in an ethical response to the cries for help encountered in the field situation. Indeed, professional interventions into complex social problems are possible to achieve only if the field researcher is prepared to *learn* from the field — truths which are uncomfortable and which challenge existing scholarly and social hierarchies. In the study of one’s own society, I have argued elsewhere, reflexivity and self transformation can ideally be encouraged to reach methodological parity. Malinowski’s *Diaries* and Gandhi’s *Autobiography*, or his ‘experiments with truth’, should both figure in the young fieldworkers reading list, to help illumine this possibility [Srinivasan 1993].

The field researcher’s own ‘right to privacy,’ personal morality, and code of ethics necessarily come in for a jolt when confronted by other ways of life and work and play. But the very criticality of this rite of passage helps produce value for the entire discipline, if it be allowed open expression and not camouflaged. This was brought home to me quite forcefully while studying the reform and revival movement surrounding the life and art of the devadasi-s of Tamil Nadu in South India.¹ My attempt then at ethnographic honesty was itself seen as a form of bias, because it displayed an unacceptable ethical ‘neutrality’ to modernity as a shared cultural value [Srinivasan 1984, 1985].

The religious, temple ‘caste’ context to the devadasi’s art and its explicit links with sexuality, made secular social scientists and fem-

¹ The devadasi-s were a class of artists who went through a pre-pubertal ceremony of dedication to the local temple deity before entering the discipline of the dance training. They were not allowed to marry but were expected to enter into public liaisons with the patron class. The children of these unions also had service rights in the temple work organisation, so long as they took their mother’s name and resided with her.

inists mistrust the clinical tone of my arguments, which did not fit her into a readymade frame of exploitation and/or victimisation. Traditionalists also rejected the inadequate attention paid in my work to what was for them a moral issue — the evolutionary emergence of temple ‘prostitution’ as a ‘social evil’, a degeneration of sacred worship brought about by the corrupt influence of Muslim ‘invasions’ on pure Hindu practices.

The polarisation of these campaigns along caste lines had already begun in the field, in the early Victorian context of colonial India and I was willy-nilly drawn into this fractious history. Even till today, the revivalist Brahmin lobby looks at me as an uncivilised, Northern ‘outsider’ who has neither the Sanskrit nor the dance skills to understand their contribution to saving the devadasi’s pure art. While the non-Brahmin reformist lobby (made up largely of the men of the devadasi community), look at me as an anti-modernist wanting to romanticise the ‘exploitation’ of their women at the hands of libidinous upper-caste men. Significantly, even academia appears hesitant to examine the implications of the devadasi household, as a matrifocal support structure for women artistes trained in a life, sex and work discipline, which is alternative to the conjugal.

Discussions on Indian Kinship and Marriage continue to ignore the professional devadasi household and include only the landed Nayar and/or Khasi tribal households, when reporting on historical Indian matriliney.¹ Why? Could it be the anthropologist’s own middle class morality and metropolitan training which prefers to examine the ‘otherness’ of matriliney in terms of dry, structural principles rather than the more threatening, sexual ethic which informs it culturally and historically? This appears to be the case in most of the post-colonial writings on Nayar matriliney, which attempt to coerce a rather unique local tradition for organising personal, sexual and property relations between men and women into the standard conceptual framework of ‘marriage’, more usual to modern kinship studies.

This standardisation of Nayar sexual codes has of course the laudable purpose of compensating for the earlier, colonial mistake made in the anthropological examination of so-called Nayar ‘promiscuity’. But in the process, no theoretical space has been made for an indigenuous understanding of Nayar sexuality which does not force it into opposed and mutually exclusive ‘wife’/‘whore’ moulds [Srinivasan 1996]. With the devadasi case, the methodological and ethical sensitivity required to understand Indian matriliney becomes even more

¹ The most recent evidence of this is [Uberoi 1994], which reprints Kathleen Gough’s celebrated article on *The Nayars and the Definition of Marriage* that first appeared thirty-five years earlier [Gough 1959].

difficult for the mainstream and so her example is simply ignored. In the process, a local regional example of social innovation in art and in life is lost to sociological scrutiny because of middle class prissiness and moral censure, more than anything else. The term ‘devadasi’, let us not forget, was forbidden to be even uttered in Kalakshetra, the first national dance Academy in Free India to revive her art as a modern amateur art form, Bharata Natyam.

Retrospectively, the need for a conceptual separation between the old and the new was that much greater because of the very real debt the new class of amateur performers owed the devadasi for actually learning the dance. In a system of oral transmission, Bharata Natyam could not have become a global art practice without the initial training provided to the revivalists by the men and women of the devadasi community, who had themselves stopped dancing after the notoriety gained in the Anti-Nautch agitation in the early twentieth century. But even today, it remains for an activist and feisty woman writer like Mahashweta Devi to highlight the exploitation and indifference female performers continue to face because of their stigmatised ‘matrilineal’ family traditions [Devi 2007]. These could be contemporary Bar dancers or women of the gypsy tribes, the Nats and Bedias, but their traditions of dance performance are being consistently demonised and censored and legislated against as ‘sex work’ and thereby lost to history. Yes, in the absence of traditional or modern patronage, many of the girls take to sex work to earn good money, but what is happening to the art in the process? Also, is it not possible to upgrade the women’s own skills as performers, rather than convert them into embroiderers or seamstresses as many NGO’s are doing?

I don’t think there can be any strategic ways out of dealing with complex, theory-related ethical dilemmas such as these. One just sticks to the need as a field worker to de-mystify social reality and to avoid the pitfalls and the pleasures both, of political-correctness. The comparative experimental truth of the devadasi ‘institution’ is in evidence everywhere today in its secular modern form, if we only turn our gaze inward. Middle class woman actors or ‘stars’ lead flagrantly unmarried, but not at all virginal lives. Upper-caste, elite women dance in temples for the promotion of state heritage tourism and prostitutes demand to be known as sex ‘workers’, not exotic victims. In such a scenario, the discerning will hopefully see the devadasi for what she was — a woman professional in the arts, permitted independence from conjugal, householding demands, while at the same time remaining protected by community, social security nets and household modes of dance training. Denying the underlying truth of the field because of one’s own alien metropolitan and moral training, has according to some, already ‘disabled’ the sociology of India and South Asia more generally:

A pre-emptive European conceptual framework of analysis has disabled us from probing central features of South Asian life, from pre-western forms of 'national' (or feminist, or communalist, or ethnic) identity or consciousness, pre-modern forms of cultural 'modernism', pre-colonial forms of colonialism [Pollock 1995: 115].

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ZINAIDA VASILYEVA

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I've often discussed the issues raised here with my colleagues in the corridors of the European University, and have come to the conclusion that few people have strongly defined ethical principles relating to fieldwork (I'm no exception to the rule). That's completely understandable, given the complexity and specificity of every concrete fieldwork situation/case/personal experience on the part of the researcher. But it also points to the topicality of this public discussion of fieldwork methods — there definitely is a problem at stake.

I think the most important thing about the present discussion is that it has problematised

research ethics in the context of Russian anthropology. I can foresee the objections of people with extensive fieldwork experience — ‘we sorted all that out for ourselves long ago’. Well, no doubt that’s fine if so. I am simply trying to focus attention on the fact that fieldwork inevitably becomes routinised and part of the everyday life of the anthropologist, but it still shouldn’t become unproblematic in ethical terms. It’s essential that every researcher (whether a novice or experienced) should reflect each time s/he goes into the field on how honest s/he is with regard to his or her informants, people who trustingly and for the most part unselfishly help with his or her work, and on how honest he or she is with reference to work colleagues.

Here let me insert a small autobiographical digression. When I was a first-year student in the History Faculty of St Petersburg State University, I found out for the first time that a scholar was a kind of surgeon who operates on reality. The more professional the scholar is, the more cynically he is likely to work — just as is true of doctors. I was, to be honest, completely shocked by this discovery, and particularly because historians, whose speciality is the logic behind the development of events, who became capable of prognosis, should not interfere in the natural sequence of things. (Though there are historians who think that passing moral judgements is precisely what they are supposed to do.) My naive idealism prompted me to think that the humanities, i.e. human sciences, were supposed to help people, or else why bother with them? A short time later, another truth (also obvious, when you think about it) also became clear — thirst for knowledge and information can turn into a passion or indeed a pathological condition. I thought hard then about what you can and may do satisfy your own curiosity or, to put it more accurately, ‘professional interests’. At that point, I came to the conclusion that ‘pure’ knowledge (knowledge for the sake of knowledge) is immoral. This conclusion was followed by a self-justifying argument: but I do this because I find it interesting. That seemed to be enough.

Not long after that, I started studying sociology at the Collège de France annexe attached to St Petersburg State University. Fieldwork in Smolensk was among the course requirements. My fieldwork partner and I studied the seminary and the theological college in Smolensk — the structure of these institutions and their place in the life of the city (the topic was formulated quite loosely, so we asked people ‘about everything’).

We interviewed both members of the teaching staff and pupils, at these events. The students at the theological college turned out to be especially good informants — open and ready to speak, sincere, well-disposed towards us. It was very clear that the girls saw us as

members of their own generation, but from a big city, living a different and very interesting life. They wanted to socialise with us and wanted to know what life was like back there, and to share the problems they had themselves mullied over for so long. They were extremely sincere, and they soon forgot about the Dictaphone we were holding and about the fact it was switched on. Though if they did remember this, they sometimes asked us to switch it off (particularly if they were telling us something about how they'd broken the rules of the College or Seminary and so on). So we did, and then they told us their secrets, or more accurately, the secrets of the College and the Seminary. In that way, we collected a huge amount of material about the informal life of the College — and, of course, we noted all these conversations in our field diary. I am dwelling on this point because material that hasn't been recorded on tape can perfectly well 'shine' in the field diary. Everything that was said 'in secret', without recourse to a Dictaphone, still remains in the researcher's memory, and whether the researcher uses it or not is up to his or her conscience.

Back then in Smolensk, I already realised that I couldn't use all this information, for writing a report or whatever. There would have been something quite improper and even treacherous about doing this. But at the same time, describing the 'underwater edge of the iceberg' was what I badly wanted to do. My fieldwork partner and I talked long and hard about all this and... ended up by reformulating the topic of our research so that we could refer as little as possible to the private revelations that our informants had passed on. This decision was to a large extent brought about by our youthful maximalism and inability to distance ourselves from the topic of study, and by the solidarity with our informants that we felt as a result. Yet at the same time, it really was our decision, the result of independent thought and of a conscious process of choice.

In my work on ancient cultures, I have caught myself feeling pleasure in the act of 'unmasking': for instance, if I find a mistake made by some author, the last thing I feel is 'sympathy'. What I feel when I am conducting an interview is something totally different, because I'm listening to real, living people discuss their lives, and hearing stories that make the narrators themselves laugh real laughter, cry real tears. Even if I do 'work over' their story in my head, I still feel sympathy for my informants, suffer with them. I see their lives behind what they are saying. Perhaps my attitude is unduly sentimental, even unscholarly, but I do think that it is the capacity for emotional experience linked with fieldwork which can justify 'invading' the informant's life. For me, anthropology reflects humanism at the highest level (because of its orientation to human beings, to individual personalities). In short, if one adopts the surgery meta-

phor, then one has to understand that anthropologists, unlike ancient historians, ‘cut into living flesh’ without using ‘anaesthetic’. Which brings important responsibilities, and the need precisely to observe that medical command, ‘do no harm’.

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In European and American scholarship, it is above all post-colonial experience that has made fieldwork an ethical issue. In D’Andrade’s view, the traumatic effects of this and the resulting refusal to see informants simply as ‘sources of knowledge’ brought about a root and branch transformation of anthropology: *‘anthropology [was] transformed from a discipline based on an objective model of the world to a discipline based on a moral model of the world’* [D’Andrade 1995: 399].

In Russian anthropology, this has not yet become a controversial issue, because Russian colonialism is still interpreted in the traditional way, as an ‘extension of borders’ rather than as a ‘conquest and enslavement of unfortunate natives’. In Russia, working is easy and pleasant because the objects of study/sources/informants are so passive. By and large, it hasn’t occurred to them that their rights (their human rights!) might be in danger, partly because it often hasn’t occurred to them that they have human rights in the first place. Even if someone does suddenly ask what’s going to happen to the information that’s being collected, a few confidently-uttered phrases, a sentence or two about anonymity and confidentiality, are generally enough to calm him or her down.

Yet the concept of anonymity is always relative. Let me recall a case from my own experience. I was working as an interviewer (rather than a researcher) for a certain project. Before I began the interview, I assured the informants, speaking in the name of the project, about guarantees of anonymity and so forth. However, not long afterwards, it emerged that an article published as part of the same project had included some biographical details about one of the informants that made it pretty clear who this person was. Of course it wasn’t anything as simple as a name. The biographical details given were limited to sex and birth date, in the usual way, but in the body of the article, the company the person worked for (a large and famous one!) was specified, as was their occupation (which happened to be rather an unusual one, thus making identification all the more likely). It was also easy to work out from the discussion exactly how much this person earned. Thus, anonymity was preserved in a formal sense only; working out the informant’s identity was childishly simple. Even though I personally wasn’t directly responsible for the publication, it made me uneasy that my own assurances of anonymity, albeit made on behalf of the group, had been undermined. When you interview someone, it’s a personal business. The interviewer relates to the informant like one

human being to another; I felt as guilty as though I'd broken the promise myself.

I think concepts from political philosophy — the ethics of legality, the ethics of morality — are helpful in discussing the topic of research in the field.

The ethics of legality assumes, to begin with, the development of a corpus of clearly formulated laws (prescriptions) that are interpreted in one particular way. It also declares equality before these laws and makes submission to them compulsory. Thus, on the one hand, the ethics of legality maximally simplifies the task of adhering to social order, making it possible to identify the right and the wrong quickly and expeditiously; on the other hand, it is inhuman and inflexible, since it cannot by definition take into consideration the complexities of a given situation.

The ethics of morality (the ethics of virtue), on the other hand, is based on the drive to understand each concrete situation, to enter into the position of every personality and to take decisions only with reference to the entire sum of relevant conditions.

In Russia (including in scholarly practice), precedence is given to the second type of ethics. Yet, however superficially attractive the second type of approach may seem, it at the same time gives rise to a host of different possible interpretations, dependent on which presuppositions we consider most important. In other words, instead of a 'flexible' or 'understanding' type of legislation, we end up with *zakon, chto dyshlo*,¹ or to put it differently, *You mustn't... but if you really want to...*

So can one formulate ethical principles for fieldwork and should one do it? I think that everything relating to the sphere of morality (including ethics) is hard to formulate. It's easy to formulate requirements such as no concealed recording, but much of what comes up in fieldwork is a lot less simple. For instance, what do you do if an informant 'got carried away' and said more than they wanted to into the tape, and then got upset about it? (This situation is perfectly possible, by the way, even if the informant has signed on the dotted line and said they're happy for you to publish material from the interview.) What's more, young informants close to you in age are quite likely to trust you and talk to you as though you were a friend, a confidant, someone you can pour your heart out to. Something like a confession can be the result. Should we keep the secrets we are told? A simple set of rules is not much use

¹ *Zakon, chto dyshlo* — *kuda povernesh, tuda i vyshlo*: saying about the permissiveness of the apparently repressive. A *dyshlo* is a single shaft, such as is used to hitch two horses, one on either side; it is much easier to break away from than the standard double-shaft system. [Editor].

in answering questions like this. A working anthropologist is first or foremost part of human life, fortuitously enacting the role of a professional. And just as he or she takes decisions in life and (in the ideal world at least) is responsible for these, so in his or her professional existence — as a fieldworker and a researcher generally — he or she is responsible for every action, whether this is in the process of interviewing or what gets published. In every situation, every researcher makes his or her moral choices and takes responsibility for these.

None the less, in my view there is some point to corporate agreements. Let me cite an example from the world of archaeology: the work of so-called ‘black archaeologists’.¹ These are people who carry out excavations without official permission, and who usually (there are exceptions) ride roughshod over professional methods, in other words, who work in an uncivilised way, with no heed at all to preserving a site as an integrated cultural complex. The purpose of such excavation is the discovery of valuable objects — to put it crudely, financial gain. Let me emphasise that not every ‘black archaeologist’ is a treasure-hunter. Many ‘cut their teeth’ during official expeditions, and some even have specialist training.

Scholarly archaeologists have a mixed view of ‘black archaeologists’. Some people simply consider them vandals, others acknowledge that their professionalism is rising, and a third group even tries to co-operate with them (for instance, in the Russian south, where there are so many sites that if you ignored these people, many discoveries would completely slide from view — not only would objects not end up in museums, but the fact of discovery would remain secret).

The hostility of most archaeologists to these people in any case makes little difference, since controlling unofficial excavations is extremely difficult, and the number of valuable, attractive objects hidden in the earth’s recesses remains high.

In contradistinction to archaeology, the materials of anthropology are hardly ever worth a fortune, and so ‘black anthropology’ hasn’t yet emerged as a concept. At the same time, there are ‘impure’ methods in use that have similar effects, and the laying down of rules could act as a way of setting up boundaries between legitimate/ethical and illegitimate anthropology. Under pressure from the scholarly establishment, ‘delinquents’ could be placed outside the law. I don’t say this because I want to divide researchers into ‘sheep’ and ‘goats’, ‘honest’ and ‘dishonest’ practitioners. What I have in mind is something else. Up to a point, issues of research

¹ In the sense of tomb-robbers, pirates, those working for sheer material gain. [Editor].

ethics are also methodological issues, and I simply want to emphasise the role of the scientific and scholarly community in defining what constitute correct and incorrect methods of working. If our fellow scholars reach some kind of agreement about research methods (by the term 'methods' I also have in mind the issue of ethical versus unethical methods), and put together a corporate agreement on this central question, then it will, I think be possible to reach an agreement that the behaviour of this or the other researcher is frankly unprofessional.

VIKTOR VORONKOV

1

In my view, the most significant ethical problem in sociological study is the relationship between researchers and informants. A sociology of understanding has a humanist character, which means that the informant is understood as an equal partner, and not simply as a source of information. The researcher ought to follow the same principle as the doctor: 'Do no harm!' If the results of a study, and most particularly of publishing the given materials, might bring about the slightest harm to informants, the sociologists (here I am referring to specialists across a wide range of social sciences, including, of course, anthropology) must keep their sources secret, and sometimes even (no matter how regrettable this seems) refrain from publishing certain data.

2

The sociologist always (let me emphasise!) approaches a given academic study precisely in his or her academic role, as sociologist. Let me recall here the story of how a certain US right-wing organisation was studied. A young researcher (and usually it is precisely young researchers who do the best work, since established academics have no time to spend on fieldwork and prefer to work in their own studies) had got a grant for a project and, having completed the first leg, gave a brilliant talk. His listeners were amazed by how much he'd managed to learn about a subject that was usually a closed book to researchers. The speaker explained that he'd secured his entrée to the field

by pretending that he shared the views of the organisation that he was researching and wanted to become ‘one of them’. The professional community reacted by cutting off the rest of his grant funding. His breach of ethical codes was not left unpunished.

I don’t think this is simply a moral problem. Of course, deception is reprehensible at the best of times. And breaking the rules of any professional organisation can bring unpleasant consequence too. But even if one sticks to pragmatic arguments, there’s the obvious fact that ‘secrets will out’. It’s hard to imagine that an intelligent person who knows that some aspect of their work is founded on a lie will manage to keep up a front: at some point or another, the facade is bound to collapse. At which point they can say goodbye to months of effort gaining entry to the field and cementing friendships with informants. The whole intricate house of cards will collapse in minutes. Not only that, the field will also become inaccessible for other researchers, since people’s trust will be breached once and for all. Alongside this set of considerations, let me also mention that any researcher has a certain conflict of roles: on the one hand, he or she is an observer, but on the other hand, he or she is also a human being. The only solution to the problem is for him or her to be honest with other people about what they are doing.

Which ‘fieldwork legend’ a researcher uses when entering the field is a separate issue. I personally think one should be honest about this too, while keeping the ‘legend’ as simple and understandable as possible. The most suitable explanation in this case is probably ‘I’m writing a dissertation (a coursework essay, a book)’ — after all, it’s usually perfectly true. In any case, the precise objectives of the study are not generally of much interest to the people one approaches. They usually work on the principle, ‘if the person seems a good thing, the study they write probably will be too’.

Honesty will also suggest all the solutions to the various other problems that may come up. I myself think that the whole question about whether you may tape informants without their knowledge is simply unethical. Of course you mustn’t. Just as you mustn’t read other people’s letters in ordinary life. I usually explain the presence of my Dictaphone (if I really need it in the first place) as a kind of substitute notebook, meant entirely for my personal use. This allows me to talk normally without worrying about how the recording is going. In fact, ethical questions aside, I try to avoid using technology wherever possible, given that the whole interview situation is an unfamiliar one for informants anyway, which brings about inescapable changes in the character of communication.

3

It would be naive to imagine that a researcher can avoid having an impact on the lives of the people he or she observes. Given that

the main method used in fieldwork is participant observation, the researcher has to adopt a role of some kind in the community (or organisation, etc.) that he or she is studying. Interaction is inevitable. In order to understand someone else (especially if they are at a 'cultural distance'), you have to live for a while alongside your informants, in a sense to become one of them, to re-socialise oneself. Yes, we can influence people, we can become 'biographical assistants' to some of our informants. But I actually think that the plain fact of involuntarily interfering in other people's lives can be to the good. Both for the researcher, who not only collects information, but also learns to understand (!) this from the point of view of the informants; and for the community, which the sociologist can help in numerous different ways, for instance as an expert (someone seen by many to be the vessel of 'sacral' knowledge) who contributes to solving problems of different kinds. But there is a difference between the role of a researcher and the role of a missionary or a social work. A researcher's help has a situational character, but without his or her trying to influence informants' value system and way of life.

I don't think that you can carry out an in-depth study, and sink yourself in the everyday life of a community, without engaging in participant observation. These days, researchers tend, if time and money are limited (or the motive of 'not influencing' informants is held to be primary), to restrict work to interviews according to a 'guide' prepared in advance (I personally, I must say, feel rather sceptical about interviews, feeling they are, for the most part, a coercive and interventionist way of collecting research materials). All in all, attempts to become an impersonal machine for collecting information and to preserve one's professional 'virginity' without doubt devalue the results of one's research.

As for participation in 'immoral' activities, then sociologists are straight moral cynics. After all, their task is to explain the rules that people live by and not to evaluate what they do on a scale of 'right and wrong'. By becoming part of a given community, sociologists internalise its rules (or if they want to achieve the result they have in mind, at any rate!) They eat and drink what their informants do, they behave so far as they can like their informants do. If a sociologist happens to be studying a criminal group, then his or her moral perceptions as an upright citizen socialised in a certain way often come into conflict with the norms of behaviour in that particular group. There are no universal recipes for success here, but I think that professional ethics ought to take precedence over civic commitment. If you find that a crime is being planned, do you go running to the police?¹ If so, then I think you should change

¹ Literally, 'to the right place', a standard euphemism for the secret police. [Editor].

your profession. (Though in fact the moral conflicts that arise in this sort of situation really need to be discussed with reference to concrete examples, for which I don't have space here.)

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We at the Centre make a practice of anonymising informants, though sometimes doing this can be very difficult — for instance, when some of them are well-known people, or when the object of a case study is the only institution of a given type in a town or city. It is important to understand that in many cases — especially if an academic study has some kind of broader social resonance — readers will rush to identify the suppliers of hidden facts or incriminating details. Since the Centre of Independent Sociological Research was set up, a couple of situations have arisen when informants were identified despite all our cunning attempts to obfuscate who they were, which led to a fair number of difficulties. As a result, we are now very aware of this issue and do our best to think round it.

It's fair to say that fieldworkers suffer constant moral dilemmas. When we note down the conversations and situations that we have observed, we feel a sense of treachery. Our informants would prefer, after all, that what they said remained private (imagine noting down and publishing things your relations and close friends had said). The Austrian sociologist Roland Girtler draws our attention to the fact that *'a sense of guilt, treachery, and dishonesty unavoidably haunts the sociologist, despite all the pseudonyms and paraphrases one uses to avoid the possibility of identification. The researcher does everything in his or her power not to "make hostages of" his or her informants'* [Girtler 2001: 174].

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Of course I don't think researchers' work should be tied up in red tape in the way it is in the US, for instance. But I do think that the general framework of professional activity should be subject to discussion and regulation. A community can function well according to rules if all its members are in agreement with these. An important part of this collection of rules should relate to ethical norms and sanctions when these are breached. If you want to be a member of a professional association, then please be sure you stick to the agreed rules.

A few years ago the St Petersburg Association of Sociologists put together its own ethical code fixing professional standards and setting down penalties for the breach of the agreed norms. In particular, the rights of informants are stated and offered protection. There is also a special ethical commission. (True, during all this time, there has never been a single *official* approach to the commission, although plenty of misdemeanours have come to light — plagiarism, for instance, is running riot. But why people don't contact the commission is a separate issue, relating to the peculiar relationship between (post)-Soviet citizens and the law.) The dis-

ciplinary elements in professional codes should, when all is said and done, exercise their role. This will come about gradually, as professional associations succeed in enhancing the prestige and social significance of those who belong to them.

I would like to finish these brief remarks (a good many ethical problems aren't even mentioned in the discussion here — the whole issue of 'leaving the field', for instance) by noting the following. An investigator's own conscience is his or her best guide. A well-motivated sociologist tries to sink as deeply as possible into informants' lives. There is no 'invention' of results here. Sociologists like this base their attitudes on those of equally open-minded and well-motivated colleagues. Of course there are lots of chancers and charlatans in the academic world, and people like this always try to get round professional rules. But the 'disrupters of convention' are easy to exclude. Since social prestige is very important for colleagues, researchers of society try to stick to professional codes — to do otherwise would mean sacrificing the professional authority they had made such efforts to achieve.

Reference

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EDITOR'S AFTERWORD

The questionnaires that we sent to participants from Russia and the former Soviet Union, and those sent to participants from elsewhere were different; so, too, not unexpectedly, were the answers. In many countries, including the UK and the USA, but by no means only there, it is now impossible to carry out 'research with human subjects' — a broad-ranging term that encompasses biological, medical, and social sciences, and also some areas of the humanities — without engaging with ethical issues, whether willingly or not. All our participants reflected this situation, expressing their own experience of the sometimes frustrating situation of catering to the often abstract demands of committees, while still doing work they consider challenging and necessary. In Russia, on the other hand, regulation by compulsory codes is still very much a development for the future. By no means everyone professionally involved in fieldwork takes a direct interest in ethical issues. Indeed, many regard such issues with total indifference, feeling that they are not terribly significant or even that the problems raised are purely imaginary.

The lack of sensitivity to ethical conflicts that one finds among many Russian specialists is both striking and potentially problematic, and so it is not surprising to find this point reflected on in several answers. As Sergei Abashin argues, a *'naively Romantic view'* still persists in Russia, according to which *nauka* (science and scholarship) is an activity of absolute benefit. This applies not only to, say, medicine, but also to anthropology and ethnography, which *'is held to embody a purely humanitarian mission'*. Very few Russian scholars have sympathy for the arguments of, for example, Michel Foucault: *'The link of scholarly knowledge with repression, as agonised over in many works by European and American anthropologists, strikes most Russian anthropologists as either distorted and inflated out of all proportion, or simply a fiction to begin with.'* As a result, ethical problems are either overlooked, or kept silent: no public discussion of issues such as informant confidentiality ever takes place.

Zhanna Kormina has some thoughts about the reasons behind this 'ethical indifference', as one might term it. In her view, *'Russian anthropology (ethnography) [...] has never been through a phase of decolonisation accompanied by recognition that making use of someone else's culture requires a sense of responsibility. For us, the "field" never seemed any distance away.'* In other words, when one is working 'at home', one does not see oneself as an outsider, and hence does not see one's own presence as intrusive. As Kormina goes on to argue, *'we simply assume that we have a full right to the cultural knowledge of our informants since this is part of "national heritage", and the fact that they know more about x, y and z than we do is simply an accident. So far as 'ethics' are recognised at all, it's in the terms of everyday morality: we've all known since we were small children how you were supposed to behave out visiting. That's usually seen as enough.'* Kormina's argument that views of this kind are widespread is in fact borne out by the comments from other participants in the discussion (see e.g. the comments of Larisa Pavlinskaya, who sees the ethical dimension of fieldwork as lying purely in the dimension of common sense, the avoidance of obviously rude and inappropriate behaviour).

Yet the fieldwork situation can hardly be described as *'like any social interaction'*, above all not from the ethical point of view. After all, few of us would be prepared simply to wade into someone else's life just like that, with no introduction or apparent purpose, in the ordinary way. However, just this is the norm in fieldwork. At the same time, this 'norm' brings many hidden complications. Visiting anthropologists put themselves, and their informants, in a very delicate situation. They become empowered, yet they also empower others and submit to others' power; they are the oppressors, but also the oppressed. As Nona Shahnazarian writes, fieldworkers are often ascribed levels of power

and roles that they may not hold (newspaper reporters, government inspectors, spies, or all three at the same time). In India, where the social situation is analogous to that obtaining in Russia, researchers may find people negotiating themselves into a dependent relationship, as Amrit Srinivasan points out. Yet anthropologists can also become completely dependent on their informants: Zhanna Kormina sets out a striking portrait of this situation.

It would be bad enough if things stopped at the inevitable complications of the ethnographer/informant relationship. In fact, as Olga Boitsova argues, *‘the problem is that anthropological investigations are unethical from the very outset. The ethnographical method itself constitutes a breach of moral norms. After all, can one call it “ethical” to spend eight hours a day participating in the life of a community and then spend two hours writing up every word and every event in one’s field diary, the very existence of which stays hidden from other members of the community?’* Sergei Abashin argues along similar lines, seeing the lack of reflection on ethical conflicts among Russian ethnographers as a kind of defensive reaction, a denial of the necessarily unethical character of ethnographical work.

It is certainly right to assert that the methodology of fieldwork was laid down at a period when academic knowledge was seen as enjoying absolute authority, and when informants were treated as the impersonal bearers of the information essential to the production of this knowledge. In Russia and in post-Soviet space generally, this situation has not altered very much. As Elena Boryak argues, one should look for this situation in the Soviet past, and particularly in attitudes to the duty on the part of the ‘masses’ to provide information where this was required: *‘Ordinary people were quite “malleable”; they had become prepared, over the years, to answer any “incomer’s” questions about whatever subject, wherever and whenever it was asked. This sense of defencelessness, complete openness, the absence of any barriers (ethical above all) on the way to acquiring desired information generated a situation where the informant’s “privacy” was a non-issue.’* She identifies a ‘consumerist’ attitude on the part of information-gatherers, which completely excluded the potential for concern for those who provided information: *‘The principle of defending the informant, of avoiding placing him or her in a degrading situation, or still worse, a dependent one, simply did not get raised.’* In other words, the fact that fieldwork ethics is a ‘non-subject’ in today’s Russia can be seen as a product not just of professional hubris about the necessary benefits of science, but also of a culture where there was a strong link between the circulation of information and entitlement. People in socially subordinate positions were supposed to provide information to order; they were not supposed to ask what would be done with this information, and did not have the right to see what had happened to it

once it had been appropriated by those with social capital, whether these were census researchers, scientific investigators, or scholars carrying out fieldwork.

For all that, the situation in the world of Russian anthropology is changing, in a process perhaps linked with the growing concern for personal privacy that is reflected in the regulations governing, say, access to archival documents.¹ The very fact that ethical discussions are beginning to take place is some indication of this. Different participants see the reasons for the emergence of such discussions in different ways. Mariya Akhmetova identifies several: one of them is of a general kind, related to the growing interest in human rights in today's society; two others are bound up with the concern, in contemporary social sciences, with modern traditions and with the study of urban subcultures. *'This appears to presuppose that the researcher to some extent identifies with those he or she is studying; it's simpler that way, is the idea, because informants can start thinking they'd like to see the academic analysis when it's finished. After all, they're part of it as well.'* In other words, once informants also become part of the 'knowledge economy', their needs and concerns become more 'visible' as far as researchers are concerned.

Elena Boryak also sees the growth of interest in ethical problems as lying in the fact that *'the traditional "field" [...] has altered considerably in recent years. Society is more open now, information circulates more freely, knowledge is more accessible and "niche audiences" have proliferated. The potential respondent is therefore more thoughtful, more demanding, more attentive. Researchers have also changed; their mentality is quite different these days. Concepts such as lyudnost (as used by Fedor Vovk) the 'people', the 'masses', the 'population', all referring to an undifferentiated object of research (cf. the questionnaire that Lyudmila Shevchenko used in the 1950s) have now vanished for good and all into the past'* Yet recognition of the changes that have come about should not prevent us from perceiving that, as Aleksandra Britsina rightly argues, *'totalitarian stereotypes have been rather slow to vanish from the consciousness of researchers'*. Irina Razumova sees the basic reason why ethics has become topical as lying in the capacity of the scholarly community for reflection and self-analysis; it follows that, in cases where this capacity is little developed, concern for ethics will also be atrophied.

¹ For instance, it is now common for repositories to forbid the consultation of documents in which *personalii*, 'personal details', are set out. This can of course function as an instrument of research censorship on political grounds, but there are cases, such as the restriction on access to personal medical records under the rule about 'secrets of personal life' (*tainy lichnoi zhizni*), where privacy concerns seem more obvious.

In countries where there is already an established tradition of coming to grips with ethical problems, not recognising the rights of informants is hardly a possible course of behaviour. US universities have their Institutional Review Boards and Offices of Research Ethics, which require researchers to take mandatory cognisance of ethical codes; British universities are now instituting similar procedures, and it is routine for grant awarding bodies, such as the government-funded Economic and Social Research Council (ESRC) and Arts and Humanities Research Council (AHRC) to require applicants to provide a link to their institution's Code of Practice and to state precisely how they propose to comply with this.

At the same time, as one would expect, attitudes to the work of regulatory boards are far from straightforward. As Donald Raleigh describes, it is often not enough simply to provide a very detailed project description, incorporating information about procedures and recruitment strategies, and about how informants' rights to consent and confidentiality will be observed. The person doing fieldwork in a modern Western university may well also find him- or herself required to undergo a process of training, which may involve the provision of yet more detailed information about the procedures and strategies that are to be used. All of this takes a great deal of time, and yet, as all our participants (and also Jane Zavisca in her associated article) make clear, often the requirements set out, and advice provided, seem rather distant from the 'live' situation to which project organisers are exposed. Suffice it to say that many of our participants are working in cultures where being required to sign a consent form generates perplexity and even panic. A stranger who foists himself on someone else is already a dubious figure; a stranger who acts like a bureaucrat is still less likely to inspire warmth. Indeed, as Raleigh points out, the whole area of '*cross-cultural interviewing*' is non-existent so far as codes are concerned. Yet, on balance, he articulates feelings about ethical regulation that are precisely mixed, introducing his comments with a reluctant expression of commitment to the process that he underwent, flawed though it often was. *'After weighing the alternatives and familiarising myself more deeply with the larger national debate that provides the context for my own experience, I must conclude that, until something better is in place, mandatory codes of ethics training/review strike me as necessary.'*

To judge by the responses we received, these mixed feelings are widely held. Most of our participants appreciate precisely the stimulus to self-conscious reflection provoked by the process of external scrutiny, at the same time as expressing annoyance at this. As Bruce

Grant puts it, *'For all of the frustrations and extra paperwork involved in the dreaded IRB, their existence reminds all of us that even the most innocent seeming projects can have extended consequences.'* And further: *'Institutional review committees may be a burden to some, but their virtues far outweigh the vices, as the saying goes, vices that many resort to when we momentarily forget that we live in a world where all can read what we write.'* Brian Schwegler voices much the same idea: *'Professional and regulatory codes of ethics can play a positive role in research. By offering researchers an opportunity to reflect on the risks and benefits of participation, ethics review processes can strengthen research design and relationships among researchers and research participants.'*

Marcus Banks, writing from a British university, where ethical codes are a relative novelty in the social and human sciences, comes to similar overall conclusions about the function of codes of ethics. While expressing significant reservations himself, he admits that *'other social anthropologists — and particularly students — have said that they find the process useful.'* He also sees the reason for this in the stimulus to self-reflection. *'While there are rarely any clear-cut answers to questions posed by code-based ethics systems, considering the questions allows researchers to focus their attention on ethical matters and to consider in advance the kinds of situations they might encounter.'* But he also points to two significant problems. The first lies in the *'reactive rather than proactive'* nature of the codes, constantly redrafted in response to *'periodic episodes in which an anthropologist or group of anthropologists is accused of unethical behaviour'*. The second is the *'jural'* character of the codes, which is fundamentally inimical to the whole character of anthropological research. *'abiding by a code in one's professional life — particularly one primarily designed to forestall legal action — serves to dehumanise not only the anthropologist but also her research subjects. Of course, much the same would be true for many other disciplines, but the nature of ethnographic research means that it is far more difficult for a social anthropologist to identify precisely which activities in the field count as "research" (and are therefore liable to the strictures of an ethics code) and which do not.'* The model for codes of ethics has been those obtaining in medical sciences, yet the practices characterising say anthropological fieldwork are very different. In contradistinction to the potentially extremely invasive nature of medical research, the actual process of research in anthropology does not carry a high risk. *'It is the subsequent publication and dissemination of this material that carries moral and ethical implications. Publications (including films and photographs [...]) may bring hurt or distress to individuals, but more generally an entire corpus of work, such as anthropological writings on 'primitive society' in the late nineteenth century, may bring hurt and distress to untold millions as a result of stereotypes*

that become established.¹ The whole nature of ‘consent’ is made problematic by the fact that, as Catriona Kelly points out, the motives which inspire informants to share material are unlikely to correspond exactly with those that govern the researcher. ‘*An individual’s treasured memories will necessarily need to be placed in a general analytical framework, or, to put it more simply, taken out of context. Someone’s key life experience of narrowly survived starvation during the War becomes “war survival story no. 49”. As anyone who has ever talked to a journalist will know, seeing one’s own words placed in someone else’s controlling narrative, given an alien “spin”, invariably creates dissatisfaction in those whose words are cited. Sometimes, just seeing off-the-cuff comments reproduced in print is enough to produce a sense of alienation. Also, informants may not necessarily like the company they are forced to keep.*’²

The gulf between the principles encoded in regulations and the real problems that occur at different phases of work with informants not only provokes scepticism in Western researchers with reference to the codes themselves, but also suspicion that the ‘*jural*’ nature of codes is derived less from a concern for informants than from ‘*a concern to protect universities against litigation*’ (Catriona Kelly). In Russia, this situation (or even suspicions of this kind) are still a long way off, though, as Sergei Abashin points out, the predominance of ‘*vertical ties*’ over ‘*horizontal ties*’ in the operation of the academic world generally would suggest that the dangers that codes might do anything other than protect informants are significant. Not surprisingly, then, the question of whether Russian academy should follow what is being done in other countries (the creation of ethical codes, regulatory bodies and so on), or whether its members should look for another approach, proves controversial.

Different participants in the discussion, as always, offer varying views, but scepticism with regard to regulation is definitely the dominant attitude. Elza-Bair Guchinova and Elena Boryak argue

¹ A striking example from the Russian context would relate to the responsibility of ethnographers producing studies of the ethnic minorities of the North and Siberia for foisting on such peoples their ideas about a ‘golden age’ of traditional culture lying in the past. As V.A. Turaev has described, ‘*I am certain it is we, the ethnographers, who were primarily responsible for the tragedy that overtook the minority populations of the Russian north in the 1990s. We spent a decade on wretched efforts to “resurrect” the local culture, which everywhere was interpreted as demanding a return to the ethnographical norm we loved so dearly — traditional culture. Today everyone realises that the “resurrection” never took place. And the problem didn’t lie in the fact that the government lacked the political will to make it happen, but in the fact that you can’t resurrect the past — any more than you can step twice into the same river.*’ (See *Antropologicheskii forum. Spetsial’nyi vypusk k VI Kongressu antropologov i etnologov Rossii*. P.152).

² Similar points are made by Olga Boitsova, who states in her contribution that inviting informants along to her talks has usually ended rather badly: ‘*My informants didn’t agree with what they’d heard me say in my talks and they took umbrage at the generalisations I was making. In their lives (their own lives, their personal lives, lives unlike any others!), I was seeing overall social phenomena, and I was seeking for something of general significance in what they considered unique.*’

that professional agreements on research ethics are useful and necessary, but on the other hand have doubts about how effective they are likely to be. *'None the less, even if such agreements only have symbolic value, they do provide points of orientation for young researchers and define the boundaries of what is permissible in ethical terms; they are a step on the way to the formation of an intellectual community of anthropologists in the Russian Federation'* (Elza-Bair Guchinova).

The outright sceptics have weighty arguments on their side. Sergei Abashin sets them out fairly fully. Added to the points about the insufficiency of codes that are raised by some of our contributors from outside 'post-Soviet space' as well, and to the argument about 'vertical ties' that has already been mentioned, he comments on the fact that self-regulation by anthropologists would have to take place in a kind of socio-cultural vacuum. *'It isn't even the anthropological community that has to be persuaded by ethical codes. Documents of this kind must be the result of a meaningful dialogue between ethnographers and "civil society" (human rights organisations, different types of public association, journalists, legal professionals, and so on). But I can't at the moment see any great public interest in research ethics, nor any social institutions that might generate it — more's the pity.'*

One has to agree with this rather depressing summary. As our contributors from the US and Britain make clear, the formation of ethical codes has been part of a huge process of social transformation, embracing attempts to make cultural institutions and professionals generally more 'accountable', and relating also to the 'audit culture' that seeks to establish that public money has been expended in ways that would be endorsed by the fiscal community that, in a final sense, supplies it. With political leaders and the business community in post-Soviet countries not 'accountable' in any real sense, it is hardly surprising that the general public is really not bothered what academics get up to. But as a matter of fact, Sergei Abashin himself offers an impressively rational suggestion for a way out of the impasse. This does not lie in *'tightening control over the work done by researchers (in any case, control of this kind will never be effective), but in making the collection of information the subject of analysis and self-reflection, so that it becomes the norm in the professional community to hold open and frank discussions of this side of our work, and so that training in fieldwork skills begins with induction in ethical issues.'* Some other participants in the discussion express similar views (e.g. Alexandra Brytsina). And it should also be said that our Western participants are precisely in favour of this sort of discussion and information exchange; what bothers them, too, is the association of ethical regulation with the emergence of new

‘vertical ties’: the entrenchment of the power of administrators, and the imposition as canonical of research measures and standards that are conceptually as well as empirically suspect. One might cite here the points made by Brian Schwegler about official representations of consent as ‘an action to be done to research participants’, or alternatively as ‘a gift’. For officials regulating fieldwork, as he points out, *‘social relationships remained external to the process of informed consent, with the exception of a nebulous conception of trust in the research enterprise’* — in other words, anthropologists were being required to ‘sign up’ to a concept of social interaction that was, from an anthropological perspective, profoundly naive.

Space has precluded giving a resume of the detailed issues raised by our discussants, important as these also are — for instance, their views on anonymity for informants, the use of technology, the right techniques for working with informants, and so on. There are very varied opinions expressed on all these topics — indeed, some participants flatly contradict the views offered by others. The answers also make clear that the questionnaire was far from exhaustive in terms of the topics that it raised (see e.g. the comments of Viktor Voronkov, Vladimir Ilyin, and others).

What emerged from the discussion was certainly not a prescription for ‘how to deal with ethical issues’; the non-Russian participants did not set out ready-made solutions to the difficulties raised by encounters with ‘human subjects’ in the field. At the same time, a consensus did emerge that ethical issues are of great importance, and that self-scrutiny by scholars when it comes to work with informants is vital, even if this does not have to be, and perhaps should not have to be, set out explicitly in codes imposed by fiat ‘from above’. While not all our Russian participants subscribed to this consensus, some did, and the division, interestingly enough, appeared to lie along generational lines. The younger Russian participants in the discussion displayed a great deal more sensitivity to the issues of ethics we raised than the older ones did. In the words of Zinaida Vasilyeva, *‘the practice of fieldwork inevitably becomes routinised with time, an everyday practice among anthropologists and ethnographers, but that does not mean that it should also become unproblematic in an ethical sense. It is vital that all researchers, whether experienced or just beginning, should reflect every time they set out on the extent to which they are able to treat their informants honestly — these people who have trustingly and usually unselfishly helped with their work — and also on their levels of honesty vis-à-vis their professional colleagues.’*

This is, perhaps, a hopeful sign for the future: maybe Russian anthropology is now reaching a point when it may jettison the tradition of indifference towards those on whom it depends for its very

existence. And perhaps, when this happens, some of the dead ends and over-regulation that have bedevilled the introduction of ethical codes in the West will prove avoidable.

Whichever way, our thanks go to all involved for their contribution to the discussion.

Albert Baiburin

Translated by Catriona Kelly