The Research Object and the Subjectivity of the Researcher

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The Research Object and the Subjectivity of the Researcher

FROM THE EDITORIAL BOARD[1]

The **'Forum'** is central to our journal, and its purpose is to facilitate the exchange of ideas between members of different disciplines: anthropologists, historians, folklorists, linguists, and others. We publish discussions here relating to current topics of central interest in the humanities and social sciences.

The issue raised in our questionnaire, of how 'the research object and the subjectivity of the researcher' may inter-relate, is, one may suppose, as old as science and scholarship themselves, touching as it does on the very fundamentals of these as knowledge and practice. The essence of the problem has long been known: we can only introduce those fragments of reality that we observe into analytical discussion if we translate them into the symbolic language that is current in a given discipline, for instance, if we write them up as an ethnographical 'fieldwork diary' or an exercise in narrative history. It is clear that any such text will bear the 'fingerprints' of its writer, of his or her creative work as author. Another observer would perceive and describe the given fragments of reality in his or her own, probably quite different, way. This process of translation is taken for granted in art, where it is recognised as the norm. Science and scholarship operate according to different rules, which have traditionally included insistence on the objective validity of the

[1] All contributions to the Forum originally written in Russian were translated by Catriona Kelly.

given data and the way that these have been interpreted in a given piece of research.

Evoking the concept of objectivity so long associated with scientific and scholarly procedure, one is reminded of what Potebnya wrote over a century ago about magical representations: *'It should be observed here, that we are inclined to abuse the terms "scientific" and "scholarly", equating these straightforwardly with* ***truth*** *or* ***reliability****. Such a view of things is clearly mistaken: it is linked with the childish perception that "knowledge" in the scholarly sense begins with the last book one finished reading. On the contrary, it began with the analysis of things; and since no-one can determine when that may have started, then it is clearly impossible to determine where knowledge itself may begin. And as for the criteria we use, the yardstick according to which one determines what is true or false, then we must consider to be true what corresponds to the complete range of data that is accessible to our observation; that is, truth does not exist in the atmosphere at large, it exists so far as a given person is concerned'* [Potebnya 1894: 30].

At different times, the problem of objectivity rises to the surface of academic discussion, a process that is generally associated with a reassessment of the nature of appropriate source material, and of the role played by the subjectivity of the researcher in the analysis of this. The increase of interest in these issues that has been clear over the last two decades is closely connected to the post-modern preoccupation with reflexivity, and with the move away from overt concern with theory, and towards descriptive methods.[1] And once description had again become 'respectable', it was inevitable that scrutiny of its character and status should take place.

In anthropology and cultural history, scrutiny of the relations between the subjectivity/analytical perspective of the researcher, and the objects of his or her research (informants, historical subjects) has generated two different types of reflexivity. On the one hand, attention is paid to the role of informants/historical subjects, to the fact that these may adopt (and generally do adopt) an active position in the representation of their culture — for instance, they may consciously archaise this, or seek to represent it in a favourable light. On the other hand, interest is provoked by the position of researchers as the bearers of cultural stereotypes, analytical preconceptions, and discursive strategies, which in one way or another influence the way these researchers see the cultural phenomena under scrutiny. According to such perceptions, an anthropological study can be imagined as the result of a dialogue, with each of the sides 'correcting' the information flow in its own way. Work in some other disciplines

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1 See further in the round-table discussion published in *Forum for Anthropology and Culture* no. 1.

(e.g. linguistics, sociology, history, cultural studies) would also be seen in this way.

These considerations prompted us to circulate the following questionnaire:

1 *Are the pretensions of traditional anthropology (history, linguistics, etc.) to objectivity in terms of the description and analysis of cultural phenomena to be understood as merely a rhetorical strategy, a 'badge of office' marking out academic commentators' corporate identity, and legitimating their strivings to control the circulation of knowledge? Or does 'objectivity' have value in its own right (as an ideal, if nothing else)?*

2 *Recognition of the inevitability of distortion when material is studied can prompt not only the renunciation of pretensions to objectivity in scholarly discourse, but also an emphasis on the subjectivity of such discourse. For instance, the academic commentator may attempt to insert him- or herself into the text of 'the other', to make his or her experiences and feelings the subject of description and analysis. How productive do you find such initiatives?*

3 *It has become not uncommon for a fusion of the discourse of the researcher and the informant to take place (as in the genre of 'auto-ethnography'). What are the results of such a fusion, in your view?*

4 *It is traditionally held that the individual characteristics and motives of the author of a given text (in the broadest sense, i.e. the informant in anthropology, the creator of the particular source in history) come between the researcher and the subject being studied, impede an adequate understanding of this. How much foundation is there for this view? Can there be such a thing as an 'adequate understanding' in the first place? Should the influence of the originator of source material on the researcher be minimised, and if so, how might this be achieved?*

We invited practitioners of a wide range of disciplines — ethnographers and anthropologists, historians, folklorists, linguists — to supply answers to our questionnaire. The participants in this discussion are not just from different scholarly and intellectual traditions; they also belong to different generations, come from a range of different countries, and are, of course, also very varied in terms of their personal histories and attitudes. Naturally, we neither expected nor indeed desired consensus. However, it is interesting to note that not one of the participants in the discussion chose to question the fact that *striving* for objectivity is a primary condition for the existence of scholarship and science. Conversely, one might observe that everyone here recognised that 'objectivity' is in practice an extremely slippery and problematic concept. In the past, analogies with the natural sciences have often been used to support the claims of the humanities and the social sciences to represent reality 'as it is'. Many of the participants here, on the other hand, see recent developments in the natural sciences as a demonstration of how elusive objectivity in fact is, laying bare 'the inevitability of a strange world' in particle physics, or pointing to the inescapable presence of the observer, to the 'operator's hand effect' during the electronic recording of data.

No dyed-in-the wool positivists found their way into the discussion, then; indeed, it is hard to imagine that any could have done, given the universal recognition here that the traditional positivistic trust in an objectivity never defined or scrutinised has had its day. The discussion that appears below, on the other hand, is precisely concerned with subjecting the concept of objectivity to definition and scrutiny.

Given the range of the discussion, a detailed summary would be inappropriate. However, some key moments may be noted:

— *Scholarly knowledge has inevitable limitations, but precisely the recognition of this fact generates the optimal perspective for any investigation;*

— *The fact that a cultural text must be perceived by an observer in a subjective way has a value of its own, and can be exploited as an intellectual reserve, enhancing the objective status of the data;*

— *The equally inevitable process by which the subjectivity of an informant makes itself felt should not be regarded as a 'distortion', but seen as a culturally important process that can be the subject of investigation in its own right.*

The discussion here, we believe, demonstrates the determination of the participants to extract from the notorious 'crisis of representations' that is said to have taken place as much benefit for scholarly work as they can. The high quality of the comments offered, eschewing banality and statements of the obvious, and the wide range of illustrative material — some cited from personal experience, touching on the ethical as well as technical issues raised by 'objectivity' when dealing with human material, some considering case-studies from ethnographical and historical practice in the past — make the comments offered interesting and valuable to readers concerned with in a wide range of social and cultural problems. We are grateful to the participants for addressing the questionnaire so thoughtfully and in such depth.

Finally, we note that the discussion here has led many of the participants to reflect, one way and another, on thorny problems to do with fieldwork ethics. Indeed, some answers are preoccupied with these questions above all. Here one senses a separate set of uncomfortable questions that is pressing for discussion; in a later issue of the journal, we shall be returning to give them space.

SERGEI ABASHIN

1 If I understand the question correctly, the meaning is that all ethnographers have to play by the rules set down by the corporate culture to which they belong, because that is the only way to participate effectively in current academic debates, to impress one's academic qualifications on others, and to make one's way up the career ladder of administrative posts. It would be hard to argue with this formulation. The perceived 'objectivity' of an academic study is without doubt the product of all these different interests, i.e., a mask behind which multiple peripheral considerations and self-serving actions are concealed. At the same time, the consciously or unconsciously self-serving character of academic activity, according to the authors of the questionnaire, is counter-balanced by a selfless belief that knowledge *is* objective, and that belief is in itself essentially a positive phenomenon, since it expresses a powerful motivating force for academic work that can act independently of the interests just mentioned.

In my opinion, the whole 'either-or' structure of this formulation is misleading or indeed deceptive. The problem is that the rules of the game in any corporate society, and in academic

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life above all, require the individual member of that society to believe in the 'purity' and nobility of the aims in view — here, those of the academic investigation — and of their neutrality with regard to group and political interests. The corporate society often (though by no means always) veils its politically engaged nature and its dependence on political and economic forces, so that distinguishing genuine faith from self-deception is more or less impossible: there is simply no boundary between the two entities. To put it crudely, an academic investigator may wholeheartedly believe in the 'objectivity', 'sincerity', and 'neutrality' of his or her work, and at the same time put this work at the service of some concrete ideology or political principle, i.e. be nothing more than a 'cog' in the 'machine' exercising 'control over the circulation of knowledge', with all the attendant consequences that such a role entails.

What interests me in this connection is a different question, one to which I am not prepared to give an unambiguous answer, but which I would like to open up for discussion by the ethnographic community in general — the question of the ethical responsibility of the scholar in the humanities. Can the sincere conviction that scholarship exists to track down 'objective' information and to 'lay bare' the laws of human development act as a justification in cases where a given scholar or the scholarly community in general places in the hands of extremists, dictators, political hotheads of every shade instruments (interpretations, arguments, mobilising symbols and so on) that can be used to foster social conflict, exploitation, the persecution of alien groups? One should bear in mind also the uncomfortable fact that this instrument is all the more dangerous, the more fervently a given scholarly community, and society more broadly, believes in the 'objectivity' or 'neutrality' of scholarship.

The question just set out could be put in a different way: is there such a thing as a 'good' (or a 'bad') theory, conceptual system, line of scholarly interpretation? And if there is not, if any ('properly formulated') search for fresh knowledge, any new interpretation, is considered acceptable, because scholarship would not be scholarship if it were constrained in any way, then where exactly does the boundary between self-expression and politics lie? And if it is possible to demarcate some theories etc. as 'bad' and others as 'good', then who decides on the demarcation? Who decides what is 'good' and 'bad' in scholarship?

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I think that this question also sets up a false dilemma. The 'quasi' factor in 'objectivity' stems not from the intrinsic falsity of that phenomenon, but from its pretensions to truthfulness. But 'subjectivity' also has pretensions to 'truthfulness', though perhaps of a more subtle kind, and in this sense it, the attempt to make one's own *'experiences and feelings the subject of description and analysis'*, fits perfectly well with the ethos of 'objectivity' or 'quasi-objectivity'. 'Subjectivity' also makes use of a whole arsenal of rhetorical devices in support of its pretensions to special insights into phenomena and processes. And I would argue that within 'subjectivity', i.e. within any text that alludes to the 'I' of the ethnographer, it is also possible to uncover manipulation of facts, construction of reality, and the effort to set up a disciplinary hierarchy.

Thus, *'recognition of the inevitability of the distortion of material'* cannot be classed as a result of the triumph of the 'objectivist' view of the research object. To be sure, in Russia such a causal connection has long been taken for granted, and every manifestation of subjectivity is either expelled from the academic world, or thoroughly marginalised. Of course, one could not say that Russian scholars are completely deaf or indifferent to personal experiences of the kind one may have in 'the field' or indeed when sitting at one's desk. The 'sub-culture' of Russian anthropology includes an off-duty tradition of recollections of fieldwork, of anecdotes and fables relating to the topic, an entire 'fieldwork folklore' of a quite specific kind. (A secondary literature on this subject is now starting to emerge.)[1] But the gold standard of academic value, to be aspired to in monographs, text-books and dissertations, remains the 'objectivist' text, one from which the author is to all intents and purposes absent. Hence the widespread use of impersonal constructions ('it might seem...'), or the replacement of 'I' by 'the author of these lines' or by an undefined 'we'. Hence the lack of interest in the methods by which information was collected, right up to the level of failing to provide details of where, when, and from whom data were collected. Hence those copious historiographical discussions in which the academic author consciously writes him- or herself into a pre-existing context of scholarly debates and disciplinary hierarchies.

Yet at the same time it cannot be said that this tradition helps to solve all the new problems that arise 'in the field today', when the objects and themes of ethnographical work are undergoing significant changes. These days, ethnographers give a great deal of attention to different manifestations of conflict: to extremist groups, to 'shadow phenomena' (in economics and in politics), to self-conscious manifestations of identity and so on. These new research topics constitute 'material' of a highly fluid and slippery kind, and collecting information is associated with significant technical, political and ethical difficulties. How is one to research conflicts between neighbours, for instance, or between different members of the same family? How does one get accepted by a group of Russian skinheads or for that matter 'New Russians'? How should one research the character of

1 See further the contribution by Tatiana Shchepanskaya to the present discussion. [Editor].

'shadow' (i.e. often criminal) resources and connections? And how to fight one's way through bureaucratic restrictions so as to study power relations in political elites?

Personally, I don't regard 'subjectivity' as a panacea for the ills of 'objectivism', but as one of many possible methods of carrying out investigations in 'the field' as it is today, of collecting information, analysing that information, and writing up a text. The history of inter-relations between researchers and people and conditions in the field, and the reflections of researchers on this subject, can be more important than detailed transcriptions of interviews, data from questionnaires, and so on. They constitute source material of a kind that it is now impossible to ignore. 'Subjectivity' should not merely be granted legitimacy; it should be regarded as an index of a researcher's professional competence. But this should not be at the expense of 'objectivity', which, as a rhetorical strategy, has just as much right to existence as has 'subjectivity'.

3 I think I've answered this question already in my answer to no. 2 above.

4 I must say that I don't understand what the authors of the questionnaire have in mind when they talk about *'an adequate understanding'* of a given subject. How is such an understanding to be measured? One might re-formulate the question as follows: is there a failsafe recipe (collection of recipes) for work with informants, a way of getting maximally full and accurate information out of them?

I think that this issue has both a methodological and a technical component. So far as methodology is concerned, the researcher can adopt one of two strategies. The first might be described as 'positivist': one decides that there exists a certain number of facts that the researcher wants to get at, and which the informants (whether consciously or not) are 'concealing' or 'distorting'. In this case, the logical conclusion is that the researcher's task is to 'dig down' to the 'truth', using any tools that he or she can — from heart-to-heart conversations to straightforward spying. True, it's not altogether clear who decides which information is to be considered 'pure' and which 'distorted'. When ethnographers take the decision-making about such things on themselves, they inevitably introduce their own evaluations and concepts, usually on the basis of reasoning a priori.

The second strategy works like this: the ethnographer decides to study not the facts as these supposedly exist beyond the consciousness of the informant, but the informant's own interpretation of these. In this case, the *'source of information'* is inseparable from the *'analyst of the source of information'*, and the researcher's task is not so much to 'minimise' the influence of the personal on the 'source of information', as to study carefully the precise formulation of individual versions of events, individual interpretations of relationships, and so on. 'Distortion' itself is transmuted into 'the research object', and becomes an indication of the status of this or that person, and of their capacity for manipulating information as a means of struggling for power or for economic and symbolic resources.

I should immediately emphasise that both strategies have their place. The 'positivist' conceptualisation of reality as an interplay of 'objective facts' can be a worthy end point of research, provided it is set out with elegance and intelligence. After all, anything interesting has the right to exist. And I would also note that the 'interpretive' strategy is rarely found in its 'pure' form. Even academics who assert their own critical attitudes to 'positivist' knowledge consistently contribute to the production of such knowledge.

As for the technical side of gathering information, this is an issue of the researcher's competence, professional skill, experience, and personal qualities. But there is no one recipe for work with informants that suits all ethnographers. In any case, an individual researcher will need to use different tactics on different occasions: sometimes tape-recording a conversation will be right, at other times one will have to commit the substance of a particular conversation, and certain key episodes, to memory; sometimes one will have to play the VIP, demanding an informant's attention, sometimes to act simple-minded, as though one had no idea what was really being talked about. Every ethnographer develops an arsenal of techniques and a store of methods for getting hold of the information that he or she needs. One could go on discussing these forever, but the debates would almost certainly not produce some kind of 'skeleton key' to bring success in any ethnographic situation.

And so I'll switch the discussion in a slightly different direction and raise another very important question that ought to be considered, if not decided for one and all. This is the issue of ethics in ethnographical research.

Let me cite an example that may seem a little surprising, but which I am sure is familiar to many other Russian ethnographers as well. Anyone who has done fieldwork will know that sharing a drink with an informant is a very effective way of getting hold of many fascinating details about life in the locality that are normally kept concealed. However, it is clear that informants would often not pass on information of this kind if they were stone cold sober. The logical conclusion has to be that information collected in this way has to be regarded as unsanctioned. Should the ethnographer avoid situations of this kind and ignore information received in this way, or quite the reverse, do their best to pour drink down the informant's throat and provoke him or her to more and more revelations? Is it in order for the ethnographer to set up a simulacrum of a heart-to-heart conversation, and to use spirits as a way of 'making an informant talk', of finding out facts and opinions to which gaining access by other means would be difficult, or indeed impossible?

Difficulties can also arise not just when material is being collected, but when it is being analysed and published. After finishing field-work, ethnographers continue to ask themselves uncomfortable questions: should they ask permission to use the material that they have managed to collect? Is it in order to publish personal information, including that received with the informant's permission (one has to bear in mind that informants do not always fully understand what is likely to be done with the material). And suppose — just hypothetically — the release of such information might harm the informant? Is it all right for an ethnographer to describe situations of conflict, to pass hostile judgements on specific individuals, to make assessments of this or the other real-life situation?

We all know that, in the effort to solve dilemmas of this kind, many ethnographers adopt a strategy of anonymity, giving place-names and personal names in code. And in principle this is the best way of dealing with fieldwork materials, especially if it is to be published somewhere beyond the academic world. But if one has done research by the 'stationary method', the entire purpose of which was to produce a study of one particular kishlak,[1] its specific history, concrete family histories, and so on, then how, sensibly, is one to conceal whereabouts the place is, what it is called, and the details of people's lives? The inhabitants themselves will easily work out what's going on — and the same is true with any close study of a village or a city locality. For one's academic colleagues, on the other hand, details of this kind don't have any emotional significance, however they are presented. And concealing the precise place where information was collected prevents other scholars from checking up on the information or supplementing it by further study in the same place.

I don't think any of the questions I have posed here have clear answers. If one follows strict ethical guidelines, then many subjects, themes, and issues will remain wholly or partly beyond the purview of scholarship. Then ethnography will lose its significance, raison d'être, and defining features (and intellectual advantages) as a discipline, and will turn into nothing more than a 'battle of words'. On the other hand, if moral imperatives are totally ignored, then scholarship turns into a destructive activity, and the noble, humanitarian foundation of ethnography, its value as an expression of the

[1] Mountain village. [Editor].

liberal humanities, becomes a fiction — when it is exactly this humanist principle that is the reason why many ethnographers love what they do.

I can't see easy ways of solving the problems of scholarly duty and morality that I have sketched out here. Even an experienced ethnographer has to confront difficulties and overcome obstacles afresh each time he or she is out in the field, always with reference to local conditions; much depends on the personality of the ethnographers concerned, their temperament, local knowledge, sense of tact, capacity to make contact with people. But discussion of such problems is long overdue. And its result should not necessarily be the execution of some 'corporate' ritual (the passing of a sort of Ethnographers' and Anthropologists' Charter — though this in itself would not be a bad idea). But what is more important is that ethnographers, and particularly those of the younger generation, should think seriously about all these questions, that they should pay more attention to reflecting and testing out what they do — and that they should maybe even engage in a bit of self-censorship every now and again. Because only a heightened sense of self-awareness and personal responsibility can act as a guarantee against various extreme manifestations in fieldwork.

LEVON ABRAHAMIAN

1 The pretensions of anthropologists and historians to objectivity in the description and analysis of cultural phenomena are, it seems to me, not reducible simply to corporate control of knowledge: a genuine belief system is in operation here. Without it, the whole meaning of academic work would be lost. The subjective in its constantly changing and fundamentally indescribable nature is more properly the domain of philosophers and anarchists, and it is not surprising that post-modernism and post-structuralism were indeed developed by philosophers on whom the social and political upheavals of the 1960s had made their mark. Belief in objectivity does not signify blind faith in the possibility of objectivity in an absolute sense; it signifies the rejection of non-objectivity in an absolute sense. What the question here posits as 'objectivity', on the other hand, really is an unachievable ideal.

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2 The distortion of the material being studied is often related to the presence of an intrusive observer. An article by Gayane Shagoyan about Armenian marriage ceremonies deals with a concrete example of this [Shagoyan 2000]. Here, the distortions in the ritual text of the marriage occurred as the result of the presence of a video cameraman, whose presence generated both conscious and unconscious changes to the traditional scenario of the ceremony. The anthropologist is confronted with roughly the same phenomenon as particle physicists were when they realised that the instruments they used to observe the microscopic world were introducing significant changes into what they were observing.[1] An academic researcher, who is a more detached and self-conscious observer than the video-operator, in turn watches this first type of observer, who has become a 'canonical' figure in the wedding ritual. But it shouldn't be ruled out that the presence of an academic observer also brings about distortions in its own right (indeed, there seems to be evidence that this can and does happen). Ideally, then, this observer too should be observed by someone else, and that someone else by someone else... and so on. One might suppose that the rapid progress of technology could altogether free rituals from intrusive observers on these different levels, leaving only a completely hidden observer, with the capacity to eavesdrop on and peep at everything without hindrance, producing absolutely unimpeded, 'complete' knowledge of what has been going on — in the manner of a TV documentary about the secret life of insects, for instance. But even supposing totally non-intrusive observation were possible at the level of recording data, intrusion would be bound to make itself felt at the level of interpretation, above all because any interpreter inevitably introduces subjective factors. Indeed, for some interpreters, interpretation is only possible if subjectivity is present. Commonly or invariably (depending on the interpreter), interpretation has to involve at least fleeting self-transformation into the people under study, which process also necessarily leaves subjective traces in the final text. When I compiled the index to my most recent book, I realised that I sometimes didn't know whether this or that observation really emanated from Australian aborigines, whether they were looking through my eyes or I through theirs. Generally speaking, too, one can strive all one likes to 'objectivise' an interpretation, only to find that the intrusion has taken place at another level — that of the anthropological source itself, as recognised in question four below. We elude subjectivity at one level of observation, only to have it creep back in at another; shown out the door, it climbs back in the window again. Still, observers who haven't 'lost their faith' in objectivity will make every effort to prevent some unexpected appearance on the part of the

[1] As discussed in [Danin 1962]. See also Mikhail Rodionov's contribution below. [Editor].

subjective — an activity that may not sound too attractive to those who want maximum sensations here and now, but which has greater appeal to observers with more patience and tact.

Another route is hinted at in the phrasing of the question itself — to exploit the subjective as a constituent part of observation, given this is inescapable. So far as I can see, both approaches, or to be more accurate, genres of writing, have their place and are productive in their own way: which is to be preferred depends on the task in hand. I myself try to work in parallel in both genres, describing the same phenomenon in two alternative ways — in different pieces of work, of course. I do this not for the sake of variety, but because some aspects of a given situation require one kind of approach and some another. The problem is that if the first approach may allow an undue level of subjectivity to 'leak in' to one's observations, the second carries the perhaps even greater danger of surrendering to subjectivity altogether, of going over into egotistical self-display. For my money, the genre of 'autobiography' sometimes heads a bit far in this direction — which brings us to question three.

3 The fusion of informant and researcher is an inescapable process, especially given the post-modern extension of the term 'author' to include also the informant as creator of a given text. What are the results of this extension? A skilled anthropologist turns an interview with an informant into a dialogue of two autonomous individuals, two 'authors', from which dialogue we can learn much that we would not have learned even in the most impartial or subtle retrospective interpretation of a traditional kind. The most attractive thing about this way of setting out material for me is that it forces academics to give up jargon and talk 'ordinary' language — otherwise they simply won't be understood. That said, knowing how to direct a dialogue of this kind without resorting to patronising questions of the kind some adults like asking children takes skill, the ability on the part of the researcher to be genuinely simple and direct — something that is particularly hard for people who are likely to be 'complex', spending a good deal of effort on concealing their natural simplicity. On the other hand, though, undue attention to what an informant says, the preservation, so to speak, of the inviolability of his or her 'copyright' can easily generate an over-abundance of interpretive 'noise', of authorial sighs and throat-clearings: all the anthropologist's effort seems to go on recording every squeak made by his or her co-author, so that essentially he or she is reduced to an appendage of the tape-recorder, and the text becomes not so much a dialogue as a monologue by the informant — valuable material, to be sure, but material that could perfectly well be obtained, these days, without the philosophical dialogue of *'researcher and informant'* needing to take place to begin with. And that's not even to mention the frequent cases when the researcher isn't so much

transformed from a participant in a dialogue to the audience of a monologue, but starts off as a passive listener to begin with, convinced that he or she is following the latest fashion in anthropological fieldwork.

It is from the dialogue of the researcher and informant of the ideal kind as just described — fragile, trusting, egalitarian — or from a skilfully conducted 'interrogation' based on a questionnaire, that 'auto-ethnography' emerges. The process can happen in two different ways. On the one hand, anthropologists may see themselves as informants; on the other, informants may have ambitions to turn into anthropologists. An analogy from the cinema comes to mind: a director wants to be an actor, or, as is more likely, an actor decides he or she wants to direct a film. As a rule, neither process is terribly successful. To be sure, there are cases where a director was originally also an actor (as with Orson Welles). Commoner (and less open to contamination) are cases when directors fleetingly appear in their own films, as Pasolini liked doing, which generates a sensation of their perpetual presence. The two types of 'auto-ethnography' have significant differences (an anthropologist who becomes an informant circulates obsessively round his or her own personality, while an informant who turns anthropologist is likely to get stuck in the rut of local history — i.e. recording of local detail for its own sake), but they are united by one peculiarity — a strong pull towards idyllicism, with the picture focusing on vividly remembered events and details. One could add that an 'auto-ethnographer' is quite likely to suffer from 'false memory syndrome', in the way that a patient on an analyst's couch will often 'recall' exactly what suits the analyst (whose equivalent in this case is the horizon of cultural expectation, or the expectations of the ethnographer him- or herself). However, I should emphasise that what has just been said relates to the genre of 'ethnographical memoirs' and is of course a crude over-generalisation, ignoring the exceptional cases of anthropologists with prodigious memories or informants who become genuine autodidact anthropologists.[1] What I have in mind here is the problems that are built into the method itself: whatever happens, the anthropologist will end up with a self-portrait of some kind,[2] and, as we know, even the most skilled self-portraitists tend to distort the image, at the very least showing their face as it appears in a mirror. I have more time for the cases where anthropologists, rather than reminiscing, describe situations in which they are themselves involved: this depends not on retreat into the self, but the description of an entire milieu, one part of which is the self — which constitutes an unsolved problem of a different kind.

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1 On such a case, see Tatiana Valodzina's remarks on Petkevich below [Editor].

2 Compare [Hayrepetyan 2001: para. д584].

4 It is not just the individual characteristics and motives of the information that influence the adequate understanding of the subject in hand (though this process is scarcely open to doubt). The question of what happens to the information they have imparted is also crucial. This is particularly clear in the case of ancient documents that have come down to us only in a late version, reflecting many subsequent redactions by generations of scribes. How do we come to an adequate view of such a document?

To give a concrete example: the life of Mesrop Mashtots, the creator of the Armenian alphabet, written by his pupil Koryun (fifth century AD), claims that he also created the Georgian and Caucasian-Albanian alphabet; however, Georgian historians consider that the passage making this claim is a later insertion by an Armenian scribe. Crucial to an adequate interpretation of this document is the fact of rivalry between two neighbouring peoples over who was responsible for creating which phenomenon of cultural significance (including, but not limited to, the alphabets in use); the exercises in graphemics and linguistics meant to demonstrate the truth or falsehood of such claims need to be taken into consideration too. In this sense, the true content of the document is a question of private interest only. There are other cases where the loss of an original text works exactly the other way round, frustrating later interpretations by depriving these of foundation. I experienced a case of this myself when I was studying the mass rallies in Yerevan in 1988. A political leader of the day gave a reading from his latest leaflet, and made some remarks whose content was crucial to the drift of my investigation. But I could only track down one, hand-written, copy of the leaflet, which on investigation turned out not to contain the passage I needed. The person doing the copying had simply left the passage out, because he didn't agree with what was being said in it. Here again we have a case where the individual motives of a secondary author deprived a text of its original meaning.

An informant need not be partial even where he or she is expressing individual traits. Sometimes, an investigator who asks a question that is impossible in terms of the prevailing conventions of etiquette will end up getting an untruthful answer — the most famous example being Margaret Mead's material on the erotic paradise of the Samoans. This needn't be a question only of direct and self-conscious lying: a badly formulated question may generate a badly formulated answer, as happened with L. H. Morgan's questionnaires on kinship terms. The phrase *'adequate understanding'* in the fourth question here assumes that the informant may have individual (i.e. aberrant) characteristics, but that the anthropologist's position will be impeccable. On the whole, however, lack of adequacy is more the fault of the latter than the former, or at any rate the product of close collaborative work on the part of two 'co-authors' engaged in dialogue: the person asking the wrong questions, and the person answering them with lies.

Finally I'd like to ask: should a researcher actually try to represent an informant's text adequately to being with? To my mind, the answer is yes, and especially if the obstacle to adequacy might be of his or her own making. Do anthropologists have the right to edit a text received from an informant? This issue takes us back to question three, and my view is that yes, editing the text is fair enough, but only if the informant is given full rights as a co-author. A good example of how this can work is the collection of Eskimo tales put together by Igor Krupnik [Krupnik 2000]. Krupnik edited down literally miles of tape on which he had recorded tales told by Eskimo elders over the course of a decade. He cut out long digressions and parallel narratives, and his own questions and remarks, and removed himself from the equation as co-author. He turned a dialogue into a series of informant tales, an Eskimo epic meant for the younger generation who had forgotten their forefathers' tales. On the surface, none of this work is evident, and the reaction might even be: OK, so he recorded a load of stories, which anthropologist doesn't do that these days. But I know myself that these tales were recorded in exactly the ideal situation that I described when I was answering question three above, and for no less a reason than this: I was myself present when some of these dialogues took place, as the observer of the observer.

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On Ethnographical Reality and the Limits to its Description

The question of the 'objectivity' of an anthropological study is directly related to the question of the epistemological status of any branch of the humanities, all of which suffer from certain well-known difficulties of verification with regard to the results that they adduce. From a positivistic point of view, the more exact and 'objective' the description and analysis of culture that is offered, the higher the status of the piece of work involved. But what exactly does an 'objective' description or concept of culture mean? What culture actually is, or what we suppose it to be? The answer is obvious and lies at the level of ontology — we cannot know the

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deep essence of culture and grasp it as a whole, since we ourselves belong to it. This is true even if we devote ourselves to the study of ‘the other’ — to exotic cultures.

Constructing an objective description requires distance from the object of study, obliquity of gaze. To paraphrase Wittgenstein’s *Tractatus Logico-Philosophicus* [1960: ch. 6, section 41] one could say that the meaning of culture lies beyond culture. In the final analysis, any study of a human culture that pretends to objectivity and definitiveness will have to go beyond the bounds of that culture. It seems highly likely that a significant number of cultural phenomena that might interest anthropologists fail to emerge from the cultural background for the simple reason that they are part of it.

This does not mean that the anthropologist should not strive towards an objective depiction of the subject in hand, but it should be borne in mind that no anthropological description of culture will get to the essence of that culture, which always remains somewhere alongside, beyond; or, to put it differently, there are as many different models of culture as there are descriptions of culture.

On the other hand, if anthropology refuses even to attempt to come to an adequate understanding of cultural phenomena, anthropological study risks turning into a *‘factory of entertainments for a small circle of intellectuals’*, into *‘round-dances of etymological, phraseological, and hermeneutic signification, loaded with ever more recondite gestures of interpretation’* [Lem 2003: 40].

I have the impression that the time of optimism in theoretical anthropology, which was sustained above all by contemporary developments in linguistics, and which anticipated the discovery of algorithms or general laws for the generation and evolution of cultural phenomena and culture in general, has come to an end. In order for any discipline to develop, it is essential for it to know its own limits.

One such limit, which is particularly relevant to the anthropological study of traditional cultures, is the recognition of the fundamental impossibility of adequate translation (meant here in the broad sense of interpretation, inter-communication, description) of *‘the language of visual images into the language of concepts’* [Freidenberg 1978: 43]. Any explanation or interpretation of cultural phenomena that takes place within the limits of conceptual, abstract-logical thought, inevitably involves a distortion of the subject under study. Some argue that ‘empathy’ — the submersion into the culture being studied and depiction of this *‘in the images and value terms used within that culture itself’* [Dmitrieva 1998: 31] might offer a way out of this situation. Here, the emphasis is placed on shared experience, on emotion, intuition; hence the marked elevation of the role of the personality of the commentator (subjectivity), of his or her human and intellectual traits. Attractive as the idea of according intuition and feeling the status of tools in the investigation may be, however, the question arises of how such investigations may satisfy the burden of proof; in addition the hope that an anthropologist might actually put into practice Victor Hugo's insight that *'it is essential to make oneself like the other, if one is to understand him,'* (which essentially is the foundation of empathy) looks, on close view, rather naïve.

Yet this does not rule out turning — at the level of a supplementary (preliminary) step in recognising the object under study, as a starting point for scholarly analysis as such — to an intuitive, serendipitous, 'metaphysical' approach to the problems of interpreting culture; such a recourse might indeed be extremely productive, even if not necessarily definitive. In fact, the anthropologist is not always well advised to rely on common sense and logic, whose proper domain is considerably narrower than his or her own object of study — culture (cf. Nils Bohr's theory of supplementarity or Carl Jung's concept of the acausality/synchronicity of events, which were formulated as a result of a retreat from European scientific thought and under the influence of the Chinese philosophical tradition).

Another limitation in anthropology is connected with the problem of the 'intrusive' action of the subject (the researcher) on the object of study (the culture being analysed). There has already been quite a lot of cogent discussion of this problem; here I will only remark that fieldwork these days is increasingly characterised by direct communication and dialogue, with the ethnographer becoming in a true sense the co-author of the information. Attention is being devoted more and more to the process of communication itself, and to the anthropologist as a participant in the dialogue that takes place in the course of teasing out information. Here the personality of the researcher becomes the most important factor in play, predetermining, in many respects, the way that a given culture is described. One issue involved is the ethics of fieldwork, i.e. the need to ensure the preservation of the dignity, privacy, etc., of the people whose stories are being recorded; another is the fact that the ethnographer, when describing ethnographic reality, notes only phenomena that have already been pre-selected, filtered through his or her own experience and world-view as a member of a different culture. One could even say that the ethnographer's individual experience is raised to the status of an instrument in methodology.

Perhaps, indeed, the very process of coming to know a different culture might turn into a 'visionary' strategy on the ethnographer's part, since it makes concrete the idea of 'cultural distantiation' —

an essential achievement on the part of anyone who wants to get to know the culture concerned. To begin with, such an observer may well notice details that escape the attention of researchers studying 'their own' culture, because of the natural tendency among 'insiders' to be blasé and take their own knowledge for granted; in addition, the experience of studying a new culture sheds a fresh light on cultures with which one is familiar.

Whichever way, the ethnographer in the field will only notice those particular events and details which can be assimilated in terms of his or her pre-existing ideas about the culture being studied, or about culture in general, and which can be fitted into established scholarly categories. This classificatory character of fieldwork became evident for the first time more than a century ago, when the collection of ethnographical material started to take the collection of material for botanical museums as a model. As a curator of the Russian museum wrote, those collecting for botanical purposes *'do not bring whole strips of turf from the meadows: every plant that is selected is removed from the connection with neighbouring plants to which it belonged in nature, and is placed in a new connection as discovered by mankind'* [Smirnov 1901: 229]. The very selection of different facts or phenomena by the observer in the field gives these an ethnographical status, yet on the other hand makes objectivity problematic, because the original contextual connections are destroyed.

Thus, the process of observation and recording itself undermines pretensions to objectivity; it creates a secondary ethnographical reality, 'the reality of observation'. Study of archival data or museum collections assembled in earlier eras indicate that these represent the cultures from which they were taken rather unevenly (from the point of view of modern ethnography); significant thematic lacunae are evident. All this is perfectly natural and easy to explain: material was collected according to particular concepts of what constituted tradition, ethnographically valid source material, and so on. It's clear that practical experience comes first: one can only ask questions about material that one in some sense already 'knows'. Therefore, any questionnaire already expresses an interpretation of the material, and ethnographical facts collected in the field cannot — pace the traditional view, still strongly held by many ethnographers in Russia — have pretensions to objectivity. Any programme for data collection inevitably reflects the mental stereotypes (myths, if you like) of its compilers, and the level of academic knowledge at a given time (a level that is constantly rising).

As an example of how the formulation of questions makes explicit a scholar's view of a given cultural milieu, and in turn impacts on the character of the material being recorded, one might cite L. N. Vinogradova's stories about Kazimir Moszynski, well known for his work in the Polesye region.[1] Moszynski concluded from his own fieldwork that beliefs about witches were rather weakly represented in the region, yet materials from the Polesye archive, collected at exactly the same time, indicate that witches figured widely in folk culture there. The co-existence of two mutually opposed 'observational realities' is traceable to research methods: Moszynski framed his questions about witches as part of a series of questions directed at beliefs in dark powers; in Polesye, however, beliefs about witches are tied in to calendar festivals, and can be tapped by asking questions about ritual practices associated with these days, especially Ioann Kupala [St John's Night].[2]

The next phase of an anthropological investigation — description or interpretation of a given culture — is associated with a predictable kind of displacement: 'observational reality' gives way to 'representational reality', which sets out ethnographical facts in a sequence dictated by one or another current academic classificatory system. The greater the degree of abstraction and the more sophisticated the analysis offered, the bigger the gulf between reality as observed and as described is likely to be. Thus, distortion of ethnographic reality itself occurs on two levels: when it is observed, and when it is interpreted.

There is also another, still more widespread, obstacle to 'objective' description of cultural phenomena. On the whole, modern fieldwork (in the East Slavonic domain anyway) collects stories about ethnographical reality — the verbalised form of culture — rather than ethnographical reality itself. At its most 'authentic', the material collected is likely to take the form of memorates — a specific genre of eyewitness testimony that, like other types of folklore, uses set formulae, verbal clichés, and traditional forms of structure and organisation. In other words, one is collecting not 'primary' information, but pre-interpreted material, filtered by the informant's awareness of talking to an outsider (among representatives of cultures themselves, there is a tendency to 'take things for granted'; events fade into the background and aren't the subject of reflexive activity). Cultural memory is highly selective in terms of the facts and events it records, and the more distant they are in terms of time and space, the more they are subject to mythologisation, the more they are reworked in terms of the canonical beliefs and values of a given culture.

In addition, oral tradition is characterised by reference to a shared pool of variables: a single concept will be conveyed by a variety of synonyms. This generates plasticity, instability, and at times even

[1] See e.g. *Polesie Wschodnie*. Warsaw 1928. [Editor].

[2] i.e. 23 June, or Midsummer Night. [Editor].

apparent contradictoriness in texts depicting the past. At the same time, all this does not, of course, reduce the ethnographical value of the given texts, because they represent the peculiarities of the thought-system and world-view shared by the bearers of a given ethno-cultural tradition.

Things become more complicated, though, when a researcher is confronted not by memorates, but by a paraphrase of a paraphrase — by a second- or third-hand interpretation. However, modern methods of fieldwork do make it possible to trace the process by which reality gets turned into folklore, for instance, by comparing interviews with eyewitnesses and interviews with people not directly involved in a given event, or by holding follow-up interviews with eyewitnesses after some time has elapsed.

One also has to bear in mind the way that informants' own characteristics and motives — for instance, their sense of what ought to be said and what ought to be left unsaid — will shape their testimony. Once again, this should not be regarded as a tiresome distortion of 'real' information — these layers of motivation themselves reflect value systems of this or that kind, different concepts of norms and ideals, in short, the attitude to life in general prevailing among the representatives of a given culture. They should be regarded as ethnographical facts in their own right. In this perspective, the attempts to overcome *'the dictatorship of individual creators of texts by allowing room to other "voices" and interpretations, particularly as produced by the informants themselves in dialogue with ethnographers or with each other'* [Sokolovsky 2003: 29] are interesting and productive. But this relates more to questions of the appropriate forms and genres for the representation of ethnographical observations.

Let me, in conclusion, look at one situation where the voice of the informant is barely to be heard, and where pretensions to objectivity in anthropological work might seem to have a firmer foundation. I am referring to the collection of 'objectivised' forms of culture — of material objects, as in the collecting activities of the Russian Ethnographical Museum, St Petersburg. As in the collection of 'informational' material, scholarly engagement here has two phases: the gathering of material, and then its interpretation.

So far as the first is concerned, then the Museum has, throughout its entire existence, been devoted to a maximally full representation of traditional culture in all its different aspects. As was set out in the *Programme for the Collection of Ethnographical Objects* [*Programma* 1902: 5]: *'all appurtenances of life and daily life should be collected, relating both to the material and the spiritual domain, since it will be found that the latter is often expressed in material objects as well'.* Yet all-encompassing collection of traditional objects did not in fact take place, despite the apparently unambiguous nature of the task as defined by the *Programme*. Over the course of the Museum's history, the material that entered the collection changed on numerous occasions. The reasons for these fluctuations in curatorial policy are traceable to changes in selection criteria for the objects put on show, which in their turn were related to shifting perceptions of culture generally and the ethnographical status of cultural phenomena particularly. All of this is well analysed in O. B. Lysenko and N. N. Prokopyeva's article 'Collections and Collectors: Ethnographic Reality and its Interpretation' [Lysenko, Prokopyeva 1998], which uses a synchronic survey of the Museum's acquisitions in the early twentieth century as the basis for valuable observations about the way that the academic preoccupations of such well-known ethnographer-collectors as F. K. Volkov, N. M. Mogilyansky and A. K. Serzhputovsky influenced acquisition policy. The collections were driven by the aim of representing the culture of Polesye as fully as possible, but in fact indicate how the ethnographical reality of the region was assimilated in fundamentally different ways. F. K. Volkov, whose main interest was in the mythic dimension of culture, was primarily concerned with collecting ritual objects that had Christian connections. For his part, N. M. Mogilyansky's main focus was on objects that could be grouped into typological series according to patterns of ornamentation, manufacturing techniques, structure, and so on. As Lysenko and Prokopyeva write [1998: 20], *'Over numerous visits of a month and a half at a time, an enormous collection of materials relating to ornament was built up [...] There is no doubt that few, if any, other areas have been so strongly shaped by theorising a priori, hypothesising, and the setting out of statements and surmises of all kinds'.* The collector who came closest to addressing the tasks of collecting as originally defined was A. K. Serzhputovsky, who considered the holistic description of culture to be the central priority, since *'a complete and living picture of the life of a given people'* could only be achieved by means of a thorough description of its material and spiritual culture [Lysenko, Prokopyeva 1998: 20]. His collections are therefore extremely varied and reflect many different sides of peasant life.

One should also note that not only the principles of collection, but also the definitions and categorisations on which these were based ('traditional', 'ethnographic', 'typical', etc.) have been subject to change. In this sense, the collections accumulated over more than a century reflect both the general level of development in ethnography as a discipline, and changing concepts of ethnic groups and cultures. One of the first Russian scholars to emphasise the relativity of the concept of 'tradition' was the great Ukrainian scholar F. K. Volkov, mentioned above. In his studies of Ukrainian ethnography, he made interesting comments about the development of 'secondary ethnographical reality', which are worth quoting in full here:

The rapid rise of the Ukrainian movement[1] *in the 1870s, and of an interest, among intellectuals, in the Ukrainian theatre, has generated a more or less stylised form of "Little Russian dress", which bears a fairly distant relation to ethnographic reality* [...] *We must confess ourselves to some astonishment on seeing local girls everywhere, from Kiev right to Novgorod-Seversky, wearing "Little Russian" costume of a kind that irresistibly reminded us of the chorus in N. V. Lysenko's opera* ***Christmas Night*** [...][2] *The costume was, it would seem, introduced to the village from outside, but has now been thoroughly assimilated, and includes such curious details as embroideries of cabbage roses and bunches of grapes wreathed in leaves on the sleeves of blouses; these decorations are certainly not part of Ukrainian folk tradition...* [Volkov 1916: 576–577].

And so we see how researchers in the field are directed by their own ideas about the culture they are witnessing, about what is characteristic in a genuine sense, an integral part of that culture, and what is present simply by chance, without 'weighting' in ethnic terms. In this sense, the selection of material objects for study always has subjective promptings: we are not so much collecting objects in the field as we are collecting our own, constantly changing, perceptions of what those objects should be. Material culture where displayed in the form of museum exhibits is just as subject to ethnographical opinion as any other kind of data.

In the words of one of the Russian Ethnographical Museum's originators, Smirnov [1901: 230], the objects relocated to the museum were supposed to *'give a full cultural and historical picture'* of the peoples included therein. But in fact the objects held in the Museum's reserve collections or put on show in its exhibition halls never did or do represent the cultures from which they were taken to any level of adequacy. The cause for the gulf between the objects exhibited and the culture that they represent lies in the essential nature of the relation 'object–culture', which is metonymical (the part stands for the whole). For the researcher — as for the visitor to the Museum — the object is always just a fragment of a reconstructed whole: traditional culture. So, a ploughshare from a given local tradition, standing for 'work in the fields', does not represent the act of ploughing itself, but merely gestures obliquely in that direction, acting as an index of that area of an ethnic group's culture. Everything else has to be imagined, reconstructed, a process which will take place in the heads of visitors according to the image they already

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1 i.e. the Ukrainian nationalist movement [Editor].

2 Nikolai Vasilyevich Lysenko, alternatively Mikola Lysenko (1842–1912), was the premier Ukrainian composer of the late nineteenth century; *Christmas Night* [Rizdvyana noch] (1882), with a libretto by Mikhaylo Starytsky, is based round a medley of traditional carols sung by a chorus in peasant dress. [Editor].

have of a given culture. The 'ethnographical reality' of a museum is, then, the product of combined efforts on the part of collectors, curators, and visitors.

Appearing as a representative part, rather than as a complete whole, the object gets turned into a kind of abridged text (or abbreviated cultural space), which, in the context of a museum exhibition or a work of scholarly analysis is expanded into a new narrative. It should be emphasised that the semantic field of an object, where this is presented as a museum exhibit, is never identical to its semantic field in its 'natural' habitat: we witness the relocation of an object, complete with its primary characteristics, into one or another kind of interpretive model (sign system).

To sum up these reflections on the provisional nature of any representation of culture: anthropological descriptions never do or can correspond to ethnographical reality, but are always located somewhere alongside this, forming a dense 'interpretive field' with reference to it and to the given culture. The recognition on the part of anthropology of the limits to objectivity in its depiction of culture and, as a correlative of this, the recognition of the right to co-existence of many different interpretive models, complementing and supplementing, rather than competing with, each other, is essential if we are to work towards producing as full a depiction of culture as is humanly possible.

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1 Discussions about objectivity in the social sciences (Geisteswissenschaften, Kulturwissenschaften) date back at least to the times of Dilthey and Max Weber, if not to Vicco. Dilthey's notion of intersubjectivity drew attention to the fact that the oppositions subject/object and interpreter/informant (except for grammatical relations) are inadequate schematisations of what in fact happens in social reality. Not only is the individual born into languages and cultures; at a simple communicative level there is always a redundancy of messages such that after a few rounds of conversation, a level of objective understanding is achieved adequate for the circumstances at hand, even if further specification and implications remain obscure or ambiguous. As Gilbert Ryle was to subse-

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quently put it, there are no private languages in the social world, and so the entire public social world is available, in principle, to objective study.

Weber introduced a heuristic notion of ideal types, which were rational schema that could be held up as predictive templates, and that were generated from a contrastive analysis in different cultures, classes, or social settings. Thus in *Economy and Society*, the notions of patrimonialism and feudalism are generated from the histories of the Ottoman, Safavid-Qajar, Mughal and Chinese empires, based in particular on the evolution of their taxation systems and legitimation ideologies; this is contrasted with the development of feudalism in Europe with its merchant city and vassalage charters. Similarly, Weber's notions of mandarin educational systems, though drawn from China, could be used as an intellectual model of the way that Greek and Latin and vernacular literary classics were used to form a 'mandarinic' state bureaucracy in Germany, England and France. At issue in Weber's methodology was the epistemological disclaimer of psychologism, or being able to get inside the heads of other minds; but he also argued that there was sufficient patterning of social action that models could be generalised from historical particulars with good predictive validity. Thus Weber's famous definition of power as the probability that an order given will be obeyed depended upon an analysis of the weakness of physical coercion (good only as long as the force is applied) and economic coercion (as in the power of monopolies) relative to power deriving from 'legitimate domination' (the feeling that an order given is legitimate, either stemming from tradition, or from legitimate political processes). Weber's *Verstehende Soziologie* (interpretive sociology, or a sociology of understanding) required the understanding of the motives, intentions, categories, and interpretations of the actors to a degree adequate to construct the ideal types needed for explanatory or policy purposes.

Paul Ricoeur and Clifford Geertz extended the ideas of Weber's *Verstehende Soziologie*, drawing upon the phenomenological work of Alfred Schutz and others, by working with the metaphor of social actions as analogous to texts that could be read: in Ricoeur's case by 'reading' the social traces left by social actions, and in Geertz's case by tracking the interpretive activities of the actors. Others who extended these methods include the philosophers Ernst Cassirer and Suzanne Langer (on symbolic forms), the rhetorician Kenneth Burke (on the dramaturgy of social life), and the anthropologists Anthony Wallace and Victor Turner (on the patterned uses of ritual processes to fuse embodied pain and emotion with cognitive-moral principles so as to reinforce or transmute social patterns).

The ability of area specialists to evaluate generalisations or claims has always depended upon checking sources and perspectives against other sources and perspectives. Two areas of importance here are language competence, and the changes incurred during the translation process, either by the process of translation itself (i.e. the substitution of one set of linguistic relations for another), or through the changed relations in the relays of addressee for whom things need to be explained (both generally as in the concept of 'cultural translation', and also in such explicit sociolinguistic contexts as a fight or a story-telling session, embracing both primary relations, between participants in the fight and between story-teller and audience, and secondary relations, between the narrator of these events to different expert, policy-maker, or student audiences).

In anthropology, this validation methodology was honed initially by applying new frames of analysis generation after generation to the data in classic ethnographies; by regional studies of variation (as in the studies carried out by anthropologists and agronomists from the Rhodes-Livingstone Institute in Northern Rhodesia (now Zambia); by comparative social -structural investigations; and increasingly, by restudies of, or further studies in, areas worked over by previous generations of anthropologists. Annette Wiener went back to the Trobriand Islands, and her work enriched, complemented, and challenged that of Malinowski, as did that of other fieldworkers who went to the Trobriands. Studies by Paul Howell, Douglas Johnson, Sharon Hutchinson, and others among the Nuer clarified and expanded Evans-Pritchard's account. In addition, Evans-Pritchard's own opening self-report of how difficult it was to collect information began a tradition of reflecting upon the biases and problems introduced when persons associated with an occupying military in a conflict area undergoing 'pacification' attempted to do research. The alternative frameworks and temporalities of the fieldwork in Tepotzlan by Robert Redfield (migration studies) and Oscar Lewis (Rorschach and other psychological probes) were long used as pedagogical teaching devices for courses on ethnographic methods. By the last quarter of the twentieth century, the understanding of such classic discussions was enormously enriched by the expansion of anthropology as a field, by the entry into the discipline of professionals from around the world, and by the changing nature of social and cultural interactions, particularly through the changes in the media/information environment (from radio and television to the internet and satellite phones), and by the massive population migrations and new diasporas challenging nineteenth- and twentieth-century nation building technologies and categories.[1]

An effort to trivialise these epistemological and methodological recognitions was mounted in the 1980s by some scholars who feared

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1 See also Nicholas Harney's contribution to Forum no. 1. [Editor].

losing their (cognitive and social) control over the academy and policy worlds. This defensiveness was reinforced by the pressures towards deliberative democracy — or what Ulrich Beck has called *'second order modernisation'*, — the pressure towards reflexive institutions that are able to incorporate and process information from various positions in their social environment so that decision-making is not inflexible and brittle in the way that it is in first-order, rule-rigid-bureaucratic, or 'experts know best' formations, The arenas where these pressures towards flexible and information-rich, multi-perspective-recognizing institutions have been most intense are in medical care, public health, and the handling of environmental issues, e.g. the disposal of toxic waste. Patient groups organised around various cancers, HIV, Huntington's Disease, and certain 'orphan' illnesses (i.e. affecting such small numbers as to make them normally uninteresting to pharmaceutical drug discovery operations) have managed to change doctor-patient, research funding, and insurance-patient power relations by using the new information tools available on the internet to collect data, to organise patient and clinical pools for experimental testing, and to marshal resources and political clout.

Reflexivity in a social institutional sense (i.e. the integration of information from multiple sources, and the evaluation of implementation processes and impacts as well as of production design and rationalised decision-making) is quite different from the trivializing claims (whether in a positive or negative light) made about reflexivity in the sense of individual observers necessarily having partial views and being situated in their knowledge (as if individuals were not operating in larger social and cultural structures), or the equally trivializing claims that evaluations of sources and perspectives are just 'rhetorical' gestures by the researcher. Rhetorical and discursive framings (what gives some rhetorical moves social power) should be recognised as part of what is studied.

2 There is an enormous difference between evaluations of perspective and coverage on the one hand, and lazy assertions that there can be nothing but subjectivity. Subjectivity (and its variants, such as subjectivisation, the formation of a subjectivity through discursive disciplining) is itself a topic for social analysis, and has become a major field of study. The explicit positioning of a writer or investigator in his or her text can be, when carried out appropriately, a productive tool in experimental ethnographic methods, both changing the dialogic field situation and exposing that situation to the reader. Anthropological interviewing tends to be different from journalistic interviewing on two fundamental grounds that are relevant to this discussion. Journalist encounters tend to be short, and often involve aggressive cross-examination. Anthropologists, ideal-typically, tend to develop long and deep knowledge of their field sites

so that they can contextualise particular statements, rants, interventions, and explanations, and they hope to elicit collaborative explorations with insiders rather than catching them out. (Journalists, of course, also cultivate informants over long periods of time, and investigative journalism can, very much like anthropological ethnography, be based on long term engagements.) Using knowledge from informants to play these off against one another can be useful both to get under defensive shields (assumptions that outsiders are naпve and essentially uninterested) and establish a deeper level of information flow. Similarly, juxtapositions of the points of view of partners to a discussion can be of help in clarifying both sides. This is the aim of what in French is called *un entretien*, a popular form of 'interview' in which the give and take between say an neuroscientist and a mathematician, or an anthropologist and a philosopher, is subjected to repeated rewritings by the interlocutors, but presented in a final text as if it had been a spontaneous dialogue. Similarly, there is an important place for the anecdote or vignette where the anthropologist's role serves as a vehicle to illuminate the structure of a social situation, to draw comparisons/contrasts, or to evoke larger stakes than the immediate interaction.

3 Autoethnography has various referents. Superficially it denotes the ethnography of one's own culture. It can also refer to the sociolinguistic study of the interview situation, albeit most usefully mediated by videotape (as in videotapes of doctor-patient interactions used to teach medical students how they may be screening out important information that patients are trying to convey by sticking too close to the established conventions of medical protocols). This second usage employs the word 'auto' in the sense that participants are active in generating the ethnography of themselves. Thirdly, Barbara Tedlock calls autoethnography a *'cultural performance that transcends self-referentiality by engaging with cultural forms that are directly involved in the creation of culture. The issue becomes not so much distance, objectivity, and neutrality as closeness, subjectivity, and engagement. This change in approach emphasises relational over autonomous patterns, interconnectedness over independence, translucence over transparency, and dialogue and performance over monologue and reading. Such once taboo subjects as admitting one's fear of physical violence as well as one's intimate encounters in the field are now not only inscribed but described and performed.'* But there really is not as much of an epistemological change from the perspective I have been outlining above as it might seem: as Clifford Geertz pointed out long ago, *'experience-near'* and *'experience-far'* are relative terms of distance and closeness, and relational terms are what have been at issue since at least the 1930s, when Radcliffe-Brown, following Durkheim and Mauss, emphasised that anthropology was a study of relations, and relations among relations, and particularly that emotions, joking patterns, and other relations were socially structured [Radcliffe-Brown 1940; Radcliffe-Brown 1922]. The conditions for feeling fear and the question of what it indexes, are sociologically, culturally, and psychologically, as important as the feeling described phenomenologically as particular to the narrator.

What is important and crucial in Tedlock's call, first of all, is the return to engagement with public issues (after arguably a period of professionalisation, Cold War caution, and confidence in modernisation theories, when involvement in public debate was, perhaps, less salient than in the era of Tylor, Malinowski, Boas, and Mead.)[1]

Second, her call for performance-linked approaches draws attention both to the emotional and community-building techniques of narrative and oral performance, and to the retooled communication environments of the twenty-first century, where scholarly textuality (in the sense of the traditional forms of self-expression via academic monographs, articles, etc.) is a minor means of public influence.

I would add that there is a powerful need not just for public engagement, but for intelligent, responsible, and powerful, socially aware ways of facilitating public evaluation of media saturation in the form of direct-to-consumer marketing, informercials, blatant manipulations of the political arena, and exploitative popular culture.

Autoethnography has mounted a challenge that goes far beyond the context of researcher and informant.

4 It is usually shoddy work to rely on a single informant or historical source. All sources need to be contextualised, analyzed, put into play against other sources and information flows. The time is long past since we could pretend that we could discover new worlds by finding a particular informant or source. New sources do appear, and can transform a particular researcher's access or understanding, but rarely do they transform general knowledge. Much more important in today's research environment is to recognise that we are always stepping into a stream of prior representations, and not to be naпve about any given one.

For further thoughts on these topics, and examples relevant to them, see [Fischer and Marcus 1999; Fischer 2003; Fischer 2004].

1 Cf. Nancy Scheper-Hughes's contribution to the present discussion. [Editor].

ELZA-BAIR GUCHINOVA

'Native Anthropologist': A Vocation, A Diagnosis, A Destiny

A while ago, someone wrote a reference for me in which they called me a native anthropologist. My immediate reaction was delight: instead of the complications and obstructions I'd been used to as a Soviet *natsmenka*,[1] I'd now be a beneficiary of Western political correctness and be able to exploit the advantages of belonging to an 'ethnic group'. The sense of support from an unexpected place reminded me of the 'affirmative action' policies for non-white minorities in the US. But it didn't take long for me to start feeling embarrassed by being categorised in this way, and I started tuning into my own thoughts and feelings more closely. What do I feel as a 'native anthropologist', and what are the circumstances that make me feel it?

The fact is, that by positioning yourself in this way, you are adopting a strategy characteristic of *natsmeny*, who have traditionally been patronised by their colleagues (from the 'centre of things', i.e. the Russian capitals) — on account of their intellectual (or historical, or cultural) limitations. It's only a short step from here to the token *natsmenka* deputies to the Supreme Soviet, who were supposed to legislate for the entire country, despite hardly being able to understand Russian, the working language of the legislature. 'Native anthropologist' is a put-down: it implies one has inadequate professional standards because of one's rootedness in the native culture, which in turn is seen as a bar to intellectual growth. (As I reread what I have just written, I realise that I'm not sure I agree with it — is this my old, unrecognised complexes at work? My Kalmyk (is that it?) complexes? Am I simply inventing the idea of well-intentioned racism to 'place' myself intellectually?)

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1 *natsmen*, plural *natsmeny*, feminine *natsmenka*: member of a 'national minority', a term introduced under the Bolsheviks and originally meant to be neutral or honorific, but which soon acquired pejorative undertones. See Terry Martin's *The Affirmative Action Empire*. Ithaca, N. Y., 2001. [Editor].

Once I'd been labelled a native anthropologist, then, I tried to exploit the advantages arising from the fusion of professional and ethnic affiliation. It turned out to be very difficult to stop being blasé, and regard any phenomenon in one's own culture as deserving close attention. It was extremely hard to explain things that seemed natural to any bearer of the culture; it was like trying to describe how you breathed. When I finished my book, *Post-Soviet Elista: Power, Business, and Beauty* [Guchinova 2003], I had to reread it many times in the effort to distance myself internally from a culture I thought of as natural, and to introduce more and more explanations of phenomena that seemed obvious to Kalmyks, but not to any other readers. But this was all post factum: while I was actually doing the research, I couldn't achieve distantiation of this kind, and didn't even recognise the problem — that is, I knew about its existence in theory, but I didn't feel in practice that geographical or historical distance was essential in order to get the right focus. But in fact, as colleagues have told me and as I've realised myself, the chapters about the diaspora, which deal with distant material (in spatial and temporal terms) are more interesting, more detailed, and provide more food for thought, than the others.

This summer, I decided to take up the chance of publishing a book on Kalmyk collaboration with the German invaders during the Second World War, and the deportation of the Kalmyks that followed. When preparing the manuscript for publication abroad, I had to distance myself again, this time from Russian and Soviet experience generally, which also turned out to be far from simple: many phenomena of Soviet life, especially during the Stalin era, are hard to communicate in rational language. Not only a foreigner, but also a post-Soviet Russian has difficulty in imagining just how unfree Soviet citizens were, the extent of their social degradation, and of the covert civil war that the Soviet government was waging against millions of its citizens.

At the same time, I tried to bear in mind the sort of accusations that are often levelled at 'native anthropologists', and to avoid idealising my own culture, romanticising its history. I was quite aware that the very subject of collaboration was one a lot of Kalmyks find uncomfortable. *'Is that all you can find to say about us?'* people like this will grumble. They think that if you decide to tell the world about your own people, you should have something 'worthwhile' to say, something your people can be proud of. Why should you 'wash dirty linen in public', bring out in the open a topic that is hard to talk about even at home, something everyone's ashamed of — treason. A professor I know at the Kalmyk State University put it like this: you ought to realise that the whole country will cut you off for ever if you get your facts wrong. He talked about *'getting my facts wrong'*, and I realised that what he actually meant by 'facts' was what would suit the general reader (in Kalmykia). This was his way of telling me to use sources that put the estimates of collaboration at their lowest. My elderly informants also laid down the law to me: *'You can't stop people knowing about the Kalmyk Corps,*[1] *but why should Kalmyks themselves make efforts to let people know about all that when they don't already? The deportation, now, that's something that is worth writing about. Everyone should know about how we suffered, about our losses. But do you really have to mention the work being done by NKVD agents*[2] *among the deportees? That's another black mark on us, after all.'*

But I am myself Kalmyk by descent and I don't want to hide the emotions I feel about all this; they seep through in any case, even into academic discussions. So should I stop working on my own culture, deliberately choose other subjects of study (subjects foreign to me), give my native culture into the hands of other researchers, foreigners, whose objectivity depends on knowing less about the community and being less involved with it? In that case, who is going to study the problems that are important to me and to other Kalmyks at the moment? Where is a researcher like this to come from? I had the opportunity to study certain topics in recent history and I did everything to make use of it, recognising that the *raison d'être* of my work lay in a combination of intellectual competence and scholarly probity.

I studied Kalmyk culture 'at home', in Elista and other areas of the republic, and in Kalmyk communities in the USA, Germany, and France. When I think back to my experience of fieldwork, I realise that both in Russia and outside I encountered difficulties of various kinds, and that I had to behave differently at home and when I was working abroad.

In 1997–8 I was doing fieldwork with Kalmyks in the US. For almost 10 months, I made daily visits to Kalmyk families, spending time with them, socialising with them, trying to associate with them on a footing of equality. I did my best to work seriously, and to come across as serious, and felt that I was 'on duty' more or less whatever I was doing. Because I didn't want to be looked down on as a 'poor relation' from Russia, I tried to dress as well as possible, knowing that clothes and accessories are important in America because they signal professional success. Academics in the US enjoy quite high

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1 The Kalmyk Cavalry Corps was set up in September 1942, after the invasion of the Kalmyk Autonomous Republic by the forces of the Third Reich. It numbered around 3500–5000 at different times. After the Second World War, the collaboration of some Kalmyks with the invader became, as with the Crimean Tatars and the Chechens, the foundation for stigmatisation as a 'traitor nation', leading to savage reprisals and mass deportations. [Editor].

2 i.e. informants etc. working for the NKVD, the Soviet secret police. So-called 'traitors', from enemies of the Red forces in the Civil War and kulaks deported during collectivisation onwards, were a primary target for surveillance work. [Editor].

respect among the population at large, and have more chance to show off their individual personalities; for instance, they can wear more or less what they like, while people working for banks, big companies, or in government, particularly on the East Coast, have to look as if they just walked out of the shop window. In the circles I moved in at Georgetown University, I wore what I would have done at home and didn't stand out at all. But my Kalmyk informants demanded much more, even though their social status was often quite low. They wanted the best for me, and even took a sense of personal pride in seeing a Kalmyk from Russia with a prestigious American fellowship, and so they started giving me advice on my wardrobe. As a relative of mine who has lived in the US for eight years (her husband is a professor at Princeton) advised me, *'You should look as if you've spent around a thousand dollars: a hundred dollars on your shoes, two hundred on your suit, a hundred and fifty on your watch, a hundred on your bag. The rest goes on your jewellery. If the suit is very expensive, you can get away with less on the jewellery. Dress like that and you'll look like someone successful, someone who deserves to be respected.'* Another woman originally from Elista told me, *'You should get rid of that silver jewellery, it's junk. After all, you're nearly a professor, you can surely afford a decent ring, with a diamond in it, say? If you don't want to spend, that's OK, just don't wear that cheap stuff.'* My beloved silver rings got up the nose of the average Kalmyk American in the way that a diamond ring would have got up the nose of my post-Soviet colleagues and informants. At the same time, the two relatively expensive designer accessories I had — some Rayban sunglasses and a Paloma Picasso handbag — were noted by all my Kalmyk American acquaintances, every one (yes!) of whom made it clear that they thought I'd shown good taste there, at least.

Unlike those of my American colleagues who had written about Kalmyk Americans and seen this as nothing more than an academic exercise, my emotions and interests were more complex. I could not stop comparing what I saw with the Kalmyk way of life back in the Russian Federation; whether I liked it or not, I saw myself not just as an anthropologist but as a Kalmyk, a Torgout on my father's side and a Debret on my mother's, a woman from Elista, Russian and Asiatic at one and the same time. And, as I realised later, my gender identity faded into the background and I did my best to act up to traditional role expectations, so as not to alienate the men from the older generation I had to deal with. It helped to be married: the ambiguity in the position of a young woman who was divorced or single would have sent out mixed signals, making my informants wonder what my real purpose in visiting was. There was, of course, no room for flirtation, or any other kind of behaviour that might have been criticised: any hostility evoked by my behaviour would have had an adverse effect on the level of trust that I inspired and the quality of information that I received, and might even have meant that some households and informants were out of bounds altogether. I had the strong impression that a researcher has to hit the happy medium: to be neither rich nor poor, extravagant nor mean, silent nor loquacious, over-bold nor prim. During this long stint of fieldwork professional interests dominated over all others; for a while the ethnologist in me pushed my identity in a general human sense to the margins. What made me realise this particularly forcefully was when a friend of mine, a woman who had been ill for a long time, eventually died, and I started feeling pleased I could now witness an entire funeral, start to finish. To be sure, I was shocked to discover these feelings in myself, and immediately felt very ashamed of them.

When I was writing about Kalmyks in the Russian Federation, on the other hand, the difficulties were of a different order. While I was living in Elista, which is not particularly large (the population is around 100,000), I had a job in a smallish institute, and had to bear in mind the local audience when I was writing up my research. Someone might well be offended that they hadn't been mentioned, someone else take umbrage for precisely the opposite reason, and it was essential to try and keep a distance from the ideological leaders in the republic if one were to have a chance of writing anything impartial.

I left Kalmykia in 1998, and visited afterwards to make trips 'into the field', for instance in 2003. By now I was living in Armenia, and had limited time to spare on visits. I repeatedly rang the local archive to try and find out the hours it was open, scrabbled out some time in my overloaded schedule, and arrived back home in Elista. My plans included work in the archive, the library, talking to experts, making videos — and I had only ten days for all of this. Every day, I'd meet dozens of friends on the streets, and everyone wanted a piece of my time, wanted to hear about my family, my children, to invite me round to visit. These were all people who'd known me since I was a child, who remembered my parents and my grandmother. They wanted to know how I was getting on, but I hadn't made room for seeing them in my timetable. I saved the last evening for my small circle of immediate relations — an hour for my great aunt, two hours for a cousin of my mother's. The presents I'd brought helped to smooth over the offence of not staying longer, but it still wasn't what they'd been expecting. Running away without spending the night or staying up talking till the small hours, hardly drinking anything! — there was no way that was a real visit. This time too, the ethnologist in me won out over the 'ordinary human being', and I managed to offend lots of worthy, kind people, who decided I'd got too stuck-up for my own good.

Having exploited the status of 'native anthropologist', I then grasped that I felt hampered in the research I was doing, that I was threatened by the danger of playing up to this primordial category, which had evidently been constructed by 'post-colonial' anthropologists not only to keep their colleagues on the straight and narrow, but in order to mark out for scholars from the former colonies their inalienably provincial niche in academic life, the intellectual boundaries signified by the geopolitical frontiers of the 'small nation' itself. Why are 'travelling anthropologists', who may not understand the first thing about the foreign culture they observe, allowed to blunder on, without fear of being labelled whatever nonsense they spout? It's not unknown for such people to make free use of the organisational resources, expertise and help with translation of colleagues from the locality, but without considering them equals: after all, academics are representatives of Western scholarship with its centuries of academic tradition, while the 'anthropologist at home' comes from a line of shamans and folk healers. Post-colonial anthropology remains fixated on the West even in the era of post-modernism. And can Russian ethnology even be considered post-colonial?

We used all to think that tsarist Russia was 'a prison of the peoples', and that the role of ethnography had been to help keep down the non-Russian population in the empire and to defend the regime's interests at home and abroad. So what are we to say about Soviet ethnography? What purposes did it serve, apart from increasing the sum of knowledge? Under Soviet power, all the republics put together a cohort of local cadres (who were all native anthropologists, and all institutes in the humanities in the capitals of Soviet and autonomous republics, the former colonial margins, had to have departments studying the ethnography of 'titular nationalities'.[1] Scholars in the republics were supposed to demonstrate the validity of Leninist ideas in their work, and to show that Soviet power had given the toiling people more than traditional culture had given them, or could give them. All in all, this *was* Soviet post-colonialism, contradictory, heavily ideologised, dominated by the centre, with quotas at metropolitan universities and planned numbers of graduate students, with stiffly regulated staffing loads at local institutes, and academic topics vetted out of Moscow.

And what about ethnology and social anthropology in Russia now? It's well-known that the real index of post-colonial revisionism is when non-Western intellectuals become leaders of Western academia. You can think of people at major US universities in this category

1 'Titular nationalities' — *titulnye narodnosti* — a term coined by V. A. Tishkov to signify those peoples of the USSR who were recognised in terms of the administrative organisation of the USSR — as the 'leading' nationalities in Union and autonomous republics, autonomous regions and provinces (e.g. the Azerbaijanis, Armenians, Chechens, Chuvash, etc.) [Editor].

right away — Edward Said, Akhil Gupta, Homi Bhabha, Arjun Appadurai, Partha Chatterjee,[1] but not, at least offhand, of post-Soviet ethnologists from the republics who have been offered work in the centre and turned into intellectual leaders.

As for colonial intellectuals, that type is not only not dead, but positively flourishing in post-Soviet space. For instance, I remember meeting a Moscow specialist on Mongolian culture who couldn't conceal her contempt for Mongol culture and Mongols themselves, and later I saw how she treated Mongols and Americans at an international conference. It might as well have been two people: all hauteur with the people from a weak economy, all affability with the scholars from the superpower.

ZHANNA KORMINA

1 There is reason to argue that it is precisely the attachment to objectivity that distinguishes work in the social sciences and humanities from, say, journalism or literature. The techniques and approaches adopted here are expressly directed towards emphasising the authenticity of the formulations that we set out. Source materials and secondary opinions are cited as testimony to the rightness of our suppositions. This, of course, also makes those we name responsible for our view of things — a process that reveals not only our need to strive for objectivity, but also our lack of certainty that it may be achievable. But I think that belief that objective description is in principle possible is one of the governing conditions for the existence of scholarship as a specific mode of descriptive and analytical practice.

2 Recently, the diary has become one of the most fashionable genres of anthropological writing, not in the sense now of a dry field record of the old sort with historical information about the locality or statistical information, but as a chronicle of inward experiences and feelings, setting out everything that surprises, distresses, infuriates, perplexes and embarrasses the observer. To

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[1] Another example would be Gayatri C. Spivak. [Editor].

put it another way, anthropologists have started to record their own emotional states and peculiar bodily experiences. Being exposed to another culture is like wearing someone else's clothes: one immediately senses the difference between one's body, shaped in one set of cultural circumstances, and its covering, created in another and meant for someone else's physique. The size is wrong, the style doesn't suit, the material brings one out in spots, the smell is disgusting. In a very real sense, the experience of another culture is a psychological fact that becomes susceptible to analysis only after one has felt alienation in a direct and physical way. The diary is an attempt to record this very direct kind of experience. A rather different issue is that the primary sense of shock or indignation can become the subject of analysis in its own right, and thus aid the understanding of the meaning and conventions of the alternative way of life we are plunged into, or, alternatively, can simply inform the final analysis without ever having been worked through, retaining a sense of raw subjectivity which will probably be of an ethnocentric, culturally suprematist kind. Success in handling these considerations depends, it would seem, on certain professional qualities of the anthropologist: his or her powers of observation, capacity for self-awareness and self-irony.

3 If anthropologists adopt themselves as ethnographical objects, the result of the process to a large extent depends on whether they are aware that they are, like Baron Munchhausen, trying to drag themselves out of a bog by their own hair. If they are aware of this, then they usually try to create a springboard (for instance, a command of relevant theoretical issues or of work on analogous topics) that provides for a distancing leap between the self as informant and the self as analyst. The danger otherwise that the resulting text will either display a strange sort of exhibitionism (just look at what I went through and eat your hearts out!) or be an exercise in apologetics (leading to idealisation of the self as object of study). The second result is common among Russian ethnographers working on Orthodoxy, who have shown how scholarship can perfectly well be harnessed to the search for one's own identity (and *perhaps* to self-justification as well), including identity in a religious sense. And indeed, the observer-as-informant does claim a double right to the knowledge of 'higher truth': as the bearer of a given tradition, and as the representative of the corporate values of the academic community. Hence, the fusion of these two figures tends to foster results of an extreme kind: pretentious would-be lyricism on the one hand, and ideological dogmatism on the other.

4 If we reject — as we indeed do reject — a teleological model of an objective form of knowledge existing beyond the 'noise' created by the social identities of those who create it, then we have to recognise that any understanding of materials that is recognised as adequate

will necessarily be one conforming to the rules of scholarly discourse as this operates at the time. Any scholar is constrained in terms of the subject that he or she can choose to study and of the sources, analytical approaches, and conceptualisations that he or she is able to use: in sum, by the governing academic ideology of the day and by the socio-historical context that defines this. Tylor, Malinowski and Geertz all studied the anthropology of religion, and if Geertz's approach now seems most satisfying (i.e. closest to a true understanding of the topic in hand), that is because his work is closest to our own in terms of date. So, when we are talking about an 'adequate' understanding, we really mean one that is satisfactory in terms of current academic appreciations of a subject, rather than in terms of the subject itself.

Sources themselves, as both anthropologists and historians well know, always need critical treatment. In order to minimise 'noise', one must understand how a source was created and what the motives of its creators were, just to begin with. The most important qualities in dealing with sources, especially in the anthropological field, are sensitivity and scepticism. The anthropologist quintessentially has to do with opinions, as expressed in the form of answers to the questions posed in interviews, and has to deal with the huge number of different variables affecting the quality of the information, from the physical condition of an informant to his or her social status. Everyone tells a true story, from his or her point of view. Where sensitivity comes in is in knowing what question to ask at which time, and where scepticism is important is in avoiding giving too much weight to one or other story over another. To cite a concrete example: today's media (a vital source both for anthropologists and historians) puts out a great deal of information about assaults on the human rights of the Russian population in Latvia, particularly with regard to the recent school reforms, which require a shift — of a fairly gradualist kind, to be sure, but compulsory none the less — to universal teaching in the Latvian language. However, when a Latvian colleague and I did fieldwork this year in an area of Latvia lying close to the Russian border, where the Russian-speaking population stands at around 80 per cent, we found no sense among our interviewees that their human rights were being assaulted, or even of frustration or dissatisfaction with regard to the issue of teaching in Latvian. Large numbers of Russians living here are bilingual, and all of them are proud of their children's successes in learning Latvian. The local folk music group, which sings *chastushki*[1] in Russian about Latvia's entry to NATO and the EU, gets invitations to festivals and celebrations of all kinds. The people that we talked

[1] i.e. short humorous rhyming ditties: see also Svetlana Adonyeva's article in *Forum* 1 (2004). [Editor].

to, ordinary villagers, are happy to be Russian in Latvia. So who's telling the truth? Maybe everyone is. The point is that there are different 'truths' depending on which social group you belong to, and a scholar should ideally preserve neutrality, not wholeheartedly endorsing any one version. Though, to be strictly honest, I have to say that I find the attitudes of the border-zone dwellers, their dynamic understanding of identity as socially constructed, a good deal more to my taste than the clamouring of the Russians in the capital (Riga), with its overtones of unworked-through imperialism.

VLADISLAV KULEMZIN[1]

1 I think any researcher — down to the level of students getting their first experience of academic work — has claims to being objective. That is where the interest of any discussion lies. An excellent example of how this works is the oral examinations relating to fieldwork that were held at the Moscow Institute of Ethnography in the 1960s, 1970s, and 1980s. Almost every discussion would end with a debate on the importance of methodology. Things were simpler in those days, though: there were set views, an accepted perception of kinship structures and of totemism. The work of many researchers was out of phase with these established ways of looking at things and thus appeared not to be 'objective'.

Now, things are more complicated, but also more interesting. We have laid bare logical weaknesses in the work of classical Russian and Soviet ethnographers, and an infinite variety of kinship structures, stages of social development, and forms of belief, and of interaction between them, has come to light. Hence the question of objectivity — the objectivity to which every scholar has claims — has begun to be of interest and value to academic discussion in its own right. We may be referring to 'objectivity' as an ideal, but the ideal has importance because reality may be compared with it. We can already

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[1] The author did not respond to an invitation to comment on the translation of this article. [Editor].

see how ethnography is ceasing to be an empirical discipline. Eventually, generalising theories that will allow particularities to be explained will come to light.

All of this is happening thanks to the opportunity of comparing materials and facts, which has made clear what approaches have no future. It is new facts, not shifts in intellectual authority, that make researchers change their minds and espouse views they formerly opposed. It's less of a blow to the self-esteem that way.

As for the dialogue between the ethnologist and the informant, and the material collected in general, then these always were, and still remain, something separate from the researcher's conceptual principles. Another person working with the same materials may easily set up a diametrically opposed schema of cultural evolution.

I think that the appeal of the material that informants give us is explained precisely by its breadth and the difficulty of pinning it down. *'There were fewer folk here, back then, and more fish* [...] *folk*[1] *didn't buy anything, they grew everything themselves* [...] *they told the future by reading their dreams, they prayed to idols, they made their own medicines.'*

It's clear that no-one would be able to conceptualise anything by moving directly from information like this. One has to decode the material, to supplement it, working as a doctor does when he or she explains to patients what their cardiogram means.

And it's here that we rely on statistics, sociological surveys, published data of various kinds, folklore, and so on — which, to our surprise, will usually inform us that there were in fact remarkably few major economic, ecological, and demographic changes over the period in question.

But what can we describe as having happened if, in the 1960s, the informants were delighted to tell us all about their lives, displaying complete friendliness and trust, while, in the 1990s, the children of those original naïve informants dog new arrivals' footsteps and, given the slightest opportunity, rootle round in their travelling bag to see if they can find any vodka? What did we miss first time round? I think we *weren't seeing the wood for the trees*, and hence missed a very important topic: human life in our own culture. The study of this material is likely to prove extremely productive, the more so since it is the starting point for a fusion between *etnografiya* and *kulturologiya*.[2]

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1 Unfortunately, English cannot retain an ambiguity in the Russian verbal plural, which could also mean 'we'. [Editor].

2 The term *kulturologiya* is almost impossible to translate into English adequately. Given that *etnografiya* in the late Soviet period was counted as a branch of the 'historical sciences' (see the contribution by Tatiana Shchepanskaya below), and that studies in this discipline were

3 When the peoples of the far North and Siberia entered into the domain of contemporary ethnographical knowledge, the question of whether it is possible to fuse the discourse of informants and investigators was answered in a direct and positive way. Tomsk University Press published a whole series of collections under the title 'The Peoples of North-Western Siberia', many articles in which were written by authors {from those peoples}[1] who had completed their candidate's degree and were working on their doctoral degree.[2] Over the last few decades, the study of Siberia has been enriched precisely by the fact that much information that was hidden from earlier figures such as K. F. Karyalainen, A. M. Kastren, V. N. Chernetsov, or V. V. Radlov, came into the open and became accessible for any researcher. The contribution of people with a knowledge of two cultures made a major contribution to this process.

Max Müller once made an observation that still holds good today: *'A person who knows only one culture knows no culture.'*

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Keeping Hope Alive in Anthropological Research

My contribution to the roundtable discussion concerns what I believe underlies anthropological research and perhaps academic knowledge more generally, what makes it possible, and what it ultimately seeks to generate — hope.

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Recently, many anthropologists have drawn at-

often concerned with 'traditional culture' (and based on material from the late nineteenth and early twentieth century), there was not really a place here for work on the present day. On the other hand, *sotsiologiya* tended to refer to work dominated by Marxist-Leninist conceptual paradigms. Hence, the term *kulturologiya* came into use to describe what in Britain or America might have been known as 'ethnography' or 'sociology'. It could also refer to a very loose kind of cultural history addressing, say, the role of the mirror in world culture since the dawn of time (cf. the comments of Evgeny Golovko to the Congress Forum at the end of the present journal). Since 1991, on the other hand, *kulturologiya* has been used as a translation for 'cultural studies' — an academic area more accurately rendered by the recent neologism *kulturnye issledovaniya*. See also Catriona Kelly, Hilary Pilkington, David Shepherd, 'Introduction', in *An Introduction to Russian Cultural Studies* (Oxford, 1998). [Editor].

1 The passage in curly brackets has been inserted in the translation for the sake of comprehensibility. [Editor].

2 Unlike the academic system in Britain or the US, but like the system in France and Germany, the Russian academic system recognises two classes of higher degree. The candidate's degree is roughly equivalent to the Ph.D (though dissertations are usually less substantial than those expected at major US and British universities), while the doctoral degree is awarded to senior academics (normally in their mid-40s or older), and is comparable with the German Habilitation or French *doctorat du troisiume cycle*). [Editor].

tention to the subject of hope. Sarah Franklin, Mary-Jo Del Vecchio Good and others have examined the discourse of hope surrounding medical professions and practices and its articulation with, and disjuncture from, patients' experience [see Franklin 1997; Good 1990]. Katherine Verdery has discussed the emergence of a discourse of hope in the context of Romania's transition from socialism to capitalism [Verdery 1995: 654–5]. Ghassan Hage has proposed to approach societies as *'mechanisms for the distribution of hope'* [Hage 2003: 3] in his critique of neo-liberalism and global capitalism. Arjun Appadurai has introduced a notion of *'capacity to aspire'* in his effort to redefine culture as a future-oriented concept in the context of development studies [Appadurai 2004].

As Vincent Crapanzano has recently noted, hope triggers a series of analytically complex questions. What is hope, as opposed to other more familiar categories in social analysis such as desire and fantasy? [Crapanzano 2003: 19–20]. To what extent does the category of hope reflect the common usage of that word in the English language? What are the implications of this linguistic specificity for the category's applicability in other linguistic contexts? [Ibid.: 11–4]. All these questions point to the slippery nature of the hope category. More importantly, for present purposes, what is the relationship between hope as a research object and the hope that underlies academic research? [Ibid.: 25]; see also [Miyazaki 2004].

Roy Wagner has observed that *'an anthropologist is someone who uses the word "culture" with hope — or even with faith'* [Wagner 1981 [1975]: 2].[1] Underlying Wagner's statement is anthropologists' now long-established commitment to demonstrating the creativity of the people they study and to seeing the hopeful content of culture in its association with a notion of multiplicity as a locus of human potential. Anthropologists have documented multiple possibilities and alternatives in actors' resistance to the process of globalisation, their competing claims to political legitimacy and their debates about tradition and modernity. In other words, anthropologists' task has been, at least implicitly, to document their research subjects' hope as a source of anthropologists' own hope for change.

I wish to suggest that the currently emergent interest in hope as an analytical category in anthropology offers an opportunity to make explicit the hope latent in anthropological research and to bring critical scrutiny to the question of how we keep that hope alive. In my own work, I have examined indigenous Fijians' efforts to *replicate* hope across different kinds of knowledge practices ranging from gift-giving to petition writing, Bible study and business management [see Miyazaki 2004]. In particular, hope emerges as a focal

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1 I thank Matthew Engelke for drawing my attention to this passage.

theme in a ritual moment in indigenous Fijian gift-giving in which gift-givers wait for gift-receivers' evaluation of their gifts. This moment of waiting, which I have called a moment of hope, is predicated on a balance between gift-givers' sense of expectation for a favourable outcome of their ritual action and their willingness to submit themselves to radical indeterminacy as to whether gift-receivers will indeed accept the gifts. As soon as gift-receivers accept the gifts, however, they immediately offer the gifts to God and create a further moment of hope. I have argued that in this way indigenous Fijians keep hope alive by replicating hope on a new terrain.

In the discussion, I juxtaposed this replicative propensity of Fijian hope with the Marxist thinker Ernst Bloch's philosophy of hope. In his book, *The Principle of Hope*, Bloch famously introduces the category of not-yet as the engine of hope. In his view, what he calls the not-yet consciousness allows hope to replicate itself despite repeated disappointment. More importantly, Bloch draws attention to the incongruity between the future orientation of hope and the retrospective orientation of philosophical contemplation and proposes to make philosophical investigation resolutely open-ended. In other words, Bloch's turn to hope as an object of investigation effectuates a temporal reorientation of his philosophical investigation to the future. I have suggested that this reorientation of philosophy is Bloch's own effort to replicate hope, the very object of his philosophical investigation, on another terrain, that is, the terrain of that investigation. In this sense, Bloch's investigation is a replication of the replicative propensity of hope.[1] In juxtaposing Bloch's hope with Fijian hope, I have argued that the production of hope for both Bloch and the Fijians I studied is predicated on the replication of its own method, that is, its own replicative propensity on a new terrain. In this context, my work surfaces as a replication of their hope on yet another terrain, that is, the terrain of my own investigation.

It is interesting in this context that there is a similar effort in the field of counselling to theorise and even instrumentalise this *replicable* quality of hope. The psychologists Wendy Edey and Ronna Jevne have recently suggested that *'Hope in the context of counselling is relevant to both clients and counsellors'* [Edey, Jevne 2003: 44]. Jevne and a group of counsellors affiliated with the University of Alberta's Department of Educational Psychology and the Hope Foundation of Alberta have developed what they have termed *'hope-focused'*

[1] Terence Turner has examined a Kayapo video of a Kayapo ceremony as a replication of the *'replicative structure of the ceremony itself'* [Turner 2002: 83]. Turner's focus is on the Kayapo's *'cultural value of replication as beauty'* [Ibid.: 83] whereas my focus is on the implications of replication as a more general operation for the problem of how to keep hope alive [see Miyazaki 2004; Miyazaki, in press].

counselling. According to them, *'Both client and therapist must possess hope in order for the therapeutic process to be successful'* [Ibid.: 46]. In contrast to the psychologist C. R. Snyder's influential goal-oriented conception of hope, Jevne and others *'emphasise the value of acknowledging hope, and de-emphasise its relationship to realistic goals'* [Ibid.: 46; emphasis added]. In other words, in their counselling practices, they seek to *'make hope visible'* [Ibid.: 47] not only for their clients but for themselves. Edey and Jevne suggest that counsellors bring up the subject of hope in their sessions with their clients. In particular, they suggest that by asking clients questions about hope, counsellors *'draw attention to hope'* and *'persuade clients to think and talk about hope'* [Ibid.: 47]. In Edey and Jevne's view, this is a doubly productive process because clients' even slightest interest in the subject of hope in turn generates renewed hope on the part of counsellors [Ibid.: 47]. The emergence of hope as an object of reflection prompts a creative collapse of distance between hope as counselling's desired outcome and the hope that keeps that counselling going.

My attention to the collapse of distance between hope and analysis in the context of the present discussion about the relationship between researchers and research subjects in anthropological research is not accidental. In recent efforts to redefine and reinvigorate ethnographic research, Donald Brenneis, Douglas Holmes, George Marcus, Bill Maurer, Annelise Riles, Marilyn Strathern, and others have studied those knowledge practices and artifacts that resemble or are anthropologists' own such as accounting and auditing practices [Maurer 2002; Strathern 2000], funding applications and recommendation letters [Brenneis 1999], networking and network analysis [Riles 2000], means-end reasoning [Riles 2004], and ethnographic research itself [Holmes, Marcus 2004]. Many of these anthropologists have begun to take the collapse of distance between themselves and their research subjects as an opportunity to refashion ethnographic research without recreating analytical distance [see, in particular, Holmes, Marcus 2004; Maurer 2002; Miyazaki, Riles 2004; Riles 2000, 2004]. In my own work, I have sought to join these efforts by examining the slippery and yet productive relationship between hope as a research object and the hope that underlies anthropological research as yet another kind of collapse of analytical distance. This juxtaposition of two different kinds of collapse of distance in turn is intended to bring to light the hope that underlies those efforts to redefine anthropological research [see Miyazaki 2003, 2004].

The ultimate purpose of my attention to hope, then, is not so much to point to hope as a hitherto under-theorised research *object* as to propose to take hope more seriously as a *method* of academic research. David Harvey and others have recently lamented the pervasive sense of lack of hope in social theory and have developed an argument for the importance of reclaiming the category of hope for the future of progressive thought and politics [see, e.g., Harvey 2000; Zournazi 2002]. I believe that the time is also ripe for anthropologists to give some serious attention to how we keep hope alive.

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1 So far as I'm concerned, striving towards 'ob*jectivity in terms of the description and analysis of cultural phenomena'* (i.e., in the final analysis, striving towards the truth itself — however much one may hedge about these two concepts) signifies something far more than an empty phrase. I'd give up academic work altogether — or at any rate, in the form in which I've devoted my life to it — if I thought that it amounted to no more than a *'rhetorical strategy'*. I firmly believe that work in the humanities is something much more than a *'glass bead game'* or an exercise in mental gymnastics: it's a form of productive labour with its own purpose and its own 'end product', which is nothing less than the constantly increasing sum of human knowledge about the world and about humanity. But if we are going down that road — why shouldn't we term mathematics or crystallography, biology or astronomy, a *'rhetorical strategy'* too? A cultural artefact exists in objective terms in exactly the same way that the objects of study in the natural sciences too; it is equally open to analysis, by exact methods if needs be (structuralism, semiotics, statistics, and so on), and such methods generate perfectly satisfactory results. I don't see how 'the humanities' are to be differentiated from 'the sciences' at this level, and indeed, the whole distinction seems to me obsolete, or at the very least inexact — the map of human knowledge now looks much more complex and ramified than the old opposition suggests.

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If we're talking about objectivity with regard to research materials, then again I don't see why disciplines studying 'cultural texts' (in the broadest, semiotic meaning of the term) are different from other human sciences — from anatomy to psychology. Who decided that human beings can be studied objectively by physiologists, but not by specialists in culture? More generally: where do *'objective description'* end and *'rhetorical strategies'* begin?

If we're talking about the anthropologist's necessary impact on the material that he or she collects, the fact that distortion is inevitable and that this can be the subject of analysis in its own right, then I'd agree, but only up to a point. Of course anthropologists working on cultures to which they are strange are bound to have an intrusive effect, and one that, above all, concerns the areas of most interest to them (the very expression of interest is enough to ensure that). In electronics, this type of phenomenon is called 'operator's hand effect'. When the operator raises the sensor to a particular point on the screen, the action simultaneously makes minute changes to the electronic parameters in the sequence being recorded.

It stands to reason that the authenticity of formal laments, spells and other texts that are dictated to folklorists by order, as it were, is highly questionable (the same applies to material whose functional status is less clearly defined), and that any collector should bear this in mind (as collectors generally do in any case). Of course, any recordings of folklore represent living tradition in a limited sort of way, but the fuller and more meticulous they are, and the more careful the attention that is paid to contextual factors, the more valuable the representation will be. Particular 'coefficients of distortion' need to be evolved here, as they have been to calculate 'operator's hand effect'. But aren't they in any case in use when folklorists discuss materials from the field and editions of these?

And besides: a frog stretched out on the zoologist's table, or a microscope slide with a tissue sample on it, bear a far from straightforward relation to the normal state and function of the organisms concerned. Today's scientists obtain most of their data in laboratory conditions, but that should not put into question the objectivity of the results that they obtain, and lead one to consider the description of them nothing more than a *'rhetorical strategy'*.

As for *'control* [of] *the circulation of knowledge'*, I'm not sure I have any idea what that might be with regard to anthropology (or for that matter other branches of the humanities). I can see how this sort of question might be relevant to law, medicine and other disciplines with a practical dimension, the practice of which really can be a route to power and money. But so far as ethnography, folklore, and literary history are concerned, then *'control of the circulation of knowledge'* ceases to have any meaning in a real sense.

Enough half-baked philosophy and inaccurate paraphrasing of Foucault: let's get back to real life. Of course one can understand any act of communication, any dialogue (for instance, of a man and a woman, an older person and a younger one, an adult and a child, etc.) as a *'manifestation of power relations'* (after all, almost any real interaction displays a power asymmetry of one kind or another); but equally, common sense tells us that seeing such interactions purely in those terms would be an over-simplification, reductionism of an unhelpful kind. It would be an equally gross over-simplification to understand the collection of material by anthropologists in this way. Of course we ought to remember that the collector always remains 'other' in the informant's eyes (the reverse is also of course the case); but this relation of 'otherness' does not necessarily have to be associated with a power asymmetry and to have a negative impact on the interviewee's readiness to speak openly. Sometime people speak more freely to an outsider than to someone from their own circle. And in reality fieldwork spans a huge range of possible variants: from different forms of bullying the informant (or buying information from him or her), to collaborative work on the basis of solidarity, or indeed friendship.

Much the same could be said of *'control of the circulation of knowledge'*. Once again, I wonder what on earth this could mean, practically speaking. Fair enough: I have come across cases where people keep what they know even from their colleagues, or take a haughty attitude to 'non-initiates', but on the other hand I've equally often observed a readiness (and a desire) to spread one's knowledge as widely as possible, including well beyond professional circles. If *'control of the circulation of knowledge'* applies only to the first two kinds of reaction, then we're dealing with a personal thing (and with a form of behaviour that isn't even all that common); if it means all the forms of behaviour mentioned, then frankly the whole question loses any meaning. I really do think that the primary and lasting motivation for any scholar is disinterested curiosity and not *'control of knowledge'* or the satisfaction of power impulses of any kind.

On the other hand, each to his own, as the saying goes. I'm very happy for *'rhetorical strategies'* and *'control of the circulation of knowledge'* to be left to those who think that's what scholarship's all about.

2 I think initiatives of this kind are, frankly, generally counter-productive — for my reasons, see above. So far as I can see, this kind of approach pushes the academic article too far in the direction of a literary essay. The usual criteria of demonstrability go by the board; the possibility of obtaining responsible, verifiable knowledge is discarded, and the door is open to caprice on the part of the researcher, so that it becomes all the same whether one argues that

2×2=4 or 2×2=5 ('well, after all, *I* think it does'). I wouldn't want to reject the possibility of cautious use of introspection as an (occasionally) useful analytical strategy, but I'm not sure that the question had that in mind.

3 Up to a point, the fusion of researcher and informant is inevitable — in the case, say, of work on contemporary urban folklore, any researcher will also be a bearer of the tradition under analysis, and any collector will also be an informant.

As I've written elsewhere, this is a situation where the principles of collection and publication are in a grey area [Neklyudov 2002; Neklyudov 2003]. Still, we can regard them as authentic in the case of bearers of a given culture, who certainly have the right to vary the texts they transmit at will. But experience of studying a culture that is so close to one's own (which to all intents and purposes *is* one's own) essentially underlines how important it is to distance oneself from one's material, to switch to the position of 'external observer', of 'the outside position', without which the objective study of the subject in hand would be scarcely possible . I'd like to emphasise once more: no matter how important it is to immerse oneself in a given culture, to find common ground and a shared language, one still has to remember that 'participant observation' has strict limits, and that even the most intensive immersion can only go so far. The observer and the object of observation can never be completely identified, whether in a psychological or a pragmatic sense. Hence the fact that the exteriorisation of the subject can be as important in methodological terms as the interiorisation of 'the other'.

4 I think that the answer to this question depends on how the 'lens' of the observer is set up and what it is focusing on: something of personal or general interest. In the first case, we are dealing with something ephemeral, specific, unrepeatable (and of potentially widely varying significance — from the level of the individual text to that of an entire national tradition). In the second, the material in view is general, typical, universal. The potential for uniqueness requires the existence of variety — of a diversity of cultural understanding, multiplicity of structural configurations, while universality demands schematism, predictability of structural configurations, the replicability of a given unit. The personal is highly specific, the universal has an underlying sameness.

If we are dealing with a case of the first sort, the individual motivations of the informant or the author of a given text should not fox the researcher — they are an essential part of what he needs to analyse. In the second case, the text has to be placed in a series of typologically homogeneous phenomena, and therefore any individual characteristics will be placed against the background of the text's overall structural schema, and emerge either as systematic variations requiring analysis in their own right, or as fortuitous details that have no relevance to the problems in hand.

It follows that one and the same text can constitute a different 'research object' with reference to different analytical ends, and that these ends also shape the issue of what is meant by an *'adequate understanding'* of the text concerned. I don't think that such an understanding exists beyond the field of enquiry constituted by those analytical ends. The next issue is the language one might use for the analysis itself, but so far as I can see, that's also something lying beyond the purview of the questionnaire as framed.

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1 I think it's likely that most people engaged in scholarly or scientific work believe that the results of their analytical endeavours do reflect the state of things existing beyond their own immediate experience — which is to say, objective reality. To be sure, the selection of any object of study is the result of a set of self-imposed constraints. The transformation of the *object* of study into the *subject* — in the sense of the target of research as defined by the methods used, the conceptual apparatus, and the task in hand — inevitably (in any discipline) intensifies the influence of the observer on what is being observed. And naturally, the type of knowledge being produced is different in the exact sciences and in the humanities: if the first are concerned with establishing laws (within the object of study — the natural world), the second are preoccupied with general directions and tendencies.

In all branches of knowledge, the answers we get are predetermined by the questions that we ask (of nature, of a human informant). As I have discussed in some of my other publications, the interest in archaic culture among Russian ethnographers and folklorists, and later among ethno-linguists as well, was driven by a view of the Russian folk as essentially pre-Christian in terms of its belief system, a view expressed also in the countless questionnaires drafted to gather

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material in a concrete sense. But on the other hand, historians and archeographers working among Old Believers, and devoting themselves primarily to written Christian tradition, depicted the Old Believers as the bearers of a pure, 'age-old Orthodox Christian' faith, which faith was held to determine their world-view and their way of life down to the smallest detail. Obviously, both the first and the second approach are one-sided: more recent study has shown that the very conservatism of the Old Believers meant that their belief system preserved much pre-Christian material, while, on the other hand, among the majority Russian population, popular Orthodoxy, far from being rooted out under Soviet power, still preserved -— albeit in starkly reduced form — the core system of Christian concepts: sin, punishment (divine retribution), repentance, prayer. Yet I would imagine that even where approaches to fieldwork differ, some of the same questions are likely to be asked: the answers to them will illuminate 'objective' distinctions between groups. It is fair to say that any comparative study of cultures (and languages) based on an integrated methodology has a greater chance of being 'objective' than a unidirectional (non-comparative) study.

There are a fair number of methods that increase the likelihood of 'objective' results. For instance, a classificatory system once applied to one set of problems turns out later to be helpful in the case of another set as well: this suggests that something in the nature of the object may have been correctly identified — as with the functions analysed by Propp,[1] for instance. In my own experience, this kind of insight was afforded in the course of a lexicographical description first of scholarly terminology, and then of the language of folklore. In this description, an entry in a dictionary represented something akin to a questionnaire, with each point dealing with semantic links that occur regularly in a given type of text. The 'answers' to the questionnaire not only make explicit the semantics of the headword, but make it possible to group them according to semantic types in quite a sophisticated way, according to the way that the points in the questionnaire are addressed. More generally, a new theory or conceptualisation that is richer in descriptive terms than a previous theory or conceptualisation will also be more likely to facilitate objectivity.

As for the juxtaposition 'subject-object' itself, then the link between the two, and, in the final analysis, the results of the investigation, will depend not only on the way we define the latter, but also the way we define the former. If the subject thinks of him- or herself only as the author of a given study and the bearer of a set of conceptual presuppositions, but not as the vessel of evaluative principles (of an

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1 Notably in *The Morphology of the Folktale* (1928 and many subsequent editions). [Editor].

ethical or whatever other kind), then manipulation of the object under study (or indeed deception of the informant) in the cause of generating new material of a kind held to be useful by the information-gatherer becomes routine: a subject may be filmed with a hidden camera, taped secretly without having given permission, and so on. All of this is met with commonly enough in fieldwork. But if the subject thinks of the informant he or she is engaging with as equal in dignity to him- or herself, then the use of such techniques becomes impossible; new, and sometimes valuable, information is withheld from scholarship on grounds that have nothing to do with scholarship. Personally, I belong to the second kind of researcher. Last year, I would have had the chance to make some unique recordings among the Molokane-Pryguny[1] if I had ignored their requests not to use a tape-recorder and not to write anything down in my notebook. I did as they said, even though it would have been easy enough to have made a recording anyway. Whether one can hold productive discussions about this kind of issue, I don't know, but I am convinced that it is essential to develop binding codes covering the relations between researchers and informants.[2]

2 'Adjusting' or distorting the object of study is unavoidable, and the researcher should try, if not to minimise, at least to reflect and record the process. Alongside the methodological preconceptions of researchers, distorting factors include the information of a general kind (evaluative principles etc.) they bring with them to the process of observation, and also the concrete ways they collect and analyse material. Thus, the notable impoverishment of Old Believer libraries and the oral tradition of spiritual songs has many causes, but these include the assiduous collecting activities of archaeographers, who in some cases did not even have the decency to supply photocopy duplicates of the precious books and manuscript verse collections they took away, which were an essential psychological support for members of the community who wanted to sing the texts included

1 The Molokane-Pryguny are an Orthodox sect — a branch of the Molokane, or 'milk-drinkers', so named from their tolerance of milk products on fast days — with some resemblance to Protestant sects such as the Pentecostalists: they believe that the Holy Spirit speaks through 'righteous men' (*deistvenniki*) and prophets. [Editor].

2 Such codes have, of course, been in circulation in the US for at least three decades, and are well-developed in the UK as well. They cover not only the need to acquire full consent for recordings, but also issues such as what may be done with transcripts afterwards (in terms of archiving etc.), the need to ensure anonymity in publications, and so on. Yet the paradox is that journalists — whose behaviour may be much more intrusive, and who are much more likely to resort to methods such as secret filming — are not subject to regulation by such codes; those who feel that their confidence has been violated have to resort to complaints (with reference to violation of general privacy legislation, etc.) after the fact. On the one hand, clearly the objects of journalism should also be entitled to protection on ethical grounds; but on the other, discussions about 'informant rights' should perhaps be balanced by some degree of sensitivity to issues of freedom of information, expansion of knowledge, and so on [Editor].

in them. Going to the other extreme, one could mention the efforts among folklorists to bring back to life lost traditions among the bearers of a given culture, or to showcase these through the organisation of performances by professional or semi-professional song and dance groups. To be sure, in this second case, unlike the first, one is dealing with experiments involving a level of co-operation and 'mutual aid' between researchers and those whose culture they study, and I think that they certainly have some value.

I'm not sure I follow the part of the question that refers to how *'the academic commentator may attempt to insert him- or herself into the text of "the other"'.* One might understand this in two ways. The first would be the composition of some kind of scholarly-popular text,[1] on folk culture, for instance, where the experiences of the researcher would naturally get blended in, but without becoming part of the subject of study as such. The second type of authorial presence would be of a much more complex kind, and has recently been the subject of a good deal of discussion — here, one is talking about the hermeneutic method in scholarly description. Unfortunately, though, while I've seen plenty of manifestos for this method as applied to folk culture, I'd never seen a detailed description of how it works, and certainly never seen anything that might pass for serious results of its application.

3 The case of fusion of informant and researcher in one person is a special one. This type of fusion is well known in linguistics by the term 'introspection'. In the case of other disciplines (ethnography, folklore studies), a fusion of this kind has productive consequences if the bearer of culture concerned has the credentials for informed scholarly analysis in his or her own right, i.e. is in command of its conceptual instruments and its metalanguage, and is capable of transcending the bounds of the culture under study. Indeed, in this case he or she will probably be in an advantageous position vis-à-vis ordinary researchers, who quite often don't have the full range of empirical information at their disposal. But if the author of an anthropological study deals with material from many cultures and is of a theoretical bent, then the question of fusing researcher and informant obviously falls by the wayside.

There is a situation where the issue of fusion takes on especial significance and is perhaps, in a final sense, unresolvable: when one is studying the culture of some religious group. Even if one sticks to

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1 'Scholarly-popular' is hard to translate adequately into English. The term refers to a short book on an academic subject (science, history, etc.) written by a specialist, but for a general audience, and without much in the way of technical information, referencing, etc. If such a book appeared in English, it would probably simply be referred to as 'a popular book' or (among the pretentious) 'a work of *haute vulgarisation*', but the Russian term is not as dismissive as either of these [Editor].

an aspect of this culture that is not directly related to the essence of religion (the language or folklore of such a group, for instance), then religious dogma, rituals, the prohibitions that are generated by these all have to be studied in depth — in other words, one has to have a full view of the world-picture of the group concerned. Many years ago, when I had just begun studying Old Believer culture, I was staying a few days in the home of an *ustavshchitsa*[1] in one of the 'priestless' (Pomorye)[2] Old Believer villages of Verkhokamye. I had already grown accustomed to the fact that my hostess would not drink the water that I brought from the stream, which was left to me (and other 'worldly' people[3] — and the cattle, that my 'worldly' cup and spoon was allowed to occupy only the section of table divided off from the rest by a hand-towel, and that any uneaten pieces of bread placed on my side of the table could only be given to the cattle. But I was astonished to realise that the hostess and I were able to share a knife. Why? *'Nothing sticks to a knife,'* the hostess explained. This was totally unexpected, so far as I was concerned; and how much else may have stayed beyond my ken as well!

Studies of religious groups encounter two kinds of restrictions. The first is superficial — the prohibitions imposed on the researcher by members of a given culture. Experience of work with Old Believers, Molokans and Dukhobors has shown how difficult it is to 'get through' to a culture that has traditionally defended its inner core from the prying gazes of outsiders: this is clear both in answers to questions the researcher may ask about aspects of religious belief and about different acts of worship (for instance, conversations about prophets that I had with Molokan-Pryguny went like this), and in the bans on audio- or video-recording (as mentioned already), or in bans even on attending acts of religious worship. For instance, in June 2004, the *ustavshchik* in a certain Old Believer village in South Siberia firmly refused to let any of a visiting group of researchers attend the prayer meeting being held on the Feast of St Peter and Paul.

The second consideration is the question facing anyone analysing a given religious culture who does not come from that culture (or perhaps is not religious at all) about whether he or she is capable of describing it meaningfully. There is no clear answer to this question

1 Among the 'priestless' Old Believers (see below), the name *ustavshchik* (masculine), *ustavshchitsa* (feminine) is given to a person who knows the order of service and is able to conduct the Mass. [Editor].

2 'Priestless' Old Believers, as the name suggests, are a sect where there is no hierarchy, and worship is organised by community leaders (as among the Quakers and some other non-conformists in Britain and America). Pomorye, in Northern Russia, was where many such groups originally settled in exile. [Editor].

3 *Mirskie* — people who do not observe the prohibitions incumbent on members of the Old Believer community. [Editor].

either. However, it is significant that the members of a given religious culture are almost always certain that describing their faith properly without feeling it in one's soul is not possible. Indicative in this regard is my interaction with Molokan-Pryguny from the Caucasus (Armenia). In explanation of why they wouldn't let me make recordings, they said, *'If thou wants to understand our faith and take it to thy heart, come to us, we will tell thee all thou needs to know, we will sing our songs to thee, and thou wilt record all this in thy heart. What need then for paper, or a tape-recorder? But if thy intent is to write of our faith for others, not having faith in thy heart, why then nothing will come of this in any case, thou wilt turn it all into a mess, and so it is not right thou shouldst record our service.'* By no means all the Molokans reason in this way (the so-called 'fixed' Molokans actively want information about their beliefs circulated), but similar attitudes are also found among some priestless communities [soglasiya] of Old Believers (for instance, the Chasovennye). In this sense, a description of the religious essence of a group written by a member would be more acceptable for that group itself and, possibly, also more representative from a general point of view.

4 The individual characteristics and motivations of the creator of any anthropological source — the informant — are inescapably present, and become part of the subject of study. If a survey embraces a large number of informants, then answers may be broken down according to the types of characteristic one is dealing with (this is essential to any approach that pretends to objectivity), and subjected to statistical analysis so that data about typicality, averages, etc., can be generated. But there is no cause to be alarmed by the presence of idiosyncrasies: these should be recorded as such and addressed by the analysis of the data.

SERGUEI OUSHAKINE

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1 Whether representational practices can actually fulfil their promises of 'capturing' for the viewer or reader facts, phenomena, events or experience, access to which may for various reasons be problematic, has rightly become one of the central issues in discussion within the humanities over the last 20–30 years. The hypothesis that a crisis of representation has occurred has laid bare the pointlessness of any striving on academic observers' part to set down an 'impartial' and 'adequate' description of what they witness, given that any description is to a signif-

icant extent pre-determined by the observer's own intellectual biography. Debates about this 'crisis of representation' have also drawn attention to the fact that the mechanism by which the practices of everyday life are transformed into discourse is to a large extent determined by the very process of *description* itself [see e.g. Clifford, Marcus 1986; Fischer, Marcus 1986]. Among the various conclusions drawn by the advocates of the 'crisis of representation', the most cogent, to my mind, are those which emphasise the existence of a qualitative rupture, a gap, a lack of correspondence between experience and the ways in which such experience is represented, between 'the symbolic' and 'the real' (see e.g. the work of Julia Kristeva). In this framework, the 'description and analysis of cultural phenomena' is always a mechanism for the transformation of these phenomena, a process of reworking events in terms of an accessible language — i.e. one that has been fully internalised.

Certainly, this insight still has its opponents, and the two aspects set out in the question — the employment of objectivity as a rhetorical strategy, and the gesturing towards objectivity as a manifestation of corporative identity — continue to make themselves felt in the practices of the humanities today. I would, perhaps, add that in contemporary Russia the striving to *'control the circulation of knowledge'* plays a considerable role, not now as a result of political censorship, but as a consequence of established institutional practices. What I have in mind, in the first instance, is the editorial process, i.e. the routine procedures through which texts are selected, published and reviewed, and thus accorded academic status, the weight of an academic norm. The organisational and institutional circumstances that might unproblematically stimulate the formation and development of new approaches — and new versions of 'objectivity' — are to a significant extent hampered by the conditions prevailing in today's Russia. For instance, the number of professional journals in the humanities and social sciences that is genuinely accessible to a wide audience of academics and students is vanishingly small. Editorial boards are to a large extent in a stagnated condition, and hence, so are 'editorial styles'. The capacity to determine to what extent submitted manuscripts 'make a genuine contribution to the field' is essentially the monopoly of editorial boards, who send material to an inner circle of tame referees. In the circumstances, debate over different views of some question, or approaches to the study of given phenomena, is often reduced to the construction of a particular 'canon of objectivity', which is then mummified in the form of a journal's thematic sections and established agenda of topics. The fact that peer reviewing is non-existent (submissions will be assessed by the editorial board alone), and that there is next to no tradition of open and friendly intellectual debate, fosters the development of research-and-publishing conglomerates within which the corporate version of 'objectivity' soon gets transformed into an intellectual party line. It's fair to say that the *'vital force of the Slavonic people'* in Siberia represents objective reality for the members of the 'Altai sociological school'[1] no less than does *'cultural evolution'* for the editorial board of *Chelovek*.[2]

But does the striving for corporate objectivity of this kind in fact have any cultural value? I would argue that it certainly does. The selection of material, of the type of argumentation employed, the politics of quotation, the thematic and social geography of an analysis, all make clear how symbolic capital circulates in the given scholarly community, and how the epistemological hierarchy is constructed in it; this in turn illustrates how given practices of problematisation and thematicisation fit into a broader pattern of social mapping used by that community. To speak in methodological terms, what is of value here is not so much the relationship between the canons of 'corporate objectivity' and a given version of reality, but the relationship between those 'canons' and practices as realised *beyond* the field of academic activity in question, which is to say, the web of relations in which this striving for objectivity acquires the status of a purposeful and planned activity. To put it another way: instead of reducing 'corporate objectivity' to various forms of repression, such as might have brought a given interest group and its view of objectivity to the status of a 'material force', it is more productive, in my view, to trace how *analogous* strivings for objectivity can realise themselves in *different* forms of activity. Which is to say, we are dealing here with the need to define the effect of homology (Pierre Bourdieu), the effect of the parallel unfolding of the same organisational logic in different areas of reality. Thus manifestations of corporate objectivity should be considered important in so far as they allow us to reconstruct the 'general world-view', the 'horizon', the social conditions, and the symbolic order which make a striving for such (corporate) objectivity possible in the first place. This emphasis on the structural origin of epistemological norms allows one also to look from a rather different perspective at the role of rhetorical strategies in generating the effect of objectivity. Hayden White, who was strongly influenced by the Russian Formalists, noted in his *Metahistory* [White 1973], that the descriptive logic espoused by historians, and the methods of analysis preferred by them, were to a large extent determined by the form ('genre') according to which discursive material was organised. The 'deep structures' revealed in the course of a historical or — we could equally well say — ethnographical, investigation would then become just reflections of dominant narrative traditions, both at the level of the subject of a given ethnographical encounter (the 'informant', 'source',

1 See e.g. [Grigoryev, Demina 2000; Semilet 2001].

2 See e.g. the review in *Chelovek*. 2004. No. 2.

etc.), and at the level of the subject of the ethnographic exposition (the 'researcher', 'scholar', 'academic author').

The effect of objectivity is attained, in the given context, by the employment of symbolic forms that are capable of recording the personal/group experience in terms that are socially meaningful. Accordingly, the rhetorical and stylistic levels of a text — i.e. the organisation of words, things and people in a given discursive and social space — transcend aesthetic categories as such and acquire the status of mechanisms for the regulation of social reality, of mechanisms whose role is especially evident in the absence of clear, universally shared, normative standards (of an ideological, religious, class, national, gender etc. character)

Where, then, does the specific epistemological value of 'rhetorical' objectivity lie? By paying attention to the structuring principles of the ethnographic description (be it an informant's narrative or an ethnographer's testimony) one can reveal the motivation behind the narrative choices of the author, that is, one can see how the *'apparatus of concatenation'* [Shklovsky 1966: 118] turns an external event into part of an individual biography as well as a discursive fact. How is this symbolic choice organised? On what principles is the orientation among the thickets of diversity of discursive forms (and their relative degrees of accessibility) directed?

The analytical appeal of 'rhetorical objectivity' would, on this analysis, be determined by a basic assumption that the established rhetorical preferences of the author (ethnographer) and the informant (their 'style', if you like) had a particular and definable basis. The explanation of the reasons behind the selection of concrete forms of linguistic organisation could certainly be interpreted according to classic theoretical models — from Marxist theories of class consciousness to Freudian theories of the unconscious. But crucial in this effort to identify the genealogy of motivation would be the same basic premise that the representation of experience — what Lacan termed as the attempt to acquire the shape of the signifier [Lacan 1998: ch. 13] — is the result of a selection among available symbolic strategies, i.e. that it is a process of conscious (or unconscious) differentiation of symbolic forms. A genealogical analysis of the different types of 'rhetorical objectivity' would therefore draw attention to the historically specific conditions that both constituted 'techniques' for the differentiation of symbolic practices, and simultaneously stimulated particular inclinations to exploit this or that symbolic method.

2 I think that the degree of subjectivity in an analytical text is to a large extent defined by the aims of the project in question. I would identify two different types of text here. On the one hand, there is the case of a commentary on documents that are known to the researcher, but which are unlikely to be known to the reader — these might be

archive materials, say, or the results of ethnographical observations. On the other, we have scholarly work that is precisely based on the author's reasoned interpretation of a given source which is already, so to speak, 'in the public domain'. I realise that the dichotomy I have just identified is traditional, and so too is the unequal status of its constituent parts. In Russia at any rate, such 'authorial interpretations' are usually classified as 'scholarly publicistics'[1] (the term 'essay' effects the same kind of marginalisation in a milder way), and open expression of personal opinion is usually interpreted as the sign of a subjective, i.e. emphatically partial, approach.[2] In many cases, the problematic character of an explicitly personalised text is essentially a problem of reception, that is, the result of 'unfulfilled expectations', when the audience's strategies of readership come into conflict with the representational model set before them. The expectation that material (of a 'scholarly' kind) should be organised in a given way works as a normative template, as an epistemological grid that predetermines the type of access to the text. For instance, in reviews of some recent collections of interdisciplinary work that I edited, I was struck by the fact that reviewers preferred to focus on the lack of a clear-cut disciplinary and stylistic 'affiliation' in the essays, rather than to engage with the ethnographical material that these articles cited, or with the interpretations that they offered, or with the arguments that they advanced. The genre preferences of the reviewers, often supported by the usual positivist gestures towards the need to 'establish the validity' of the 'data' concerned, were (all too predictably) realised as an attempt to police disciplinary boundaries, allegedly capable of producing an unambigious distinction between 'scholarly' and 'non-scholarly' texts.[3] Ernst Gombrich, in his classic study *Art and Illusion*, well captured the mechanism that underlies perceptions of this kind. Gombrich concluded, on the basis of a meticulous study of representational material, that:

the individual visual information... [is] *entered, as it were, upon a pre-existing blank or formulary. And, as often happens with blanks, if they have no provisions for certain kinds of information we consider essential, it is just too bad for the information... And just as the lawyer or the statistician could plead that he could never get hold of the individual case without some sort of framework provided by his forms or blanks, so the artist could argue that it makes no sense to look at a motif unless one has learned how to classify and catch it within the network of a schematic form* [Gombrich 1989: 73].

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1 i.e. a sub-genre of not-quite-scholarship close to literary journalism [Editor].

2 See e.g. the recent debate on quantitative and qualitative methods in *Neprikosnovennyi zapas* No. 35. URL: <http://www.nz-online.ru/index.phtml?cid=3>. Cf. [Bikbov, Gavrilenko 2002]; [Shpakova 2003].

3 See e.g. *Chelovek*. 2003. No. 3. P. 182.

It's clear that the ability to 'catch' or 'to be caught' within the networks of meaning is simply a question of the time and experience needed for new conventions of expectation to be formed, and for the techniques of representation to be perfected. The problem is that it is precisely the subjective elements in a given piece of writing that are often used as an excuse to avoid considering the possibility of reassessing the parameters of scholarly *'blanks'* and *'formularies'*, to repeat Gombrich's terms.

I'd also like to mention here another kind of 'personalised' approach to a scholarly text. In this case, the potential problem has to do not so much with difficulties in the dynamic balance between the 'informational'/'interpretational' drives of the writer on the one hand, and the reading habits of his or her audience on the other, but with another kind of fusion. I'm talking here about texts in which the authorial position with regard to a given question is overshadowed by the description of the *process of communication* between the author and the objects of his or her attention. What I see as significant here is not even so much the tacit replacement of the analytical procedure by the impressionistic reflexivity with regard to the relations *between* the author and the subject under study ('substitution', in Freud or Lacan's terminology), as the nature of the subject position of the imaginary reader-addressee which is constructed as a result of this substitution. Constructing a personalised academic narrative serves, whether consciously or not, as a justification of textual exercises in narcissism. The attempt to *convince* the readership that the author's line of arguments makes sense is elided by the author's striving to *demonstrate* his or her experience; the personalised text turns the reader into an interested observer (at best), and an unwilling voyeur (at worst). The process of critical writing/reading is neutralised by the activation of mechanisms of compassion (in the sense used by Hannah Arendt) between author and reader; the possibility of a critical dialogue between author and reader is effectively eliminated by the reader's enforced identification with the author's emotions and experiences.

As in many comparable cases, the exaggeration of the narrative device makes it easy to recognise the inner mechanics of the authorial style, and to trace more clearly to the sources of the inflection of rhetorical strategies discussed above. The US anthropologist Paul Rabinow's memoir of his fieldwork in Morocco [Rabinow 1977] is a good example of a text of this kind;[1] so too is the scholarly prose of Victor Shklovsky.[2] In both cases, the radical stylistic exposure of the author's relationship to the text under study

1 For a discussion see [Oushakine 2004].

2 See e.g. his analytical memoirs [Shklovsky 2004]; [Shklovsky 1977].

has at least two functions. Defamiliarisation of ordinary clichés and linguistic formulae allows a new, dynamic, de-automatised response to the text (of the other). At the same time, the purposive construction of the authorial text as an artefact that is designed to be *read* sustains the possibility of response on behalf of the readership the analysis is addressed to. Here, the subjectification of the text acts as a mechanism that facilitates the triangulation of relations between the source, the analytical resources of the author, and the perceptive abilities of the audience.

3 In the best cases, 'ethnography of the self' can be very useful in providing a form for the articulation of, and an interpretative trajectory for, experience that has been left on the margins of established scholarly tradition. For instance, feminist anthropology and 'native anthropology' are good examples of this approach. In both cases it has allowed women and members of ethnic communities respectively to alter the accepted analytical prisms and thus to break out of a situation where members of these social groups were traditionally impeded from exercising any notable influence on research agendas and research methodologies.[1] Such 'first-person' studies have broadened the spectrum of issues that are recognised as 'important', and the range of forms in which these are articulated; they have designated a whole new geography of analytical positions — whether in the form of *history from below*,[2] of *subaltern studies*,[3] or of *postcolonial studies*.[4]

Yet, when one's own experience is turned into one's own research object, this always raises concomitant questions about the consequences of the resulting absence of physical/analytical/critical distance between the object and subject of the discussion. The literal fusion of the object and the subject of research often undermines the boundaries between auto-ethnography and the politics of identity. The choice of the research object — 'the author's own experience' — is normally accompanied by a defence of the author's subjectivity, beyond which the experience would not exist.

Participating in recent debates about the evolution of women's studies, an area where auto-ethnography is pervasive, Wendy Brown, the well-known American feminist and political theorist, recently pointed out that institutionalisation of academic disciplines that are built around the notion of identity tends to transform these disciplines from a mode of criticism of dominant norms and hierarchies

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1 For a good example of a study espousing both a feminist and a 'native anthropology' line, see [Seremetakis 1991]

2 See e.g. [Krantz 1988].

3 See e.g. [Guha, Spivak 1988]; [Guha 1997].

4 See e.g. [Bhabha 1994]; Chakrabarty 2000].

into a basis for creating new norms and hierarchies. The observation seems to me cogent. Placed at the root of an academic project, the author's identity can foster the transformation of the project into a form of 'idententious' [**ident**-ity + tend-**entious**] textual practices aimed at expressing corporate, rather than gnoseological, interests — even if the 'corporate' here is hidden behind an attempt to explore the ontology of this or that version of the feminine.[1]

As with the textual representations fostered by 'corporate objectivity', the materials of auto-ethnography are generally of value less for their own analytical and interpretational achievements than as the material for secondary analysis, allowing the evolution of group and individual self-consciousness to be traced. A good example of how work of this kind can be carried out with auto-ethnographical texts is the publication of diaries written by Evgeniya Kiseleva, and discovered by Nataliya Kozlova and Irina Sandomirskaya in the Narodnyi arkhiv [People's Archive], Moscow [Kozlova, Sandomirskaya 1996]. Written over several decades, Kiseleva's notebooks of autobiographical text are not analytical in character, yet they provide rich ethnographic material about the life of an ordinary, little-educated woman in a provincial Soviet town. What, however, makes this publication interesting and unusual, is the form taken by the textual dialogue between the 'naïve writing' of an uneducated provincial woman, and the interpretation by the two scholars concerned to discover the discursive strategies that made this autobiographical text possible. An extensive theoretical introduction supplied by Kozlova and Sandomirskaya offers models of conceptualisation and interpretation of the documents, and simultaneously channels the reader's attention. Kiseleva's text both defines the theoretical generalisations and is defined by them.

4 I'm not sure I can agree that additional information about the author of a given document might be an obstacle to the researcher's analytical position. There is no such a thing as too much information. Especially ethnographical information. The question for the ethnographer is how to avoid an easy fusion of the author of a given ethnographical document and the document itself. The question is, what are the social and analytic mechanisms that could constantly reveal the fact that there is a gap between a textually produced representation and an actually lived life? There probably isn't a fail-safe recipe for all occasions; in my own fieldwork, my strategy is to try and use a maximally diverse range of sources. For instance, in my work on the Soldiers' Mothers Committee in Barnaul, I used materials from local archives and museums alongside interviews with members of the organisation and direct observation, videos of

[1] [Brown 1997]. See also [Scott 2002]; [Scott 2000].

meetings, the women's correspondence, the Books of Memory published by the Mothers,[1] and official documents. The variety of positions and opinions allows one to see a diversity of different responses to the same situation (family experience of war losses); each statement becomes contingent on every other one, creating, in total, a kind of palimpsest. The variety of different opinions also destabilises any authorial position: analytical techniques have to be constantly varied according to different situations. The problem with such a fragmentary/constantly fragmenting ethnography then becomes how one maintains any thematic unity at all, given that every different fragment has the potential to acquire a life of its own.

MIKHAIL RODIONOV

1

The usual reaction to an academic author who vehemently lays claim to objectivity is suspicion: he or she must, one supposes, be either lacking in sincerity, or in basic intelligence. The only thing worse is a person who flaunts their own *lack* of objectivity, political commitment, dependence on some particular set of ideas or circumstances, or hard-and-fast methodology. But the *striving* towards objectivity — not in the sense of some kind of exquisite erudition or of pretension to divine omniscience, but in the sense of humaneness, tolerance, and openness — is and should be valued quite differently. And these qualities are easier to manifest for an observer on the side, a representative of a culture other than the one under contemplation. I should emphasise: I am not speaking here of ethnographical theorists, whose concern is the textualisation (in the Ricoeurian sense: see [Ricoeur 1973]) of knowledge, but in the first instance of ethnographers working in the field, and needing to address the behaviour norms, practices, linguistic strategies, and so on, that prevail among those they encounter.

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It is generally agreed that science and scholarship should take nothing on trust. However, in anthropology, a discipline that is quintessential-

[1] See e.g [Khramtsova 1992].

ly concerned with the study of the general by means of the particular, many publications are simply not open to verification, nor can the material in them be subjected to the scientific proof of experimental replication. How authentic and representative our suppositions are is the key question here, which leads some to consider anthropology no more than an exercise in description, a form of folk tradition, or indeed an art form. As a matter of fact, one has the impression that the academic study of the humanities, both here in post-Soviet Russia and in the West, defends its right to exercise control over knowledge in a more or less inert sort of way; there seems to be the sense that the corporate privileges of the past are lost for good and all. All living forms of scholarship and science are constantly gripped by a sense of crisis, but often this is represented as something characteristic only of the present day, and which has to be 'overcome' by thrashing out general, objective criteria of one kind or another. In this context, the concept of 'objectivity', in itself empty of content, acquires an obvious value for dogmatists, who assign to it specific meanings of their own in the struggle to uphold, or to demolish, theoretical and methodological unanimity.

2 When old interpretative schemas with pretensions to objectivity are reassessed, the results to a large extent depend on the person who is undertaking the reassessment. Field work in ethnography and anthropology, like classical philology, depends for its effectiveness more on the personality of the scholar involved than on methodological principles: on the observer's levels of education and knowledge, breadth of vision, sense of proportion, personal decency and honesty, and ability to think flexibly, changing direction when an unexpected situation requires this.

Back in the 1960s, Daniil Danin's book *The Inevitability of a Strange World* [Danin 1962] introduced us to the idea that the apparatus used for an experiment in physics will inescapably distort the results. It did not take long for this sense of 'strangeness' to grip the world of the humanities as well. The subject and object of research came to be recognised as interdependent — one is reminded of the Nietzschean insight, *'Remember when you are looking into the abyss that the abyss is also looking into you'.* At the end of the 1980s, John Van Maanen's book *Tales of the Field: On Writing Ethnography* [Van Maanen 1988] drew some conclusions about the diversity of studies based on field work. He distinguished three genres: realist (impersonal, with pretensions to strict standards of scholarly detachment and to completeness), confessional (the ethnographer analyses his or her own feelings, errors, and uncertainties, depicts his or her entry into the situation and exit from it), and impressionistic (concerned with the poetics of investigation, rather than with the researcher or the object of research). Van Maanen hypothesised that the 'right' to publish texts in the second category was often a sort of professional reward for success in producing work in the first; the third category had, in his view, a marginal status, belonging more to oral exchanges (dinner-party anecdotes about field work, chat in the coffee-breaks of conferences, etc.) However, he also pointed out that there were plenty of cases where all three genres came together between two book covers. I'd also remark on my own account that Van Maanen's classification was by no means original, going back to Hayden White's *Metahistory* [White 1973], which represents the 'realist' trend in nineteenth-century historiography according to various patterns: *'romance'* (Michelet), *'comedy'* (Ranke), *'tragedy'* (Tocqueville), or *'satire'* (Buckhardt), and Marx, Nietzsche, and Croce as executing respectively *'the philosophical defence of history in the metonymical mode'*, *'the poetical defence of history in the metaphorical mode'*, and *'the philosophical defence of history in the ironic mode'*. Literary-critical analysis of this kind could perfectly well be applied to ethnographic materials too — as indeed it was in Edward Said's brilliant, if at times polemically overstated, *Orientalism* [Said 1979].

As for attempts to introduce oneself into the discourse of the 'other', these can be remarkably productive. In the area that I myself know most about, study of the Arab world, a key figure is of course Clifford Geertz, both a practitioner and a theoretician of the transcendence of objectivity (take, for example, *Local Knowledge*, first published in 1983), and also someone who has successfully applied the comparative method in his anthropological work — and as such a significant rarity. I am also impressed by the success of my closest colleagues in the Anglo-American world — Stephen Caton, whose particular area of study is the ethnography of oral and written poetry in the tribes of North Yemen [Caton 1990], Brinkley Messick, who brought to life the traditional poetics of the Yemeni written document in his book *The Calligraphic State* [Messick 1993], and Paul Dresch, who used materials gathered during field-work to characterise the interrelations of the North Yemeni tribes with the state [Dresch 1989], and who is also the author of a study of modern Yemeni history in ethnographical perspective [Dresch 2000]. It's not hard to think of less successful excursions in the 'impressionist' genre, where 'inspiration' takes the place of solid scholarship, but to list them here would, unfortunately, take up more space than I am prepared to sacrifice.

3 The pioneer of 'autoethnography' is usually taken to be David Hayano, whose study of Californian poker-players was based on his own experience, as the title of one of his essays makes clear [Hayano 1979]. I'm sure that this genre will become ever more popular, despite the fact that to date, so far as I can see, there has been little really strong work in this direction. As a rule, the autoethnographer locates him- or herself on the border of two (or indeed more) cultural

domains, but tends to gravitate more strongly in one or other direction. This state of cultural schizophrenia demands exceptional levels of honesty and ruthlessness towards the self and others in an observer, which is likely to be traumatic, no matter how harmonious the observer believes him- or herself to be. Edward Said, who until his recent untimely death was Professor of English and Comparative Literature at Columbia University, also spent a great deal of time writing and thinking about the ethno-cultural ramifications of the crisis in the Middle East, and here he wrote both as an observer, and as the object of his own reflections. The punning title of his wonderful autobiography *Out of Place* [Said 1999] referred not only to his family's migrations from Jerusalem to Lebanon, and then to Egypt and finally the USA, but also to his conviction that an independent intellectual has in our time to be a spiritual exile and a social marginal. In the Arab milieu, the splitting of consciousness in an educated specialist in the humanities — a person in whom the subject-object dichotomy is directly located — can be exacerbated by bilingualism in ordinary life: such a person will command both a local demotic idiom enjoying low prestige (whose use conservative Arab intellectuals would see as a sign of ignorance), and literary Arabic, which is far from always able to express local realia with precision. At any rate, for many of my colleagues from the Arab world, exercises in auto-ethnography have a purely documentary status, requiring separate commentary and analysis in their turn.

Another kind of fusion of the subject-object standpoint can currently be observed among ethnographers of Eastern and Central Europe who are themselves citizens of the societies they seek to document. When they analyse the crisis in humanities in the post-Soviet era, they are studying themselves, and trying, in collaboration with their Western colleagues from, say, the Max-Plank-Institut für Gesellschaftsforschung or from Harvard to understand who they are: are they still ethnographers (as in the past), are they ethnologists, or have they now attained the status of fully-fledged anthropologists? Often, an ironic quip made by Grażyna Kubica-Heller is used in all seriousness as a starting-point for self-definition: *'In Eastern and Central Europe the word "anthropologist" means an ethnographer, or in some cases, sociologist, who has a command of the English language'* [Skalnik 2002: XIV].

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As early as my first field trip to Southern Arabia in 1983, I started to feel convinced that contact with informants was most successful when it was focused on some traditional text, whether this was a poetic turn of phrase that was in common use, a sultanic decree about the proper conduct of wedding ceremonies, or a reminiscence about a decision in a court of arbitration. Subsequent field trips confirmed this conviction. And in 2003, I spent two three-month stints of fieldwork in Hadhramaut extracting ethnographical information from the archives of the Sultans of al-Qasiri and other family and tribal documents. The lively commentaries that my informants offered, the wrangles that spring between them, always throw up interesting details, which, besides, illuminated the particular subject positions adopted by different speakers. It is easier to understand not only what the document contains, but what it leaves out. As one can see, therefore, the individual traits of a given source (provided scholars recognise them as such) are helpful, rather than detrimental, to understanding, making this more complete and representative, though one has of course to bear in mind that no single text can ever be interpreted in a final sense. The context for a text will never be revealed in its entirety, and its possible intertextual resonances never exhausted. Of course, it is perfectly possible to 'minimise' the impact of sources upon us. But this would lead to a break in contact between the ethnographer and his or her informant: the latter is always conscious of the effect that a given statement will have on the former. A skilled informant will always affect the viewpoint of the academic observer, and individual scholars have to decide how far they are capable of separating one viewpoint from another.

No doubt there are some ethnographers and anthropologists whose drive to objectivity and completeness prompts them to present their field records in impersonal form, as inventories or data-bases. Even today's technology makes this straightforward, and no doubt in the future things will become easier — perhaps even justifying what Evans-Pritchard is once reported to have said, *'Anybody who is not a complete idiot can do fieldwork'* [Clarke 1975].

I feel, on the other hand, that all types of narrations from the field have their merits: all of them complement each other, making up a kind of harmonious chorus. In literature, we can see marginal genres becoming central: it's not impossible that the same thing will happen in ethnography.

NANCY SCHEPER-HUGHES

Anthropology and the Suspension of the Ethical

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In what follows, I relate the questions raised in your Forum about 'objectivity' and 'subjectivity' to issues that have haunted me over the years in my own field research projects regarding object/subject relations and anthropological ethics. Anthropology has always existed as a moral project, one that demanded an explicit ethical orientation to 'the other'. Historically, this was understood as a respectful distance, a reluctance to name wrongs, to judge, to intervene, or to prescribe change even in the face of considerable human misery. Anthropology (like theology) implied a leap of faith to an unknown, opaque other-than-myself, before whom a kind of reverence and awe was required. Fieldwork was to be transformative of the self, while putting few or no demands on 'the other'. If the physician's injunction was to 'do no harm', the anthropologist's was to 'see no evil, hear no evil, and speak no evil' in reporting back from the field.

For much of its history, cultural anthropology was concerned with understanding divergent rationalities — with explaining how and why various cultural 'others' thought, reasoned, and lived-in-the-world as they did — best exemplified in the great witchcraft and rationality debates of earlier decades.[1] Ideally, modern cultural anthropology was poised to liberate 'truth' from its unexamined, Eurocentric presuppositions and to engage respectfully with divergent truths, rationalities and subjectivities. The practice of anthropology was also guided by a complex form of modern pessimism rooted in the discipline's tortured relationship to the colonial world, and its ruthless and wonton destruction of native lands and peoples. Anthropology came of age amidst genocides and ethnocides of non-western peoples whose precarious lives (and deaths) provided anthropologists with their livelihood. True, anthropologists were, at best, 'reluctant imperialists'[2] who, even when working for colonial administrations in Africa and Asia or for internal bureaus of Indian affairs in the Americas, tried their best to reduce the damages to native peoples. However, because of its origins as go-between in the clash of indigenous cultures and colonizing civilisations, anthropological thinking emerged as radically conservative with respect to its suspiciousness of any universalizing proposals advocating social change, economic development, modernisation, human rights, and the like. Anthropologists were keenly aware of how often even the 'best intended' interventions had used against those traditional, non-secular, and communal people who happened to stand in the way of dominant cultural and economic expansion. Thus, an acute sensibility to cultural difference and to cultural and moral relativism became the hallmark of modernist anthropology, one that served the discipline (and the peoples studied) well in certain times and places, but which need to be reviewed and re-evaluated in terms of the interdependent, post colonial global world we now inhabit.

1 Some of these debates in anthropology are discussed in [Mohanty l989]; [Hollis and Lukes 1982]; [Wilson 1985]; [Tambiah 1990].

2 See [James 1973].

At the beginning of the twenty-first century the world, the 'objects' of anthropological study and our fieldwork relations have changed almost beyond recognition. Anthropologists have had to refashion themselves in response to postcolonial critiques of traditional ethnographic research and engagements. It is one thing to challenge an epistemology; it is quite another to transform the self in the daily performance of one's profession as anthropologists have had to do in the past few decades. To take an example: Interpreting the cultural logic of Azande witchcraft (as Evans-Pritchard did)[1] is one thing; interpreting the burning or necklacing[2] of witches in South African squatter camps during the anti-apartheid struggle in South Africa [Scheper-Hughes 1995] is quite another. In South African squatter camps, as in the AIDS sanatoria of Cuba, as in the hungry and parched lands of Northeast Brazil, as in the clandestine and illicit transplant clinics of Turkey, Brazil, South Africa, the Philippines, and Israel, I stumbled upon a central dilemma and challenge to cultural anthropology. That is, in bracketing certain 'western' Enlightenment truths held and defended as self-evident 'at home' in order to engage intellectually with a multiplicity of alternative 'truths' and divergent subjectivities in the field, were we perhaps *'suspending the ethical'* [Buber 1952] toward a class of overly differentiated 'others', especially those whose vulnerable bodies and fragile lives are most at stake?

In North-East Brazil during the military years (1964–1984), I encountered a post-colonial situation in which some impoverished women living on the edges of starvation in the sugar plantation zone of Pernambuco, seemed to have 'suspended the ethical' — compassion, empathy and care — toward some of their weak and sickly children, whom they 'allowed', and sometimes even 'helped', to die of strategic and mortal neglect in the face of overwhelming difficulties [Scheper-Hughes 1993]. 'Good, *menos um*', one less for my little bit of *angu* [manioc gruel], a shantytown father said in response to the death of his infant. In the squatter camps of South Africa in 1993–1994, I encountered the revolutionary logic and sentiment among some ANC comrades that 'one less' police collaborator, or even one less thief, whipped, flogged, and necklaced, made good sense in terms of social and community hygiene. Today, in the clandestine transplant clinics of the world, I have encountered surgeons and their patients who are willing (indeed eager) to plunge into the bodies of the poor, the unlucky, the hungry, the mentally deficient, the imprisoned, and the homeless — the desperate and the displaced — for a 'spare' kidney, half a liver, even an eye.[3]

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1 See his classic work: *Witchcraft, Oracles and Magic Among the Azande* [1937].

2 Setting victims aflame with rubber tyres around their neck.

3 See [Scheper-Hughes, 2002]; [Scheper-Hughes 2003a]; [Scheper-Hughes 2003b].

The survivor 'logic' that guided shantytown mothers' actions toward their weak babies was understandable but nonetheless tragic. The revolutionary logic that saw in the pressured but self-serving acts of a young police collaborator the sorcery of a scarcely human witch or devil was also understandable but open to political, ethical and (I am arguing) anthropological critique and debate. The medical logic that encourages transplants relying on the tempting 'bio-availability' of the organs of the healthy-poor also enters into a gray zone of moral ambiguity. Each complex situation gives reason to pause, and to consider how often the oppressed (or the sick) turn into their own oppressors or, worse still into the oppressors of others, especially those structurally weaker than themselves. The question I am raising is whether the goal of anthropology ends with interpretation and with 'making sense' of the world, and of violence and human suffering, from the classical stance of the objective, distanced, relativising spectator, what Clifford Geertz [1988] called anthropological *'I-witnessing.'*

The Primacy of the Ethical

There is, I am proposing here, an alternative position: to identify social ills in a spirit of solidarity, and following a 'womanly ethic' of care and responsibility [Gilligan 1982; Ruddick 1989]. For anthropologists to deny, because it is 'politically incorrect' to do so, the extent to which formerly colonised and dominated peoples can come to play the role of their own executioners, is to collaborate with the relations of power and silence that allow the destruction and the suffering to continue.

To speak of the 'primacy of the ethical' is to suggest certain transcendent, pre-cultural first principles. Whereas historically cultural anthropologists have tended to understand morality as contingent on, and embedded within, specific and local cultural assumptions about human life, there is another philosophical position that posits the ethical as existing prior to culture: *'Morality'*, writes Immanuel Levinas [1987: 100], *'does not* ***belong*** *to culture:* [morality] *enables one to judge it.'* Responsibility, accountability, answerability to 'the other' — the ethical as I am defining it here — is *pre-cultural* in that human existence demands the presence and the existence of another. The extreme cultural relativist position assumes that thought, emotion, and reflexivity come into existence with words, and words come into being with language and culture. But language itself presupposes, as Sartre wrote in *Being and Nothingness* [1956], a relationship with another subject: *'If speaking means existence with others, what creates this reciprocal bond is the silence that precedes speech, the silence in which two subjects take stock of each other at a distance'.*

For anthropologists — who are not, of course, moral philosophers — the challenge remains: the search for a standard, or for divergent ethical standards, that might take into account — but not privilege — 'western', cultural presuppositions, and that invites radically different ethical sensibilities to participate in anthropological thinking. Following the lead of Lila Abu-Lughod's argument for *'writing against culture'* [1991], this could mean abandoning two sacred cows of anthropology: the idea of culture and with it cultural and moral relativism, and even political neutrality.

Culture as Fetish

The idea of culture has often been used to obscure the social relations, political economy, and the formal institutions of violence that promote and produce human suffering. Cultures do not only generate meaning they produce ideologies and justifications for institutionalised inequality, exploitation, and domination. As a Xhosa undergraduate student at the University of Cape Town, where I taught in 1993–94, queried: *'Just because culture is a design for living, Prof., it doesn't necessarily mean that it's* ***good*** *design, does it?'* The culture concept has been used to exaggerate and to mystify the differences between the anthropologist and his or her subjects, as in the implicit suggestion that because they are *'from different* ***cultures,*** *they are* [also therefore] *of different* ***worlds,*** *and of different* ***times'*** [Farmer 2003]. This 'denial of co-evalness' is deeply ingrained in our discipline, exemplified each time we speak., as Geertz has, of the impenetrable *opacity* of culture or of the incommensurability of cultural systems of thought, meaning, and practice. The idea of culture may disguise an incipient, underlying racism — a pseudo-speciation of humans into discrete types, orders, and kinds, reifying difference.

Anthropologists who are privileged to observe human events close up and over time, and who become privy to state and to community secrets that are generally hidden from the view of outsiders until much later — after the collective graves have been discovered and the body counts made — are beginning to recognise another ethical position: to identify the structural sources of violence that often passed for 'cultural differences', in a new spirit of active engagement.

'Basic strangeness' — as the psychoanalyst Maria Piers [1978] labeled the profound shock of mis-recognition reported by some mothers in their first encounters with a newborn — offers a prototype of all other alienated 'othering' relations, including that of the anthropologist and her subjects. Just as many women may fail to recognise a human kinship with the newborn and see it as strange, exotic, other — a bird, a crocodile, a changeling, one to be returned to sky or water rather than adopted or claimed — so the transplant tourists of the world can view their living paid kidney donors as living cadavers, non-

persons, not worthy of compassion, concern or responsibility, and so the anthropologist herself can view her subjects as intrinsically other, belonging to another time, another (cultural) world altogether. But if it is to be ethical in nature — and taking up an ethical position of some kind is inescapable for the human sciences — the work of anthropology requires a different set of relationships. In minimalist terms this might be described as the difference between the anthropologist as objective 'spectator' and the anthropologist as engaged 'witness'.

An Anthropology of Evil

Classical cultural anthropology and its particular moral sensibility orients us like so many inverse bloodhounds on the trail of the good and the righteous in the societies that we study. Some have even suggested that evil is not a 'proper subject' for the anthropologist. Thus, some of my anthropological colleagues have reacted with anger to my ethnographic descriptions and analyses of such vexing topics as: the scape-goating and psychological crippling of last born, farm-inheriting Irish sons within the structure of economically failing farm families; the medicalisation of hunger among Brazilian sugarcane cutters and the mortal selective neglect of infants; the execution of street kids in the cities of Brazil; the government sponsored health insurance reimbursements to Israeli transplant tourists who travel for surgeries using the purloined kidneys of poor Brazilians, Turks, Palestinians, Iraqis, and Eastern European peasants. In all these, contexts layers of collective 'bad faith' and complicity join the oppressed and their oppressors in a macabre dance of death. What was I after, after all?

Chronic hunger, of the sort I described in rural Brazil, was not unusual, the anthropologist James Peacock commented, at a faculty seminar given at the University of North Carolina, Chapel Hill several years ago. Many of the Indonesian villagers he had been studying for years were surviving on a comparably meagre and deficient diets to the Brazilian cane cutters of Northeast Brazil. Why did I make *that* — the mundane concreteness of chronic hunger and its eroding effects on the human spirit — the driving force and focus of my work?

'Is this an anthropology of evil'? the late Paul Riesman challenged me at a symposium of the annual meetings of the American Anthropological Association in response to my paper on 'The Madness of Hunger' that illustrated the bad faith of clinic doctors in failing to see the starvation that lay just beneath the skin of their folk diagnosis of infantile 'nervousness', irritability and 'delirium' in the babies, allowing them to prescribe painkillers, phenobarbitol, antibiotics, and sleeping pills to medicate the starving. The folk diagnosis of *nervoso infantil* was, I argued, the somatisation of a mortal social and

political illness, The bad faith existed on many levels: among doctors and pharmacists who allowed their knowledge and skills to be abused; among local politicians who presented themselves as community benefactors, while knowing full well what they were doing in distributing tranquillisers and appetite stimulants to hungry people from the over-stocked drawers of municipal file cabinets; among the sick-poor themselves, who even while critical of the medical mistreatment they received, continued to hold out for a medical-technical solution to their political and economic troubles; and, finally, among medical anthropologists themselves, whose fascination with metaphors, signs, and symbols can blind them to the banal materiality of human suffering and prevent us from developing a political discourse on those hungry populations of the third world who generously provide us with our livelihoods.

Riesman contested: *'It seems to me that when we act in critical situations of the sort that Scheper-Hughes describes for Northeast Brazil,* ***we leave anthropology behind****. We leave it behind because we abandon what I believe to be a fundamental axiom of the creed we share, namely that all humans are equal in the sight of anthropology. Though Scheper-Hughes does not put it this way, the struggle she is urging anthropologists to join is a struggle against evil. Once we identify an evil, I think we give up trying to understand the situation as a human reality. Instead we see it as in some sense inhuman, and all we then try to understand is how best to combat it. At this point we* [leave anthropology behind] *and we enter the political process.'*

But why is it assumed that when anthropologists enter the *luta* — whether it is a political, revolutionary, or a human rights — struggle, we inevitably surrender our anthropological credentials? Since when is *evil* (or more often weakness, complicity, cupidity, even stupidity) exempt from the human and from anthropological study and analysis?

Tristes Antropologiques

In his professional memoir, *After the Fact,* Clifford Geertz [1995] notes somewhat wryly that he always had the uncomfortable feeling of arriving too early or too late to observe the really large and significant political events and the violent upheavals that descended on his respective field sites in Morocco and Java. Later, Geertz notes that he consciously *avoided* the political conflicts by carefully moving back and forth between his respective field-sites during relative periods of calm, always managing to *'miss the revolution'* [Starn 1995], as it were.

Consequently, there is nothing in Geertz's ethnographic writings that hinted at the 'killing fields' that were beginning to engulf Indonesia soon after he had departed from the field, a massacre of suspected Communists by Islamic fundamentalists in 1965 that rivalled more recent events in Rwanda. Unless, that is, one interprets Geertz's celebrated analysis of the Balinese cock-fight as a coded expression (and warning) of the fierce aggression lying just beneath the surface of a people whom the anthropologist otherwise described as among the most poised, controlled, and decorous in the world.

In *After the Fact*, Geertz returns anthropology to its problematic origins as a pastime for a leisured class of well-travelled Victorian gentlemen *'where the sport resides in not knowing too much about what one is after'* and not being too engaged in one's findings. But when the stakes are high and concern the description and analysis of mass destruction, the calm, removed, objectively distanced stance of the anthropologist/observer strikes a discordant note. Those privileged to observe human events close up and over time and who are thereby privy to local, community, and even state secrets that are generally hidden from view until much later, are beginning to recognise another ethical position — to name and to identify the sources, structures, and institutions of mass violence. This new mood of political and ethical engagement has resulted in considerable soul-searching among some anthropologists, even if long... 'after the fact'.

Claude Levi-Strauss fast approaching the end of his long and distinguished career, opened his photographic memoir, *Saudades do Brasil* [Homesickness for Brazil, 1995], with a sobering caveat. He warned the reader that the lyrically beautiful images of 'pristine' rain forest Brazilian Indians about to be presented — photos taken by him between 1935-1939 in the interior of Brazil — should *not* be trusted. The images were illusory, he cautioned. The world they portray no longer exists. The starkly beautiful, seemingly timeless Nambikwara, Caduveo, and Bororo Indians captured in his photos bear no resemblance to the reduced populations one might find today camped out by the sides of busy truck routes or loitering in urban villages looking like slums carved out of a gutted wilderness. The Nambiquara and their Amerindian neighbours had been devastated by wage labor, gold prospecting, prostitution, and the diseases of cultural contact: smallpox, TB, AIDS, syphilis.

Earlier, Lévi-Strauss [1966: 126] described the central quandary which *demands* that anthropology be a 'vocation' rather than merely a scholarly pursuit: *'Anthropology is **not** a dispassionate science like astronomy, which springs from the contemplation of things at a distance. It is the outcome of a historical process which has made the larger part of mankind subservient to the other, and during which millions of innocent human beings have had their resources plundered and their institutions and beliefs destroyed whilst they themselves were ruthlessly killed, thrown into bondage, and contaminated by diseases they were* *unable to resist. Anthropology is the daughter to this era of violence... in which one part of mankind treated the other as an object.'*

Before his death, the archetypal neo-colonialist anthropologist, Bronislaw Malinowski, likewise came to see anthropology as a profession with a specific moral obligation. In [1945: 3] he wrote: *'The duty of the anthropologist is to be a fair and true interpreter of the Native and... to register that Europeans in the past sometimes exterminated whole island peoples; that they expropriated most of the patrimony of savage races; that they introduced slavery in a specially cruel and pernicious form.'* He saw the commonalties in the destructiveness of the European colonisations of New Guinea, the New World, and sub-Saharan Africa. But these later writings were largely discredited by his profession as the irresponsible babbling of an old man past his intellectual prime.

In the *Anthropology of Reason* [1996], Paul Rabinow characterised one dominant mode of social science practice, the vigilant virtuoso, which he exemplified in the work of Pierre Bourdieu. The defining moment for Bourdieu's reflexive sociology — was (Rabinow argues) the unveiling of the lie — *the illusio* — those forms of collective and individual self-deception necessary to maintain any social group — a marriage, a family, a profession, a community, or a society. The power of the sociological-anthropological imagination derives from the social scientist's claim to occupy a privileged position outside the social field and to the powerful interests at play within it. Achieving such extraordinary clarity of vision demands a sacrifice — the refusal of all social action and the rejection of all personal interest in the meaning and stakes of social life. Rabinow characterised this stance as based on a set of ascetic techniques, central to which is a renunciation of the 'real' world — the world of power, action, and high stakes.

But in fact, Bourdieu had long embraced what he himself called in one of his last public lectures (delivered in Athens in May 2001) the life of an *'engaged and militant intellectual'* [Bourdieu 2002]. For Bourdieu this meant an active (sometimes combative) engagement with social movements, labor unions, the homeless, displaced rural workers and against the world-wide embrace of economic and cultural globalisation which Bourdieu insisted was a dangerous *theory* of social reality rather than a *description* of it. In his public lecture in Tokyo in October 2000, 'Unite and Rule' Bourdieu refered to the 'Global Market' as a political creation, and to globalisation itself as a pseudo-concept, masking a kind of neo-evolutionary model of the world in which a city or a nation's insertions into the global market (where cities are classified as 'global', for example) are diagnostic of a new kind of developmental trajectory. Given the gravity of the world situation, Bourdieu called for the practice of *'a scholarship with commitment'* toward building collective structures capable of giving birth to new social movements and new sites for international action.

Anthropologist as Spectator, Anthropologist as Witness

In his book on white South Africans, Vincent Crapanzano invoked the generative metaphor of *'waiting'* to describe the intellectual and moral paralysis of rural white farmers, both Afrikaner and English, on the eve of the inevitable unravelling of apartheid. *'Waiting',* wrote Crapanzano*, 'means to be oriented in time in a special way.... It is a sort of holding action — a lingering. (In its extreme form waiting can lead to paralysis)* [...] *The world in its immediacy slips away. It is de-realised. It is without elan, vitality, creative force. It is numb, muted, dead* [...] [Waiting] *is marked by contingency — by the perhaps — and all the anxiety* [and all the powerlessness, helplessness, vulnerability and infantile rage] *that comes with the experience of contingency* [and passivity]*'* [Crapanzano 1985: 44].

These phrases irked my white South African colleagues at Cape Town to a point of murderous rage. The words seemed to cast aspersions on all white South Africans and to ignore the role of those courageous whites who joined the political struggle that eventually brought the apartheid state to its knees. However, while 'waiting' may or may not be an adequate metaphor describing the existential situation of South African whites, it struck me as an apt metaphor for anthropology itself where 'watchful waiting', above and outside the fray, was the dominant moral position.

I think, for example, of my late mentor Hortense Powdermaker [1939/ 1968], and of her enormous pride in her ability to negotiate skillfully between and around the 'color line' in Sunflower County, Mississippi in the 1930s, managing to maintain open and courteous relations with both the white aristocracy and 'their' Black sharecropper families. But there seems little virtue today in neutrality in face of the broad political and moral dramas of life and death, good and evil, as these are played out in the everyday lives and everyday violence practiced against the people with whom we temporarily cast our lots.

If observation links anthropology to the natural sciences, witnessing links anthropology to history. Observation, the anthropologist as 'fearless spectator', is a passive act which positions the anthropologist above and outside human events as a 'neutral' and 'objective' seeing I/ eye. Witnessing, where the anthropologist acts as *companheira*, uses the active voice, and it positions the anthropologist inside human events as a responsive, reflexive, and committed being. The spectator is accountable to science; the witness is accountable to history. The anthropologist as witness is accountable for what she sees and for what she fails to see, how he acts and how he fails to act, in critical situations.

I have asked what anthropology might become if it existed on two fronts: as a field of knowledge (a discipline) and as a field of action, a force field and a site of struggle. Anthropological writing can be a site of resistance and the anthropologist can resemble what the radical Italian psychiatrist Franco Basaglia [1987] called the *'negative intellectual worker'*. The negative worker is a species of class traitor— a doctor, a teacher, a lawyer, psychologist, a social worker, a manager, a social scientist, even — who colludes with the powerless to identify their needs against the interests of the bourgeois institution: the university, the hospital, the factory.

I have argued [Scheper-Hughes 1993, 1995] that we can practice an *antropologia-pe-no-chao*, an anthropology-with-one's feet on the ground, a committed, grounded, 'barefoot' anthropology. We can write books that go against the grain by avoiding impenetrable prose so as to be accessible to the people we say we represent. We can make ourselves available, not just as friends or as 'patrons' in the old colonialist sense, but as *companheiras* (with all the demands and responsibilities that this word implies) to the people who are the subjects of our writings, and whose lives and whose suffering (as Mick Taussig once put it) have provided us with a livelihood. We can, as Michel De Certeau [1984] suggested, exchange gifts based on our labors, use book royalties to support radical actions, and seek to avoid the deadening treadmill of academic achievement. We can, at one and the same time, be anthropologists and *companheiras.*

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Auto-Ethnography: Some Remarks

The questions put to us in this *Forum* address the tendency in modern anthropology to include descriptions of the fieldworker's personal experiences in the worked-up ethnographical description itself. In professional journals, a number of terms are employed to name the different strategies by which such experience may be invoked: 'reflexive ethnography' (alternatively, 'the confessional style') [Jenkins 1993], 'auto-ethnography/auto-anthropology', and 'anthropology at home'. Each of these terms is multivalent, and each overlaps with the others to some extent.

The issues here emerged for me particularly sharply in connection with my own study of

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expeditions into the field as an ethnographical phenomenon, as a set of practices expressing symbolic, organisational, and self-identifying significance in a range of professional contexts.[1] At first, this theme was one of many others I was investigating in the framework of a large-scale comparative project on the anthropology of the professions: both ethnographical expeditions and my own experience came within my purview, the latter also as an object of analytical reflection. In other words, I myself was faced by the fusion of 'subject' and 'object' addressed by the third question of this Forum.

The situation did not seem to fit the traditional scenarios of ethnographical investigation; clearly, it needed to be placed in the framework of debates on 'auto-ethnography' and 'auto-anthropology'. 'Auto-ethnography' and 'auto-anthropology', as I found, were used with a variety of different meanings in methodological discussions. In the broad sense, they signified study of 'one's own' society, community, country, region, religious or professional milieu i.e. the one to which the researcher him- or herself belonged. In this meaning, auto-anthropology signified something close to the term 'anthropology at home', i.e. the study of the *'culturally familiar'* [Strathern 1987], being applied to situations where Western culture was under address by Western anthropologists, or conversely, or where non-Western societies were the subject of scrutiny on the part of local ('indigenous') anthropologists. For instance, Michael Moffatt used the term for the ethnography of American culture (i.e. the culture of the continental USA excluding the culture of native Americans), but also argued that only those relatively few studies written by ethnographers who were themselves members of the culture being studied could be described as auto-ethnographic in a full sense [Moffatt 1992]. In Deborah Reed-Danahay's interpretation, on the other hand, 'auto-ethnography' meant not only the study of something familiar and near at hand, but also the employment of a reflexive analytical position based on biographical studies and the analysis of life histories — on the personal experience of the ethnographer as well as those 'others' whose lives he or she was investigating [Reed-Danahay 1977].

A third view came from Penelope Harvey, who saw auto-anthropology as a model of analysis drawing on the concepts current in the society or culture under study: that is, in essence, as a particular culture's self-presentation, or its representation from the perspective of the bearers of that culture. Harvey addressed the problems of 'auto-anthropology' in the context of a case-study of the 'universal exhibition', which she saw as a representation of cultural diversity

1 The first results of these experiments were set out in [Shchepanskaya 2003a]; see also [Shchepanskaya 2003b].

as imbricated in systems of global communication in the conditions of late capitalist consumerism (the 'universal exhibition' was discussed by her alongside the formation of 'virtual world's fairs'). Auto-anthropology, as a form of representation of a culture in the terms of that culture (or a form of self-representation in these terms) allowed the researcher, in Harvey's opinion, to escape the problems of difference and 'incommensurability' that were generated by the concepts current in traditional anthropology, and permitted all cultures to become part of the unifying context of the 'universal exhibition'. In this context, all interpretations were seen as equal in value, and the knowledge of professional anthropologists lost its authority; the representatives of any culture had as much right to represent that culture for the 'universal exhibition' as anyone else, whether they were professionals or not [Harvey 1996: 12–3]. In this signification, 'auto-anthropology' intersected in part with the concept of 'native anthropology', although it could also include work by 'non-native' anthropologists where this invoked the concepts current in a given culture, i.e. set out 'insider' models of that culture's self-presentation.

In my opinion, it is this last definition of auto-ethnography/auto-anthropology that is most helpful. The ethic/cultural origins of an anthropologist are of less importance than is the fact of the employment of discursive models current within a given ethnic group/ culture. I would apply the term 'auto-ethnography' to any analysis that cites or is based on the discourse of a given culture, regardless of where the researcher's origins in fact lie. A non-native analyst is perfectly capable of understanding and representing a culture in the terms of that culture, while, on the other hand, a 'local' or 'native' observer is not always able to distance him- or herself from Western models (as has often been observed, a Western-trained anthropologist *'lives and thinks like a Western academic'* whatever his or her actual origins [Diamond 1980: 11–2]).

I also prefer the narrower, and to my mind more accurate, understanding of 'auto-ethnography' (more commonly 'auto-anthropology', a term that emphasises analytical as well as methodological considerations) to signify the study of **the professional community**, i.e. the milieu, communicative practices, and modes of research of professional ethnographers and anthropologists [Peirano 1998]. The analysis of anthropology by anthropological methods is far from new, yet it is still (and I think, not only in Russia) seen as a relatively exotic departure for the discipline.

In the 1970s, the focus was on the history of anthropology, above all the cultural context in which Western work in the field had developed [Halowell 1974]. In the 1980s, another aspect of auto-anthropology came to the fore — the study of how different national traditions in anthropology came to be formed. Among the subjects under discussion was the nature of disciplinary boundaries in different countries (at international conferences held during the 1980s, Western anthropologists would express surprise at the fact that in the USSR anthropology was considered a sub-division of history, rather than a social science) [Strathern 1985: 25–6]; the specifics of education and career structure also received attention. The discussion of such distinctions took place in the framework of overarching concepts such as 'cultural context' or 'centre-periphery relations', where the 'mainland' was held to be made up by the traditions of Anglophone and Francophone anthropology, while other national traditions were represented as the 'archipelago'. George Stocking, on the other hand, was more concerned with the political context, drawing a distinction between *'the anthropology of empire-building'* and *'the anthropology of nation-building'* [Peirano 1998: 110]; [Stocking 1982].

By the 1990s, auto-ethnography was discussed mainly in connection with debates on ethnographical methodology. The discussions of methods and of the difficulties of verification and representation of ethnographical data that had taken place in professional journals in the course of the 1980s had made it essential for anthropology not only to consider its own professional practices, but to research these in depth: anthropological concepts themselves had become the object of ethnographical description and analysis [Aunger 1995: 97] — though, to be sure, this tended to take the form of reflexivity rather than the application of ethnographical methods as such. Explanations of distinctions in styles of fieldwork, and interpretation in terms of subjective factors — personal experience and differences in the standpoint of the observer — of the kind that had dominated discussions in the 1980s were supplemented by a search for distortions of a systemic variety, such as could be traced to the context of academic work. Hence the suggestions that anthropological work itself should be studied as a form of culturally determined practice, employing for the purpose analytical methods largely drawn from anthropology. If anthropological fieldwork was regarded as a historically sited phenomenon [Peirano 1998: 108], then it made sense to scrutinise the forms according to which ethnographical studies have traditionally been organised. In this vein, P. S. Sangren proposed taking discussion of methodology *'beyond individual studies to the imagined community of social scientists'*, and represented *'science'* as *'itself an intersubjective process'* [Sangren 1995: 122].

It was precisely this aspect of auto-ethnographical discussion (I use this term here advisedly, since what concerns me is the nature of fieldwork) that seemed most relevant to contemporary Russian ethnography. Here, the collective nature of fieldwork had much more weight than traditionally in the West. There, the ideal of fieldwork was the extended residence in some remote region beyond the borders of the country where the researcher had been trained and where he or she had their academic position [Bart 1995: 52]. In Soviet, and later post-Soviet Russian, ethnography and ethnology, on the other hand, the standard method of organisation was group visits, most often within the country itself, lasting not years, but 'fieldwork seasons' — from a few weeks to at most a few months. The extended visit system (the so-called 'stationary method', where a researcher lived for several years in a given research environment, working as a teacher, doctor, etc.), was brought to an end in the 1920s.[1]

The differences in organisation of 'the field' in turn fostered various differences in the way that the practices of fieldwork were understood. The fact that expeditions into the field were organised collectively tended to level out subjective factors and the influence of a given researcher's personality; it allowed data to be checked both in the field and afterwards, during the discussions at academic conferences. There was also another reason why experience of a given region or district was necessarily supra-individual. Entire departments or sectors in the state-sponsored research institutes specialised in the study of particular places, often organising expeditions to exactly the areas visited by previous expeditions.[2]

All of this made the issue of the exclusivity of the 'ethnographer expert' with regard to a given place, and of the possible subjective distortions in the information that he or she collected, of less significance. All the same, it is important to recognise the possibility that material might be distorted, or, shall we say, modified by, the existence of collectively shared conventions, by the stereotypes existing in a given academic milieu. Indeed, the danger of intrusion at this level may be greater than in individual cases, since collective perceptions tend to be accepted as given, and not to be subjected to much in the way of reflexive scrutiny or criticism within the community that holds them. But on the other hand, the methods of our discipline are particularly well suited to lay bare stereotypes, tenets of faith, and unwritten rules, even where these are not explicitly

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1 As a result both of ideological changes with regard to 'voluntarist' work by individuals, and of the socio-political catastrophe of collectivisation, which had made survival difficult in many remote parts of the Soviet rural landscape [Editor].

2 See e.g. the publications on two different expeditions to Pinega (Arkhangelsk province): in 1987–9 (organised by the Department of East Slavonic Ethnography of the Institute of Ethnography, Academy of Sciences of the USSR — now the Museum of Anthropology and Ethnography of the Russian Academy of Sciences), and in the mid-1990s (by Moscow State University): [Bernshtam 1995]; [Kalutskov 1998]. Not just the itinerary of the two expeditions (right down to the individual places visited), but the thematic agendas of the given expeditions were closely similar. This situation is not at all exceptional in terms of Soviet and post-Soviet ethnography; indeed, it might even be considered typical.

verbalised or recognised as such by the members of a community themselves. This allows one to suppose that the stereotypes of our own professional community that have not been subjected to reflexive scrutiny, or addressed in the kinds of open discussion commonly held among us, might be brought to light if they were subjected to the methods of anthropological/ethnographical research that we routinely apply to material from different social environments.

These considerations give an additional significance to the study of the ethnographic community in Russia, of its everyday practices, and especially the practices of the expedition into the field, which in various professional discussions is accorded the significance not only of a data source, but the basis of a professionally specific way of seeing, and an idiosyncratic ritual form.[1]

There are no institutional or conceptual obstacles to the study of 'one's own' professional milieu — the community of one's immediate colleagues is as worthy a subject of study as any other professional community. The only possible hitch is the tradition of selecting for study distant, 'remote' communities, and preferably ones that have not yet been the subject of scholarly investigation, and can hence be the subject of exclusive knowledge. From this point of view, studying ethnographers themselves is an extreme case of the disruption of the accepted rules for the construction of the object of study in classical ethnography. We have the chance to perform a controlled experiment: how far do the methods we use depend on the characteristics of the objects under study? To what extent do they reflect cognitive principles that may be applied to any milieu or culture?

In the auto-ethnographic investigation (in the narrow sense understood here), the object of study is to all intents and purposes identical with the agent of the investigation; the community under analysis is coterminous with the audience addressed by the analysis. It follows that the auditorium is both the bearer of the discourse under study, and of the discourse used for the study itself. This contrasts sharply with the traditions of classical ethnography, where the ethnographer has to employ 'cultural translation', transmuting the materials drawn from 'another' culture into the conceptual system employed in the academic auditorium. But by repeating this process of 'cultural translation' with reference to our own community, we are able to demonstrate how that process itself functions; it is made transparent, laid open to scrutiny, since the audience that we address knows both the starting and finishing points of the discussion ('experience' and 'ethnographical analysis').

It is clear that, in this situation, an ethnographical analyst is

[1] See e.g. [Elfimov 1996], esp. the comments by S. Sokolovsky, A. Nikishenkov, p. 7, p. 24.

especially vulnerable to criticism. Almost any colleague can put forward alternative interpretations, and ones that will carry equal weight, since the colleagues in question 'were also *there*' (i.e., are familiar with the community under study). In my own practice of 'auto-ethnography', I have come across a curious phenomenon. When I set out my analysis of the ethnography of a given expedition into the field, some of my colleagues will immediately raise objections, saying that my account is incomplete, and referring to the fact that they 'were on that expedition too', i.e. to their own 'insider' experience. Sometimes alternative accounts of the expedition will be asserted, which simply goes to show the multiplicity of possible 'insider' viewpoints — after all, I, the researcher, was part of the expedition myself. The curious thing about all this is that these 'insider' interpretations are usually given the status of scholarly analyses whose authority challenges my own — though so far the outlet for them has remained conversations during the coffee-breaks at conferences, rather than anything more considered.

By extension, this situation models, in extreme form, a significant problem with which contemporary anthropology has to come to terms: these days, ethnographical texts reach not only a community of near colleagues, but also people from the communities under study (from political activists to students and 'local' anthropologists), who are, indeed, now becoming one of the main groups of consumers of anthropology's textual production. Increasingly, the audience for ethnography is converging with the object of ethnography, as Clifford Geertz remarked some time ago. This issue is still at the forefront of discussion, since the situation demands reassessment of professional practices and discourses. The 'object' (community under study) can no longer be described as 'external' to the audience, given that members of the former may well be present in the latter. All of this makes the possibility of an objectivised viewpoint highly problematic. As noted in discussions on this topic, 'authority to represent an alternative reality' no longer depends simply on the opinions of one's colleagues: the approval of the group being studied is also crucial. Moshe Shokeid terms this a kind of 'test': can the people who are the object of study *'identify themselves in the ethnographic hall of mirrors, as distorted as that image might be in their own eyes? This is probably* [the] *crucial test of the reliability, morality, and usefulness of our work'* [Shokeid 1997: 631].

When we are talking about a study dealing with the community of our fellow scholars, this test cannot be passed by, and we can expect a lot more criticism than we might otherwise, given that scholarly communities are in any case much given to critical self-scrutiny. The authority of any researcher who turns to this topic will therefore be subject to assessment; but so too will the method of analysis, which is usually passed over in silence in the scholarly community. Now, even its non-verbalised characteristics, those that are usually taken for granted, will be out in the open. Any criticism of particular interpretations of this set or the other set of ethnographical facts where drawn from the everyday life of the scholarly community will make visible the elements of methodology not customarily subjected to reflexive scrutiny, and hence bring these into the field of analytical discussion. Therefore, though a project of this kind may well threaten the personal reputation of its co-ordinator, it will also be extremely useful from the point of view of the evolution of the discipline (above all because it will open up perspectives for the rational improvement of the methodology used). The abyss of subjectivity may ever threaten, but it should finally be seen as a resource for perfecting objectivity, or at the very least, rational enquiry.

Another aspect associated with the study of a particular community is the resistance to objectivisation that we encounter on the part of representatives of 'other' communities. By this I mean the hostility they may feel to interpretations of their life experience that are recognised to be 'objective' in the sense of authoritative and authentic, and hence favoured in public discourse (as opposed to everyday discourse, which is distinguished by a multiplicity of conflicting interpretations). Where an academic community is under study, this multiplicity of conflicting interpretations is certain to boil up in public scholarly discourse. I experienced something like this myself, at an informal level, in my colleagues' responses to my 'field materials' about the practices and narrative strategies in use during ethnographical expeditions.

When working among my colleagues, my data collection (the purposes of which I, it stands to reason, make no attempts to conceal) is regarded as rather offbeat, although it also excites interest and readiness to help. The ethnographical methods I use include participant observation (drawing on my more than 20 years' experience of fieldwork at first hand), supplemented more recently by systematic recording of so-called 'expedition folklore' — the songs, epigrams, skits, funny stories etc. that are put together in the course of expeditions, or composed later about expeditions, and which may later get wider dissemination in the professional milieu (in the domain of oral/'folk' discourse, of course). So far as possible, I record such texts when they are customarily performed in any case — i.e., I work according to the principles customary in ethnographical fieldwork. The reasons why I might be making the recordings get discussed by my colleagues every now and then in an informal way, so that these colleagues share with me the divided role of a bearer of culture and a researcher in the field.

This situation makes impossible the preservation of a subject-object paradigm, and, more broadly, of pretensions to objectivity and to professional expertise (to interpretation from a position of privilege), since the community itself decisively resists my 'right' to any such things, constantly setting forward alternative interpretations. From this example we can see that it is essential not only to organise the collection of field materials in a dialogic way — as ethnographers have long been used to doing — but also to present them in a dialogic way: the audience must be able to hear 'different voices', interpreting experience from different positions.

In a situation where the milieu under study is coterminous with the milieu performing an analysis and interpretation, a question presses itself forward: how can 'field data' be differentiated from the interpretations determined by the aims the researcher has in view and by the paradigms that he or she is employing? Isn't it possible that my colleagues' own analytical positions pre-determine what they say about expeditions? I think that a possible solution to these difficulties lies precisely in the recording of professional folklore — i.e. of texts that have already gone through a process of transformation into stereotype independent of the process of my recording them. These texts are oriented to shared symbols and values, and they reflect collective norms (of the group carrying out the expedition, on the one hand, and of the 'off-duty' preoccupations and behaviour of the ethnographers among whom they are customarily performed, on the other). Accordingly, they are much less subject to modification on an individual level than is the impromptu discourse current in the same milieu.

Thus, in studying our own community, we have the opportunity to model and to isolate certain aspects of the ethnographical method that ordinarily remain beyond the bounds of critical reflection and rational consideration. In the context of Russian ethnography, this task is especially important, and has its own contextual specificity, which is related to the considerable role played by informal traditions in the transmission of professional practices. My own observations, and the material I have collected over the past two years suggest that it is possible to speak here of a powerful ingrained culture relating to expeditions (not just ethnographical expeditions, given that folklorists, dialectologists, and sometimes archaeologists usually come along as well), and a clearly expressed complex of stereotypical academic practices related to fieldwork, as expressed for instance in the discourse of fieldwork anecdotes, songs and other such texts, as customarily grouped together under the term 'professional folklore'. All of this is bound to be reflected in the stereotypes according to which fieldwork is set down, and it is precisely the isolation of these influences that I would deem the most important, although the most difficult, task in Russian auto-ethnography at present.

The analysis of Russian (as of other national or regional) traditions of fieldwork (including its 'underground' or 'folk' elements) is topical in terms of the current international directions in ethnography too, given the shifts in the objects of study and the development of many different 'national anthropologies', with centres scattered all over the world, not to speak of the 'narrowing down' of the globe (i.e. the growing accessibility of formerly 'remote' places). Thus, we find James Clifford discussing the likely spread of the practice of repeated short visits, of the kind that were formerly the norm in the 'national' ethnographies of various 'non-Western' countries, offset by the decline of extended residencies of the kind traditional in Anglo-American ethnography.[1] In a 'shrinking' world, where the number of physically remote places is becoming ever smaller, the possibility of repeat visits becomes more realistic than in the 'classical' days of the 'fathers of fieldwork' And an even greater influence is exercised on fieldwork by critical discussions on the problems of representation, which have demanded that new methods of verifying ethnographic data are evolved. One might cite Bart's comments on the importance of the collective principle in fieldwork, of the fact that those taking part in the process should be diverse in terms of personality [Bart 1995]. It is notable that various forms of fieldwork organisation (group expeditions, repeat expeditions) that were previously not characteristic of Western anthropology, but were standard in various other national traditions, including Russian anthropology, have now started to become more popular on an international level. From another perspective, in Russia, there is now a drift towards individualisation of fieldwork, which is partly derived from economic causes (the reduction in the number of state-funded group expeditions and the rise in the availability of grants for individual research). This background makes it easy to understand the rising interest in the experience and traditions of different national anthropologies, such as I have encountered myself when talking to colleagues from France,[2] Japan, and Britain,[3] all of whom cross-questioned me and my colleagues in Russia about the organisation of fieldwork. It was even suggested I should write a short essay on the subject, and interest was expressed in observing Russian expeditions at first hand. (No doubt I was playing the role of the 'native anthropologist' here.)

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1 See [Peirano 1998: 113]; [Clifford 1998: 90].

2 For example, I have held long discussions with Lise Gruel-Apert, who told me of the high level of interest among French anthropologists in the practices of Russian ethnographical expeditions, especially since the publication of her note [Gruel-Apert 1996] (this was part of an issue dedicated to Russia and Russian ethnography).

3 I derived great benefit from my discussions with Patrick Heady when the latter visited the Department of Cultural Anthropology at St Petersburg State University. He was keen to take part in an expedition with Russian colleagues (and was in fact able to arrange this); it would be intriguing to hear what his impressions of the expedition and of Russian fieldwork generally may have been.

Thus, the ethnographic study of one's own research milieu goes beyond being simply a case-study of a professional community like any other: it also affords a unique opportunity to test out the methods we use. Are they useful for the study of any object, or only for the scrutiny of 'remote' (for which read, 'exotic') cultures? — which would suggest in turn they were essentially an instrument of representing/constructing concepts and ideologies of difference.

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The laying of claims by the discipline of history (my primary discipline) to objectivity is, of course, largely a modern phenomenon, most famously captured in the 19th century by Leopold Ranke in his renowned insistence that we write history *'as it actually was'* (*wie es eigentlich gewesen ist*). I would say, therefore, that the pretension to objectivity has been more than a *'badge of office'* or a sign and ritual of *'corporate identity'* — one can see the claims to objectivity in research and writing as intimately connected with the construction of modernity itself as a form of comprehending and ordering the world. That said, most of us have long grown sceptical of such positivistic confidence (though, at a recent conference in St. Petersburg, the question of how 'objectively factual' history is or should be was a subject of some disagreement). Just as the modern itself is not simply the story of the march of science, reason, and order, but a profound struggle between what Zygmunt Bauman called *'meaning-legislating reason'* and another, no less essential, face of the modern: that which challenges assumptions, upends stabilities, and questions knowability in the world. Practically, then, if we are to make humanistic research fully 'modern' (including by understanding the fractures and self-deceits in modernity that postmodernism sharply reminds us of) we can neither jettison the effort to uncover 'facts' (data that can be proven or disproven with sufficient evidence) nor forget that the human world, and all the evidence of it we choose to study, is vital and

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alive precisely in its endemic constructedness through people's minds and imaginations.

In a word, the truth remains elusive but not irrelevant in our work. E. H. Carr said long ago that history (and, he might have said the same of other humanistic and social disciplines) is a practice of interrogating the past with better and better questions. Knowing the limitations of our knowledge about others (and, of course, about our own selves) and the shaping effect of our own subjectivities — i.e. questioning our own assumptions about how people live and think — brings us closer to something we might dare to call the truth; we might dare, as long as we know that what we offer when we write up our work is still only an argument. For most of us, I think, the important thing is that it be an honest argument — honest in our pursuing what facts we can and honest in both acknowledging and continually questioning and undermining our assumptions. This may be obvious, but it shapes how we work in very tangible ways.

Such persistent questioning and self-questioning as a method of inquiry is all the more difficult, and important, when we are studying culture than when we are studying, say, economic production or how a war was fought (though, as some quantitative historians have shown, we need carefully to question the making of even the most materialist evidence). The chief question posed in the questionnaire is about the *'subjectivity of the researcher.'* In my own work, I have found this problem especially important and full of hazards when it is the subjectivity of the 'research object' that interests us. Cultural history — much informed by anthropological notions of culture as structures of constructed and enacted meaning as well as by philosophical and psychological notions of how to reconstruct and interpret thought — faces this problem directly. My own recent work, like that of many others, tries to recover the subjectivity of people in the past: in my case, to look beyond the usual questions of how Russian workers formed their social identities and political allegiances to explore how workers thought and even felt about such elusive questions as self, modernity, and the sacred. A senior historian whom I greatly respect, precisely for his efforts to look deeply at the mentalities of Russian workers, circled in a chapter of a manuscript I asked him to review my repeated uses of the verb 'felt' to describe what I thought I found. He warned me that I was on dangerous interpretive ground. I agree. It is hard enough to determine and explicate the ideas and values of people long dead, with whom the analytical dialogue of questions and answers is already severely limited by the fixity and many silences in the sources, without trying to ask questions about sentiment and mood, about subjectivity in its most essential aspect. Yet, for all the risks, I join others in recognizing that emotion was a crucial part of the meaning of the past for those we are seeking to understand. It may be more prudent to resist trying to look into layers of consciousness that we can only, at best, infer from our sources, and which our own subjectivities are most likely to cause us to misunderstand. But prudence does not change the fact that human experience and action are composed of emotion as well as rational perception, of moral sensibilities as well as ethical conviction, of what Russian worker writers themselves called 'life feeling' (*zhizneoshchushchenie*), and thus that it is our work to explore this with whatever tools we can.

So, in direct response to some of your questions: First, yes, objectivity has value, but only as a goal toward which we strive but that we know will continually slip over the horizon, and we accept this. As long as we honestly believe we are moving in the right direction, but do not imagine we have reached the reflected mirage, we are on solid ground. My talk of 'honesty' in scholarship—questioning our sources and ourselves in a good-faith pursuit of facts we believe to be 'out there' in the world while never deluding ourselves or our readers that we have spoken the 'final word' — may seem simplistic and naïve. But it allows many of us to work critically without succumbing to scientific hubris about what can be known about human experience or succumbing to (or being deceptively liberated by) absolute relativism and scepticism. Second, yes, it would be an interesting experiment in narrative form for a historian to insert his or her explicit self into a work the way anthropologists often do, to make our questioning dialogue with the past and with ourselves explicitly visible, but this is still not the custom. There have been some steps in this direction in the work of Natalie Davis, Joan Scott, Simon Schama, and others. But the narrative conceit of the historian acting as if she or he is outside the story being told — however much we all understand that this *is* a conceit, that we are offering no more than an interpretation based on our own particular, but hopefully honest, engagement with the evidence — remains largely intact (apart from various admissions of authorial subjectivity in the introduction). But it need not remain the only way to write history. Finally, yes, in this case tradition is right (though not always acknowledged): it is corrosive of what I am calling honest (as opposed to objective) history to minimise the mediating subjectivities (and, let's be blunt, their occasional fabrications and lies) of the sources we study. Of course, authors (whether ourselves or those who created the texts we study) are not autonomous individuals. Of course, their subjectivities are entwined with their times and hence can be treated as evidence of those times. Still, we must never stop asking questions — and listening carefully for answers — about the past, about our sources, about how the past was recorded and told, and about the assumptions in our own questions and in shaping the answers we believe. Stubborn questioning will not result in final answers. This interrogatory work stops when we decide to publish, and, in peer-review publishing, our peers accept what we say as at least tentatively compelling. For myself, I admit that I reach this point when I 'feel' that I know something valuable and not fully known before about some part of the past. That feeling — what could be more subjective? — is grounded on a measure of certainty that I have honestly tried to know something real and true — yes, I would even say, objective — about the past. This self-conscious paradox of subjective objectivity, this never resolved ambiguity, this unrequited desire to know, are the conditions under which many of us work. I see no good reason to have any more certainty in our practices than this.

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1 I agree that objectivity should indeed be seen in terms of a rhetorical strategy and the expression of a corporate identity. Indeed, reference to 'objectivity' in the context of the humanities smacks of sleight of hand. The methods of research and analysis used, their object, and the means by which they are communicated are so deeply imbued with subjectivity that the proportion of evaluations and interpretations that can really be considered objective, verifiable, impartial, free of personal input is vanishingly small. So what about objectivity as an ideal? Personally, I believe in ideals. But if we are dealing with belief, then how can 'objective objectivity' be relevant? Everyone creates their own variant of the invariant, and in reality one can only speak about the 'percentage of objectivity', while being aware this will never reach anything like 100.

2 This question made me wonder about the issue of how far ethnologists may try to 'fill holes in the ozone layer' (i.e. evidential mismatches of all kinds). Weakly representative data is cloaked in endless plural verbs ('the locals consider... have the following practices...' and so on and so on); gaps in material are passed over in silence; insufficient knowledge of the given culture is masked by narration of personal impressions and observations on the process of getting immersed. It's not really surprising that reflexivity

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is most widely represented in the work of beginning ethnologists or of journalists writing about traditional culture, and in popular books, works of *belles lettres* and so on. In solid, well-thought out scholarly studies this emotional aspect is less strongly represented, and appears in well-regulated doses.

3 This fusion of the subjective and the objective has become extremely widespread, and to a considerable extent that is to be welcomed. One could take the dissertations written by members of the Khanti people, who have moved, under the guidance of experienced ethnologists, from being reindeer-herders and cultural activists to the role not just of active informants, but of informants who set down their contributions in writing (a process that became evident during the 1990s). It is clear that the material set out by such writers is of unique value: in order to collect even a fraction of it, anyone from outside the culture would have to spend years immersing him- or herself in Khanti culture. Of course, they may not be able to analyse the material collected from the perspective of 'elite' scholarship, or place it in a cross-cultural context, or engage in ratiocinations about scholarly aims and methods — but you can't have everything, or at least not all at once. It is precisely the breadth and quality of the materials here that are of value, rather than the way they are analysed. The function of such writers is analogous to that of the translator, where the essence of the job lies in the accuracy and adequacy of the translation — no-one would dream of expecting a critical evaluation of the original. And informants of this kind have not only to translate from one culture into another, but from the language of symbols and images that is used to express the traditional world-view into the logically ordered language of scholarship.

But on the other hand, excursions into the mythic world of other cultures on the part of observers trained in the methods of logical analysis, such as those carried out by Eva Schmidt, the Viennese anthropologist, do not provoke much desire to go in that direction.

I myself have occasionally ended up in the role of an informant when foreign researchers (young anthropologists from Germany and the USA) arrived to work on the culture that I work on myself (of the southern Selkups). I acted as a guide, an intermediary in the complicated business of immersion in the milieu, and a source of primary information. When I recall my contacts with them, I have to smile — spinning round in my head is a phrase from a school poster about civil defence: 'A Chatterbox is the Spy's Best Friend'. They would solemnly note down my idle remarks over a cup of tea, or record them on tape, though I couldn't even get as far as checking I'd been properly understood. (All comprehension is necessarily partial, and what's more, their Russian was far from perfect to begin with). And how they managed to put together a scholarly commentary, using my comments as sources among other things, remained forever beyond my ken...

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I'm not sure I understand this question very well, and I'd rather concentrate on how an informant or a historical document (and the author that stands behind it) can help a researcher gain some kind of idea about the subject (phenomenon) in hand. During this process, the adequacy of comprehension depends on the methods used to do the research, on the professional standards of the researcher, and especially on the representativeness or otherwise of the data. A range of other factors, including the researcher's own level of knowledge and world-view, and his or her own motivation and characteristics, is also vital. The motivation and characteristics of the informant or the author of a given document, on the other hand, constitute the material that researchers have to work with: their task is to try and understand these as fully as possible. The only way to reduce the danger of distortion in the process of comprehension of a given cultural reality that is brought by the partiality of any single informant or author is to maximise the number of informants (including those whose characteristics and motives may differ).

TATIANA VALODZINA

3

A classic example of the fusion of researcher and informant in one person is Cheslav Petkevich (1856–1936), the Belorussian and Polish ethnographer and folklorist. His is an unusual scholarly biography: he embarked on an academic career when he was over sixty years old. His parents being impoverished gentlefolk, Petkevich was forced to run the estate and to do every kind of manual work; later, he worked as the estate manager. He had soon learned all the skills and trades practised in the villages around. Crucial in terms of his future was an encounter with Kazimir Moszynski,[1] who at the time was still a young man. Petkevich was a dream informant for a novice ethnographer; with his help Moszynski organised an expedition to Rechitskoe Polesye. However, it then turned out that Moszynski was still well short of the materials he needed for the study he was plan-

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1 See the notes to Baranov above. [Editor].

ning to write. Moszynski accordingly went back to Petkevich, who was noted for his phenomenal memory. Later, helped by Moszynski, who was by now an established scholar, Petkevich himself went to Warsaw and began work in the Ethnology Department in the Anthropological and Ethnological Institute of the Warsaw Scholarly Society, where his gift as an ethnographical researcher and painter on ethnographical themes was able to realise itself to the full.

Most of Petkevich's life was associated with Polesye. This was where he grew up and acquired his knowledge and experience. His scholarly works were of the first rank intellectually, but they were not so much studies of specific themes as they were expositions of his view of life in general. Petkevich did not record his material in the form of questionnaires or interviews, he represented what he wrote about from the inside; he had absorbed his material in the peasant world itself, aided by the conditions he had lived in and which had shaped him as a researcher. The subject of his research is inseparable from his own biography, and his Polesye studies can be considered exercises in autobiography of an idiosyncratic kind.

His first book was a wide-ranging monograph on Rechitskoe Polesye, *Polesie Rzeczyckie: Materiały etnograficzne. Cz. I. Kultura materialna* [Rechitskoe Polesye: Ethnographical Materials. Part 1. Material Culture] (Warsaw, 1928). As Kazimir Moszynski wrote in his preface, *'This is a unique document, composed as it is by an intellectual who has lived the life of a peasant and is, in addition, gifted with a wonderful memory, so that he is able to recall the past with photographic accuracy.'* The book's fourteen sections deal with almost all the occupations and trades of the Polesyans, and almost every phenomenon in peasant life is recorded by Petkevich's meticulous drawings (the book contains nearly 300 of these) as well as in words.

Petkevich's second book — *Kultura duchowa Polesia Rzeczyckiego: Materiały etnograficzne* [The Spiritual Culture of Rechitskoe Polesye: Ethnographical Materials] (Warsaw, 1938) — also embraces a huge range of themes, here set out in twenty different sections. Among these are such unusual, in terms of folkloric study, themes as 'The Measurement of Time and Space', 'Hygiene in the Life of the Polessians', 'Beliefs about Psychology', and so on. Every page of the book bears witness to the author's deep organic feeling for language. Of course, it is no surprise that Petkevich could retrieve all these proverbs and sayings, jokes and riddles, from his capacious memory, but there are also longer texts — tales and legends — set down with what appears to be stunning accuracy in every linguistic detail. And this despite the fact that he did not keep working notebooks; he simply sat down in Warsaw and recapitulated all these oral traditions with the authenticity that they would have had in folk speech itself. His representation of them can't be termed stylised, and the word 'pseudo-folklore' is clearly totally inappropriate: they were set down by someone of the highest scholarly integrity in a language that was not simply one he knew well, but that was his own native language, which he used daily for many years. Petkevich did not write any conclusions to his books or annotate them with generalisations of any kind; no doubt, having come late to academic work, he fought shy of theorisation. But his readers have been left with the most fascinating and exhaustive record of the folk life and folk culture of Polesye.[1]

4 The forms and methods by which interviews are conducted are directly linked with the anticipated destiny of the information collected. If fieldwork is concerned with more than the gathering of statistical information, then the motivation of the informant is relevant to the first stages of information collection. It is often this motivation that in turn acts as the stimulus to further work, and to the construction of an understanding of the subject in hand. Let me turn to an example from my own fieldwork. During an expedition I made the following recording from an elderly woman: *'My daughter's hair was long and thick, so lovely, you'd never tire looking at it. And she loved the cat so much. She'd take it for walks and even sleep with it. And she had such lovely hair, and then suddenly she went bald. It's all from the cat. We call it* katsinka.*'*[2] Here, it is obviously essential to understand what is meant by the illness named as *katsinka*, whose symptoms are rapid hair-loss. The situation so named is completely real and can be empirically observed in peasant life, though it is not all that common. Categorising the reference as 'medical' is problematic in that there is no comparable term in the literature, although there are plenty of words of analogous formation — cf. *shchetinka* [from *shcheta*, a bristle], *svinka* [mumps, from *svinya*, a pig], the Belorussian dialect term *krylyska* [from *krysa*, a rat], and so on. The defining element here is the explanation given by the informant: *'It's all from the cat.'*

It's also important that the information here wasn't provoked by a specific question on the part of the interviewers ('What do you do in cases of *rozha* –erysipelas. What exactly is *koltun* and how do you get rid of it?'), but followed on a passage about childhood illnesses as expressed in night-time crying, restive behaviour, and so on. *'If a pregnant woman kicks a cat, then her child will have trouble sleeping, will roll its head round and round and scream. When you're pregnant, you mustn't ever touch anything with your feet, not a cat nor a dog. If*

[1] I am indebted here to [Vasilevich 1991].

[2] Arkhiv IIEF NANB f. 8 op. 2002 d. 315. Recorded by T. Valodzina and T. Pshenko, Velikii Polsvizh, Lepelskii district, Vitebsk province.

you do, your child will have hair all over its back. It'll lie there screaming. Well, then you should touch the muscles on its back with some fresh bread, and then give the bread to a dog. Sometimes the hair will fall off as soon as you pull the bread in pieces.' Belorussians would call ailments of this kind *kraktuny* or *krylishchy*, Russians would name them as *shchetinka* and so on, and the supernatural hairiness would be understood as a sign of the child's closeness to nature, its not-yet-overcome affinity with the other world.

Both excessive hairiness and hair-loss on the part of the child are linked with unregulated, intensive, or even aggressive contact with domestic animals on by the child or by his or her mother when pregnant. The rituals customarily performed in the child's first year of life also emphasise the consciousness of some norm of hair growth deviation from which provokes morbid symptoms and which is understood not only as the result of illness but as the result of external forces, maybe even other-worldly ones.

Thus, there are several levels constituting an 'adequate understanding' of the given folk concept and the phenomenon that it describes:

— the empirically observed hair-loss named as *katsinka*;

— the possible grouping of this ailment with skin diseases of the ringworm type, such as really are transmitted by domestic animals;

— the complex of mythological ideas linked with the universal notion of the parallel between biological/social status and the extent of 'hair bearing'.

The link between the two first elements (which reflect the map of bodily reality current in the given community) is the individual motivation of the informant, which combines two ailments that in the view of official medicine are utterly distinct, interpreting the differentiated signifiers of 'hairiness' and 'animal nature' as equally significant in cognitive terms, as is made clear in the symbolic/metaphorical character of the naming patterns used. Without doubt, any reconstruction demands a degree of alienation from the processes in hand, but the motivation of the informant here, which often brings to the fore motifs from what is known as 'folk etymology', points to the manifestation on the surface of background perceptions and connotations linked with the notion of 'hairy illnesses' in general. For this reason, the immediate motivation of some phenomenon may emerge as stimulants to, or even generators of, reflections on material 'in the researcher's study'. Not infrequently, folk etymology acts as a distraction, generating a 'sensational' interpretation of some phenomenon or event, but it does often genuinely illuminate aspects of the functioning of world-view. For this reason, I am convinced that the folklorist should take seriously the individual motivations of the informant as bearer of traditional knowledge, though treating these with a requisite degree of critical awareness when final interpretations are drawn.

Fair enough: here we have been dealing with a case where the collector and the interpreter are the same person. At the same time, another crucial question relates to the role played by the motivations of informants and the ways these should be represented in published collections of folklore. Because such motivations lie beyond the generic classifications of folklore, embracing as they do comments, evaluations, and so on, they are often not recorded when tests are collected, and thus remain beyond the purview of later commentators, which leaves later generations of folklorists to rely on their own interpretations of texts, ignoring the fact that researchers and those who are actually part of a given tradition will take very different views of a given fragment of reality. In recognition of this, contemporary Belorussian collections of folklore may now include snatches of conversation and dialogues of a purely everyday kind that don't involve narratives or artistic texts. Such texts are named as 'oral folk discourse', 'comments on world-view', and record the spontaneous opinions of the bearers of a given tradition about past and present, about life on this earth and life after death.[1]

VALENTIN VYDRIN

Contemplation vs. interaction: the researcher, the object of research, and the informant in fieldwork

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In linguistics generally, studies of the problematics of fieldwork and indeed studies based on fieldwork, occupy a fairly marginal position. In the case of work relating to Africa, however, things are quite different. Even scholars specialising in the major African languages (e.g. Swahili, Hausa, Manding, Pular-Fulfulde), not to speak of smaller languages, mostly come from outside the continent (which is not to underestimate the importance of their achievements). In addition, study of the majority of African languages has gone no further than preliminary

[1] A. Boganeva. 'Suchasnae bytavanne narodnai nyakazkavai prozy: aspekty naratyva i dyskursa [Contemporary Everyday Folk Prose: Aspects of Narrative Structure and Discourse]' // *Falklarystychnyya dasledavanni. Kantekst, typalogiya, suvyazi*. Minsk, 2004. Pp. 246–7.

data collection: even basic grammars and reliable dictionaries have still to be produced. *For this reason, issues to do with fieldwork and its methodology are topical to the highest degree.*

On the other hand, obviously meditations on method can only be useful if there is some preliminary agreement as to the aims in view. My assumption here is that such agreement does exist. The object of study is language in its multifarious aspects; the linguist working in the field strives to record data as fully and accurately as possible and to construct as exact a model of as possible of the language under study, which would require the complete articulation, at every level, of its systemic features.

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In abstract terms, one can identify two diametrically opposed views of the *means of retrieving linguistic information from speakers of a given language.*

1.1. The first method, which has deep philosophical underpinnings, assumes long-term immersion (probably over a number of years) in the linguistic community concerned; by and large work is carried out without recourse to an intermediary language. The assumption is here that any impact by the researcher on the informant makes the linguistic material useless: in the effort to say what the linguist wants, the native speaker of the language may well assess as 'normal' a construction or phrase that he or she would never dream of using in the ordinary way. Especial opprobrium is extended, *within the framework of this approach*, to any attempt by the researcher to construct phrases independently in the given language, which are then presented to the informant; the assumption is that the unnatural, extraordinary nature of this situation is bound in itself to confuse the interlocutor, making it impossible to discriminate between permissible and impermissible forms and constructions. An important argument in favour of these cautions is the unarguable fact that even an experienced linguist working on his or her own language by the introspective method may well be mistaken about the permissibility and, more particularly, the frequency of certain linguistic phenomena.

Thus, it is only spontaneous texts (ideally recorded with a hidden microphone — the sight of a tape recorder is enough to make a speaker lurch into unnatural modes of speaking), and texts in every sort of fixed folkloric genre, that constitute acceptable material for linguistic analysis. Written texts can also be helpful up to a point (assuming that one is dealing with a language that has a written form). However, here one has to make one important qualification: for native speakers of a language that has begun to be written down only recently, the construction of a written text is an unnatural exercise in its own right, and one that robs the end result of spontaneity. In addition, any bilingual speaker is likely, whether consciously or unconsciously, to employ the syntactic and perhaps even the morphological constructions of the culturally dominant language.

The method just outlined could be termed 'contemplative', or 'the method of total immersion'; it is in essence similar to participant observation as practised in sociology and anthropology. It has considerable merits, but also a number of important defects. One of these is potential obsolescence in terms of the dynamics of contemporary evolution in linguistic science: the field is changing so fast that someone committed to 'total immersion' (in its extreme form) will obtain information so slowly that it will be useless by the time that it is ready.

Another peculiarity of this system is that it is designed for individualists, for 'the chosen few', and hence over-determined by the researcher's particular traits of character and personality. The method is hard to learn and therefore hardly suited to work in a group, to research expeditions of the kind traditionally organised in Russia. In essence intuitive, it demands that a researcher steep him- or herself so thoroughly in the given language that he or she can rely on *Sprachgefühl*. Of course, one can't argue with the fact that a good working knowledge of a given language makes it possible to record this language with a high degree of accuracy. But at the same time, one has to remember the dangers when researchers overestimate their feeling for the language and their own capacity for intuition: the subjective sense of complete fusion with the linguistic group under study and its language can turn out to be illusory (at the level of details, at any rate). And here an analytical approach can be more productive.

One also has to bear in mind that even a strict commitment to using only spontaneously-generated texts does not protect a researcher completely against the possibility of ending up with ungrammatical constructions of a sort that most informants would repudiate.[1]

1.2. The second type of approach (let's term it the *active* approach) is one where experimentation is not only considered permissible, but also the best means of proceeding. Elicidation (i.e. surveys conducted using a pre-prepared questionnaire containing a list of words, and by interviewing informants on the basis of sentences that the researcher has made up him- or herself), becomes the main method of enquiry, rather than a prohibited strategy. As for the anxiety that the informant may not be able to distinguish between correct and incorrect (natural and unnatural) constructions, then that is set at rest as follows.

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1 So, for instance, when I was working with a dictionary of the Bamana language in which the abundant illustrative material was taken almost exclusively from spontaneous speech or impromptu written texts, I found myself confronted with a situation when native speakers of the language rejected many of them as abnormal (and in particular, passages taken from the oral epic tradition). These cases must have involved idiosyncrasies or incidental slips of the tongue.

It is indeed fair to say that when informants are confronted by the unnatural situation of a survey they may make mistakes about whether a given construction or phrase is correct or in general use — particularly since the researcher working in this direction will deliberately present them with many borderline cases. After all, the whole point of research of this kind is to establish the boundaries between the normal and abnormal, the 'correct' and 'incorrect' in language — though the danger of course is that with time, the informant's sense of the language will get worn down, and he or she will stop knowing where the boundary lies.

All the same, the risk has to be considered worthwhile, given that in some cases fixing linguistic norms in a language that is not one's own is simply impossible. If we only analyse spontaneously generated texts, we can never establish, for instance, whether a certain verbal form represents a regular derivation or not. If the full range of possible derivations for all verbs has not been met with in the textual material we have, then we have no evidence to resolve the question of whether a given derivation is regular or not; its absence in the texts may indicate that it is impermissible, but may equally well be the result of chance. For this reason, the 'active' method constitutes not only the quickest and most direct way of gaining access to the material needed for a grammatical or semantic study of the kind that might satisfy the demands of modern linguistics, but essentially the only way of gaining access to such material.

As for the possibility of mistakes on the part of the informant, let me introduce here an argument set forward by Vladimir Nedyalkov.[1] Let us assume that the informant is mistaken in as many as 30 per cent of cases. If we carry out a thorough investigation on the basis of a detailed questionnaire, then we have obtained information that is 70 per cent correct. If, on the other hand, we try working with texts to obtain the material we want, then we are only likely to get 40 per cent of this, even if we check through an enormous number of texts. And in any case, if we use the 'active' method we can reduce the number of incorrect answers if we question a few other informants on a selective basis (generally, the logic of a given language, which one comes to feel in the process of an investigation, will indicate where an informant is likely to be doubtful or mistaken). On the other hand, an increase in the number of 'spontaneous' texts for analysis such as would be obtainable by efforts to a commensurate level would only increase the amount of useful material by a small fraction.

Let us not forget also that the questionnaire method requires interaction (long-distance contact with informants — by post etc. —

[1] Pers. inf.

really does generate unacceptable levels of informant error), and that an experienced linguist is usually able to steer an informant in the right direction and keep him or her from reacting in the wrong way. And, as a matter of fact, when showing informants 'home-made' sentences and constructions, the researcher is able to generate a wide range of responses: not just 'correct' and 'incorrect', but a whole gamut of intermediate ones, such as 'Yes, I know what you mean, but no-one would ever say that'; 'OK, you could say that, but what we would say is...'; 'That's correct, but you hardly ever hear it,' and so on. It is precisely the analysis of these intermediate responses that often allows one to sketch the unstable boundaries of the norm — the central purpose of the investigation to begin with.

All in all, the 'active method' allows, because of its analytical character, more systematic conclusions to be drawn. It assumes that the data collected is verifiable in terms of a model that exposes the systematic and paradigmatic particularities (and irregularities above all) of every element described — which let me repeat, is hard to achieve by the 'contemplative method'.

Another advantage of the 'active' method over the 'contemplative' is located in the *relationship to the obvious* that is involved. An advocate of the former method bases everything he or she does on what is plainly in view; this is the touchstone of all argumentation — yet at the same time the nature of 'the obvious' is never addressed. The 'active' method, on the other hand, being analytical to start with, allows transcendence of the obvious, an annihilation of its tyranny, since what is obvious quintessentially cannot be the adequate basis of any analysis. Hence a description of a language that is framed by analysis is more satisfactory in terms of the demands of a reader located beyond the boundaries of the language and culture described.

1.3. I recognise that I have represented these two positions in an exaggerated way. In actual research, a compromise will invariably be found between these two extremes: advocates of the 'active' method will be prepared to make use of 'spontaneous' texts — indeed, the employment of these is usually considered essential at a given stage of the project so that experimental data (i.e. those produced by means of questionnaire work) may be verified, and to establish further contexts that turn out to have been omitted from the original process of elicidation. On the other hand, advocates of the 'contemplative' method are also compelled — however reluctantly and half-heartedly — to work with informants using direct questions.

It might be possible also to imagine another kind of compromise solution that might reconcile the two positions up to a point, in the form of corpus linguistics. If a data-base of millions of usages can be assembled, then the disadvantages of both the methods outlined may be overcome. Such a data-base may be compiled exclusively from texts that can be considered spontaneous, which satisfies the demands of the 'contemplative' method, while, assuming the corpus is of sufficient size (6–10 million words), then the absence in it of some form or signification can reliably be taken as an indication of its absence from usage; by extension, the representation of a given form or signification at a level lower than some empirically determined norm will indicate that this form (or signification) is rare. Overall, the study of language by electronic means is capable of taking work to totally new levels.

Unfortunately, though, it is still too early to talk about a representative corpus of any African language (even ones of 'pan-African' significance); one has after all to remember that work on the corpus of even the Russian language is quite a novelty, let alone with reference to languages which only recently acquired a written tradition. Projects of this kind are costly, and demanding both from a technical and a theoretical point of view, and every language sets different challenges. In any case, the issue of electronic data-bases is really outside the theme under discussion here — the methodology of fieldwork.

1.5. As will readily be understood, I myself feel that the 'active' method is definitely to be preferred. It is also the one that undoubtedly prevails in the work of Russian linguists dealing with 'exotic' languages.

It is equally easy to see that the 'active' method demands a great deal of the informant and the researcher alike. If these demands are not met, then the project fails — and failures of this kind inevitably get used to buttress the arguments of those supporting the 'contemplative' method. One can also put this argument another way: the disadvantages of the 'active' method, compared with the method of 'total immersion' are indeed significant, and the need to neutralise their possible negative effects means that those who employ the method have to be particularly careful about doing so in a scrupulous and systematic way.

I want to introduce a few reflections from my own experience in Africa (on an individual basis in Mali and Guinea, and as part of a group in Côte d'Ivoire) to try and isolate the factors, and the psychological characteristics of the informant/researcher relationship, that foster the success or failure of linguistic work in the field.

The interaction between the linguist and the informant can be set up in various different ways.[1] These depend on the gender, age, and

1 One should also bear something else in mind: in our group, the practice is to pay informants a small fee, which is quite attractive in terms of village economics (though it would not be so in an urban setting). This creates 'market competition' and has a marked effect on informants' commitment.

psychological characteristics of the two people involved, and on their social status (or to be more accurate, what *appears* to be their social status — which can be quite different from the social status that a given person actually occupies in his or her own milieu).

2 The informant

At risk of a certain over-simplification, one can identify several types of informant. Which type one assigns the informant to depends on objective data, but also on the relationship between him or her and the informant. In other words, categorisation is fairly fluid, and the social status and mode of behaviour of the linguist can affect it in one or another way.

2.1. If we are talking about the *statistically average native speaker* in Côte d'Ivoire, then we mean a man (less often a woman), who has perhaps, though not necessarily, been educated to primary level, and who has some knowledge (though not necessarily much) of the French language. Working with an informant of this kind, one can use a lexical questionnaire, collect material for a phonological study, work through an interview questionnaire (of restricted dimensions); collecting plant names is usually reasonably successful (this can be done around the village itself, or by using a herbarium), as is collecting animal names (here one uses pictures from a guide-book or suchlike). But going through a questionnaire on grammar with informants of this kind is, on the whole, both demanding and unproductive, and they should be called on only if there is no choice — in which case the linguist will need the ability to work creatively and a certain histrionic talent in terms of modelling real-life situations in which grammatical constructions might be used.

One can plot two vectors leading off from the 'statistically average' speaker, on which different types of informant are located. Their usefulness for the linguist is diametrically opposed, although there is only one factor separating them: the informant's motivation when carrying out the tasks assigned.

2.2. Negative scale

2.2.1. The 'half-educated' informant

Here the main problem from the linguist's point of view is this person's reluctance to admit how little he or she knows. This informant will never acknowledge that he or she is not in possession of information, whether relating to the language itself (as everyone knows, no-one has a 100 per cent knowledge even of their native language), or to the intermediary language (in this case, French). In the effort to 'monopolise' the researcher, the informant will often not admit that he or she does not understand questions addressed to him or her in French. For instance, a researcher in the group with which I visited Côte d'Ivoire, and who was working with an informant of this kind, ended up feeling after two months that the language she was learning was in the process of dying out: sentences with totally different meanings were getting translated in exactly the same way, and many things seemed not to have any names or to be called by the same names as other quite separate ones. Later, when working with better-qualified informants, she discovered that the 'half-educated' informant in question simply didn't know very much French and had been translating sentences on the basis of crude guesswork.

2.2.2. The 'would-be educated' informant

He or she has a reasonable knowledge of the language of communication, and has gone through secondary (or perhaps even higher) education. The main problem with this person is his or her overt desire to show off their supposedly specialised linguistic, cultural and other knowledge (which in fact amounts to no more than what they were taught at school). The desire to pin down complex linguistic reality in terms of the available level of knowledge can prompt utterances that would be quite impossible in ordinary language. In the final analysis, it can turn out that one simply has to junk the material collected — it is easier to check things all over again with a better informant than to decide which of these answers are right and which are not.

2.3. The positive scale

2.3.1. The 'autodidact' ('people's intellectual')

These informants have a good knowledge of the language of communication (or, where they do not, are not afraid to say so), and are well qualified to compare and contrast urban life and the traditional life of the ethnic group they come from. In addition, the whole idea of writing a dictionary of their native language is attractive to them, and encourages their interest in work with the linguist. Informants of this kind respond well to the linguist's suggestions (when one says, 'if you don't understand, ask me to explain', 'if you understand the sentence, but you'd never say that, don't tell me it's all right', and so on), and do what they can to comply with them. Experience shows that these informants will quickly learn the rules of transcription for their language and will help the linguist record words. In turn, 'work to models' of this kind is very useful — phonemes that are hard for the researcher to record correctly can be decoded by comparison with words where they have already been identified.

2.3.2. The 'linguist'

In addition to all the above positive qualities, this person also has experience of working in illiteracy programmes, on translations of the Bible, and in other areas of practical work with the language. For one reason or another, they mostly do not carry out scholarly work themselves, but they have an understanding of the linguist's concerns. When working with such a person, linguists can allow themselves much more freedom than with other informants, and move directly towards goals that could otherwise only be attained slowly and painfully. On the other hand, caution is needed here too: even 'superinformants' have their limits. And we should remember A. E. Kibrik's warning that *'lack of linguistic sophistication is a very positive feature'* [Kibrik 1972: 84]. After all, it's a mere step from the 'linguist' to the 'would-be educated' informant. 'Forbidden' questions (of the order, 'So why does this linguistic feature work like that in your language?') and undue dependence by the researcher on this informant can be a push in the wrong direction. The researcher's job is not to let things go that way.

2.4. The 'indifferent' type: Sees no reason for the study of his or her native language and therefore does not show any interest in co-operating. Work with this kind of informant is more or less pointless. However, this type, while quite common in the former USSR, is relatively unusual in Africa, partly because of the practice of paying informants something for their help.

3 The researcher

The linguist/informant relationship is prolonged and very intensive. Naturally, the linguist's role here is as crucial as the informant's. So let us now consider which factors can have a positive effect on the relationship — or, by contrast, impede it — from the other side.

3.1. Age and gender

The interaction researcher/informant is over-determined by status. In African society, where gender and age boundaries are sharply defined, these aspects of the researcher's personality are very influential. If the informant happens to be male, and the researcher female (a fairly common situation in the experience of the group I was working with), then the relationship is likely to develop in two ways:

(a) the informant, in line with the traditional model of gender relations, tries to dominate the relationship, imposing his ideas about how it should work, advice, help, pace, style of interaction, and so on, and trying to control the people the researcher associates with;

(b) the informant tries to flirt with the researcher or to initiate sexual relations with her.

In both situations, the behaviour of the woman linguist herself (lack of self-confidence, readiness to manifest dependence), and her manner of dress (short skirt, obvious make-up, striking jewellery, long loose hair, or, in some Muslim societies, even bare arms) may prompt the relationship to slide into one or other of these two directions. Obviously, either outcome threatens the mission with failure if the researcher herself doesn't manage to correct matters (helpful to this are the stereotypes about 'African/European' relations that have survived since colonial times in rural areas). It should in all fairness be pointed out that we did not encounter gross sexual harassment (of the kind you might come across in the Caucasus or Central Asia) during our trips to West Africa; flirtation wasn't uncommon among African men, but it never took on an aggressive character.

If both the researcher and the informant are of the same sex, then on the whole, relations proceed more smoothly; if the researcher handles things properly, a friendship may well get cemented. But here too one has to be wary of attempts by the informant to dominate the situation, especially if the latter happens to be older.

On the other hand, if the researcher is older, this is a thoroughly positive factor — in that case the strict stratification by age in African society works in his or her favour.

3.2. Psychological stability. The researcher's life experience and experience in the field

These factors play a vital role in directing contact in a positive way, and can neutralise the negative effects of anything else. If the linguist is nervous and moody, informants will immediately notice this, and regard it with disapproval: every researcher should be aware that in traditional societies the capacity to control one's mood, behaviour, and emotions is regarded very highly. Anyone who does not manifest this capacity will be regarded, at best, with pity and condescension, and the way is then left open for domination on the part of informants. Otherwise, he or she will be avoided, since people will not want to get involved in ambiguous and psychologically problematic situations; complete isolation may well be the result.

An important question that often preoccupies young linguists is whether researchers should imitate the way of life, manners and behaviour of the informant, or 'stay as they are'. To my mind, it makes sense to avoid either of the two extremes:

(a) Trying to recreate one's usual way of life as nearly as possible, by bringing all sorts of urban objects along, buying the most expensive goods in the local shop, etc. Of course, I don't suggest that one should dump toothbrushes, computers, and recording machines at home — indeed, objects of this kind can actually help contact with the locals, since they are likely to inspire curiosity and interest. But the point is that the researcher's behaviour shouldn't be interpreted by locals as a display of superiority, which will make establishing emotional contact much more difficult;

(b) Trying to 'blend in' from the minute one arrives in the village — and then getting sick as a result of drinking contaminated water and eating food that one isn't used to, and ending up in all kinds of daft situations because one doesn't know the local customs, but plunges in none the less (a typical example is the young male linguist rushing to help women with work that, according to local custom, it is ridiculous and degrading for men to perform).

Try though one might to bond with the object of study, it is important to remember that the researcher, when he or she first arrives in a new community, has the role of a guest, which has to be lived up to. Tact and caution in the highest degree is needed on both sides: the guest will be forgiven his or her inevitable breaches of etiquette (*dúnan nyɛ́ntannci,* 'the guest is blind', say the Bamana), but in return he or she must behave modestly and not, for instance, try to worm his or her way into the local secrets. The status of a guest allows a researcher to avoid unnecessary misunderstandings and gives him or her time to get to know local customs and acquire a niche in the local networks of kin, neighbours, and friends. It is not a good idea to force a way into the status of 'tolerated outsider' — otherwise you can end up there without being ready for what it means.

Ideally, an expedition should be preceded by special training in psychological endurance, which would involve talking through the smallest details of fieldwork experience and modelling typical situations — in daily life, work, social relations, etc. 'Fieldwork feedback' — the collection and description of real-life situations, difficulties, and conflicts, and ways of resolving these, would be very useful here.

And, of course, it is essential for participants in linguistic expeditions to familiarise themselves thoroughly with existing ethnographical descriptions of the ethnic groups they are working with.

3.3. The level of linguistic preparation

Experience has shown that a practised, competent linguist will spend a good deal less time on an investigation in the field than someone who has not done proper advance preparation. This requires thorough advance work before every session in the field, and acquisition of the skills necessary to explain rapidly to informants what is needed, so that their precious time does not get wasted on idle conversation. But a sensitive novice will usually be in the position to acquire the techniques of fieldwork in a season or so.

3.5. Knowledge of the intermediary language

Researchers working in Africa (unlike those travelling to parts of the former Soviet Union) cannot use Russian as an intermediary language (metalanguage).[1] Hence, competence in the relevant local *lingua franca* — English, French, Dyula-Bamana, etc. (depending where one is) is absolutely vital. It is also essential to have a grasp of the local variations of international languages — Côte d'Ivoire French, Liberian English, and so on. Insufficient attention to this produces all sorts of absurdities and misunderstandings, which often escape researchers' attention when the material is written up, and which sometimes even find their way into published work, creating 'noise' of their own. In metropolitan French, *palmier* means a palm tree (as a generic identifier, any member of the Palmae family), but in Côte d'Ivoire French, it signifies 'an oil palm' (Elaeis guineensis). The verb *envoyer* means respectively 'to send' and 'to give', etc. There are thousands of examples of such semantic discrepancies, which in many cases involve fine shades of meaning, and are hard to capture (this is especially true of abstract concepts and those relating to psychological realia). It is therefore essential to consult dictionaries and secondary sources about the given regional variant of African French or African English, whilst also bearing in mind that the discrepancies of usage may well not be exhaustively catalogued there.

If the researcher does not have a good command of the intermediary language (even in its standard form), then that will make fieldwork much more difficult (it will be hard to explain the nature of the questions to the informant and the answers may well not be properly understood). Obviously, the risk of mistakes in what is collected will be increased.

3.6. I should emphasise that these remarks are not intended to be a full and complete study of the problems involved in fieldwork. I want simply to note some of the interesting details that have emerged in the course of recent research expeditions to West Africa organised out of St Petersburg. To what extent these problems also preoccupy those working in other areas is something that I hope will emerge in the course of this Forum.

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1 True, there are exceptions, such as when an informant studied in the USSR and therefore does know Russian, but even in these cases the person concerned will generally have a better knowledge of English or French. Trying to ease one's task by using Russian as the intermediary language therefore runs the risk of turning up the levels of 'noise' in the interaction.

MARGARITA ZHUIKOVA

Objectivity in the Description and Analysis of Cultural Phenomena: Some Thoughts

It's clear that all commentators on culture are part of the cultural space where they function, and cannot escape from, or even distance themselves from, this. As a result, any description, and still more analysis, of cultural phenomena inevitably bears traces of the influence of the values of a given investigator and/or the social group to which he or she belongs; the possibility of objectivity is therefore to a significant sense a mirage.

By putting the question in this way, we accept the presupposition that something exists beyond the consciousness of the individual human observer, which he or she attempts to perceive. The task, given this presupposition, would be to achieve perception of whatever this extra-individual phenomenon might be and to describe it adequately in terms of some concrete system of concepts. The object of study of this kind could, it stands to reason, perfectly well be the observer him- or herself, but if that were to happen, two different sub-aspects of the person under scrutiny — as observer and as the subject of observation — would undeniably end up on different sides of an inner barrier. Yet in disciplines concerned with the study of human beings themselves — their behaviour, spiritual world, cognitive abilities — the object of study is often only brought into existence in the process of recognition and reflection, and is to a significant extent created by the results of the investigation itself.

I want to discuss this question in more detail on the basis of linguistic material. When linguistics shifted from the stage of recording and collecting material to the phase of interpreting linguistic phenomena and of identifying causal connections in this material, of modelling linguistic phenomena of a kind eluding direct observation, there was a good deal of discussion of exactly what the discipline was identifying, an-

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alysing, and modelling. Did the object of study exist in any objective sense, or was it entirely created in the process of work by linguists?

I'd like to take note here of a situation that is familiar to linguists modelling the internal structure of lexical signification. The components of this structure, one could say in general terms, are concepts, mental entities that are sometimes of a fairly complex kind. They may be verbalised in the way that is standard for language, or represented in the form of mental but quasi-empirical images (as happens, for instance, in the case of words embodying a so-called 'empirical' component, where description of the significance has to involve citation of physical experience). And in connection with this, a question arises: is the semantic structure of a given word encoded in the collective linguistic consciousness once and for all, as a given, independently of the consciousness of the person studying that word? Or does the person carrying out the analysis create, in the fullest sense of the word, this structure, which in reality, objectively, so to speak, simply doesn't exist? On the face of it, the question seems slightly silly: after all, native speakers of a given language know what words mean, otherwise they wouldn't be able to use them. But let me cite two examples of lexical signification here to show what I have in mind.

Some time ago, Juri Apresjan,[1] in his monograph *Leksicheskaya semantika* (Lexical Semantics, 1974), set out the following differentiation of the lexical significance of the Russian synonyms *mokryi, vlazhnyi, syroi.*[2] *Mokryi* signified *'containing a good deal of moisture in or on itself'* (hands, clothes, tarmac can be described as *mokrye*); *vlazhnyi 'containing some moisture in or on itself'* (as with washing on the way to drying, sweaty hands, etc.); *syroi* meant *'containing excessive moisture in or on itself'* (as with sheets, bread, houses, etc.)[3] In the last case, of course, 'excessive' means 'excessive' in the eyes of the human observer who is the source of the relevant interpretations and assessments of realia. The word *syroi*, then, does not record merely a perception of reality as such, but an evaluation: it's bad when moisture gets into places where it's not supposed to be. And fair enough, eating soggy bread, breathing damp air and sleeping in damp sheets is indeed nasty. The evaluative nuances of the word *syroi* also explain its metaphorical potential, when applied to an article or an academic paper, for instance,[4] suggesting that these are under-

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1 This is the author's preferred spelling of a name that in our normal transliteration would be 'Yury Apresyan'. [Editor].

2 Approximately, 'wet, moist, damp'. [Editor].

3 Here, with some of the substantives listed, the more appropriate English translation would be 'soggy' [Editor].

4 where 'half-baked' in English would reflect the meaning more closely than 'damp' (*syroi* also means 'raw', 'uncooked'); one could compare the use of 'wet' in British slang to mean 'feeble, spineless' (of a person) [Editor].

prepared, 'not fit for' human consumption, to be regarded in a negative light.

Apresjan, then, was able to establish the existence of a particular semantic, i.e. mental, phenomenon determining the usage of the adjective *syroi* in particular texts. But now let's put the question another way: did this mental phenomenon in fact exist before Apresjan described it? Can it really be considered something objective, in the sense of independent from the consciousness of a particular observer? On the one hand, the very fact of the existence of the adjective *syroi* — which is close, but not identical, in meaning to *mokryi* and *vlazhnyi* — appears to offer testimony of the objective existence of the mental phenomenon in question. But on the other hand, which native speaker is really aware of the fact that the semantics of *syroi* include not only *'containing moisture'*, but also *'the moisture is excessive in terms of the person wanting to use a given object'*, and genuinely recognises the second component in the word's semantics? At the point when Apresjan formulated this semantic phenomenon in analytical terms, found a verbal expression for it, it became obvious, explicit. But can it be said to have existed *before* he addressed himself to it?

The second example I want to discuss comes from work I did myself, published in 1997. In Russian and Ukrainian, there are two verbs of very similar meaning: *spustit* and *opustit* [to lower, to let drop]. Dictionaries refer the reader to one when glossing the meaning of another: so, *opustit* will be defined as 'to move something down, to lower [*spustit*]', while *spustit* appears as 'to move something down, to lower [*opustit'*]'. And there certainly are situations that can be named equally well with one verb, or with the other: lowering a bucket into a well, for instance, or a flag, a raised bridge, or the curtain in a theatre. On the other hand, there are contexts where only *one* of the two verbs will do: for instance, 'letting drop' a boat into the water can be named as *spustit*, but not **opustit*; but on the other hand you do 'lower' oars into the water [*opustit*], and the same verb is used for children raising and lowering their hands in class, or lowering your head and your hands, though you can also *spustit* your legs, for instance, let them drop down from a bed, assuming that you're sitting on it and your legs don't quite reach the floor. So it emerges that speakers' choices are influenced by certain peculiarities in the situations where objects are transferred to a lower level, and that these are implicitly present in the semantics of the words *spustit* and *opustit*. And in turn, it is possible to identify these peculiarities analytically.

Let me first draw attention to the fact that when objects are transferred to a lower level, two distinct sub-processes can be identified:

(a) as a result of the transferral, the object ceases to occupy level l_1,

(b) as a result of the transferral, the object now occupies level l_2.

As a rule, native speakers of a language tend to concentrate on *one* of the two sub-processes, (a) or (b). And this choice will in its turn pre-determine the choice of verb used to describe the process. The verb *spustit* is used when sub-process (a) is of more interest; if, on the other hand, sub-process (b) is in the centre of attention, then *opustit* will be preferred. Or to put it differently, *spustit* primarily signifies to move an object *down* (from start level l_1), while *opustit* means to transfer an object so that it is now placed on a lower level (to final level l_2). So now let's reconsider concrete examples. The verb used with level-crossing barriers is 'lower' (*opustit*), because the horizontal position is more fundamental than the raised one, conveying the prohibition on crossing the railway-line. For this reason, the fact that the result of the movement is transference to the *lower* level will be stressed. On the other hand, in knitting, the expression used is *spustit petlyu*;[1] evidently, the important thing for the speaker here is to emphasise the *absence* of the stitch on the higher level (i.e. the knitting needle), and the question of where the stitch may have gone is secondary. The expression *spuskat s lestnitsy* [lit., 'to let drop downstairs', idiomatically, 'to kick downstairs', 'kick out'] is used of an unwanted guest — here again the main thing is to mark the person's absence in the initial (raised) position, and so on. In a case where both the initial and the terminal position are considered equally relevant, either verb will do. Thus, the distinctions in the semantics of the two verbs are connected not with the objective characteristics of the situation represented., but with the ways in which these are registered in the consciousness of the speaker.

The example of these verbs raises the same issue about whether semantic phenomena can exist independently of the consciousness of an observer, in other words, about whether they can exist in objective terms. The question of whether speakers know about this distinction in the semantics of the two verbs when they choose which to use has to be answered in the negative. As it turns out, even lexicographers aren't aware of the difference. But if we ask how it then is that people manage to use the two verbs 'correctly', i.e. in harmony with the principles outlined above, then the following answer might suggest itself. There are relatively few situations where one or other verb might be used, and it is not difficult simply to remember these; new situations not previously verbalised will be handled by analogy with the cases that are already familiar. In other words, native speakers of a given language have no need to know what lies behind the distinctions between *opustit* and *spustit*, or

1 In English, one also says to 'drop [i.e. let drop] a stitch' [Editor].

mokryi, *vlazhnyi*, *syroi*. They are passed ready-made formulae of usage and simply follow these when making their own utterances.

Thus, there do exist objectively-verifiable data — the units of a given language and the rules according to which these may be combined (both grammatical and semantic). The semantics of given signs is expressed by their arrangement in a given text. The analyst of semantics identifies in the internal organisation of words certain components of signification — ideal or mental structures, often of a hidden and unexpected kind (for instance, a study carried out by me of the semantics of the verbal phrase *vzyat s soboi* [to take with one, take along] illustrated how usage was determined by a concept of clothes as an inseparable part of a person). But what, finally, do these mental entities signify? How did they come into being? Can we really consider that we are uncovering something hidden from view when we discuss them, or are we in fact creating, constructing something that previously did not exist? If we decide on the first interpretation, then it brings with it a logical follow-up question: whose consciousness registered these subtle discrepancies — in fact, not just 'registered' them, but also embodied them in the semantics of linguistic units, as reflected in the rules according to which these words are used?

Obviously, ethnologists reconstructing the archaic world picture or the semiotics of behavioural forms are also faced with these questions. When they combine material from different literary and ethnographical texts, normative guides to behaviour, and linguistic data, and extrapolate concepts from such sources, and then attribute these to the consciousness of the bearers of archaic culture, ethnologists have no guarantee that the knowledge so meticulously gleaned by the application of their conceptual structures actually corresponds to anything in the consciousness of the human beings whose lives they are studying.

This whole issue of the adequacy of knowledge strikes me as immensely complicated, and I must say I see no easy answer to it. Therefore, I look forward to hearing what other participants in the Forum will have to say.

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