

Participants in Forum 20: Ten Years and Twenty Numbers On

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Forum 20: Ten Years (and Twenty Numbers) On

A decade ago, in 2004, the first number of *Antropologicheskii forum* appeared (closely followed by No. 1 of the English-language version). The Forum in both the numbers was dedicated to the most significant developments in the study of culture, particularly anthropology, in recent times.¹ We had originally considered returning to look at these issues again, but time has left the discussion behind, and many of the questions have now lost their topicality. The shift from rural to urban topics, away from traditional folklore to the study of the present day, and from 'traditions' to the present day, is now taken for granted.

At the same time, reflection on the state of things in the academic world and the study of culture has, obviously, not at all lost its relevance, and we decided to initiate a discussion on the developments that have taken place over the last decade. The following questions, in particular, suggested themselves:

1

What are the most important changes that have taken place in your particular area of specialisation, and in your discipline as a whole, over the last ten years?

¹ 'Sovremennye tendentsii v antropologicheskikh issledovaniyakh' // *Antropologicheskii forum* 2004. No. 1. Pp. 6–101. <http://anthropologie.kunstkamera.ru/files/pdf/001/01_01_forum.pdf>; 'Cultural Anthropology: The State of the Field' // *Forum for Anthropology and Culture*. 2004. No. 1. Pp. 10–106. <http://anthropologie.kunstkamera.ru/files/pdf/eng001/eng1_forum.pdf>.

2 *How has the make-up of your field changed in terms of the people working in it (their age, the topics they are working on, etc.)? To what extent do interests differ between generations (or not!)? Which particular subjects are currently the focus of attention, and which would you like to see become the focus of attention?*

3 *How have the relations between academia and the external world (the institutions of state, society/the broader public) altered? What changes would you identify in the relations between the different disciplines?*

4 *Have your own academic and scholarly interests, methods, topics, and the 'geography' of your studies altered over the last decade, and if so, in what way?*

The answers from the seventeen participants, including anthropologists, historians, folklorists, and specialists in cultural studies, appear below.

LEVON ABRAHAMIAN

1

The *Forum* questions have made me look back over what has happened during the last ten years in the field in which I am working, and I have realised that there has not been much change in the scope of the problems. I do not see anything good in this — for myself, that is. Like the rest of my colleagues in the Contemporary Anthropology section (of the Institute of Archaeology and Ethnography of the National Academy of Sciences of the Republic of Armenia) I am continuing to study everything that surrounds us, disappears and changes. When I answered the questions of the first *Forum*, I was ambitious enough to suggest that an anthropologist might, in principle, record everything and describe the present day on the micro-ethnographic level in such a way as to leave little for the micro-historians of the future to do. I no longer think so. Far from it, our arbitrary descriptions are more likely just to create more confusion for the historians of the future. I have tried to convey this indefiniteness and openness in my book *Armenian Identity in a Changing World* [Abrahamian 2006], using the structure of the book,

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the chapters of which are constructed like avenues and paths through the public park of identity, which on one level might turn into the Garden of Eden, and on the other into a labyrinth, a ‘garden of forking paths’ with no way out. In another book, published soon after the first *Forum* [Abrahamian 2006], I tried to express the indefiniteness and ambiguities of the field in which I work in another framework, which some people will regard as not entirely scholarly, and others, indeed, as not scholarly at all. The *Conversations* attempt to return to a condition in which poetry, magic, scholarship and art were not yet divided, but taking into account our contemporary knowledge and our irreversible fragmentation. To a certain extent this approach is akin to the proximity between anthropological participant observation and the creative process recently discussed by Tim Ingold [Ingold 2013a; 2013b]. The feeling close to mystical experience undergone by anthropologists studying the present day, and which can be discerned in Tim Ingold’s conclusions, is connected, I think, with the existential experience of observing/existing in a system open to any form of change, including change in the witness/anthropologist. It is typical that he defines the described past, as one might say, as ‘ethnography’. It is the ‘-graphy’ that is important to him: if there were such a word as ‘anthropography’, that is no doubt the word that he would have chosen. While I share to the full his feeling/understanding of an object of observation open to the future, I might add that an involvement with an ethnographic trace left in the past is for me a no less exciting ‘participant observation’, but one directed towards the past. I said something of this in the first *Forum* — the anthropologist as shaman. Over the last ten years these two feelings of involvement — with the past, interpreted through its remains, and with the future, interpreted through its omens — have become even more acute. The two interpretations may be compared with the two hermeneutics of Vartan Hairapetian — the scholarly hermeneutic, which looks to the past and reconstructs, and the popular hermeneutic, which looks to the future and predicts [Hairapetian 2011: d121; d1211; d63].

2

The ‘younger’ generation on the whole chooses topics ‘that will fly’ — particularly if they can get grants for them. The titles are often more enticing than the contents. This, though, can be seen in international anthropological practice too — one has but to compare the headings of the articles in the leading journals with their content.

In the 1990s the Armenian Academy of Sciences decided to go over to an entirely grant-based system: the budgets for long-term topics were withdrawn, and finance was given to short-term thematic groups. I was a member of the commission that considered applications for funding in the social sciences, and I was struck by the fact that every thematic group was anxious to submit an application with an engaging title, in order to beat the competition. Topics were financed

for three years only, and the commission had to be convinced that the topic was important, profound and original. It is clear that fundamental topics do not always sound engaging — they can, indeed, even sound boring; moreover, applications which promised fundamental research could not guarantee to complete it, because the more fundamental and engaging the topic sounded, the fewer chances there were that it could be completed at the requisite level. In their next application, researchers would submit a different engaging topic, fearing that they would not get funding for the same one twice. As a result we got shallow work with profound titles, followed by new applications for deeply serious topics which would obviously be studied superficially. We have now returned to ‘boring’ subject areas with permanent budgets, and only a very few topics funded by grants. The younger generation’s ‘topics that will fly’ remind me of the failed financial experiment just described.

Another problem, foisted upon the social sciences by colleagues from the natural sciences, is the bean-counting practice of impact factors and having to publish in specific prestigious or pseudo-prestigious journals in a standard format, excluding or minimising everything that I discussed under point 1. Here too I am in complete agreement with Tim Ingold, who has remarked that nowadays people write more for the pleasure of the publisher than the reader: one of his interviews has the eloquent subtitle ‘Tell me in what journal you publish and I will tell you who you are!’ [De Lauri 2013]. At the same time I must reluctantly admit that this is an effective means of defending professional anthropologists from ethnographic impostors (on whom, see point 3) and senior officials who want a degree in the social sciences.¹

The relationship between the ‘older’ and ‘younger’ generations is also evident in the context of the questions raised under point 4.

3

The professional anthropologist has ceased to be the key figure that he or she still was, in a certain sense, ten years ago. They were such in the 1980s and early 1990s, at the time of the collapse of the USSR and the formation of the new republics. This was a phenomenon that included the whole of the USSR. Igor Krupnik told me about the situation in Moscow, when anthropologists (or ethnographers, as they still were then) were expected to explain what was happening, and not only by the government, which required expert consultations, but by ordinary people too. It was the same in Yerevan. On the other hand, anthropologists themselves suddenly realised that they — or rather their discipline — was involved in the unexpected changes;

¹ In recent years it has become modish for senior government officials in the Russian Federation and other post-socialist countries to ‘collect’ academic degrees, including even higher doctorates. These require publications in learned journals. [Eds.]

it is no accident that there were anthropologists among the politicians of those troubled times, the best of whom paid with their lives for this shift in professional interest.¹

In today's Armenia the state and society have little or no need of professional anthropologists, but there are plenty of self-styled ethnographers on our television screens and in the press. I think that the Armenian name of the discipline, *azgagrutyun* (calqued from *ethnography*), has played a part here. The point is that instead of the foreign word *ethnos* (which already exists in Armenian in the form *het'anos*, meaning 'pagan'), the native word *azg* is used; this has a wide range of meanings from 'tribe' or 'people' to 'nation', acquiring the last of these at a time when the word *nation* (as in English) appeared in various languages with a single specific meaning (or with two or three such meanings). But the Armenian root *azg* is found in such ordinary words as *azgakan* 'relative, relation' or *azganun* 'surname', and the word *azg* itself may be understood in its original meaning of 'kin'. Therefore Armenian nationalism leans towards ethnic concepts (and demonstrably also towards concepts of kinship [Abrahamian 2007: 267–277]), assisted also by the ethnic homogeneity of the republic (98.6 % Armenian). It is not surprising that in these conditions ethnography may be understood as 'nationography', so that the discipline becomes a 'natural' instrument of various patriotic, primordialist and nationalist constructs. It is obvious that such processes of self-assertion are typical of all new or renewed national formations, but in Armenia they received supplementary 'disciplinary' support. Some people see a way out of the situation in 'surrendering' ethnography to the nationalists and defining themselves professionally as 'cultural/social anthropologists' (in Armenian translation). (As we see, the differentiation between ethnography and anthropology here is quite different from that discussed under point 1.) Other people would find a way out by creating an alternative, non-institutional field which would not only compete with ethnography in the narrow nationalistic orientation, but blur the strict distinctions between the social disciplines that are maintained by academic institutions and universities. One could regard as an example of such a field the weekly methodological seminars at the Institute of Archaeology and Ethnography of the National Academy of Sciences of the Republic of Armenia which for the last two years have been attracting audiences via social networks to discuss various problems of anthropology and related fields. But this is by no means the only example.

It is instructive that, despite what I said at the beginning of my comments on this point, society — or at least a small part of it — does

¹ One key example is Galina Starovoitova, from St Petersburg, who was murdered in 1998, in circumstances that are still not fully clear. [Eds.].

still to a certain extent require anthropologists. Thus the activists from the civic protest movement, who in the winter of 2012 were defending a public space (a square in the middle of Yerevan) against a project to build boutiques on it, and who continued their social activity on many other occasions, sought answers (as my colleague who studied the movement tells me) to the questions that arose in the course of the movement from none other than anthropologists. With a certain degree of exaggeration one could conclude that ethnographers (in the terms I have alluded to here) are required from above (by the nationally orientated media, state structures and ruling parties) and anthropologists are required from below, by an admittedly small but nevertheless important part of civil society.

4

The two parts of this question are interrelated, or at least so it seems to me in respect of contemporary Armenia. The vectors of development of the discipline are set by the works of individual researchers, which ultimately form distinct tendencies, which in principle might be called schools. I have in mind the inner structure of the academic sphere, and not the exterior ideas and schools on which this structure might be founded. Overall it is an opposition between the ‘older’ and ‘younger’ generations of scholars, though that opposition is not necessarily one of age. The ‘older’ scholars essentially reproduce previously existing principles and norms of ethnography, directed primarily towards ‘saving’ [Dabaghyan 2011] and preserving that which is swiftly being lost (today one could say, swiftly being forgotten) and also towards preserving the principles of an outlook on life and methodology that not infrequently belong to the nineteenth and early twentieth centuries. In each case the ‘elders’, with consistent citation, reach the incontrovertible authority of one or another scholar (living or dead) or of national priorities, while the ‘younger’ researchers attempt to interpret their observations by means of different, sometimes contradictory theories and methods, or else attempt to deconstruct the canonical structures of the ‘elders’. On the whole these groups (and there are several of them) are quite tolerant of each other and live parallel lives, so that the contradictions between them may flare up only on the rare occasions when their paths cross — during the infrequent conferences in which both take part, or common publications. A typical example of this sort of opposition (admittedly historical) is the long quarrel over the first volume of the four-volume academic history of the Armenian people. (The first parts of the third and fourth volumes appeared in 2010.) It is indicative that unlike the eight-volume *History of the Armenian People* published in Soviet times (1967–1984), it was decided to entitle the new publication on the traditional early mediaeval model, ‘Hayots patmut‘yun’, which in those days could be taken to mean both ‘history of Armenia’ and ‘history of the Armenians’: in classical Armenian both meanings are perfectly

grammatical, and both are used in translation, the former in Russian translations and the latter in English ones, with far-reaching and opposite conclusions about the nature and beginning of the formation of the Armenian identity.¹ The new edition is defined in its Russian abstract (and the Russian-language abstract is the only one it has) as the 'History of Armenia', but the first volume, obviously, has to deal with the prehistory of the Armenian people, which is understood differently by authors from the two opposing schools, the two 'generations', who see the history of their people in different ways.

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¹ See the discussion of the question in [Abrahamian 2006: 35–36, 137–138].

YURI BEREZKIN

1

I suspect that my answers will be very different from everyone else's. The field of problems with which I am concerned has not changed, being unconnected with the tendencies in the development of anthropology under discussion. I am interested in the past, for preference more than a thousand, or even better, three or four thousand years ago. The number of relevant publications, which were not numerous in 2003, continues to decrease.

2

The older generation of those people with whom it is interesting and useful for me to discuss professional topics is dying out. We have, for example, just lost Elena Efimovna Kuzmina, an exceptional archaeologist and a leading specialist in the dispersal of the Indo-Europeans and in the determination of the original homeland of the speakers of Iranian languages. Some other people are still alive, but no longer active. The younger generation (or rather, by now, the two younger generations) scarcely exists, or to be more accurate there are too few of them to establish the necessary density of connexions, either personal or, more particularly, professional. This position is unlikely to change in the foreseeable future. The vast majority of the people who leave school or university do not possess what I would consider to be the essential knowledge, and their professional interests rarely come into contact with mine. Over the past ten years — and even more over the past fifteen or twenty years — society's informational background has changed substantially. I am by no means inclined to believe that young people nowadays know less than I did when I was twenty or twenty-five. But they know about other things — computer programmes, iPhones and tablets, how to get grants, ticket requests and hosting, and presumably also scientific theories and schools. But their knowledge of history, geography, astronomy, and most probably literature is ever more limited. Their knowledge of languages is also unsatisfactory.

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In the best case, decent English, but not often German, French, and so on. And this at a time when the opportunities for learning languages and using this knowledge at work and in everyday life are not to be compared with conditions behind the iron curtain. I would say that tendencies of this sort go hand in hand with other tendencies that are typical of our society, from the introduction of the Unified State Examination to the liquidation of the Academy of Sciences. Science and education are amalgamated with business, where what matters is the sort of efficiency that can be measured with tables, indices and ratings. And it would be odd to affirm that efficiency and productivity are unnecessary. It is like the transition from cottage industries to the Ford production line. It would be foolish to mourn for the good old days, but I am unlikely to fall in love with modern times. I have always had a critical view of Thomas Kuhn's hypothesis, viz.: the evolution of science is the succession of the ideas and methods which the majority in the scientific community chooses as the most convenient, but the question of which paradigm best reflects reality is not a correct one. I shall never agree that there is no 'how it really is'. But Kuhn is partly right: when paradigms shift, researchers' and society's attention is shifted to other objects and topics, and what until recently appeared important and interesting no longer attracts attention. Society even took the confirmation of the existence of the Higgs Boson as a minor episode in yet another soap opera, so what are we to say of the early pottery of Eastern Asia or the settlement routes of the New World?

Over recent years there have been more and more people amongst the school-leavers applying to the Faculty of Anthropology at the European University who have lacked the relevant education and have known little about the discipline to which they were evidently prepared to commit themselves. Surely this cannot mean that anthropology is perceived as a 'soft' discipline, requiring not so much knowledge as the gift of the gab? Another alarming symptom, from my point of view, is the textbook of ethnology/ethnography 'for graduates' prepared by the Faculty of History of St Petersburg State University which is about to be published. The last thing I want to do is criticise individual authors (of whom I am one, with my 'peoples of America'), but the use of the term 'protocivilisation' or the placing of the homeland of the 'Nostratic community' in north-east Africa, say, send a shiver down my spine. The transliteration *stoki* (English 'stocks') in the sense of 'macrofamilies' is ridiculous — it reminds one of the Bulgarian *khranitelni stoki* ('groceries'). One gets the impression that some of the authors have lived as hermits for decades, and now have tried to speed-read S.I. Bruk.¹ But that is not the point.

¹ Solomon Bruk (1920–1995) was a leading Soviet geographer and ethnologist and author of many publications on 'the peoples of the world'. [Eds.].

Students, in all probability, find the ‘Nostrates’ as irrelevant and incomprehensible as anything that is said about the Austronesian or ‘Ando-Equatorial’ families. This is the sort of information that could be rearranged at random without having any effect on the results. The problem is that it is boring (though a bit more interesting when it comes to Lev Gumilev, to whom, it seems the author of the text is not indifferent). A normal living being, after reading a textbook like that, would not want to take up anthropology. There is feedback in the system: bad textbooks mean uninterested students, and a lack of interest on the part of the students means that there is no point in putting any effort into the textbook. There are of course those who will learn anyway, if they have had the necessary impulse at home, but when one thinks how many people capable of giving such an impulse to their children have left Russia, the prospects do not look bright.

3

I cannot answer for America, Europe or Japan, but in Russia neither society nor the élite have any need of scholarship. If our institutes and all their staff were suddenly to disappear, no one would take the slightest notice. It is indicative that the remarkable discoveries made in the last twenty years have passed unnoticed in society. It would be interesting to conduct a survey to find out who, and in what social groups, knows anything about dark matter or about Göbekli Tepe — I fear that it would be less than one percent even of university graduates.

I watched a programme on ORT about the sham doctoral theses submitted by state officials. It turns out that it is not so bad if the man has copied a whole chapter from something published abroad — the main thing is that he has put a bit of effort in, and the actual chapter is a secondary consideration. And if paragraphs taken from other people’s work are not placed in quotation marks, that is just because it was done carelessly or in a hurry. All this was said, without a hint of irony, by people involved in the academic world. It is not hard to guess what the officials themselves think of academia, or their attitude to it.

The economic crisis has also complicated the situation. Since 2008 American grant awarding bodies have become more careful with their money and it is not often that they are prepared to pay the travelling expenses of conference participants from overseas. The *dolce vita* of the middle of the first decade of the century, when one had but to choose between Santa Fe and Anchorage, is over. As for connexions between anthropology and other related research areas, they are all on one side — with sociology and psychology. I hesitate to say how fruitful they are, I simply do not know. However, the links with the historical disciplines are all but severed.

It is no use moaning about this, but it would be interesting to determine the *longue durée* in which these processes are situated. It seems to me (though the question is highly debatable) that its roots go back to the nineteenth century, or, if more recently, to the collapse of German scholarship after the Second World War. German anthropology was strange and, to put it bluntly, twisted. Reading some of the arguments of Leo Frobenius or K. T. Preuss, one really does begin to wonder whether they were in their right minds. Nevertheless, it was a great academic school, and without German research in South America, Africa and Oceania, our fundamental knowledge of the folklore and ethnography of the peoples of the world would be pitiful. (And indeed, the two geniuses of physics, Albert Einstein and Max Planck, were also born in Germany, and not, say, in America.) German anthropology could not continue to exist in the state in which it had been in the 1930s, it was simply an anachronism. But instead of evolving and modernising itself, the German school, as a distinct tendency, directed towards the study of the past (and not the present, like the English school), ceased to exist. Something similar is happening in Russia. Russian scholarship is connected with German scholarship in its origins, but certain events a hundred years ago prevented it from developing naturally. The isolation in which Soviet scholarship existed is yet another tragic page in the history of our country. But after the collapse of the Soviet system, our scholarship did not so much make use of the opportunities that had opened before it, as set about liquidating itself. Something, of course, will remain — people do good work in Germany these days too. But it will not be possible for Russia to continue to exist as an independent academic centre. And this is not only bad for Russia (who cares about that now?), but first and foremost for the discipline, which is putting all its eggs in one American basket.

4

My interests have not changed; my methods, I hope, have improved a little, and the subjects and geography of my research have shifted somewhat. The world folklore database <<http://www.ruthenia.ru/folklore/berezkin>> was begun in the 1990s to solve a particular problem — the settlement routes of the New World, and to find those regions of Asia where the folklore and mythology have the greatest number of parallels to the folklore and mythology of the Native Americans. For this reason, until the middle of the first decade of this century it basically contained those motifs and subjects recorded in the Old World that have analogies in America. However, my interests gradually shifted to Eurasia and Africa. Africa is reasonably clear — there are a handful of early motifs that were already known to primitive *Homo sapiens*, and almost everything else is a recent import from Asia. But sorting out Eurasian folklore — that is what you call a challenge.

KONSTANTIN BOGDANOV

1

The last ten years have changed a lot. These changes, I think, concern not the content of anthropological research, but the social and psychological circumstances that justified it ‘then’ and justify it ‘now’. Ethnography and folklore studies at the beginning of the century — speaking of the situation in Moscow and St Petersburg — seem in retrospect to have lived through a time of somewhat vague but very exciting hopes and expectations. The very fact of the appearance of *Forum for Anthropology and Culture* may serve as evidence, on the one hand, of an awareness of the disciplinary, methodological, and institutional problems of the ethnography and folklore studies of that time, and on the other the realisation of the changes in qualifications in that transitional area of folklore and ethnographic studies which has come to be known in its Russian variant by the somewhat imprecise but nevertheless inspiring name of ‘cultural anthropology’. The role played by *Forum* in encouraging the research drive of scholars intent on correlating their thematic interests with the innovations of world ethnology and associated disciplines cannot be exaggerated. At a pinch one can even say that *AF* filled the gap left at that time by the Academy journals *Etnograficheskoe obozrenie* [Ethnographic Review], *Russkii folklor* [Russian Folklore], *Sotsiologicheskie issledovaniya* [Sociological Research] and *Voprosy yazykoznaniiya* [Questions of Linguistics]. Even before then there had been no thematic barriers to enlightened curiosity, but thenceforth its frontiers had acquired another source of corporate, informatics, and editorial support.

Engaged as I am in research on historical fragments of discursive and social experience, I find it hard to subdue them to any discipline, but it is easier for me to imagine the means of conceptualising these fragments in terms of ‘cultural anthropology’ than, say, literary studies — and I consider myself fortunate even

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to have such a choice. Therefore I take a positive view of the changes that have taken place over recent years, as a whole, in that field of knowledge which corresponds to the articles and materials that are published in *AF*. But as it is an ill wind that blows nobody good, so it is an unreal good wind that blows nobody ill. It seems to me that the freedom of choice of topics and subjects, which at the beginning of the century served as a stimulus to work and to the discovery of like-minded people in the corporate or 'imaginary' community of anthropological researchers, is now less attractive to many of those whom I should like to regard as my colleagues. If the past of ethnography and folklore studies under the old regime is usually illustrated by examples of methodological scholasticism, rote-learning and tedious impositions, nowadays the appearance of quite a few 'anthropological' texts is due to their predetermined correspondence to academic fundraising, and the emphasis they place on certain easily identifiable ideological, political, and polemical motifs. The substantive value of such works often amounts to no more than their titles, which consist of a series of variations in different keys on the stereotypes of explanatory 'constructivism', social determinism, and everything else that their authors imagine will 'sell' on the academic grants market.

2

The sociologists of science should make their voices heard in answer to this question. It would hardly be fair to judge this 'by eye', since the change of generations takes place differently in different academic communities and institutions. And the difference between the scholarly output of a 'young' scholar and a 'mature' one (to put the latter euphemistically) is not only thematic. Their methodological and even stylistic inclinations are also important: the history of any discipline in the humanities (and in this country too) is full of interconnections between expert pronouncements and rhetorically coloured value judgments — not only in terms of what is said, but in terms of how it is said also. The once revolutionary, now commonplace opinion that the humanities and historical sciences are, whatever else they may be, also *écriture* — that is a form of literature — seems to me both correct and, in a heuristic sense, fruitful. As for the change in academic interests from one generation to another, it might perhaps also be possible to discuss what the young and old prefer to write: the former incline more to manifestos, the latter to memoirs.

3

Both attitudes have changed. The discussions about the reorganisation of the Russian Academy of Sciences have made blatantly obvious the mechanisms and motives designed to transform a learned society into a management group acting in the interests of state officials who speak in the name of the state, of society, of 'social goods' and so on. The future of anthropological disciplines depends to a large extent on how far real, and not imaginary communities are seduced by the powerful methods of demagogic (self-)persuasion regarding the

efficiency/inefficiency of academic institutions. That these disciplines have had a certain success in fundraising shows that the transformation of their scope is already to a certain extent determined by their legitimisation within the structure of the ‘project mentality’ of grant-awarding bodies. The process of the ‘statification’ of scholarship intensifies this situation *nec plus ultra*. One might suppose that a shift in the boundaries of academic knowledge will in this case (the worst case for Russian scholarship) be determined not by the epistemological rules of research and educational practice, but by a valorisation of the ‘scientific product’ offered.

4

The definition of anthropological disciplines is problematic and contingent. This case is a particularly striking confirmation of Davidson’s old thesis concerning the incompleteness of conceptual schemes in scholarship. History — society — culture — human being: these are all concepts which any ideological project can easily turn into operational markers to be used in the self-representation of this or that ‘school of thought’. In my own work I am interested in the works of particular researchers. That one can also speak of a degree of ‘scalability’ of those academic trends within the course of which such works are written is another matter. But here I will allow myself to descend to truism, preferring those which are based on the rules of verifiability and falsifiability of research data and which display subjective relevance in terms of those questions which we ask ourselves and others.

VLADIMIR BOGDANOV

The last ten years have been very productive for fieldwork. The development of the material-technical and informational base has significantly stimulated work in this direction: the search and analysis methodology has been improved, as have the methods of collecting and recording material. The removal of the boundaries between researcher and informant (first and foremost thanks to oral history) makes for a better dialogue between them.¹ At the same time researchers are making ever more active use of previously collected field material.²

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¹ There was much discussion of this in on of the previous ‘forums’ in this journal [Ustnye materialy 2012]. On the erasure of boundaries between researcher and informant by oral history, see also [Tompson 2003: 24–26].

² All these tendencies can be seen very well in [0 svoei zemle 2012].

In the process the actual set of problems addressed has undoubtedly changed. Whereas formerly researchers were concerned with questions of local history or the social practices belonging to a particular community, now there are more and more works appearing that attempt to lift the curtain on forbidden (or at least, once forbidden) knowledge. When recording material in the field it has been noticed that the bearers of the tradition do not observe the prohibitions so strictly now, and are, if not pleased, at least not particularly embarrassed to talk about witchcraft, evil spirits and so on. E. B. Smilyanskaya has quite rightly remarked, ‘Even the old women who are most strictly bound to the congregation¹ are almost always understanding towards their “bewitched” neighbours when they have recourse to “powerful means”, meaning spells (as above — “special words”) or the help of “outsiders”, “stronger” enchanters and “seers”. Any means are acceptable in the struggle against the world of evil, even the notorious cultural and confessional separateness of the Old Believers breaks down, and they “seek healing” from their neighbours the Udmurts, the Niconian Russians, and the Komi. True, only twenty years or so ago, if members of the congregation had recourse to such forms of healing and anti-magical practices (as indeed if they consulted doctors and took medicine), they would have to suffer a heavy penance and long exclusion from communal prayers. But now such rules are less and less often remembered’ [Smilyanskaya 2012: 141]. It is noteworthy that of eighteen narratives about evil spirits and fifteen narratives about enchanters recorded in the Verkhokamye, only one of each was recorded before the 1990s [Materialy 2013].

Moreover, the relaxation of prohibitions concerns not only the study of the phenomenon of witchcraft, but also such spheres as conflict, deviant behaviour, certain illnesses, etc. within closed communities. All this has become possible because the informants are more open in comparison with former times. In some areas unexpected discoveries have been made. For example, V. L. Dyachkov’s research has shown what health problems were experienced by women in the USSR.² It turns out that every one in two women in the USSR had had an abortion [Dyachkov 2008: 163]. This somewhat delicate subject could hardly have been broached even fifteen years ago. It was not acceptable to talk about it, even if it had already been raised in the cinema.³ It would appear that society as a whole has tended towards

¹ This refers to Priestless Old Believers, for whom the centres of spiritual life are the ‘congregations’ (in the Verkhokam’ye) or ‘brotherhoods’ (on the Southern Vyatka), who are supposed to live righteously, keeping as far away as possible from worldly vanities.

² The work in question names no names and preserves absolute anonymity in this area.

³ In the film *Surgeon Mishkin’s Days* (1976) O. N. Efremov’s hero, a provincial doctor, remembers how he often had to carry out abortions in his youth, but is no longer prepared to undertake them. In the film *White Dews* (1983), V. V. Sanaev’s hero reproaches his son because he and his wife ‘had left their

a sort of ‘spiritual striptease’. Whereas formerly the informant might abruptly interrupt the interviewer if certain delicate questions were asked, now s/he is happy to answer them. This is evidently reinforced by the influence of the mass media, where such tendencies, taking the form of talk shows and such like, have long since become the norm. Where once gossip was the preserve of a narrow social milieu, now thanks to the mass media it becomes known to the wide readership of the tabloids and ‘women’s magazines’ and to television viewers and listeners to the wireless. Accordingly the researchers’ attitude to it also changes.¹ In particular, one would hardly have expected to find the phrase ‘one really need not take any notice of gossip’ in serious literature a decade or more ago. The reaction would have been ‘not that one need not, one must not’. Researchers now often elicit intimate details from their informants, carefully recording them in their field diaries or on their Dictaphones.

The influence of the mass media has made itself felt much more strongly over the last ten years. In 2009 the spiritual father of the local Old Believers (Filippovtsy) asked the children who had come to visit their relatives in the country for the annual memorial service, ‘Is it nice to have a small house in the country?’² A year earlier members of an expedition had been told (admittedly by an ordinary member of one of the brotherhoods), that the end of the world would be ‘on 21 December 2012 — so the scientists say. The planets, Mars, Venus and Saturn, will be arranged in a particular way, “in the same direction from the sun”. The Lord said that I will let you confess all your secrets, but I will not let you live’. Later they were told that a rocket was being made in France to uncover the mysteries of the origin of the Earth, but that it might bring about the end of the world (Author’s diary, 4 August 2008. Ch—na F. E., born 1940, a Filippovka-Shikhalevka from Shurma, Urzhum Region, Kirov Province) [AMAL 2008]. Both the ‘Small House in the Country’ advertisements and the hadron collider (though this was being built in the Czech Republic, not France) had become known through television, though until very recent times members of the brotherhood (not to mention their spiritual leader) were not supposed to have a set. Besides, thanks to the Internet, researchers’ work may become accessible to the bearers of the tradition, which inevitably serves as a corrective to scholarly investigations.³

children at the clinics’ [a euphemism for aborting them. — Eds.]. It may be that literature and film featuring fictional characters based on real life could be used by researchers to permit a more objective study of delicate topics, allowing a more abstract approach to them. When they are discussed with real people, the question of ethics is never fully resolved.

¹ There is interesting work by Anna Kushkova on this [Kushkova 2003a; 2003b] etc.

² ‘Small House in the Country’ (Domik v derevne) is the name of a well-known brand of dairy products. [Transl.].

³ For more detail see [Ustnye materialy 2012: 23–24].

At the level of the present dichotomy between the town and country, one must say something about the further interpenetration of the two subcultures. In particular, some spiritual leaders in the villages are now also pastors to their flock in the towns. For example, the spiritual father of the Pomortsy-Maksimovtsy in the Verkhokamyë, who lives in the village of Sokolovo, ‘receives’ Pomortsy from Perm, and has moreover the appropriate permission to do so, issued in 2010 by the Russian Council of the Old-Orthodox Pomorye Church in Moscow. The reverse also applies. Some activists from village communities try to ‘establish themselves’ in urban communities, even though at home they are not always able to acquire the necessary authority for leadership.¹

As for the general shift of research focus ‘from the country to the town’, this process is evolving faster than ever. And the reason for this is as a rule nothing to do with the development of scholarship in a particular direction, but the result of external conditions — the catastrophic depopulation of the countryside, which, indeed, is becoming unrecognisable. This process was already discernible in the 1970s and 1980s. As I. V. Pozdeeva remembers, ‘dozens of villages vanished before our very eyes during the first two or three years of our work’ [Pozdeeva 1996: 13]. A letter such as this is eloquent testimony to the process: ‘Fotiya took Marya Ovdiiykha away, they live somewhere on the other side of Belova, and Marya died on 20 March, the people have all gone away not many are left Marko has gone, all to different places, Sinka Mitina has gone Sanka Sinkin has gone Zhulanikha has gone Igolnikov has gone Semen has gone Yeremikha has gone Marusika Vanina has gone Mishkha has gone Mishka Mikeshyn has gone Tanya has gone Onton Zhidanov has gone Lenika Yegrafov has gone Savva has gone Viketika has gone’ [Materialy 2013: 186]. Since 2010, in particular, the four most literate informants on the Southern Vyatka have left the countryside and gone to live in the central towns of the province and its regions. When the leader of the Fedoseevets community in the village of Ruskii Turek left, that was the end of the local Fedoseevets tradition: the brotherhood fell apart, and the last ‘old woman of the brotherhood’ died in 2012. Besides, there is not much difference now between life in the village and life in the town (except perhaps in the amenities). As a rule, the villagers have stopped keeping cattle and reduced the size of their vegetable plots, which results in changes to their pattern of life, and thus to the tradition.

If in 2003 the confession of one of the participants in the *Forum*, that one of the subjects of his fieldwork had himself sought him out, sounded very touching and rather unusual [Sovremennye tendentsii

¹ For the relations between the town and country communities of Fedoseyevtsy see [Soldatov 2012].

2004: 9], now this is, to a significant degree, a general tendency. However, in this story told by Levon Abrahamian there is already a note of alarm on account of the shift in the perspective of the research: since rural culture, being less subject to change than urban culture, has long been considered as the model of national culture, the very fact of a shift of interest towards urban culture 'indicates a decline of interest in the roots of culture, in the problem of origins...' It is worth remembering that Pushkin's epigraph to the second chapter of *Evgeny Onegin* is 'O rus!.. O Rus!', a play on the Latin *rus* (countryside) and the traditional name of the Russian state.

The changes in the scholarly community itself are that new leaders have appeared and the position of the old ones has weakened. This weakening is often due to subjective factors. In particular, over the last ten years a number of fieldworkers have left the Inter-Departmental Archaeographic Laboratory at the History Faculty of Moscow State University: V. I. Yerofeeva, who was for many years in charge of documentary practice (in the field as well), E. V. Gradoboinova, A. V. Dadykin, V. V. Makarovskaya, I. I. Pryakhina¹ and others. One cannot but be concerned for the future of this centre (which even earlier had lost such well-known scholars as E. B. Smilyanskaya and E. A. Ageeva). As for new centres, this century has so far seen a real boom in fieldwork. In particular, I. Yu. Trushkova's school has been very active in a wide range of anthropological problems, usefully collecting and studying field material in Kirov Province. Unfortunately, the various fieldwork centres do not always co-ordinate their activities. The result is that not only do different expeditions follow each other round the same places, but also a palpable weakness in elaborating the historiography of various questions. Researchers are frequently convinced that they are examining a topic that no one has considered before, while the very same topic is being examined in parallel at another centre. In this context one might remark on E. I. Krivosheina's article [Krivosheina 2010], in which she studies the Old Belief on the Southern Vyatka, taking no absolutely no account of the work of her Muscovite colleagues² and with only a partial awareness of the work of scholars

¹ It should be pointed out that these authors are responsible for a number of important works. E. V. Gradoboinova posed the question whether conflict, as a factor in the development of a *socium*, is a permanent presence in the Old Belief and an essential characteristic of it [Gradoboinova 2008], M. V. Makarovskaya and I. I. Pryakhina wrote on the tradition of spiritual verses in communal life, using the *Verkhokam'ye* as an example [Komu povem 2007], and A. V. Dadykin on the circulation of early-printed Cyrillic books at the end of the twentieth and beginning of the twenty-first centuries, concluding in particular, on the basis of concrete examples, that early-printed books served the same purposes in the Old Believer community in the present day (or at least until very recently) as they were intended for 300–350 years ago [Dadykin 2013]. The field diaries of these authors were the basis for such publications as [Materialy 2012; Materialy 2013].

² By 2010 such works as [Filippovskoye rodosloviye 2004; Iserov 2008; Gradoboinova 2008] had already been published.

from Kirov.¹ This is the more surprising in that the informational space in which historians, ethnographers, sociologists and others are working has expanded considerably, and the search for the requisite information (particularly the historiography of a question) has become that much easier. We should not omit to mention here the positive effect that eLIBRARY.RU has had on the research process, as has the availability on the world-wide web of a number of periodicals (including *Antropologicheskii forum*), dissertation conspectuses, and so on.

However, the end to funding for student practicals announced in 2013 must be a cause for concern. This will mean a considerable reduction in the activities carried out by universities in the field. At the same time the funding of a number of directions through grants (noteworthy in this context is the award of a mega-grant to Tomsk University for developing research in social anthropology²) allows us to believe that the situation in this area is not hopeless.

Abbreviations

AMAL — Arkhiv Mezhkafedralnoi arkheograficheskoi laboratorii [Archive of the Inter-Departmental Archaeographic Laboratory]

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¹ Kirov is the political and cultural capital of the region of Vyatka. [Eds.].

² <http://tsu.ru/content/news/news_tsu/39680/>.

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MARK EDELE AND DEBRA MCDOUGALL

1

'Whatever you people do,' the historian among us was advised in the early 2000s by an anthropologist he met at a conference, 'stop citing Clifford Geertz!' This frustrated outburst encapsulates part of the relationship between anthropology and cultural history ever since the 'cultural turn': historians tend to take their cues from anthropological theory, while anthropologists move on to the next big thing. Two of the most important buzzwords which migrated into cultural history during the last decade were 'transnationalism' and 'emotions'. Ten years ago, there were a few manifestos to acquaint students with what 'entangled history' might be [Werner and Zimmerman 2002; 2003]; today one can buy textbook introductions to the subject [Iriye 2013; Saunier 2013] and even

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a *Dictionary of Transnational History* [Iriye and Saunier 2009]. Likewise, back in 2003, when participants at a history conference at the University of Chicago still grappled with William Reddy's path-breaking *Navigation of Feelings: A Framework for the History of Emotions* (2001), senior historians proclaimed that they might engage in this field as something of a hobby, but advised PhD students against working on such 'soft topics'. Today, the legitimacy of what once was something of a recreational diversion for the establishment has been ensured by a lengthy discussion in the American flagship journal, *The American Historical Review* (2012), detailed methodological guides [Plamper 2012], a research centre at the Max Planck Institute for Human Development (Berlin), not to speak of a Centre of Excellence in the History of Emotions at the University of Western Australia, funded by a large grant of the Australian Research Council.

If transnationalism and emotions are hot trends in cultural history, they are now somewhat old news in its sister discipline. By the mid-1990s, prominent anthropologists had already outlined research agendas for studying processes of cultural globalisation through innovative, often multi-sited, research projects [Kearney 1995; Marcus 1995; Appadurai 1996]. Interest in emotions in anthropology peaked a decade before this transnational turn. Lutz and White [1986] noted that transdisciplinary interest in the topic had grown since the 1960s; by the 1980s, anthropologists within and beyond the subfield of psychological anthropology wrote ethnographies centrally focused on affect (e.g. [Briggs 1970; Schieffelin 1976; Abu-Lughod 1986; Myers 1986; Lutz 1988; Burbank 1994]).

Meanwhile, back in the sister discipline and among historians of Russia both transnational history [David-Fox 2006; 2011a; 2011b; Geyer and Fitzpatrick 2009; David-Fox et al 2012; Edele 2014a] and the history of emotions [Boym 2001; Fitzpatrick 2001, 2004; Suny 2004; Slavic Review 2009; Elie, Plamper, Schahadat 2010; Steinberg and Sobol 2011] have found early adherents. Nevertheless, neither had much bearing on the single most influential debate within this field in the last twenty years [Chatterjee, Petrone 2008]. The idea of a 'Stalinist subjectivity', indeed, was premised on the assumption that the Soviet Union was a civilization *sui generis*, despite its 'modernity' essentially different and closed off from western civilization [Malia 1994; Kotkin 1995]. Such a subjectivity was something that took shape within a Soviet habitat, not a transnational one, and it took place more as an intellectual engagement of individuals with ideology than as an immersion in turbulent emotional seas. It was no accident that the most prominent historian in this field focused initially on the 1930s [Hellbeck 2006], where the assumption of the closed nature of the system makes somewhat more sense than for the war or postwar years [Edele 2006]. The debate has now moved ahead and reached the years of the war [Krylova 2010; Reese 2011;

Johnston 2011; Hellbeck 2012; Markwick and Cardona 2012; Edele 2013; 2014b]. We will see if the old suppositions can hold in these years of both heightened emotions and increased transnational flows of people, weapons, and ideas.

If history seems to follow anthropology's theoretical footsteps, the reverse is true for research methods, with anthropologists of recent generations following historians into the archives. In the 1980s, studies of colonialism moved into the mainstream of the discipline (e.g., [Cohn 1987; Comaroff and Comaroff 1991]). At the same time structuralism's timelessness was opened up by practice theory and studies of structure and history [Sahlins 1985]. With the turn toward history, anthropologists began to take Christianity as a focus of study, a move of particular importance in the anthropology of Oceania (e.g., [Barker 1990; Robbins 2004; Keane 2007; Tomlinson and McDougall 2013]), though Chris Hann and others have been redressing the overemphasis on Pentecostalism and the Pacific with anthropological studies of Eurasian, Eastern European, and post-Soviet Christianities [Hann and Goltz 2010]. Anthropological studies of Christianity have helped to renew interest in the anthropology of religion more generally, a topic of study from the inception of the discipline.

Theoretical trends change quickly within anthropology. Subjects once studied under the banner of 'transnationalism' are now being reconfigured in new ways, for example, as 'cosmopolitanism' (e.g., [Werbner 2008]). It is the *practice* of anthropology, rather than its theory, however, that has changed most dramatically over the last decade or two. Twenty years ago, it was considered as professionally risky for PhD students to undertake research in the US, Europe, or other Western, urban, modern contexts, as it would have been for their peers in history to 'worry about emotions' [Rosenwein 2002]. Today it is the reverse: If an earlier generation of scholars had to justify 'studying up' [Nader 1972], scholars now find it necessary to justify their interest in social forms like 'the village' [Stasch 2013]. While many anthropologists do continue to fieldwork among people who are culturally unfamiliar to them, some of the most prominent anthropologists are working on social worlds much closer to home.

2

By all accounts, we should be in the middle of a generational change in the makeup of the social and human sciences. The baby boomers (those born in the late 1940s and early 1950s) are on their way out, and several cohorts of younger scholars, with very different life-experiences, expectations, and sensibilities are poised to take their place. The effects of this generational shift, however, are somewhat muted, and for several reasons. First and foremost, there seems to be (in both the US and Australia) a deepening bifurcation of teaching and research, which is related to the 'adjunctisation' of the academy.

A new generation of scholars might be teaching students, but they are often not writing the materials taught. Moreover, baby boomers do not all retire at the same time and many of the most productive senior scholars do not retire at all. Even if they resign from their chairs, they remain immensely productive, and indeed can produce at a rate even their tenured younger peers cannot match. Engulfed in teaching and administration, often raising families in two-career households, and not infrequently stuck in complex long-distance arrangements if both spouses are academics (as they now often are), the younger scholars simply have less time, and also much less accumulated research, than their allegedly retired peers. Thirdly, neither the older nor the younger cohorts agree internally on politics, ideology, method, or aesthetics. The provocations in Edele's *Stalinist Society* [2011], for example, have attracted both praise and scorn from scholars of all generations, in the process confirming one of the arguments of the book's final chapter. And we can see no clear pattern even on choice of topic among generations, either.

There are overall trends, though, which are not connected to generational change. In both of our fields, we see a move away from the poor, the uneducated, and the exploited towards those more favoured in life — what has been called, for anthropology, a 'metropolitanisation' of the discipline (Kapferer in: [Anthropologists Are Talking 2007]). The world has changed in ways that make the framing devices of some canonical earlier ethnographies — bracketing a social world in time and space, minimizing discussion of connections beyond — untenable. It is certainly a positive thing that anthropology no longer occupies the 'savage slot' [Trouillot 1991] in the social sciences. Yet it seems to us that anthropology has been too quick to abandon, indeed, abdicate its tradition, too quick to focus most attention on relatively affluent, literate, metropolitan populations that have long provided the subject matter for most of the other social sciences and humanities. Anthropologists have traditionally sought to move outside of the Western intellectual traditions of which they are a part and to bring those traditions into dialogue with very different ways of seeing the world, namely those non-Western, often non-literate, people who have been our classical interlocutors. Half a century of internal critique, discussed by Susan Gal in this journal ten years ago, has undermined the discipline's capacity to generate such dialogue. Critics of anthropology suggested that the very project of studying others was epistemologically and ethically suspect, especially when those others are less powerful in a global political economy than scholars themselves. As fewer anthropologists do work among the global poor, the discipline may be avoiding some sort of exploitation, but we may also be narrowing the scope of human experience taken as a focus of scholarly study.

The trend to ‘metropolitanisation’ has been less marked in history, partially because history has long been metropolitan in the first place. This has to do with the discipline’s original entanglement with nationalisms and nation states, but also reflects constraints imposed by the sources: elites write and hence create the raw material most historians depend upon. Social history had tried to broaden the subject of study by employing quantitative methods in an attempt to get to experiences not reflected in the written record; the new cultural history of the 1980s and early 1990s [Hunt 1989] also drew energy from the study of the life worlds of non-elites [Ginzburg 1980; Behringer 1994]. Today, however, cultural historians are more likely to write about state discourse or the experiences of elites. Detractors are few and far in between, although they certainly do exist [Diamant 2009; Zubkova and Zhukova 2010]. Partially, this ‘elitisation’ is the result of the interest of cultural historians in thick symbolic artifacts more likely to be produced by well-educated people, as Bill Sewell [2001] pointed out a long time ago. Incidentally, the history of emotions is subject to the same logic pushing its practitioners, as it were, ‘upwards’.

This move away from the poor and downtrodden is also reinforced by the shift from the local to the global, the national to the transnational, emplaced anthropology to entangled history. The local, if it is studied at all, has become the ‘glocal’, and ‘cosmopolitanism’ is usually understood as meaning to study the metropole. The fact that more than half of the world’s population now reside in cities (<<http://www.who.int>>) does not make the lives of those continuing to live in rural areas any less important, but most historians and anthropologists seem most interested in obviously global, mobile, urban subjects. The rise of transnational history — while a blessing insofar as it shows one way to escape the confines of the nation state — is a rather two-edged sword in this regard: mostly, the transfers and entanglements studied are those between urban and urbane elites.

3

We have witnessed a number of changes over the past decade, but many of them reflect our own altered context as we moved from the national system in which we were trained (the US) to a new system (that of Australia). The contrasts between the two are striking, but nevertheless we see similar trends, in terms of the expansion of administration, the bifurcation of teaching and research, and the casualisation of university labour. Once first among equals and expected to return to teaching and research, senior university administrators are now a separate, and exponentially higher remunerated, caste. Academics have minimal input into the governance of Australian universities, though our strong union has helped to secure comfortable wages and generous benefits — something that we appreciate when considering the situations of our peers in the US. In Australia, research money is available for humanities and social

sciences (though a recently elected conservative government has resolved to eliminate ‘wasteful’ funding, by which it appears to mean most humanities research and indeed anything non-applied), but the funding has been unequally distributed across, and within, universities. Although most universities’ income is generated by teaching, the Australia-wide scramble for high standing in national and international rankings of research excellence has sucked funding away from education. Whether such changes have led to better research is questionable, but there is no doubt that they have created a very clear status system within universities. At the top of the pecking order, serial holders of large Australian Research Council grants do minimal teaching and may also avoid administrative duties. Below them are tenured academics who enjoy varying success in grant-getting and publication quantity. In our faculty, teaching loads are calibrated to reflect publishing and grant-getting success (measured by what is called — without irony — the ‘Socratic Index’); more publications mean less teaching. At the bottom of the scale are scholars who are employed on contracts and completely dependent on the benevolence of grant-winning professors. Everywhere, academic systems continue to produce more PhDs than could ever expect academic jobs. Australians seem to be ahead of their American peers in responding to this problem by telling our doctoral candidates that a career in university teaching and research is unlikely. But US universities are quickly catching up: during a recent visit to our alma mater, the University of Chicago, we learned that even this professor-factory now encourages its graduate students to at least contemplate careers outside of academe.

There were also more positive developments in the last decade. For one, serious historians and anthropologists seem eager to engage with a wider public. One place where this happened is *Wikipedia*. Once ridiculed for its frequent factual mistakes, in fields like Russian history entries are now sometimes better than those in commercially produced handbooks, because professional historians have engaged in putting things right. This underground movement among Russianists was led by one of the electronic visionaries of our field — Marshall Poe (cf. [Poe 2009]) — who more recently has embarked on a new venture also intended to bring academic knowledge to the non-academic public: ‘New Books in History’ (<<http://newbooksinhistory.com/>>) uses podcasting to disseminate historical research to those interested in history but lacking time or energy to actually read books [Poe 2012]. Indeed, the possibilities of the new media to overcome the gap between academics and other intelligent people are sometimes quite stunning. Take anthropologist David Graeber’s recent article in an online magazine (‘On the Phenomenon of Bullshit Jobs’, <<http://www.strikemag.org/bullshit-jobs/>>), which went viral and was picked up by the mainline press within weeks.

The example of Graeber's essay, which crossed the boundaries not only between academia and 'the rest of the world' but also between 'new' and 'old' media, shows that increased engagement with a wider, non-academic public need not be confined to the internet. Indeed, increasing numbers of serious, but accessibly written, academic books (e.g., [Graeber 2011; Luhrmann 2012] in anthropology or [Bourke 2011; Clark 2013] in history) are reaching audiences well beyond what most academics aspire to. That such books can now at times be found even in airport bookstores should be seen as a mark of success in breaking down the walls of the ivory tower rather than as distasteful populism, as purists scoff. Indeed, we can only hope that the trend continues.

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BRUCE GRANT

1

As an ethnographer of the Caucasus, perhaps one of the most significant observations I might make on the last ten years is the absence of growth. For all of the building of new universities and the restructuring of former Soviet academies of science, anthropological work in the region —

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indeed, fieldwork in general — remains modest. There are some crucial exceptions, perhaps most strikingly in the pathbreaking, polyglot work being done by Vladimir Bobrovnikov, Rebecca Gould, and Michael Kemper. But relative to most other world areas, we are seeing no flowering of anthropological work, especially in terms of monographs.

What are some of the reasons? In the North Caucasus, security of course plays a role for many. In the South, the entry level for graduate students — especially in terms of language learning and historical consciousness — remains intimidatingly high. Scholarship in the region is also challenged by the absence of a ready academic home. Like many former Soviet republics, countries of the South Caucasus have been eager to shed their ties to the communist world. Yet at the same time, despite their pivotal locations between former Russian, Ottoman, and Persian empires, the Caucasus world is not necessarily part of a broader Middle East in any institutional way.

Of the three South Caucasus republics, Azerbaijan, with a very tough setting for the kinds of free speech required in regular fieldwork — alongside the region's most restrictive visa regime — is perhaps the least studied. In my own experience, despite ten years of visiting the same family in the same rural community, security police began shadowing almost every person with whom I spoke, closing down what I had once conceived as a long-term project. Armenia, with its large diaspora and relatively more open universities, puzzles for its absence of new scholars entering the fold. Georgia, by contrast, is the one bright light: with its green landscapes, wine, khinkali, and visa-friendly setting, it remains the workhorse of the region.

2 Demographically, at North American meetings of ASEEEES (the Association for Slavic, East European, and Eurasian Studies), CESS (the Central Eurasian Studies Society), the ASN (Association for the Study of Nationalities), and others, we see the steady rise of colleagues working at institutions in former socialist space. This is a terrific move that wholly strengthens the terms of debate. Even more interestingly, universities in metropolises such as Moscow, Petersburg, or Astana are recruiting globally for new positions.

3 In the broadest terms, North American colleges and universities have taken a significant turn for the worse. Books such as *The Fall of the Faculty* by Benjamin Ginsberg, with his astute remarks on 'the rise of the all-administrative university', well capture a seemingly brave new world where the number of faculty hires steadily fall, while hiring of highly paid administrators, consultants, and public relations staff skyrockets. Profit-driven administrators seem to increasingly view faculty as obstacles to growth rather than the core staff of an educational mission. Anthropology, with its firm requirement for extended fieldwork away from the university (but indeed, all the

humanities, and humanistically-inclined social sciences) stands to suffer significantly in this new climate. Any program that privileges open exploration for research, as opposed to tighter timelines for specific data collection and analysis, challenges university cost-seeking measures. Students who 'extend' their programs for fieldwork abroad, I was told by a dean a few years back at my own institution, are 'a disproportionate draw on administrative resources', because they keep downloading articles and keep requiring annual reports (!).

JOACHIM OTTO HABECK

I vividly remember the publication of the first issue of *Antropologicheskii forum* and the discussion included there. It is an honour and pleasure to be invited to provide a personal comment on how anthropological research on Siberia has developed over the last ten years.

The Editorial Board has provided us with four questions, which I am going to tackle in reverse order.

4

As to the regional setting, methods and approaches of my own fieldwork, these have changed very significantly in the last ten years, which is due to both personal and institutional reasons. Earlier my focus was on reindeer-herding communities in the north of Komi Republic and adjacent Nenets Autonomous District [Habeck 2005; 2006]. My personal reasons for leaving that area were a mixture of annoyance with the regional secret service and the insight that my presence in Komi villages might cause local people discomfort or unnecessary problems. In addition, being employed as coordinator of the Siberian Studies Centre, it made sense to conduct fieldwork in Siberia proper. Novosibirsk and surroundings became my new research region in 2005; the music scene, cultural diversity and slightly later the House of Culture became my new objects of study [Habeck 2007]. My colleagues agreed to conduct jointly a comparative research project on the social significance of the House of Culture, which also meant trying out new methods

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and research instruments, which we jointly designed before we set off to our respective field sites [Donahoe et al. 2011]. Starting from 2008, I designed a second comparative research project, this time on lifestyles in Siberia, the results of which will be published soon. Both these projects have meant a shift from what can be understood as classical Siberian ethnographies of different ethnic groups (the value of which should not be under-estimated) towards sociology, in three different ways: methodologically; in terms of research content; and also in terms of the body of literature that my colleagues and I have drawn upon and the academic debates that we hope to contribute to. I think that anthropological research in Siberia can learn a great deal from sociological theory in order to extend its knowledge basis and its social relevance, notably in such fields as integration and exclusion, identity and inequality, individual aspirations and collective projects.

Methodologically, long-term periods in a definite place and participant observation remain core assets of ethnographic research. In the 1990s, this Malinowski-like, 'individualist' research design gradually gained popularity over the earlier *ekspeditsiya* type of fieldwork; more recently, however, one can observe yet another shift: as mobile telephony and Internet are now widely accessible, fieldwork does not end with the researcher's departure, it often turns into a continual process of communication.

3

What has changed very significantly, I believe, is authorship and audience. This leads me to the third question, that of the relation between academia and the rest of the world. 'Local' (Siberian) residents are no less able to reach out to a large audience than researchers are. In fact, many of my interlocutors in Novosibirsk and elsewhere in Siberia are sharper when it comes to smart phones and web-based communication than I am. Some of them have their own self-presentation on the Web, aptly participating in the global exchange of information. Physical mobility too has generally increased, also across international borders (see below). Is the anthropologist's job still special in any way, then? Probably the only domain that is still exclusive is that of academic writing. And here change has been comparatively slow. With regard to the conventions of academic writing, practices of peer-reviewed publishing and criteria of evaluating scholarship, I do not see major changes in the discipline of social anthropology over the last ten years, despite the establishment of online editions of many journals (for a new approach, see [Da Col, Graeber 2011: ix]). This renders academic publications (and entire formats of publishing) somewhat unattractive to society more broadly, which in turn is one of the reasons for social anthropology being out of pace with many societal changes. This criticism is, of course, not new; and conversely, one may claim that the author's distance from the object, created by academic conventions and the usually long duration of the publishing process,

is somehow advantageous because it promises to have a longer legacy. But traditional academic writing remains largely text-based and stolid. By contrast, an encouraging example of how anthropological research on Siberia is directly fed into the Web, thus being made available for immediate consultation and discussion, is the Arctic Anthropology blog, run by the Anthropology Research Team at the Arctic Centre in Rovaniemi, Finland.

Concerning the place of anthropology in the web of scientific disciplines, Siberia seems divided into two halves, each of them with its own disciplinary configuration. Anthropologists working in the Northern half, in tundra and forest tundra regions, are increasingly benefiting from, but also increasingly absorbed into, research agendas on global climate change (e.g., [Crate 2008]) and the pertinent discourse that heavily draws on eco-system notions, such as vulnerability and resilience. A very positive development is the growing (and, as of late, sincere) acknowledgement of the import of social sciences among our colleagues working in the natural sciences. On these grounds, cooperation with environmental and life sciences has increased in scope and productivity (e.g., [Forbes, Stammer 2009; Walker et al. 2006]). Anthropologists working in the southern half, in the big cities along the TransSib and the southern belt of republics (Altai, Tyva, Khakassia, Buryatia) also occasionally cooperate with natural sciences, and there are of course valid reasons for that, and here I see a somewhat stronger tendency to maintain cooperation with historians, political scientists, experts on religion, etc., which reflects the more classic ‘orientalist’ (*vostokovedenie*) connection. At any rate, the long-standing preoccupation with the contrast between the ‘traditional’ vs ‘modern’ life of Siberian indigenous peoples has given way to much more multi-faceted and vivid descriptions, in Russian academic journals (for a recent collection, see [Funk 2013]).

Siberia has often served for comparisons with adjacent regions, either in the circumpolar North or in adjacent parts of Asia, notably Central Asia, Mongolia, and China; connections and exchanges with neighbouring regions have been studied increasingly over the last few years (to quote just some authors writing about Chinese migration to Siberia: [Diatlov 2007a; 2007b; Haglanova 2011; Rabogoshvili 2012; Safonova, Sántha 2010]). Still small and yet increasing is the number of projects and publications that draw comparisons between Siberia and regions of the Global South (e.g., [Ulturgasheva et al. 2012]), even though there is a clear demand, owing to the shared experiences of a (post-)colonial situation.

2

This ties into my reply to the second question, about the things that unite or fragment the community of researchers and students. The community continues to be made up of individuals who live in Russia, European countries, North America and Japan. Thus far,

colleagues from India, China or Mexico are few and far between (notable exceptions are [Chatterjee et al. 2009; Vodichev 2007]). This reflects old colonial connections and voids in the asymmetric production of academic knowledge. Concerning former asymmetries among scholars from Russia vs scholars from the West, this gap has gradually diminished, for at least three reasons: first, there are now several universities and research institutes that have sufficient funds to finance fieldwork; second, the language barrier is now less of an impediment than it used to be; and third, as a consequence, communication has increased not only in on-line forums but also in the way we communicate about theory and research results. Particularly worthwhile, from my experience, is the exercise of inviting Western scholars to present their research through Russian, as has been the case at the conference 'Russian Field' in St Petersburg in 2009 (see [Bogdanova, Gabowitsch 2011; Kormina 2009]) and the 'IX Siberian Studies Conference' in that very city in 2013. Formats like these help create equality between research partners and challenge the expectation that fieldwork be done through Russian or a local language whereas academic debate should be conducted through English.

1

While I do think that scholars from Russia and Western countries are now more willing and able to understand each other and make themselves mutually understood, I am worried about the disenchantment of Russia with the West and the no less disenchanted attitude of Western media and the wider public towards Russia. To be sure, scholarly contacts and visits are very frequent, and they constitute part of a much larger exchange — namely, a constant and fuzzy flow of individuals between Russia and other countries, be they tourists, commuters, or permanent residents. There is a sustained curiosity about Siberia among people outside Russia, and a growing curiosity among Siberians about travel to North America, China and Europe (and not just the Mediterranean seaside resorts). Nonetheless, this curiosity has come to be tainted by old and new stereotypes.

On the Western part, it must be said, Russia for some reason continues to be perceived by most as somehow 'backward' and idiosyncratic (our research project on Lifestyle Plurality may help to question such stereotypes). On the Russian part, the disenchantment and sometimes open hostility seems to stem from a markedly conservative and inward-looking turn among wide circles of Russian society (exemplified by growing concerns about 'proper' ways of living, loving, intimacy and education). This is my reply to the first question posed by the Editorial Board, about the most important changes in the area and for the discipline, for it clearly has repercussions on the conditions of field research, not only in those vast areas subjected to 'borderland' regime but also in other parts of Russia. Suspicion and apprehensiveness have been growing among officials, pedagogues

and ‘ordinary folks’ to such an extent that there is the saddening prospect that one day, again, it may become necessary to abandon the field site, for reasons similar to those that made me leave my previous fieldwork location about ten years ago. Curiosity and friendship are stronger than disenchantment, though, and new enchantments may be just around the corner.

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Ethnography in Times of Uncertainty

Over the last decade, in the wake of the 'war on terror', migration across international borders has been increasingly accompanied by 'moral panics' in many industrialised countries and enhanced forms of security and surveillance for non-citizens seeking entry into their territorial space. While this security and exclusion dynamic has intensified, changes wrought in the world's economies have been profound. Recall, despite some colossal and notable failures such as Enron and Global Crossing and some slow growth in the early years of the millennium post-internet bubble, 2004 and 2005 were seen as robust years for the global economy. The world's economy was fuelled by the energy sector, resource extraction, the rise of the BRICS, easy consumer credit and, more generally, the rapid expansion of financial services globally [UN 2004]. Few predicted the transformative changes that would

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occur in 2008–2009 with the financial and banking crisis that upended many other segments of the world's economies and socio-political realities and has created a prolonged and painful global economic slump. Anthropologists attempting to make sense of the ideological and structural changes in the last decade are confronted with these seemingly epochal changes in capitalism, forms of exclusion, governance and technology that challenge our disciplines, understandings of social relations, subjectivity and our object of study. Coping with these changes is not simply a matter of casting about for conceptual models that somehow have some purchase on these rapidly shifting circumstances, but also one of method. In a disciplinary tradition founded on a methodological approach that emphasises local, empirical research in 'place', known broadly as ethnography, anthropologists have been challenged to consider how to think about the 'global inside the heart of the local', just as the intimacy of the local, a crucial site for anthropological knowledge production, has seemed to disappear [Herzfeld 2003: 2; Appadurai 1997:115], also see [Comaroff and Comaroff 2003].

It is in this context that I began new fieldwork in Naples, Italy among irregular migrants shortly after my last contribution. In my 2004 essay for this journal, I was concerned with tracing the ways in which transnational ties for post-war Italians complicated the concept of diaspora, effected identity formation and forms of national belonging. This more recent research based in Naples extends my concern that research on migration and migrants should address the larger structural changes discussed above more directly. Given that so much of migration research is funded by government and focussed on immediate policy outcomes, I suggest migration studies has not adequately seized opportunities to address the wider implications of mobile peoples for the major issues of the day. By isolating migrants in this policy context, scholars and the work they produce, even if unintentionally, reinforce the marginalisation and exclusion migrants face in their new homes by framing them as passive actors in the face of globalising changes in economic life, politics and culture.

Over the past two decades, anthropologists have struggled to find the methodological postures appropriate for an intensely globalising world, which seems to demand alternative ways of apprehending putatively universal forms, social relations stretched across space and time, the interests and structures behind them and indeed engagement with non-human technical forms such as the internet and long-distance, computer-mediated financial instruments. Even though there is an urgent need to apprehend the structural and systemic forms of knowledge production that seek to structure our ways of thinking, material possibilities and hopes for the future, I argue that we must still embrace anthropology's intensive on-the-ground learning in intimate sites of interaction.

To attempt to capture the ‘global inside the local’, I orient my research on migration in Naples around the dissemination and practice of contemporary corporate forms of thinking and governance circulating around the globe through the discourses and practices of NGOs, IOs, corporations and multilateral organizations. This research adapts Ferguson and Gupta’s [1997] intriguing ideas about transnational governmentality and follows from the way they reimagine metaphors such as ‘verticality’ and ‘encompassment’ relative to state presence. By governance, I mean the unfolding global political project by which social issues are increasingly depoliticised in favor of a more management-based, technical rendering of those same issues. Pierre [2000: 3–4] refers to governance as ‘sustaining co-ordination and coherence among a wide variety of actors with different purposes and objectives such as political actors and institutions, corporate interests, civil society, and transnational governments’. It is a particular policy-making style present in neoliberal regimes that places limits on state action and control because of market-ideology and informs new public management. Li [2007] following [Ferguson 1994] notes that governance requires both ‘problematizing’ and then rendering technical that problem for solution. It thereby seeks to ‘de-politicise’ governing, and hence also reduces democratic accountability. In general this dissemination of ‘universalising’ forms of management discourse and practice is part of the neoliberal project of economic and political globalisation, which assumes such characteristics as fiscal discipline, government spending cutbacks, labour deregulation, financial speculation, open markets and privatisation.

Anthropologists have quite rightly been attendant to the powerful and violent reconfiguration of state practices of control, surveillance, and management, to the racialised exclusions of neoliberal governance and to the hierarchical forms of citizenship these practices create [DeGenova 2010; Feldman 2012; Fassin 2011], following [Agamben 1995]. Nevertheless, there are neoliberal effects beyond the juridical and new securocracy, with its extended network of disparate securitising forms that enhance the administration of publics. While these juridical forms undoubtedly frame the life worlds of the migrants and Neapolitans, my fieldwork in Naples insists upon a more expansive reading of how universal forms of neoliberal governance are localised.

For over a century, Southern Italy was the source of one of the greatest displacements of population and creative mobility ever seen. The port city of Naples, in the region of Campania was not only the source of many of these migrants but also the transit point of hundreds of thousands of migrants from Italy’s southern regions in the late nineteenth century and the post-1945 period. The earlier emigration formed part of the expansion of an industrialising Atlantic economy,

with the growth of industrial capitalism and modernist narratives of nation-building in the Americas and northern Europe. More than a century after the great migrations began and a quarter-century since that emigration diminished, and new non-EU migrants made the peninsula a net-migration receiving country, between 2004 and 2013, I conducted twenty months of fieldwork in Naples and its frontiers. I wanted to document the new migration into Naples and its surroundings in an effort to understand how the experiences and knowledge work of these new migrants could provide us with some critical purchase on the transformations in the global economy as seen through the spatial-temporal perspectives offered through the city's mobilities. Ethnographic fieldwork in Naples forces a researcher to ask what it means to study informal economies when there seems to be no formal economy and what it is to be misgoverned in a place in which 'governance', as characterised by scholars of neoliberalism, appears as more incomplete than entrenched, more an imaginative possibility than an expected form of governing. In this context, fieldwork experience forced me to analyse conceptual themes about knowledge work and economies, the practice of the audit, corporate social responsibility and entrepreneurship in light of the racialised and gendered experiences of migrants. Naples is a city that raises questions about the discourses of modernity, tradition, stereotypes and reifications. The people I worked with use these prevailing representations of Naples to make sense of where they fit in an imagined globalising frame. Against this form of governance, I trace the creative forms of knowledge practices — intimate, affective, experiential — that migrants use to survive in their precarious state.

Walter Benjamin [1925] described Naples as a 'porous city', one that absorbs changes, movements and spatial-temporal layerings, a site of potentiality and improvisation. Its porosity was both material and semiotic. Despite the numerical inversion in mobility, the region of Campania, with the worst GDP per capita in Italy, continues to send people out in search of work. The imaginations and existential hopes and fears of these two mobilities meet through this ethnography. The official statistics must be tempered by an awareness of Naples' significant, vibrant, and exploitative underground economy — one which serves as a magnet for Neapolitans and migrants searching for informal work to earn the necessary cash to move up the peninsula and beyond into the larger continent. The improvisational potentiality of underground economies situates my ethnographic entry into studies of knowledge work and creative economies, as it follows (ir)regular migrants as they navigate the uncertain terrain of their new city to survive and retain hope for the future.

If precarity has served as shorthand for a social movement of disaffected European citizens faced with the limited economic prospects of a Europe under austerity and the retrenchment of welfare-state

guarantees, its potential as a way of conceptualising the peculiar circumstances of migrants on the ‘periphery’ of Europe — a conceptual geography of inequality but nevertheless a powerful framing that shapes the lives of people in Naples themselves — has yet to be adequately explored. Migrants in Naples offer a view therefore, of precarity inside precarity. Precarity for European citizens signals the way of being in the world of flexible, temporary contracts, diminished welfare-state supports and graduated forms of exclusions of belonging, but for migrants this insecurity is exponentially experienced. Undergirding the inequities and uncertainties are the very universal notions that were purported to offer surety.

Knowledge work, audit, and CSR — the imagination of financial futures¹

The Lisbon Agenda announced in 2000 by the European Union (EU) established an ambitious goal that by 2010 the EU should be ‘the most competitive and dynamic knowledge-based economy in the world’, but the focus was not on the thousands of migrants clandestinely and irregularly entering the European space. Just a few years later, the demographic weight of this non-EU irregular migrant presence in Italy alone was starkly revealed. As part of the 2002 ‘Bossi-Fini’ immigration legislation (Law No.189), an amnesty was offered for irregular (undocumented) migrants already present in Italy (and with proof of lawful arrival). The results were that the Italian government gave amnesty to just under 700 000 migrants!

The existential experience of migrants in Naples, and their modes of survival, critique the would-be exclusionary universal notion of knowledge work that restricts migration visas to the highly-educated and serves then as a vehicle for visa and citizenship acquisition and shibboleth of ‘freedom of movement’ across borders that are, in practice, increasingly securitised. How do they cope in this volatile situation? Knowledge as a commodified form has received considerable attention, but rumour, a crucial pedagogical device for migrants, has been less considered [2006]. Rumour has been interpreted by some scholars as a way to promote community cohesion and by others as an attempt to promote the self-interest of those circulating it. Rumour is seen as a communicative device that offers information or news for evaluation and is a central means of distributing information in all economies [Allport, Postman 1947; Stewart, Strathern 2004]. Here, migrants use the information circulating in rumours to interpret their migratory chances in Italy in

¹ CSR refers to Corporate Social Responsibility and is a overall term for a wide range of practices. In general, it refers to a business strategy and the self-regulating effort by corporations to ensure their business practices conform to community and international ethical standards, environmental sustainability and quality of life issues for workers (see [Blowfield and Frynas 2005]).

general, and more specifically, entrepreneurial conditions available to those engaged in informal economic activities in the Neapolitan economy. These rumours travel beyond a circumscribed racialised, group to have significance in wider social fields. In this case, as subjective representations of economic behaviour, these rumours offer models for entrepreneurial activities to be admired, mimicked, condemned or avoided. My ethnographic material suggests greater attention should be paid to how subjective understandings spur social action, as reflected here through the use of rumours as a kind of knowledge to be assessed and interpreted to form the basis of decisions about economic behaviour among migrants in Naples.

I seek, in my work, to re-evaluate the knowledge used by migrants in their efforts to survive. There is a form of specialist knowledge that has developed among migrants. Migrant intercultural mediators serve as knowledge workers and brokers within the continuing informal economies of southern Italy. Their presence argues for a reconsideration of whom we might think of as knowledge workers and for a reassessment of how knowledge and its distribution are inseparable from power. I explore the connections between knowledge, the entrepreneurial activity of precarious migrants, their inventions of self-identity, and the limitations they face from institutions and Italian citizens in the city. These migrants must negotiate competing representations of themselves as emblems of particular cultures, supposed evidence of social problems and instability, understood as illustrative of an undifferentiated migrant condition, alleged sources of innovation and economic renewal, and assumed evidence of the continuing informal economic conditions of the south of Italy.

The formalised 'knowledge-economy' discourse refracted through a managerial framework disembeds knowledge from power, people and experience by its focus on skills and measurements. Ironically, this very premise undermines precisely the value of knowledge as situational, experiential and affective learning. As Acciaioli [1981: 23] noted 'knowledge, however interesting, can never be disinterested'. Migrants I interviewed who work as intercultural mediators in the precarious employ of Italian state agencies, or non-for-profit public companies, were fully aware that their useful knowledge operated within power relationships. In their positions as brokers providing 'expert' knowledge for both the Italian state and migrants they were hardly ever characterised as knowledge-workers; yet, the abstract thinking and problem solving at the core of how business consultants and management guides describe knowledge work were central to what they did. Drawing on their experiential knowledge gained through the everyday, and racialised, interactions they had with the Italian state, they balanced the complex political space of an intermediary between unequal groups (see [Gluckman 1949]). Their

work required that they made available their embodied experiential, social, or tacit knowledge to gain legitimacy from the migrants they counselled, and the Italian agencies that sought their embodied knowledge to solve problems.

Moving ethnographically from the experiential knowledge work of these migrants, my interest began to shift towards Neapolitan institutions. How did universalising forms of management thinking, emerging out of a fitful political process in post-1989 reforms in the Italian state, and the layering of demands of European integration and standards, manifest through real institutions struggling with organising and operating in the everyday realities of southern Italy? A key part of my fieldwork was time spent as an ethnographer and volunteer alongside people at a Neapolitan transit company to observe how they elaborated these contemporary knowledge practices and forms of management. Reformed as part of the governance transformations in Naples in the 1990s, this transit company adopted 'best practices' in its operations and management to seek industry ISO standards as it moved from an institution dominated by traditional patronage networks to a model of a public/private enterprise.¹ In my work, I explored the practices of indigenised neoliberal governance, and the disjunctures between these universalising management knowledge practices and their situated entanglements with more localised challenges to governance and governing.

For instance, I examined the socialisation of Neapolitans and migrants into the knowledge intensive work activity of intercultural mediation. Empirically speaking, I participated in the intensive two-week session for intercultural mediators joining a project by a local transit company to resolve tensions on their service. This training was part of the 'best practice' disseminated in neoliberal corporate governance across public and private sectors to encourage entrepreneurial workers, self-actualising and self-managing so that they use their emotional and affective resources to improve their work environment. These training days organised by the transit company for its workers were encumbered by divergent agendas and expectations about the project. The sessions were seen by management and the subcontracted trainer from Caritas, the Catholic social service agency, as crucial for the socialization of the individuals involved, and the 'mixed teams' that would be let loose on the transit line to interact directly with passengers. On the other hand, the participant-workers from the company had greater ambivalence about solving these problems, which they expressed with resignation

¹ International Standards Organization [ISO] is a network of national organisations that establishes international standards through consensus-based decision-making for products and services that companies can voluntarily use as measures and indicators of their own goods, services and practices (see <<http://www.iso.org/iso/home/about.htm>>).

(in Benjaminian terms) about the limits of the potential for change in Neapolitan behaviour. At a practical level, the migrant mediators required the job to renew their visas (evidence of work), and at an intellectual level, there was a concern to educate fellow migrants about the difficult realities in Italy for migrants.

The ways in which this managerialist social accounting of African migrants in Naples, Italy provided insight into the social and economic dimensions of irregular African migrants working in the city's underground economy were sometimes unexpected. In practice, the social audit, which was originally presented as an effort to monitor and to control petty crime, mostly in the form of fare evasions, revealed something more to the mediators. The audit produced an excess of knowledge about the lives and trajectories of African migrants that extended beyond its procedural purpose. African and Ukrainian migrants, as it turned out, usually had tickets, but did not validate them unless forced to, thereby saving a lot of money. Ironically, transit company staff found it was mostly the Neapolitan riders who were ticketless. The effort by the company to encourage these migrants to develop the habit of validating tickets or buying monthly passes, which provided a significant discount, ran into still greater complications. It turned out that many of the migrants were not staying very long in the Naples region. Once 'integrated' from the transit company's perspective, migrants would disappear and new faces appear on the bus routes. Migrants had moved on in their migratory journey north, into the richer regions of Italy or Europe. New ones would arrive needing to be 'instructed' all over again. In doing the audit, mediators also gained significant knowledge about the informal economies in the Neapolitan region and of remittances sent to Africa.

In the process of developing a social audit and accounting for these migrants, the local company I studied came to the conclusion that the audit simply did not sufficiently address the 'business conditions', and management decided to develop another framework that has become common in contemporary management — CSR. Management adapted this 'universal' neoliberal form of technical governance in the Naples region to aid with the 'integration' of migrants in the area. The expectations for a kind of 'universal' standard for both the Neapolitan managers and the migrants must be read through the local conditions that confront Naples. The city is constantly referred to as 'in crisis' by both external and internal commentators, whether as a result of organised crime, social and cultural intransigence, political corruption or economic deprivation. Continuing historical inequalities between the north and south of Italy and related dominant cultural representations about modernity and pre-modernity, crime and corruption, frame the way regular and irregular migrants in Naples encounter 'Europe'. These conditions also

inform how Neapolitans in the company think of the project. As a result, rather than interpreting this socially responsible project by the transit company as simply another version of CSR, a form of privatising formerly state responsibilities but also potentially developing for corporations new sites of value, I found it useful to return to earlier sociological ideas. Naples faced considerable governing issues, and while neoliberal governance language certainly circulated and informed some of the ideas in management, what emerged from informants was a call for a much more mundane, but no less important ideal. They searched for what sounded like a Weberian idea of an ethical state (not an iron-cage), with rules and certainty, mixed with Simmel's notion of sociability, a desire for a kind of associative equality.

A porous city? In this view, Naples soaks in all that enters its territory, like the volcanic rock that sustains the city's foundations, Benjamin transposes the physical to the cultural and suggests that a city with centuries of invasions, imperial domination and migrations still finds a way to overwhelm the new and immerse it into the rhythms of the place. This ethnographic project, if you will, seeks to study two of these invasions — one of people (migrants) and another of ideas (neoliberal governance). The forms of thinking in neoliberalism pretend to represent the eternal verities of 'best practice'. Like the (neo)modern desire for perfecting knowledge management and skills, once delimited, disembedded, formalised, we should be expected to conquer the opportunities available in the knowledge economy. In Naples, these certainties get challenged both by the informal economies and social relations that structure the city's everyday life and by the uncertainties of human mobility. While as an ethnographer, I grasped for the intimacy of a local site (such as the tight quarters of a bus, a street corner and a manager's office) to conjure up the romantic empiricism of participant observation, I was also challenged to think of abstract manifestations of otherness such as management practices, ISO standards, and other places on the migrant's journey's. Indeed, Naples absorbed me. As part of my participant observation on for the transit company, I was issued a company's identity card with a photo, which I was to wear on the bus routes when working with the mediators. Despite my writing my full name for them, on the finished card my family's century-old migration to America, along with my Irish (American) surname, were lost, the inter-mixing of Italians, Irish, French Canadians, Swedish and Jewish ancestors was erased, and I was simply 'Nicolas Demaria', perhaps like my ancestor who migrated to America from Benevento outside Naples in the 1890s. Yet, I am not so sure the issues of integration are always so straightforward. As I was leaving Naples to return to my University I encountered a Bangladeshi man I had spent some time with street trading. He was arranging to bring

his wife over. We chatted about international travel, kids, and marriage, then he said, with a wry smile, ‘when I saw Naples I realised I could make it in Europe,’ giggled, and left.

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I began thinking about the decade that has passed since we published the first issue of *Antropologicheskii forum* (in what now seems another world) by looking back at my original comments from 2004. While I referred early on to 'the move away from governing interpretative paradigms or "grand narratives"', I later qualified this by talking about the emergence of 'a post-industrial explicatory schema that inverted the earlier concern with labour and production in favour of a pre-occupation with what had formerly been seen as ancillary phenomena — consumption and leisure'. I traced the presence, in a good many publications on cultural theory and practice back in the 1990s and early 2000s, of 'a celebration of consumption and performance' without much reference to socio-economic conditions.

In the intervening years, I would argue, study of 'consumption and performance', at any rate in the post-Socialist world, has become considerably less celebratory and more reflective. The trailblazers have lost some of their enthusiasm and become more thoughtful. Some excellent work — (for example, [Patico 2008; Shevchenko 2009; Vonderau 2010]) — has emphasised that consumption can be a site of resistance to dominant social trends, and that its multiple meanings are inflected by perceptions of crisis as well as beliefs of enhanced prosperity. On the other hand, the sense of the post-Soviet 1990s as a kind of ludic free space is not one

that I would entirely want to lose: the late 2000s model of the period as beset by instability and everyday Angst has its own ideological drive, one that pays insufficient attention to the extent of worry over rising prices in the second and third decades of the post-Soviet Russian state's existence.

But a more remarkable feature of the 2000s seems to me to have been the unmistakable resurfacing of a 'grand narrative' that I hardly referred to back in 2004 — the grand narrative of national triumphalism. Certainly, even at the time, some of the contributors to our debate, for example Konstantin Bogdanov, did mention this narrative, but more in terms of a tenacious survival from the past than as an assertive presence to be reckoned with at the time of writing. Back then, A. A. Gorelov's spouting about the prophylactic effects of folklore in the face of institutional collapse seemed an illustration merely of inertia, of how 'the kind of texts printed as "folklore" in textbooks and anthologies of the Soviet era are still taken by many to be the bulwark of national spirituality and a panacea against social ills' [Bogdanov 2004: 32–33]. I recall that when I first put together a grant proposal on national identity in Russia and submitted it to a major UK grant awarding body in 2005, the reviews were sceptical, indeed scathing: how could the expenditure of so much money on the investigation of this marginal topic possibly be justified? A year later, the assessors for another grant awarding body, the Arts and Humanities Research Council, expressed a completely different position and warmly approved the project. I think the shift was a reflection not just of the unpredictable preferences that grant-awarding committees may have, but of rising awareness that the ideological line promoted by the Russian central government had begun to shift.

While the project itself was underway, it started to become increasingly topical — and not always in ways that the participants themselves found congenial. Right at the moment when we began work, an instruction went out to the Institute of History of the Academy of Sciences that listed *natsionalnaya identichnost* [national identity] among recommended topics for investigation.¹ For academic managers to encourage analysis of a subject of emerging importance in the political arena is of course fair enough. Considerably more worrying is the encouragement of analysis of a quite specific kind — the rise of an expectation that commentary on Russia's past and present should serve 'patriotic' ends.²

¹ I was passed this memorandum in the autumn of 2007.

² These comments were written in January 2014, and I have not attempted to update them. However, I do not think they are less relevant as of August 2014 — indeed, unfortunately, I would say that the reverse is true. Michael Gove was no longer responsible for education policy in England, as of 15 July 2014, but the 'general line' in Russia proved, as I anticipated, longer-lasting — including the

This development is not limited to Russia. A British government minister, Michael Gove, recently used the approaching anniversary of the start of the First World War in order to present a highly tendentious view of the way in which history should be understood. The 1914–1918 conflict, he asserted, had been a ‘just war’. ‘The ruthless social Darwinism of the German elites, the pitiless approach they took to occupation, their aggressively expansionist war aims and their scorn for the international order all made resistance more than justified’. Yet this insight, he claimed, had to be rescued from a fog of misrepresentation: ‘Our understanding of the war has been overlaid by misunderstandings, and misrepresentations which reflect an, at best, ambiguous attitude to this country and, at worst, an unhappy compulsion on the part of some to denigrate virtues such as patriotism, honour and courage’. Behind this set of ‘misunderstandings’ and ‘misrepresentations’, Gove argued, lay not just popular TV shows such as the sitcom *Blackadder*, but the disinformation spread by certain ‘left-wing historians’ (Professor Richard Evans of the University of Cambridge figured as standard-bearer for these). While Gove conceded, ‘There is, of course, no unchallenged consensus,’ the entire tenor of the article was to suggest that those who questioned the First World War’s status as a patriotic history of self-sacrifice and triumph were not just deluded, but cynical and unpatriotic as well.¹ It is hard to remember a case from the recent past when a government minister has mounted so crude an ideological campaign, or such a specific attack (including named names) on professional historians.

As Gove’s comments make clear, his strictures on the First World War were offered immediately after the school curriculum in England had undergone a wide-ranging reform [National Curriculum 2013]. As he boastfully put it: ‘The changes we’ve made to the history curriculum have been welcomed by top academics as a way to give all children a proper rounded understanding of our country’s past and its place in the world’. The ‘changes’ actually served to resurrect a version of what used to be called ‘Our Island Story’ (most of the recommended, if not mandatory, examples related to national figures, for instance ‘British resistance’ to the Roman invasion under ‘Boudica’).² This reassertion of national values has also come at

accordance of a new significance to Crimea in Russian history books (see Elena Mukhametshina, ‘Istoriya prisoednila Krym’, *Gazeta.ru*, 2 June 2014 <<http://www.gazeta.ru/social/2014/06/02/6056877.shtml>>).

¹ For the complete article, with an approving introduction by Tim Shipman that commends its explosion of ‘left-wing myths’, see [Gove 2014]. It provoked an extensive debate in the British press and online. For an excerpt from *Blackadder* that shows the stereotypical asinine British Army officer in characteristic form, see <<http://www.youtube.com/watch?v=IDQ1jlnSjU>>.

² When I was eight or nine or so, I was donated, by the elderly mother of a family friend (I think she was born in the 1890s) a history of Britain that had been hers as a child, and published, if memory serves, in the 1890s. Boudica was then known as ‘Boadicea’, but her stirring place in the history of resistance

a time when academics are under pressure to demonstrate that their work has relevance to policymakers, a concatenation of events that might provoke some worrying reflections on the state of academic autonomy.¹ In an article championing the school reforms, the historian Niall Ferguson, who has been acting as advisor on the school curriculum, rushed into the fray. Lambasting opponents of curriculum reform as ‘the big guns of Oxbridge’ (alongside Richard Evans, the Soviet historian David Priestland, from Oxford, figured on Ferguson’s ‘board of shame’), he ridiculed their objections as out of touch with reality:

‘Quite why the professors feel obliged to defend a status quo that so many teachers, parents and pupils agree is indefensible I cannot work out. Is it sheer ignorance? Or partisan prejudice?’

Surely they can’t sincerely think it’s acceptable for children to leave school (as mine have all done) knowing nothing whatever about the Norman conquest, the English civil war or the Glorious Revolution, but plenty (well, a bit) about the Third Reich, the New Deal and the civil rights movement?’ [Ferguson 2013].

Ferguson emphasised his own authority in discussing the subject: ‘I know because I have watched three of my children go through the English system, because I have regularly visited schools and talked to history teachers, and because (unlike Evans and Priestland, authors of rather dry works on, respectively, Nazi Germany and Soviet Russia) I have written and presented popular history’ [ibid]. The ‘dry’ efforts of academic historians cannot measure up to the expertise of those who have ‘written and presented popular history’, have fathered children, and made school visits, though in what way these amount to specialised expertise in child psychology and effective pedagogy might provoke serious head-scratching.

to invasion was just the same, illustrated by an oddly racy steel engraving that showed her and her daughters, stripped to the waist, being flogged by the Romans. Thus does moralising narrative get drawn into inconsistencies (perhaps fortunately, as I shall argue below).

¹ In the past, comments by UK ministers of education that invoked supposedly neutral terms such as ‘efficiency’ often had a political undercurrent, since the core assumption was that publishing levels etc. were an indication of intellectual respectability. Margaret Thatcher was known for disparaging comments about the ‘luxury’ of research activities that had no practical application (e.g. medieval history). But ministers generally steered clear of making suggestions about appropriate research agendas, let alone interpretations. Recent years have seen increasingly urgent insistence on topics of importance (at the broadest, targeted funding for STEM [Science, Technology, Engineering, Mathematics] subjects). In 2011, a major row broke out when Chief Executive of the Arts and Humanities Research Council, the major body for state funding in the Humanities, announced that there would be special funding for work on ‘big society’ — a buzzword of David Cameron and the Conservative Party [Malik 2011]. For the first time, the national research productivity audit of 2014 (newly renamed the Research Excellence Framework) included assessment on ‘impact’, or the practical and social effects of research ‘output’, including work with policy-makers etc. (in the words of the Higher Education Funding Council, ‘wider social and economic benefits’) (<<https://www.hefce.ac.uk/news/newsarchive/2011/news62310.html>>).

It hardly needs to be said that the effort to tame the past and render it 'usable' in terms of immediate political objectives, such as the inculcation of patriotism in children, has in recent years been a striking feature of the contemporary Russian government also (for a discussion placing this in international context, see [MacMillan 2009]; for an outline setting out its relationship to Soviet historical thinking, see [Koposov 2011]).¹ Many of the central tenets of the philosophy are set out in a recent article by Nikolai Dorofeev, published in the unimpeachably orthodox *Rossiiskaya Federatsiya segodnya* [The Russian Federation Today]. Dorofeev begins by suggesting that Soviet definitions of historical practice, duly adjusted, are still relevant now:

'So what is history and why are attempts to falsify it so dangerous? There are many different definitions of history. Let us cite one from the Great Soviet Encyclopaedia, omitting only the quotations from the works of the classics of Marxism-Leninism. "History (from the Greek *historia* [sic.] is a tale about past events, a narrative about what has come to be known and studied. 1) Every process of development in nature and society... 2) A science that studies the past of human society in all its concrete and varied character, which is investigated in order to understand the present of that society and its prospects in the future." And now here is another definition, also from the Soviet period, but of a more journalistic kind: "History is a science that makes man into a citizen." I am convinced that both the first and the second definitions are so well tested and correct in their essence that they can still perfectly well be used today' [Dorofeev 2013].

The idea that one can simply recycle a definition 'omitting only the quotations from the works of the classics of Marxism-Leninism' recalls Vladimir Putin's suggestion, in January 2014, that textbooks for schools should be rewritten to remove 'ideological junk' [*ideologicheskii musor*].² Interpretation and facts are seen as easily extricable. And indeed, Dorofeev soon reinforces that idea. The central issue in history is 'what historical facts one should adopt and

¹ A particularly controversial publication was Aleksandr Filippov's *Noveishaya istoriya Rossii 1945–2006*, which emphasised the importance of political repression to the ends of state centralization and the creation of a new ruling class: 'Modern researchers are inclined to see rational causes for the use of force in terms of the drive to ensure the maximum efficiency of the governing stratum in the capacity of the mobilising force of society in order to achieve ends that could not otherwise have been achieved. Stalin followed the logic of Peter I: demand that the representatives of the executive do what is impossible in order to exact from them the maximum possible' [Filippov 2007]. The book ignores recent research suggesting the diversity of political repression, including the suppression of supposedly dangerous national minorities and other 'social marginals', and the extermination of suspected political opponents. The word 'rational' is also, to put it mildly, contentious.

² 'What I'm talking about now is not the conscious denigration of the role of the Soviet people in the fight with fascism, there are deeper things at stake there, even. It's just sort of ideological junk. That's what we need to get rid of' [Putin potreboval 2014].

with reference to what instance, and how to interpret and set them out'. As he continues:

'In this respect history is defenceless material, in contradistinction to the exact sciences — physics, chemistry, and maths, where interpretation of formulae is not allowed, and where mistakes in calculation lead simply to a false result, which may easily be corrected. With history everything is much more complicated, if one bears in mind the fact that its purpose is to turn man into citizen' [Dorofeev 2013].

For his part, Gove did not use the word 'fact': instead, he kept harping on phrases to do with interpretation. 'a proper historical debate'; 'laid out the ethical case'; 'proper study'; 'seen in a new light'. But the underlying idea was the same. Historical events are like the characters in a realist novel, simply waiting until the author of genius picks up his [certainly not 'her'!] pen and portrays them: 'Even the battle of the Somme, once considered the epitome of military futility, has now been analysed in depth by the military historian William Philpott and recast as a precursor of allied victory'. [Gove 2014]. In normal English, a battle cannot be a 'precursor': this is reserved for people. For Dorofeev also, historical truth is anthropomorphised: in its 'defencelessness it recalls a shrinking virgin, forever under threat of violation by any passing historian with malevolent intentions. It resembles, one could say, a kind of junior version of Mother Russia herself.

Facts, truth, and patriotism lie in a straight line. 'History and concepts such as citizenship and patriotism are inextricably connected, this can hardly arouse doubt' [Dorofeev 2013].¹ That 'citizenship' and 'patriotism' might ever come into some kind of conflict with each other is excluded from view, as is the question of what to do with 'facts' that may be inconvenient to both or either (one recalls the Soviet-era term 'objectionable facts', *vozmutilnye fakty*). Just as unproblematic, for Michael Gove, is the issue of perceiving the past 'properly', which likewise is held to depend upon a duly patriotic interpretation.

In England, the schoolchild historians who emerge from the revised school syllabus are supposed to be inculcated in something more like professional academic practice as it is commonly understood: they are meant to 'understand historical concepts such as continuity and change, cause and consequence, similarity, difference and significance' and 'understand how evidence is used rigorously to make historical claims' [National Curriculum 2013]. One suspects, however, that a sense of how to work 'rigorously' might not necessarily

¹ It would seem that Dorofeev means, 'this can hardly arouse doubt', or something similar, but his sloppy phrasing is indicative.

help them in a world where the ‘evidence’ can be slippery and contradictory, making problematic whatever ‘claims’ one might choose to base on it. The question of how such evidence-sifting might combine with the moral drive of the history syllabus (which is supposed to introduce children to, for instance, ‘the achievements and follies of mankind’ [National Curriculum 2013]) is not addressed.

In sum, there is little to choose between the two visions of historical practice and historical teaching, and also (one suspects) between the *Realpolitik* that inspired them. In both cases, a political interest group that is under pressure from considerably more aggressive versions of nationalism is aiming to offer its own version of ‘the national story’ as the ‘reasonable’ and ‘mainstream’ alternative.¹ Certainly, the triumphalist historical narrative current in the Russian Federation has more chance of a lasting impact, given the much better-established traditions lying behind national curricula (compulsory school programmes), as well as the fact that government ministers are traditionally more dispensable than presidents.² Yet in some respects, the discussions in the Russian Federation are slightly less narrow-minded than those currently being carried out in England. V. V. Putin has talked of the need to confer with ‘European’ colleagues about ‘memory work’ going on there (something that has definitely not happened in England).³ And the debates in Russia do display some sensitivity to the problems of inculcating ‘national’ values in a society where there is more than one *natsionalnost*. As Dorofeev recognises, some serious headaches await the would-be patriotic historian:

‘They will have to explain many events of Russian history in the way that was recently demanded at a meeting of the Presidential Council on International Relations by President Vladimir Putin: without internal contradictions and double interpretations. The authors of these books will have to rack their brains over how to explain an issue that is a hot topic at the moment: was there such a thing as the

¹ For Russia, see the interesting discussion in [Sokolov 2009] of the decline in the political capital of extreme nationalism as groups splintered after 2000, accompanied by the assertion of nationalism as a key constituent of the policies promoted by the ruling party, United Russia. In the UK, the strongest pressure is from anti-European groups such as the UK Independence Party. In both countries, though, strong personal convictions on the part of key members of the ruling elite (Putin on the one hand, Gove on the other) appear also to play a key role.

² This was indeed demonstrated by Gove’s move to another position in July 2014 (see above). That said, Michael Gove is regularly tipped as a possible future Prime Minister, at which point his ideological leverage would become considerably greater — though still with more checks and balances than that of the President of the RF. As for compulsory curricula, in Russia they have been a persistent feature (apart from the period 1917–1927), while the so-called ‘national curriculum’ in the UK was introduced in 1988.

³ See [Putin predlozhit 2014]. At the same time, on the ground, UK professional societies such as the Historical Association are signed up to the Euroclio network, which attempts to foster, for instance, collaborative initiatives such as joint German-French history textbooks, and a generally non-partisan approach to the teaching of history: see <www.euroclio.eu>.

notorious “Tatar-Mongol yoke”? Or again, how should we evaluate the actions of Ivan the Terrible when he destroyed the Kazan and Astrakhan khanates and tried to chop through a “window on to Europe” in the Baltic long before Peter I? How will they elucidate the actions and methods of that first Russian Emperor? How will these authors set out the history of the Caucasian War, which officially lasted from 1817 till 1864, but in reality for much longer? They will have to find an explanation for why General Ermolov is a hero in the heart of Russia, but in the Caucasus his name is used as a bogey to frighten children. And compromises will need to be found about important dates also. Why, to take a particularly striking example, should 23 February be a day of celebration for Russia generally,¹ but for Chechnya and Ingushetia the national Day of Sorrow, in memory of the deportation of these two peoples?’ [Dorofeev 2013].

This dilemma — how to explain a situation where there are two clear points of view, yet avoid ‘double interpretations’ — is a primal wound in the official Russian ideological narrative that might give us hope. Niall Ferguson, on the other hand, chooses to ignore the fact that there are parts of Britain, notably Northern Ireland, where the ‘Glorious Revolution’ is every bit as controversial as the 1817–1864 Caucasian War.² The curriculum itself, with its list of non-compulsory but recommended figures almost uniformly from English history (except for a couple of token ‘multi-cultural’ presences, such as the Caribbean nurse Mary Seacole) evades this issue as well.

So far as the pupils subjected to these coherent, would-be patriotic, programmes are concerned, one’s anxieties can perhaps be allayed by the notorious tendency of pupils to develop precisely the opposite views from those that are thrust down their throats at school. The programmes are formulated without any sense at all of the actual composition of classrooms, or the dynamics within them, which can be sensed if one reads reports such as the excellent ‘Teaching Emotive and Controversial History’ published by the Historical Association [Teaching 2007].³ The amount of impact they will have is therefore likely to be limited. (It is interesting, too, that no-one so far has suggested reforming the literature syllabus in English schools,

¹ 23 February was celebrated as Soviet Army Day before 1991, and is now Defender of the Motherland Day.

² The ‘Glorious Revolution’, initiated by the Battle of the Boyne, is regularly honoured by Protestant marches in Northern Ireland, leading, equally regularly, to bouts of violence when the marchers press home their historic ‘right’ to march through Catholic areas.

³ This report raises, for example, the issue of how to teach classes of children about the Holocaust when the composition of the group is 75% Islamic and the local mosque is circulating propaganda describing the Holocaust as ‘a lie’. This was picked up by the right-wing and xenophobic newspaper *The Daily Mail* and turned into a story about the censorship of Holocaust teaching by individual teachers [Teachers Ban 2007]. This then ‘went viral’ in online networks — in fact, I first got to hear about the story myself when Russian friends wrote to ask whether it could possibly be true. On teaching in schools, see also [Phillips 2014].

perhaps because finding patriotic authors of sufficient stature might be a problem on its own.)¹

More problematic are the challenges faced by those employed in the university system, given the increasing replacement of direct funding to universities by targeted funding awarded on a competitive basis to those whose research is defined as ‘excellent’. While it is constantly underlined that the measure of this is and will remain peer review, the application of categories such as ‘strategic importance’ becomes threatening once governments have started to spell their strategies out in an ideological way. An assault on academic autonomy for reasons of political expediency may become a serious possibility (to justify state funding, grant awarding authorities start to underline in their reports that they are supporting a representative percentage of research in ‘key areas’ such as national victories, the biographies of ‘heroes’ and so on).

This is not to say that intellectual quiescence is certain to result. The late nineteenth century in Russia, a period when there was intense political pressure on universities, was also a period when the teaching staff developed a strong sense of professional independence, in spite of or indeed because of the interference [Byford 2007]. As everyone knows, an external threat can be a great stimulus to corporate identity. But the late nineteenth and early twentieth centuries were also a time when positivism, and the emphasis on fact, went into the ascendancy. The defence of academic value is unlikely to get the public ear by celebrating the joys of uncertainty. Official intolerance of diverse explanations will certainly not lead to overall consensus, given the complicated societies we live in. But it may well change the academy in ways that will make the carefree debates of 2004 lose their topicality — not because we now take for granted the capacity to create our own narratives at will, as the questionnaire suggests, but because Realpolitik means that we are forced to mount narratives that directly repudiate, if they do not espouse, the ‘strategic priorities’ that have been nominated by government departments.²

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¹ Most of the major authors in the English canon could be described, even by the most hopeful, as ambivalently patriotic at best.

² Cf. [Poe 2012] on the need to challenge online comment by mounting an alternative. I happen to be sympathetic to this argument, but there are some historiographical debates which are too complicated to make easily digestible for non-specialist online audiences. It will be a shame if ‘as well as’ becomes ‘instead of’.

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Neither a Candle for God nor a Poker for the Devil (Some Thoughts on the Anthropology of Religion in Russia)¹

The fate of that narrow area of the humanities in which I am engaged, the anthropology of religion, is an unhappy one, and one which in this mirrors both the object of research (religion, or rather religions) and the discipline itself (anthropology). The post-Soviet 'freedom from everything' of the early 1990s, when the spiritual searching of the late Soviet period was able to institutionalise itself in the form of completely new, authentic movements and groups, as well as some that were not altogether new and brought in from outside, and others that were not new at all, was soon replaced by a conception of freedom as the consciousness of necessity. People in the former Soviet Union, having lived through times of uncertainty and privation, naturally desired a normal bourgeois level of prosperity, and clarity of moral signposts. They wanted the future — and indeed the past — to be definite and positive at last. The result of this conservative revolution was precisely the situation which society had a moment before been running away from — a mistrust of diversity. Definiteness took the form of an orientation towards two ideals that would seem to be poles apart: an imaginary Western standard for everything concerning the life of the body and individual personality in general, and a no less imaginary national standard in things

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¹ The title cites a proverbial expression akin to 'Neither fish, fowl, nor good red herring' in English. [Eds.].

concerning public, social life. In these conditions, where there was an acute need for cultural symbols that could unite the nation, a boom in Orthodoxy was perfectly natural. But in the first half of the 1990s, this had seemed a very doubtful prospect: Orthodoxy was the religion of a minority and was not particularly noticeable among the great diversity of wares offered in the religious markets that had suddenly opened. Out of this cross-fertilisation of neo-liberal and conservative ideas grew the religious needs (assuming that there are such things) of the average modern Russian, a New Ager on the inside and an Orthodox on the outside. As a result of these processes religion came to be surrounded by a general atmosphere of discontent: categorically secular people (mostly from the older generation) are irritated by the Orthodox dominance that imposes itself on public life and by the discourse of religious nationalism, while believers are annoyed that all kinds of ignorant people are proclaiming themselves Orthodox by right of birth and attaching themselves to the Church, and the Orthodox masses are angry that the goods and services they receive are not of the advertised quality: for them religion is responsible above all for ethics and morality, the established religious institution is supposed to be the nation's conscience, and that conscience should be clear. Clear of economic Realpolitik and the passions associated with it.

Social irritation is probably not the best stimulus to academic work, and that is one of the reasons for a reluctance to study contemporary religion, particularly the dominant type. There is no significant growth in the number of works in this field, nor, what is most important, in that field of religious studies which is conventionally called the anthropology of religion, which assumes prolonged contact with the group being studied, habits of self-reflection, and participant observation taking up a large amount of time. Anthropology is a slow subject, and nowadays anyone commissioning work expects swift and precise answers, and best of all, questionnaires, mass surveys, tables and graphs, so as to look academically respectable. The other extreme is represented by work in political studies, which deals in the main with the public discourse concerning religion. All this, of course, is by no means meaningless, and would even be useful if only there were not such a void in the area of the anthropology of religion, which is, I repeat, a slow subject. It requires the observance of simple, but not particularly convenient rules: a calm and benevolent attitude to informants, an academic, and not just a personal interest in the field, the capacity to distance oneself, and in addition to all that, the time to test, to think, to decipher interviews, to review the diaries and photographs taken in the field over and over again. There is nothing new in this; it is ordinary anthropology, but in the field of religious studies it is not, to put it mildly, the methodology for the systematic addition to the sum of our knowledge that is most in demand.

It should be noted that the situation is far better in respect of the study of religion in its historical aspect (particularly in the Soviet period), at least in quantitative terms (for more detail see [Mitrokhin 2012]). Some of these publications have an anthropological twist, as the expression goes, and make interesting and useful reading for anthropologists. This sort of research may be found, for example, in two Russian academic journals devoted to religious studies: the newly re-launched *Gosudarstvo, religiya, tserkov v Rossii i za rubezhom* [State, Religion and Church in Russia and Abroad] and occasionally in *Religievedenie* [Religious Studies], and also in historical and intellectual publications that do not specialise in religion.

The present situation is all the more peculiar in relation to the situation ten years ago in that the elements that initiate research in the social sciences are not the same. Then it was the Western funding agencies that called the tune, in the service not only of the maintenance in academic life, by financial support, of those who might otherwise have left it, but also of enlightenment, and also of the integration of research in the social sciences and humanities into world scholarship (primarily in English): it was the grant competitions of these funding bodies that were the conduit (though not of course the only one) for 'Western' academic knowledge at that period. It was through the Soros, MacArthur and Ford foundations that we discovered what was going on there, what research questions were being asked, what problems studied, and by what methods. Of course, this had its drawbacks, not every dress either suits you or fits you, but the funding bodies really did allow us to see how 'we' differed from 'them' and gave us the opportunity to participate in world scholarship, if there is such a thing. For the sake of honesty one should add the caveat that even in those liberal times when grants were awarded by open competition there was a definite asymmetry, a sort of unfairness towards those who lived not in the great cities, but in small provincial towns, where such information was not easily available and the internet became generally accessible only recently, by which time the bodies in question were already winding down their activities in Russia.

This all ended a while ago, partly because the funding bodies had achieved what they set out to do and stopped investing in the development of academia in the new independent states because they were no longer new, and partly on the initiative of the states themselves, motivated by the ideas of a new nationalism. Now scholarship, in addition to the state, is finding new patrons in the form of business, and one can see how this is working from the European University in St Petersburg, which has established named chairs and research studentships with money from business. This is of course not a new idea, and Bronisław Malinowski made his name and turned anthropology into a field discipline essentially because he had been

able to persuade business to give the participants in his field seminar at the LSE grants to carry out prolonged fieldwork. In my academic field I know of only one instance where business has taken a direct part in supporting research — the Moscow independent sociological service ‘Sreda’, which so far is engaged more in educational than academic activities, but is doing it by no means badly.

However, it is basically society and not business that commissions academic knowledge, and this commissioning usually reaches me in the form of the topics chosen by students for their coursework and dissertations and infrequent invitations from the mass media in the capacity of an expert. And this is what is curious: the media want commentaries on Orthodoxy, without any criticism or analysis, so that the viewer can see a nice, recognisable picture that he will find it easy and enjoyable to watch and listen to, whereas the students are prepared to write about absolutely anything so long as it is not Orthodoxy. All the time I have been working with students in my university I can remember only one piece of work on Orthodoxy based on the student’s own field material. It was about the practice of catechising young people at St Vladimir’s Cathedral in St Petersburg, an unusual, indeed unique, project with ‘a whiff of Protestantism about it’ (not my words), of which many people in the Russian Orthodox Church disapproved. Usually students choose exotic topics (which is perhaps understandable, given that their work is anthropological), but such as are at the same time close to their hearts or to which they are sympathetic, usually about various New Age groups, from Wiccans to the followers of Roerich.¹

Research — in the social sciences at least — is always a dialogue: we write with a specific image in front of us, of the reader, the opponent or the expert. The texts we write or the lectures we deliver depend on how we imagine this person, including the choice of material, the analytical models, the rhetoric, and even the language we use, simplified or esoteric. It has so turned out that I have never been in the situation of working within a circle of close colleagues — I do have such a circle, I am glad to say, but much of my working time is spent in a much wider circle of colleagues who are not anthropologists and not specialists in religion. I have on several occasions encountered prejudice against religious studies: there are people who regard all believers as hypocrites, people who regard the subject as too marginal, and people who do not believe that there are any methods by which religious life can be adequately studied. One cannot help remembering Durkheim and his rigid division (afterwards disputed by many as unconvincing) of the world into the sacred and the profane: sometimes it seems to me that some of my colleagues, who do not

¹ The painter Nicholas Roerich (Nikolai Rerikh, 1874–1947), known for his mystical paintings and theories; the creator of the first stage designs for Stravinsky and Benois’s *The Rite of Spring*. [Eds.].

study religious practice, almost instinctively place religion in the sacred sphere, thereby excluding it from study by the methods of profane science. If this religious field is at some historical or geographical distance, then science still has a chance, but if it is here and now, then it is better not to touch it. This sort of critical attitude towards religion as a subject and towards the methodology (the ethnographical method) means that one always has to allow for this sort of mistrust and teach the students to explain why their work has meaning and scholarly value. It is easier to regard believers as freaks or psychopaths than normal, rational human beings: no one has yet abolished the escape into ethnocentricity (that is, I am normal, and anyone who is not like me is not).

I am reluctant to end on such a mournful note. A lot of good things have happened in Russian anthropological sciences (and even in the anthropology of religion) over the past ten years. Although it is still marginal within the social sciences in general in Russia, young people are appearing who want to study it and who continue their education in English-language PhD programmes, extended post-doctoral programmes or participating in international projects, and are in the process of overcoming the distance between 'Western' and 'native' scholarship in their own careers.¹ It is also a good thing that our Western colleagues are ready to participate in Russian academic life by participating in conferences and publishing in journals and miscellanies (see, for example: [Russele 2100; Tosheva 2011; Leète 2013]).²

I shall conclude by saying that it is already possible to speak of certain specific features of Russian anthropology: for example, that its boundaries remain deliberately vague (as can be seen from the topics of articles published in *Antropologicheskii Forum*), and it transforms itself now into social history, now into folklore studies, now into cultural research. Returning to the textile metaphor, our anthropology is a patchwork — we make something new out of whatever we find in the trunk. The result is variegated but (and?) cheerful. I think that this is a good thing: movable boundaries are the other side of the active life that is taking place within them, and the result is a delight to the eye.

Of course, the signal event of the last ten years was the re-naming a few years ago of the Faculty of Ethnology of the European University as the Faculty of Anthropology. All we have to do now is convince the academic bureaucracy of the need to introduce the speciality of

¹ To name but two of the international projects in the anthropology of religion supported by American funding bodies: 'Pentecostal and charismatic movements in contemporary Russia' (led by A. A. Pancheko and P. Plattet), and 'Sensory Spirituality: Prayer as Transformative Practice in Eastern Christianity' (led by Sonja Luehrman).

² I shall at least mention the recent conference on 'Religious Nationalism as Concept and Practice' at the Research Institute of the Higher School of Economics (St Petersburg) in October 2013, an account of which will be published in *AF* [Smelova 2014].

‘social/cultural anthropology’ into the associated list of subjects, so that we can award doctorates and other higher degrees in anthropology. This might happen some time during the next ten years. Who knows?

As for the anthropology of religion — perhaps its fate in this country should not be different from anywhere else.

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Honestly, I don’t understand anything at all. Neither, I think, does anyone else. But a lot of people are pretending that they understand. And the most successful strategy is to keep quiet. Not to do nothing, but to say nothing. It is just that not many people can allow themselves to do this. Words are, after all, academics’ livelihood.

I do not understand what is going on today. For example, there is a call for papers for a conference about hares. So I applied. But it turns out that I also have to fill in a form giving a bibliographical descriptions of those three of my works ‘which are closest to the topic of the paper offered’. Three publications on hares? Or will any animal do? Against the background of my general failure to understand what was going on, this was really too much. A conference on hares. Just to spite the conductor I shall buy a ticket and go on foot.¹ Medinsky² said that academics are studying nonsense, because they are studying the philosophy of hares. Therefore just to spite Medinsky we shall follow his pointer and organise a conference on hares. And if he mentions guinea pigs tomorrow, we shall study them too. Everyone understands that this is a gesture. At least something. At least a gesture.

But I secretly think that studying hares is studying nonsense. Not because hares are nonsense, but because there has to be something that is not determined by Medinsky. It turns out that there is nothing moving scholarship, and in that case, why should it not move at a wave of Medinsky’s magic wand?

This is much easier than asking ourselves what we are actually doing. Forget about the bureaucracy, the career ladder, the attributes of symbolic status, the PRND,³ citation indexes, reports and so on and so forth. What do we actually do? And, more to the point, why? I am not at all inclined to say that we are doing everything wrong and that there is no reason why we should. I am even using the pronoun ‘we’, and not ‘you’ or ‘they’. Because it seems to me that there is some point to scholarship. In the heat of academic passions I even wrote an emotive note (which, admittedly, was not published at the time of the event that had provoked it, and afterwards it was somehow no longer *à propos*), part of which I shall recycle here.

The question: ‘What do they actually do?’ is an astonishing one. Would it ever occur to anyone to ask what an iOS developer actually does? Do you know what his duties are? He must ‘be aware of all contemporary technical trends, the latest frameworks, the inner workings of Objective-C and SDK, and keep up with the latest “cool” offers; must be quick at writing code that he won’t be ashamed to show anyone, and even quicker at creating dirty prototypes where no one is going to look at the code; he must know more than us about programming patterns; he must be able to defend with ease the position that the logic should be in the models and not in the

¹ A passenger who travels without a ticket is colloquially known as a ‘hare’ in Russian. [Transl.].

² Vladimir Medinsky, Minister of Culture of the Russian Federation. He really did say this, though it is not known to what he was referring. The conference was organised in response to his remarks. [Transl.].

³ The Russian equivalent of the RAE/REF in the UK, i.e. a national research audit. [Transl.].

controllers'. This is quoted from a real job advertisement <<http://siliconrus.com/2013/04/silnyiy-ios-razrabotchik/>>. Do you know what it's all about? Neither do I. But there again, why should I? The firm exists, it sells a certain product, but who makes it, and how they make it, nobody cares.

'This is not the case with academic work. Everybody wants to know why we should study the decoration of a nineteenth-century Setu pinafore dress, or the Ubykh language, which nobody speaks any more. Everybody wants to know why. And this is all because nobody understands what the finished product is, who sells it, who buys it, and what for. Therefore we have to explain that a specialist in Ubykh is like an iOS developer, only much, much cooler. And the finished product is our understanding of the world about us. Everything that we know about it is the result of academic activity. Any objections?'

I have quoted myself. I spend the long autumn evenings expounding this thought to myself. I still think that it is right, but in the morning I forget it, and again I stop understanding why we should study the decoration of a nineteenth-century Setu pinafore dress.

If we leave aside my lack of understanding, it seems that all is well with Russian anthropology. Russian anthropologists have been partially integrated into the international milieu. The Western milieu has been included into our national historiography. I meet my fellow-countrymen at every international academic event, and as a rule they look very well, not only in no way inferior to their Western colleagues, but often surpassing them in their command of theory and of the material, and their ability to prepare it so as to leave everyone open-mouthed.

It seems to me that international academia has already absorbed Russia. Not all of it, of course. But neither in the USA, nor in Germany, nor in Britain does it ever happen that scholars take part in the broad discussion otherwise than on an equal footing. So everything is fine. Though it is still a question whether there is any broad discussion.

When I think of Russian anthropology, for some reason I want to write about Western anthropology. There is now a curious tendency to entitle articles with strings of words (for example, 'Converting Difference: Metaculture, Missionaries, and the Politics of Locality'), in such a way that it is impossible to tell what they are about. I think that we shall soon be doing that as well.

Another recent phenomenon — the mega-grant. That is when there is a lot of money. It is remarkable that when mega-projects are announced, they hardly ever mention their purpose, nor the material to be used, nor, O horror! the expected results. Just the sums of money involved. This is another of the things that I don't understand.

Recently I have often evaluated publications on the basis of whether one can give them to students to read. There are now many more Russian-language publications that one can give them without anxiety than there were ten years ago. Over the last few years many high-quality articles have appeared in this country. But for some reason the only monograph (meaning in the field of anthropology and ethnography) that comes to mind is Shnirelman, and that was in 2006, so it probably doesn't count any more.¹

For some reason it very often happens, when I begin to read someone's work, that I realise that its merit is that I know what will come next. That is, the author has arranged his subject on the operating table and chosen such a set of tools that I know for sure that the surgeon is a first-class one and that the operation will proceed without complications. Unfortunately at this point I usually stop reading, because everything is already clear.

And another thing — so many people have left academia. I don't mean those who have left the country. When it comes down to it, I don't understand why nobody worries when a scholar moves from Germany to Britain, but when they move there from Russia it is the 'brain drain'. The people I mean are those who, being of sound mind, established scholars, even distinguished scholars, have made a conscious choice to leave academia, but stay in Russia. In order, for example, to give private lessons. It is not only a matter of money. (And yes, I do understand that this would also be an excellent subject for a research project.) It is just that, when you exist, and you have certain knowledge, and in front of you sits someone else who needs that knowledge, everything becomes somehow clearer. The more so that, when you teach someone what the Standoff on the Ugra was, you might also incidentally teach him what the Ugra is, where it is and what else there is in the locality. He may, besides, learn how to read, speak, write and think. And, quite exceptionally, you might teach him the difference between right and wrong. You can teach all these things while preparing people for University Entrance. And you can't do it while doing research. That is the paradox.

Because scholarship, with all its vast machinery, has not been able to teach people that evolution is not 'just another hypothesis'. That the colour of a person's skin is no reason to murder him. It has not been able to explain that medicine is better than folk-healing. Scholarship has never taught anybody anything. Perhaps it isn't even supposed to. More than that, it seems to have been doing its best to prove the opposite. The idea that magic, science and religion are equivalent explicatory models had become a commonplace of scholarly thought

¹ Viktor Shnirelman. *Byt alanami. Intellektualy i politika na Severnom Kavkaze XX stoletiya*. M.: Novoe literaturnoe obozrenie, 2006. [Eds.].

as a result of the long evolution of interpretative theories in the social sciences. So what can you do but shrug your shoulders? Practice confirms theory. But that doesn't make you feel any better.

There is no crisis of scholarship. Unfortunately there has already been a decent amount written about the concept of 'crisis' in the historiography of science, so there is no prospect of making a mega-project out of it. What there is is an emotionally-charged blundering about in the dark. An attempt to substitute political motivation for scholarly motivation. A lack of a clear understanding of what scholarly motivation is. Leaving aside competitions for grants, projects that automatically suck in the drifters, and one's own experience, which is, of course, unavoidable.

Are there now schools that determine the discipline's vectors of development, or are these vectors created by the work of individual researchers?

Schools continue to exist. For example, the Muscovites, about whose books people actually say 'Did you see what the Muscovites have published now?' I have in mind that group of researchers who have long been working under Neklyudov's leadership and have with admirable persistence been ploughing the fields of contemporary folklore. In St Petersburg there is the Faculty of Literary Studies, where Svetlana Adonyeva and Inna Veselova work. And of course the European University, which has already become a recognisable brand. One could certainly say nowadays that there is a European University School. Despite differences between faculties, programmes and methods, despite arguments on various subjects, people who have been through the European University speak the same language and can recognise each other in any lecture theatre.

On the other hand, I do think that the vectors of scholarship are nevertheless created by individual researchers. Therefore it is much more important to subscribe to a particular person than to a particular journal. And therefore the appearance of Academia.edu is, in my opinion, one of the most important events of the last ten years.

None of what has been said above applies to scholarship in its full-scale academic sense. The distinction between 'provincial' and 'native' science, suggested by Sokolov and Titaev [Sokolov, Titaev 2013] disturbs me. While I do not accept it, and while I consider it important to avoid any expression that might tend towards the affirmation of these metaphors, I am perfectly aware that there exist different understandings of what 'being a scholar' means. But these understandings, in my view, are connected with much more global processes than the transformation of the Russian scholarly community as such.

I do not know about other disciplines, but in ethnography there are nowadays at least two radically different types of academic work, each

of which is connected with a particular understanding of the place of ethnographic knowledge in the overall system of knowledge, its role and significance in the modern world and, accordingly, that very scholarly motivation that determines the vector of academic activity.

One of these is anthropological research in the 'traditional' sense, which grows out of the history of the subject, fills in the gaps that are found to exist in academic knowledge, and is determined by the state of the subject and by personal interests. The second is applied ethnography, for which there is certainly no demand on the international academic market, but which is directed towards the needs which have unexpectedly arisen in Russian society itself.

Ethnographers lecture to choreographers and stage directors, prepare folk costume patterns for publication, publish books of traditional recipes, participate in master classes in traditional crafts, and so on. This new niche for ethnography, which has appeared over the last ten or fifteen years, is evidence for serious change, not so much in the discipline itself as in the social context in which it exists. For a long time ethnography was a science that produced knowledge about 'something else'. Now it has become a science about 'us'. Ethnographers and anthropologists used to be required as experts on other people's culture, and even when it was a question of studying 'our own' people, the subject had to be exoticised as a necessary part of the scientific examination. Today, ethnographic knowledge is in demand as knowledge of our own culture, 'alienated, forgotten and lost'. And ethnographers play the part of the conservators and experts who can help to 'restore, return and renew' it.

This very phenomenon, connected as it is with a revolution in ethnographic knowledge and knowledge of the past and of cultural roots, requires a special analysis. Does this transformation mean that traditional ethnography is now being replaced by two distinct sciences? Or is it disappearing as a science? Is ethnography becoming part of the cultural heritage industry, a provider of material for the (re)construction of the past? And what does that mean for the evolution of our cultural memory itself. I do not know.

The main thing is, that I do not understand how all this can co-exist. Mega-grants, conferences on hares, master classes on traditional weaving, congresses of the ethnographers and anthropologists of Russia, advantageous placements, participation in international forums and so on and so forth. But I am not entirely convinced that this is the sort of failure of understanding that usually results in a research problem.

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1

In principle, perhaps nothing has changed. That is, the existing trend towards the expansion of the scope of folklore studies has not changed, and is continuing in more or less the same directions as in the 1990s:

- the urban oral tradition;
- the general forms in which folklore, including ‘classical’ folklore, exists today;
- texts, practices and artefacts which have proved susceptible to folkloristic analysis (the results of religious activity, mass culture, mass literature, web communication, etc.)

The last of these, however, is not so much a broadening of the scope of folklore studies as a use of its tools by related disciplines, including relatively inchoate fields of research (the Internet as a new form of cultural tradition, say). It is understandable that both the increase in the number of objects regarded as ‘facts of folklore’ and the use of means of analysis borrowed from other fields (linguistics, anthropology, sociology, psychology, biology and mathematics) make contemporary folklore studies more and more interdisciplinary and make its methodology more and more universal. This process seems to me highly productive, both for the humanities in general and for folklore studies in particular, and from my point of view its methodological potential is still far from being fully realised. Finally, at some unspecified time in the future, there will evidently be a return to a number of half-forgotten topics connected with the structure of texts, the problem of their generic stratification and with experiments in the historical reconstruction of folk traditions, but for the time being one can only note the preparation of a new, improved set of tools to be used when this takes place.

2

Over a relatively short period the generation of long-lived scholars who were my teachers has passed away, while the generation of my older

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pupils has definitively matured. This represents a qualitative change to the 'strata' that make up our milieu, and the stage of academic life is now set by researchers whose academic, and indeed cultural foundation is very different from that of the previous generation. This affects both the topics they prefer (see point 1) and their methodological approaches, though I cannot see that it has given rise to any particular tension. If there are any disagreements over principle in our area, they are ideological rather than generational in character. I, at least, have never encountered such problems, and am in complete sympathy with my younger colleagues, which I hope is mutual.

3

They have changed quite markedly, in my opinion, though again these changes did not begin in the last ten years, but much earlier. As happens in periods of social and ideological crisis, society becomes disillusioned with rational knowledge, which inevitably brings forth monsters of the wildest obscurantism, examples of which can be found not so long ago. Learning is 'out of fashion' nowadays, there is no shame in being an ignoramus, what you need to be is 'successful', and that in the socio-economic (or rather financial) sense. This particularly concerns 'pure', i.e. 'fundamental' knowledge (the 'purity' of which could be the subject of a separate discussion, as could its connexion with 'practical' knowledge), the value of which has fallen almost to zero.

The current anti-intellectualism is paradoxical. There was, perhaps, never a time when human well-being depended so much and so obviously on scientific and scholarly research. The very conditions for the viability of modern society, including its technology, food supply and medicine, were created not in mystic visions or religious ecstasy, but as a result of scientific investigations. And today's man in the street expresses his disillusion with fundamental research while sitting at his computer and then sending his opinions *urbi et orbi* onto social networks on the Internet. Truly, so long as there are acorns...¹

And the state? It is, after all, a part of society, an organ of its direct action.

4

My methodological base is a semiotic and structuro-typological approach to oral traditions, but, obviously, in a modern, revived form. To a greater or lesser degree I seem to be succeeding in convincing my younger colleagues of the usefulness of this toolkit. Naturally this does not prevent me from having recourse to the analytical possibilities of other currents within folklore studies which are consistent with our theoretical outlook. Academic schools within folklore studies do, of course, exist at the present time.

¹ *So long as there are acorns*: a quotation from Ivan Krylov's fable 'The Pig beneath the Oak Tree', in which the Pig, informed by a Crow that its assault on the Oak is damaging the tree, replies 'Who cares? So long as there are acorns...' [Transl.].

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1

2

3

If we are speaking principally of Russian scholarship and more about the practical than the institutional side of the matter, then it seems to me that the main outcome of the last decade has been a very significant blurring of the boundaries between disciplines and thematic hierarchies in the ‘anthropological’ and ‘historical and literary’ fields. Those things which we spoke about in the first issue of *Forum* as possible, desirable, or actually happening have now become self-evident realities. And these realities have not only opened up important and interesting intellectual prospects, but also faced us with new problems.

I think that the changes that have taken place may be explained not so much as the results of our theoretical debates (although these did probably play some part in the transformation of academic study) as of more general processes connected with the informational changes of the modern world. The development of electronic networks and means of saving and searching for information have substantially changed the ‘craft’ of specialised research in the humanities. One can often hear people say that Google and Yandex have ‘killed’ scholarly erudition, and there is much truth in this, although the way information is now presented on the Internet still requires a scholar to have specialised factual knowledge and special skills in searching. It is, however, not a matter of erudition as such; it has become evident that the ‘old’ methods of preserving and exploiting information were among the major factors in the formation and maintenance of the boundaries between disciplines. In many disciplines and academic communities academic work had very nearly been reduced to the struggle to maintain an illusory control over informational resources — not to mention a certain type of librarian and archivist (who are doomed to extinction, but still have quite a lot of life in them) who saw their vocation as making it harder

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rather than easier for 'outsiders' to have access to the information in their charge. This can all of course be explained by a specific Soviet/Russian paranoia concerning the generation of knowledge, but even so it seems to me possible to speak of more general tendencies in twentieth-century world academia.

The situation has since changed, and there are fewer and fewer scholarly publications that deal with source material and, while they are important for solving particular applied problems, lack a wider analytical significance. We should now, in fact, be considering ways to optimise the traditional means of scholarly publication and commentary on the sources. Thus academic editions of 'works of literature and folklore' are gradually losing any significance, insofar as in a society where the role of the book and of 'traditional' forms of reading is rapidly diminishing, academic 'collected works' and 'folklore corpora' are of prime or exclusive interest to professionals. Evidently the old academic editions must give way to electronic databases with convenient interfaces for researchers and means of presenting texts, commentaries and analyses. I think that analogous means of publishing sources should also appear in other areas of the humanities.

But I am digressing somewhat from my topic. The accessibility of information (be it sources or analytical works) and the related penetrability of discipline boundaries have shown that both the 'tools of the scholar's trade' and the landmarks by which she or he has been guided in theory and methodology, and which were regarded as the foundations of one or another area of knowledge, have a sufficient capacity for 'interchange', and that questions discussed within one 'narrow circle of specialists' as a rule have 'twins' in neighbouring, or sometimes in quite remote research communities. It is in fact the very criterion of 'transferability' that now allows one to distinguish a problem from a pseudo-problem. If a researcher cannot express the meaning of his or her work in the 'foreign' language of another discipline, either s/he is engaged in nonsense or else in an extremely narrow study of sources without analytical significance.

In practice this means that academic work that formally belongs to the department of, say, folklore studies, cannot be regarded a professional if it takes no account of the data, methods and theories of various directions in anthropology, sociology, history and maybe other disciplines, depending on the particular topic studied. In essence, the papers that I have heard in recent years at conferences of the international associations of folklore, anthropology and religious studies have really differed very little from each other in either their terminological or methodological aspect. Moreover, it is to a large extent the same people reading the papers in seemingly different disciplinary contexts. Such a situation, it seems to me, in no way

reduces the importance of expert knowledge, which remains the foundation of academic activity. However, it is clearly no longer enough for a scholar to have only empirical or specialised erudition: every qualified researcher must be able to find his way around the disciplinary diversity of the modern theories and methods which have some sort of connexion with what he is studying.

This naturally provokes a more serious discourse about the institutional frameworks for knowledge in the humanities and about the relationship between scholarship and society in the modern world. If the boundaries between disciplines have turned out to be illusory and penetrable, what should modern scholarly institutions look like? I have had occasion to write, for example, that both essentialist notions of folklore as a special area of culture regulated by imagined objective 'laws', and romantic and nationalist fantasies have not only led folklore studies up an analytical blind alley, but have compromised it as a scholarly discipline. However, does this mean that the university programmes and departments and research centres engaged in folklore studies ought to be closed? I think that that would be quite unnecessary. It is precisely this understanding of the conditional nature of boundaries between disciplines that allows us to keep and even protect the old institutional 'brands' and provide them with new research content. It is another question, whether the 'narrow specialists' who have built their careers on thoroughly empirical erudition and a rigid subject identity are prepared to do this. It is quite possible that they are not, but it seems to me that they will still not be able to maintain the status quo.

It is of course another matter that a scholar living in Russia has to make allowances for the peculiarities of the society in whose midst he is and whose future seems to me quite unpredictable. It is not only a question of the mass reversion to feral Orthodox patriotism (not that that can be ignored), but also of the formation of a new totalitarian regime, supported, as is proper to such regimes, by a formidable bureaucracy, and spreading bureaucratic metastases in all areas of professional activity, including higher education and academic study. A large proportion of the output (monographs, textbooks and lecture courses alike) of the humanities in the Soviet Union during the period of 'mature socialism' consisted, so to speak, of 'intellectual rubbish' devoted to so-called 'Marxism-Leninism', 'dialectical' and other forms of 'materialism', 'scientific atheism' and so on. The socio-economic crisis of the 1990s of course forced many qualified specialists to leave the country, and particularly complicated the lives of the new generation of researchers, but at the same time it expelled the 'rubbish scholars', whose interests lay in money and bureaucratic power rather than intellectual activity, from academic life. Now it seems we can look forward to their return. Higher education has already been quite seriously crippled by bureaucratic

innovations, and the coming reform of the Russian Academy of Sciences will probably do something similar to research institutes. I will not take it upon myself to say how precisely the ‘new bureaucrats’ will behave in relation to scholarly institutions and the practices of intellectual work, but we can hardly expect anything good. An obvious example of the sort of thing that can happen is the destruction of the Russian Institute of Art History at the hands of the boorish minister Medinsky and his appointees.¹ However, I must stress once again that I consider it quite difficult to make predictions regarding even the immediate future of Russia as society or state.

One further problem which has become quite noticeable in recent years and is to a certain degree connected with the informational processes already mentioned is the relationship between academic research and so-called para- and pseudo-science. This was in fact the subject of a discussion in volume 18 of *Antropologicheskii forum*, in which I was unfortunately unable to take part [Forum 2013]. Therefore I shall confine myself now to a few short remarks which have, I think, a certain relevance to the question about scholarship and society. One cannot but agree that the avalanche of ‘alternative’ scientific practices and ideologies at the beginning of the twenty-first century is in itself due to a large extent to the new means of disseminating information, and thereby blurring the boundaries between disciplines, and also to the collapse of the rigid hierarchical structures by which knowledge was generated. At the same time it should probably also be a question of more general features of modern society, in which key concepts such as ‘science’ and ‘religion’ are subject to constant competition and discursive (re)interpretation. On the one hand I am ready to agree that ‘legitimate scholarship’ should take para-scientific discourse ‘seriously’, and that the difference between ‘pseudo-scientists’ and ‘real’ scientists is not as vital as some people think. However, my special studies of various forms of the ‘(para)scientific imagination’ in the culture of contemporary religious movements allow me to suggest that academic science and pseudo-science do in fact appeal to different meta-narratives and answer different questions, which can hardly be reduced simply to the human need ‘to make sense of the environment by whatever means available’. It is no accident, for example, that one constant companion of contemporary para-scientific discourse is the conspiracy narrative, which as a ‘means of making sense’ is, if generally accessible, nevertheless in its own way very specific. In short, it seems to me that pseudo-scientific texts, ideas and practices should not only be the objects of debunking and indignation, but also of

¹ The Russian Institute of Art History in St Petersburg was subjected to a programme of reforms in 2012–2013 which led to its amalgamation with the University of Culture and effective end of its existence as a scholarly research centre. Many leading members of staff resigned or retired in protest.

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anthropological research. Moreover, such research is already taking place, and has been for some years. At the same time, however — and this must apply not only to para-science, but also to other discourses and social groups that have become the object of our analytical research — we are in no position to regard our research stance as altogether ‘monolithic’. When we write about para-scientific conspiracy theories as ‘a reaction against modernisation’ or as ‘a means of constructing an identity’ we ought to remember that these conspiracy theories are used by other people in an attempt, if not to study, then at least to make sense of our own existence. In such a way the subject and object of academic research become in one way equal participants in a social dialogue. Although the final results of such a dialogue are by no means always obvious, and it may take a considerable emotional toll on both sides, I still find it a more tempting prospect than a renaissance of bureaucratic hierarchies and of ‘rubbish science’ on the Soviet model. I think, however, that in the long term a return to these and other twentieth-century institutional forms of scholarship will in any case be impossible.

It seems to me that the time of ‘academic schools’ in any area of the humanities is past. The very meaning of this concept does in fact deserve a special discussion, but if we understand a school to be a permanent research group united by a strict explicatory model, methodology or subject orientation, then I do not think that there is much future for this kind of scholarly community. Furthermore, modern scholarship is organised not as a hierarchy but as a network, that is, it does not consist of groups each headed by a charismatic leader, but of informal (usually international) communities which incline towards an egalitarian character. In this context I find it more interesting and useful to follow the work of different researchers, both those who are part of ‘my’ network and those who are closer to other informal communities. It must be borne in mind that the concept of a ‘school’ is connected not only with notions of academic hierarchy, but also with the idea of epistemological monism, the orientation towards ‘single’ and ‘universal’ explanatory models. I think that this sort of approach is also becoming a thing of the past. Though I cannot call myself a consistent supporter of Paul Feyerabend’s ideas, I presume that the formation of interpretative models and ‘structures of interest’ in modern scholarship is not subject to the monist principle and is based on more complex processes, deserving both of conceptualisation and of separate investigation.

MADELEINE REEVES

**The Anthropology of Central Asia
Ten Years After ‘The State of the Field’:
A Cup Half Full or Half Empty?**

When the first issue of *Antropologicheskii forum* appeared in 2004, the number of published ethnographic monographs in English addressing the five states of post-Soviet Central Asia could be counted on the fingers of one hand.¹ Anthropologists trained outside the Soviet Union had been undertaking fieldwork-based doctoral dissertations on Central Asia since the mid-1980s, when long-term ethnographic research in the region became a realistic possibility for the first time.² By the early 2000s, doctoral field research undertaken in the preceding decade was making scholarly contributions to debates as diverse as kinship, marriage and gender relations in Tajikistan [Tett 1995; Harris 2000], Kazakh household networks [Werner 1997], the reconstitution of former collective farms in Uzbekistan [Abramson 1998; Zanca 1999], material culture and domestic space [Bunn 2000], local dynamics of power and political authority [Pétric 2000; Rasanayagam 2002], Muslim religious practice and collective memory [Privratsky 2001], the political economy of medicine in war-torn Tajik Badakhshan [Keshavjee 1998], inter-generational and inter-ethnic dynamics in Kyrgyzstan [Kuehnast 1997; Liu 2002], sexual relations in Almaty [Rigi 1999], nationhood and the reconstitution of

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¹ Given the brief I was presented for this ‘state of the field’ review, I primarily address ethnographic literature in English relating to Kazakhstan, Kyrgyzstan, Tajikistan, Turkmenistan and Uzbekistan, with comparative reference to anthropological literatures in French and Russian. For reasons of space, I do not address the significant contributions that have been made to the broader anthropology of the Central Asian region, which would include Afghanistan and Xinjiang. Likewise, I do not address the study of culturally allied regions of Mongolia and Inner Asia. For an excellent synthesis of the important conversation between the scholarship of Central and Southwest Asia to have emerged in the last decade, see [Marsden 2012].

² Schoeberlein-Engel [1994] makes the claim to primacy in his 1994 doctoral dissertation on *Identity in Central Asia*, drawing on research between 1986 and 1991. Three decades earlier, Elizabeth Bacon published a monograph on *Central Asia Under Russian Rule*, based on fieldwork in the 1930s. The foreword to the 1980 version of that book, however, makes clear that the only fieldwork that Bacon was able to undertake in Soviet Central Asia consisted of a stay of a few weeks in Almaty in the summer of 1934. The remainder of the fieldwork for the book was conducted in Afghanistan and Iran and extrapolated to Central Asia [Bacon 1980].

a once privileged cultural elite in Uzbekistan [Adams 1998; Doi 1999] and human-animal relations in Kyrgyzstan [Jacquesson 2000]. Much of this important doctoral research in the 1990s, however, was either not turned into books, or was published only later the following decade (e.g. [Uehling 2004; Adams 2010; Rasanayagam 2010; Zanca 2011; Jacquesson 2011; Liu 2012]). For scholars teaching on the region in English, the early 2000s were still something of a desert in terms of published book-length ethnographies on Central Asia.

In Russia and former Soviet states the landscape of publications in the early 2000s was equally sparse, but for different reasons. Ethnographic research expeditions to Central Asia in the 1960s — 1980s had enabled a generation of scholars, trained in historical and ethnographic methods, to undertake sustained team-based field and archival research. In its heyday, the then division (*otdel*) of Central Asia and Kazakhstan within the Institute of Ethnology and Anthropology of the Russian Academy of Sciences had sponsored large collective research projects on rural and urban family life. This generated an impressive volume of field materials and reports, many of them still unpublished to this day. This was matched by an array of historical, archeological and textual research sponsored by the *Kunstkamera* in St. Petersburg and the Institute of Oriental Studies in Moscow. The economic crisis of the 1990s, together with changing political priorities, however, meant that many of the scholars trained within these institutes either abandoned academia, switched geographical focus, or came to concentrate on applied questions of conflict and inter-ethnic tension in the immediate post-Soviet period (see, for instance, the Academy of Sciences series, *Research on Applied and Urgent Anthropology* [Issledovaniya po prikladnoi i neotlozhnoi etnologii], launched in 1990). In 2005 the division of Central Asia and Kazakhstan within the Institute for Anthropology and Ethnology of the Russian Academy of Sciences was downgraded to a research group within the Centre for Asian and Pacific Research. Despite the appearance of some important research monographs (e.g. [Bushkov 1995] on the population of northern Tajikistan; [Kalandarov 2004] on the Shugnan population of Tajik Badakhshan) in the early 2000s there was little market for ethnographic monographs on a now radically transformed rural life in Central Asia, and funding for new field based research largely dried up as previously well-resourced academic institutes responded to precipitous falls in their budgets.

In the states of Central Asia ethnographic research was in an equally precarious financial state in the early 2000s, and often recruited to articulating (competing) national narratives or exploring the deep past of the nation. A full assessment of the changing field of anthropological scholarship and teaching in Central Asia is beyond the scope of this article (see [Laruelle 2010] for the case of Uzbekistan;

[Gullette 2010] for Kyrgyzstan). Nonetheless, certain significant trends can be identified, which have analogues in other states of the former Soviet Union. These include a prioritizing of archaeological and archeo-ethnological research projects over those primarily concerned with contemporary social and cultural developments; the discursive domination of particular theoretical approaches, including in Kazakhstan and Kyrgyzstan the elevation of Lev Gumilev's vitalist ideas of national *passionarnost* to the status of *de facto* official ideology;¹ and the emergence of a considerable disjuncture between a generation of scholars trained predominantly in anglophone or francophone academic environments and oriented to understanding how categories of 'nation', 'tribe', 'gender', 'ethnic group' are socially constituted and politically mobilized, and an older generation operating with the more substantivist understanding of 'ethnos' emerging from the late Soviet scholarship of Yulian Bromlei and his pupils.

With notable exceptions, 'official' anthropology, as conducted within national academies of science in Central Asia and displayed in museums of ethnology, was and remains predominantly a descriptive discipline rather than a critical one, and in some instances aligned quite explicitly with political pronouncements of national autochthony or historical primary. In Tajikistan, for instance, scholarly ethnography has been recruited to the task of exploring a distinct Aryan heritage to the Tajik nation. One Tajik ethnologist, who during perestroika researched an authoritative monograph on land use and agricultural traditions in Uzbekistan's Tajik-majority Sokh valley [Dzhakhonov 1989], became a vocal advocate of Aryanism as a national ideology for Tajikistan, supposedly as a counter-weight to the nationalisms of Central Asia's Turkic populations.² In Uzbekistan, meanwhile, ethnology by the early 2000s had become, in Marlene Laruelle's words, 'one of the reigning sciences of Nationhood', tasked with demonstrating 'indisputable foundations for the pre-eminence of the Uzbek people over other national groups in their titular state' [Laruelle 2010: 104], see also [Abashin 2009].

The combination of new nationalisms, a radically transformed publishing environment (such that publishing scholarly monographs

¹ Gumilev's understandings of national survival and passionarity form a central plank of President Askar Akaev's 2004 treatise on Kyrgyz Statehood and the Manas Epic [Akaev 2004].

² Aryanism is needed now, Dzhakhonov is reported to have said in 2005, because we need to 'demonstrate and prove to others where our place is. Each nation should know its place' [Saizadimova 2005]. Interestingly, Dzhakhonov commented to me in an extended series of recorded conversations that we undertook in March 2005 that he considered himself an 'internationalist', citing as an example his own, ethnically mixed, family in Khujand: 'There were times in the past, in the nineteenth century when there were feudal relations, when there wasn't yet any Soviet power, when there was no nation [togda ne bylo natsii], without a nation we went as far as China and nobody asked if I was Tajik! And now they ask you at every step, you see they've learned [vot oni nauchilis!] In the capitalist world [national] consciousness came together already a long time ago' (Interview, Khujand, 22.03.2005).

either required substantial grant funding or considerable personal wealth), and an enduring assumption that ethnology is tasked with connecting the present of a nation to its deep past had led to a rather bleak environment for anthropological scholarship in Central Asia in the early 2000s. Renewed political pressures also meant that Central Asian scholars, particularly in the highly restrictive academic environments of Uzbekistan and Turkmenistan, often avoided topics that could in any way be felt politically sensitive — which largely excluded any research that did not celebrate the enduring spiritual culture of the titular nation. One Uzbekistani scholar, a Tajik speaker who had defended his candidate of science dissertation in Dushanbe in the early 1990s, confided to me in 2005 that he had been reluctant even to bring the dissertation with him back to Uzbekistan, despite its decidedly apolitical exploration of marriage rituals. Its focus on a Tajik-majority region of Uzbekistan made it politically incorrect, he explained: *zamon senga bokmasa, sen zamonga bok!* — roughly, ‘if the times are not looking out for you, you better look out for the times’.¹

The state of the field in 2014

In assessing the ‘state of the field’ in 2014, the relatively sparse landscape of scholarly anthropology on the region a decade earlier should be taken into consideration. The first important change over the last decade concerns volume of publications. The scale of scholarly output in English, both in terms of raw numbers of doctoral students in anthropology focusing on Central Asia and the range of published monographs has grown quite significantly from the mid-2000s and continues to grow, albeit at a less dramatic rate, since 2010.² Arguments have been made for the value of integrating ethnographic research into the study of Central Asia from the disciplinary perspectives of geography [Megan 2002; 2006; Bichsel 2009], political science [Schatz 2004; Heathershaw 2009; Spector 2009; Satybaldieva 2010] and even criminology [Botoeva 2014]. There has been a growing conversation in recent years between political science and anthropology to develop new accounts of the ‘weak state’ in Central Asia [Heathershaw and Schatz 2013; Reeves, Rasanayagam, Beyer 2014], to understand the relationship between politics and Islam [Heathershaw and Roche 2011], and to shed light on local politics and political mobilization [Satybaldieva 2010; Doolotkel-dieva in progress]. Perhaps because of the relatively small size of the

¹ For this reason, I have also not listed this dissertation in the bibliography.

² Among the published monographs focusing ethnographically on Central Asia to have appeared since 2004 are: [Harris 2004; Louw 2007; van der Heide 2008; Kehl-Bodrogi 2008; Gullette 2010; Rasanayagam 2010; Trevisani 2010; Hilgers 2011; Zanca 2011; Liu 2012; Pétric 2013; Finke 2014; Reeves 2014; Roche 2014]. Many more dissertations are currently being turned into books.

field, the range and diversity of scholarly collaborations across disciplines particularly deserves note, between anthropologists and political scientists, for instance, or between anthropologists and historians (e.g [Sahadeo and Zanca 2007]).

Central Asian anthropology has also benefitted from the increasingly diverse range of prior training that scholars are bringing to the field. During the 1990s it was typically the case that researchers beginning work on Central Asia came to the field through a background in Russian/Soviet ethnography and language training in Russian. By the mid-2000s scholars of Central Asia were as likely to have a background in Turkic or Persian studies, or to have undertaken prior fieldwork in the Middle East or South Asia as they were to have previously conducted research in a Russian-speaking context. A striking number of recent US PhDs on Central Asia began doctoral study with a background in the US Peace Corps, bringing to their research the linguistic subtlety, cultural fluency and social network that comes from two years' immersion in village life (three notable examples in the case of Kyrgyzstan are the dissertations by Borbieva [2007], Montgomery [2007] and McBrien [2008]).

The field has expanded conceptually too. Central Asia has benefitted from comparative research by established scholars, whose prior research has expanded debates about colonialism and post-colonialism, migration and livelihood strategies; the anthropology of the state, and gender and development (e.g. Ruth Mandel in Germany, Catherine Alexander in Turkey, Mathijs Pelkmans in Georgia, Roland Hardenberg in India, Magnus Marsden in Pakistan, Peter Finke in Mongolia, Susan Thieme in Nepal). In some cases, researchers have explicitly sought to question the lingering global divisions of cold war area studies, by conducting research that draws together South and Central Asia, or Central Asia and the Middle East. Magnus Marsden, for instance, has conducted innovative research among Afghan traders that moves between Kabul, Dushanbe and Dubai [Marsden 2014]; Roland Hardenberg has brought the insights of South Asian ethnography to an understanding of kin relations in rural Kyrgyzstan [Hardenberg 2009].

Alongside this growth in numbers of scholars, there has been a growth in topics of research and modes of enquiry: recent projects, for instance, have brought debates from Science and Technology Studies, legal and political anthropology, and economic anthropology to bear on Central Asian material as diverse as microfinance initiatives, the writing of new constitutions and the everyday materialization of new roads and international borders. There has been a take-off in urban anthropology, with notable studies of cosmopolitan life in Bukhara, the architectural seductions of contemporary Astana, and inter-group relations in a Bishkek *mikroraion*

among the notable recent studies (see [Humphrey et al 2009] on Bukhara; [Bissenova 2012] and [Laszczkowski 2012] on Astana, [Nasritdinov 2007] and [Schröder 2011] on Bishkek). Studies of religious conversion and religious revivalism have brought recent debates from the anthropology of religion and the anthropology of ethics to bear on Central Asian material [McBrien and Pelkmans 2008]. There have been important contributions to understanding the changing experience of non-titular minorities and their 'adaptive strategies' [Kosmarskaya 2006; Flynn and Kosmarskaya 2011; Liu 2012]. A major research project sponsored by the Christensen fund has allowed for a detailed mapping of sacred sites in Kyrgyzstan and a series of publications on *mazar* worshippers and the guardians of sacred spaces [Aitpaeva et al 2007]. There have also been some notable (though as yet, relatively isolated) contributions to understandings of linguistic anthropology, ethnomusicology and demographic anthropology (see respectively [Dubuisson 2009; Pritchard 2011; Roche 2014]) and to explorations of memory, history writing and the politics of commemoration [Jacquesson et al 2013]. If in 2004 contributors to *Antropologicheskii forum* lamented the relative lack of engagement with 'contemporary' topics by scholars conducting research in the former Soviet Union, this critique no longer holds today. By 2014 anthropologists are as likely to be conducting research among miners in Karaganda [Kesk la, in progress], architects in Astana [Bissenova 2012], road-builders in mountainous Badakhshan [Mostowlansky 2013], artists in Almaty [Nauruzbaeva 2011], lawyers in Bishkek [Beyer 2013] or pop singers in Tashkent [Klenke, in progress] as they are among herders or farmers. They were also as likely to be following livelihoods on the move as they were in place. Anthropologists of Central Asia, for instance, have made significant contributions to understanding the politics of resettlement between Mongolia and Kazakhstan or Kyrgyzstan and Russia [Flynn 2004; Sancak 2007; Barcus and Werner 2010]. They have provided nuanced accounts of Central Asian migrant experience in Russia and Kazakhstan, as well as the role of remittances in sustaining home beyond home [Isabaeva 2011; Rahmonova-Schwarz 2012, Reeves 2012, Thieme 2012; Aitieva in progress]. An emergent literature has paid attention to contemporary mobile livelihoods, among traders, missionaries and merchants [Nasritdinov 2012; Marsden 2014].

Other encouraging trends deserve note: there is more exchange between scholars located in different institutional settings and different countries than there was a decade earlier. Inter-governmental and non-governmental organisations such as the Open Society Institute have enabled an increasing (though by comparative standards still very small) number of scholars from Central Asia to study for and defend doctoral dissertations and undertake post-

doctoral research in western Universities. In Kazakhstan, the government *Bolashak* programme has funded overseas research visits to scholars who would otherwise be unable to conduct archival or field research due to the pressures of their teaching load. At the same time the visibility of Central Asia anthropology at international conferences has increased, and the broader regional field of Central Asian studies has been institutionalized through the efforts of organizations such as the Central Eurasian Studies Society (CESS), the European Association for Central Asian Studies (ESCAS), and the Association for Slavic, East European and Eurasian Studies (ASEEES). Several multi-author volumes focusing ethnographically on Central Asia have been published as books and journal special issues [Kandiyoti and Mandel 1998; Rasanayagam 2006; Sahadeo and Zanca 2007; Reeves 2011; Montgomery 2013; Reeves, Rasanayagam and Beyer 2014], and the region has increasingly figured in comparative collections engaging, for instance, questions of everyday morality and religious reasoning [Hann 2006; Heintz 2009], the relationship between Islam and politics in diverse global settings [Osella and Soares 2010], the ethics of hospitality [Candea and Da Col 2012], conversion after socialism [Pelkmans 2009], varieties of contemporary nomadism [Oushakine 2012] and the changing social dynamics of ‘post-cosmopolitan’ cities [Humphrey and Skvirskaja 2009]. The field has been strengthened with the help of new institutes and departments that have foregrounded Central Asian ethnography as part of comparative research programmes, just as it has by new book series (such as the Halle Studies in the Anthropology of Eurasia published by Lit-Verlag, the Inner Asia Book series at Brill and the *Central Eurasia in Context* series with Pittsburgh University Press) and the rejuvenation of the leading regional scholarly journal, *Central Asian Survey*, under the editorship of an anthropologically trained scholar of international development, Deniz Kandiyoti.

Within Central Asia, moreover there have been some significant initiatives to institutionalise a comparative, critical, and theoretically informed discipline of social or cultural anthropology over the last decade. In 2001 the American University in Central Asia reformed the existing Department of Kyrgyz Ethnology into a department of Cultural Anthropology and Archaeology, which has since developed a substantially new and impressively broad curriculum, teaching courses on the anthropology of Oceania and the Middle East as well as Central Asia, and theoretically-led courses such as ‘language, culture and power’. In Kazakhstan, a new Anthropology department at Astana’s flagship Nazarbaev University is staffed by a clutch of US-trained anthropologists, including Schoeberlein, whose 1994 dissertation was a pioneer in the sustained ethnographic study of identity in Central Asia. Such programmes are training a new generation of

anthropologists conversant with Anglophone anthropological theory and debates, though their longer range impact on anthropological scholarship in and on the region remains to be seen.

Half full, half empty?

The developments noted above point, perhaps, to a cup half full rather than half empty. But the picture needs to be qualified. One important proviso to the portrait conjured above is that the growth in recent ethnographic scholarship on Central Asia has been unevenly distributed. Turkmenistan, for instance, is still effectively inaccessible to any sustained ethnographic fieldwork that might prove critical of the distinct brand of authoritarian paternalism that characterises that state. Most published scholarship on the region tends to be either historical (e.g. Edgar, whose masterful monograph [2004] draws on archival *fonds* that are now largely inaccessible to western scholars), or focused on macro-level political transformations (e.g. [Peyrouse 2012]).¹ At the other extreme, Kyrgyzstan, with its more open political culture, relatively less intrusive security services, broad network of international organizations and supportive research climate, has been much more accessible, both to foreign anthropologists undertaking long-term ethnographic research, and to Kyrgyzstani scholars undertaking ethnographically-informed doctorates in Russia, Japan, Australia, North America and Western Europe (there is no recognized doctorate in *etnologiya* or *etnografiya* in Kyrgyzstan). Unsurprisingly, therefore, Kyrgyzstan remains disproportionately represented in the anthropological literature on Central Asia, with at least twenty ethnographically-informed doctoral dissertations completed or in progress since 2004, within and beyond the disciplinary domain of anthropology.²

The anthropology of Kazakhstan has witnessed some significant new contributions to scholarship among recent doctoral dissertations, focusing on topics ranging from staged recitation competitions between improvisers of *Aitys* poetry [Dubuisson 2009]; the material seduction of a new capital city in Astana [Bissenova 2012; Laszczkowski 2012]; the delicate political challenges associated with the painful memories of a polluted past in Semipalatinsk [Werner and Purvis-Roberts 2014], the politics of privatization in Almaty

¹ Though see [Blackwell 2001] for an illuminating exception.

² See, indicatively, the completed doctoral dissertations by [Gullette 2005; Borbieva 2007; Kuchumkulova 2007; Montgomery 2007; Nasritdinov 2007; van der Heide 2008; McBrien 2008; Reeves 2008; Beyer 2009; Féaux de la Croix 2010; Satybaldieva 2010; Rakhimov 2011; Schröder 2011; Ismailbekova 2012; Reynolds 2013; Botoeva 2014] and dissertations in progress by Aitieva, Alymbaeva, A. Botoeva, Doolotgeldieva, Feldman, Isabaeva, Pine, Pritchard and Rubinov. This list should not be considered exhaustive. The lack of a single reliable catalogue of ethnographically informed doctoral dissertations on Central Asia means that doctoral projects often only come to one's attention 'after the fact', when they are published.

[Alexander 2004], new modes of patronage for artistic production [Nauruzbaeva 2011], and the ambivalent 'return' of Kazakh Germans and Russians [Diener 2004]. Tajikistan, wracked by civil war between 1992 and 1997, has been comparatively less well researched, but this picture is changing. In recent years ethnographically informed doctoral research has focused on the 'youth bulge' and the impact of civil war [Roche 2014], understandings of modernity and marginality in the high Pamirs [Remtilla 2012; Mostowlansky 2013], experiences of time and subjectivity in Kulob [Ibanez-Tirado 2013], Sufi practitioners' engagement with a sacred past [Gatling 2012], religious education and moral reasoning [Stephan 2010], and local ontologies of environmental change and risk [Ismail-Beben, in progress].

Research on Uzbekistan over the last decade and a half provides a less heartening story. In the brief period of rapprochement between the governments of Central Asia and the US and other western states in the wake of the 'war on terror', western researchers faced fewer restrictions on their movement than they have enjoyed before or since. In Uzbekistan, it was possible in the first decade and a half of independence to develop scholarly collaborations, to organize joint student conferences, and to conduct relatively unencumbered ethnographic fieldwork among farm workers and village elites [Wall 2008; Rasanayagam 2010; Trevisani 2010; Zanca 2011; Finke 2014], in urban *mahallas* (residential neighbourhoods) [Baykal 2007], with rural-urban migrants [Turaeva 2010], with religious practitioners and converts [Hilgers 2011], or with artists, cultural practitioners and elites involved in state construction (see [Trevisani 2010; Adams 2010; Pétric 2002] respectively).

Foreign research institutes, including the French Institute for the Study of Central Asia (IFEAC), which was instrumental in facilitating many scholars' research visits, were able to operate without excessive interference. In April of 2002 I was even invited to give a lecture series in Andijan State University on 'civil society' under the auspices of the Open Society Institute of Uzbekistan. A decade later, such a visit would be almost unthinkable. Many of the scholars who conducted path-breaking research on Uzbekistan in the 1990s and early 2000s are now no longer able to enter the country, or find themselves trailed by the security services if they do. One notable scholar of Uzbekistan has foregone in-country research to focus on the digital organizing of the Uzbek opposition-in-exile [Kendzior 2012]. Uzbekistani scholars who have completed doctoral dissertations at western institutions (e.g. [Turaeva 2010]) may find their expertise unwelcome in their country of origin or choose to publish their research only under pseudonyms. The Open Society Institute was forced to close in 2004, with which one of the major sponsors of support for higher education vanished from the scene. IFEAC, which nurtured an impressive body of historical, archeological, literary and

anthropological scholarship, was summarily closed down in 2010 and its archive confiscated.

There are other challenges to the articulation of a distinct field of regional scholarship. The lack of ‘classic texts’ on Central Asia that will be familiar to colleagues working on Papua New Guinea or Sub-Saharan Africa means that anthropologists often have the experience of being rather marginal to wider debates. In her 2007 monograph on Uzbek religious experience in Bukhara, based on research in the early 2000s, Maria Louw notes that while Central Asia is fortunate not to be plagued by the kind of ‘gate-keeping concepts’ that have become associated with other world regions (‘the gift’ in Melanesia; ‘the tribe’ in Africa), it can be difficult to avoid the feeling of having to ‘reinvent the wheel’ with each scholarly monograph. The relative dearth of anthropological analysis and theoretical arguments based on empirical material from Central Asia, Louw argues, can leave scholars feeling ‘in an anthropological no man’s land, condemned to a kind of analytical bricolage, and wavering between the fear of making points that are banal to other anthropologists on the one hand, and the temptation to draw sweeping conclusions on the basis of limited material, on the other’ [2007: 18].

Perhaps the greatest challenge to the institutional development of a field of Central Asian anthropology outside Central Asia concerns the broader nature of the employment market in social and cultural anthropology. In North America, where prospective appointments tend to identify a desired regional specialisation, ‘Eurasia’, (still less ‘Central Asia’) rarely figures in job descriptions. This has generated a situation where many excellent anthropologists of Central Asia find themselves moving between one postdoc or adjunct position and another, with little job security and few opportunities for developing a research career. One scholar of Central Asia with a PhD in Cultural Anthropology, commented starkly in a recent interview that Central Asian studies was a ‘dying field’. Many of the experts on the region are now unemployed, the author noted, ‘or doing work that has nothing to do with Central Asia’ (Kendzior quoted in [Tucker 2014]). The irony, in this reading of the situation, is that the very generation who were first able to conduct sustained, long-term local language ethnographic fieldwork are not able to find permanent employment that draws upon their specialist knowledge.

Kendzior’s is a particularly pessimistic — and it should be noted, a particularly US-centric — view of the current state of the field. It perhaps attaches excessive importance to US government funding for area studies training, and ignores the degree to which many US-trained anthropologists of Central Asia have found niches to conduct research on the region beyond the academy. It does remind, however, of the way in which the anthropological study of Central

Asia is constrained as much by the realities of Congressional deadlock in North America as it is by the vagaries of visa regimes and presidential politics in Central Asian capitals. Whether the cup is half full or half empty, then, remains open to debate. Certainly the landscape of anthropological publications on the region is richer and more diverse in 2014 than it was in 2004. What remains less clear are the long-term research prospects in a region where scholarly access is more challenging than a decade ago, and which research funders often see as marginal to their strategic interests.

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SERGEY SOKOLOVSKIY

1

Every diagnosis assumes the existence of a person to make the diagnosis (his experience, and frequently also his special instruments and methods), a basis for comparison (a set of prototypes or precedents, against which the present state of the object to be diagnosed may be judged), and the object itself. To set this apparatus in motion some confidence in the person making the diagnosis and his methods is essential, as is the presence (or at least belief in the existence) of the object before his eyes (or at least his mind's eye), and if there is no question of the object's being accessible to immediate perception, then it must at least be reachable by the instruments and methods chosen for the purpose. Equally necessary is the confidence in the relevance of the precedent or prototype taken as a point of comparison to the object in question (in other words, a confidence or belief in the existence of the object at the time when the prototype is determined and at the time when the diagnosis takes place). In such a way,

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in order to answer a question which is only in appearance such a simple one, we must not only try on the role of the person making the diagnosis, but also have available some set of methods and instruments and at least believe in the existence of the object to be diagnosed during the period of time indicated in the question. Hence a number of problems arise. An academic discipline is not an easy object to perceive. In a certain sense it resembles other imagined communities, and in order to compare its present condition with its former condition we must retain our faith in its existence or be in possession of irrefragable arguments in favour of its existence during the period in question. What do we in fact possess when we consider that to which the authors of the question have given the wise and streamlined designation 'our discipline'?

Anyone with at least a passing acquaintance with the history of that mass of nowadays highly amorphous fields of teaching and research, which confronts us like the Lernaean Hydra now with three heads, now with five, each with its own name (ethnography, ethnology, social anthropology, cultural anthropology, socio-cultural anthropology) is free to doubt that it is a matter of one and the same discipline. 'Enough!' the sceptic will cry, after comparing the ethnography courses of, say, the Ethnography Department in the History Faculty of St Petersburg University with the anthropology courses of the European University (or, for example, the social anthropology courses at the Russian State University of the Humanities with the ethnology courses at Moscow State University — not to mention the altogether exotic treatment of the subject in the so-called supernumerary faculties, where staff and students alike experience severe difficulties in identifying themselves with a discipline or determining the scope of the discipline which gives its name to their department), 'can all these heads really belong to the same creature?'

Attempts to unify diverse research practices under a single disciplinary heading have fared no better: the enthusiasms of ethno-sociologists and 'ethno-politologists' have no point of contact with the problems and concerns of specialists in 'gender studies', who also regard themselves as part of the anthropological community, and researchers into tolerance and conflict do not read works on museum studies, religious studies or folklore studies; specialists on Siberia have no great interest in the works of specialists on the Caucasus, and Americanists and Africanists know little about each other's work. However, all these people, often divided not only by their theoretical outlook and research interests, but also by the circumstances that led to their becoming researchers (sometimes the same topic is being studied by geographers, historians, linguists and representatives of a dozen other disciplines whom fate has brought together in the same research centre or university department or research project) are united institutionally by their workplaces, journals, funding sources

or publishers, attend the same conferences and consider themselves as belonging to the same profession, but continue to ignore each other's work, with the exception of a handful of like-minded people. These hermetically sealed worlds are hard to measure with a single ruler, and their successes and failures, breakthroughs and blind alleys, active growth or stagnation pass unnoticed from the outside. It is only the broad trends that gather many admirers of different stamps under their banner (structuralism, hermeneutics, post-structuralism, pragmatism, etc.), that attract the attention of historians of science by their noise victories and defeats. At the same time, for example, the revolution in the study of material culture taking place in a 'parallel world' (to be precise, in the community engaged in the anthropology of science and technology, better known as the STS) passes unnoticed even amongst people who are studying material culture.¹ Nevertheless, in all these hermetically sealed communities, concentrating solely on their own interests, unique abilities are being honed and knowledge obtained that cannot be encountered anywhere else.

In a book published a quarter of a century ago [Oakeshott 1989], expressing his opposition to a university reform directed towards utilitarian and 'relevant' teaching, Michael Oakeshott gave a description of disciplinary specialisation which I find better than many: 'Each *techne* is, or involves, a particular manner of thinking, and the notion that you can think but without thinking in a particular manner, without reference to some definite universe of discourse, is a philosophical illusion. Every true '*techne*' profoundly studied, knows something of its own limits, because it has some insight into its own presuppositions,' and those who try to establish a single template for education are 'unreliable guides whose immoderate thirst has conjured up a mirage' [Oakeshott 1989: 134]. David Mills, a British anthropologist who studies the anthropology of education, commenting on these thoughts of the distinguished philosopher, writes that according to this logic disciplines provide teaching, and a high degree of devotion to the disciplinary identity represents a more profound assimilation of knowledge: 'Disciplines are their own pedagogy, their own rationale' [Mills 2008: 175].

In this case the difficulty for the person making the diagnosis (or, to put it more conventionally, the historian of science, who evaluates its

¹ I doubt whether Russian ethnographers specialising in costume and clothing have paid any attention, for example, to the 'Manifesto for the Study of Denim' [Miller, Woodward 2007] published in a leading British journal of social anthropology. The reason is simple: jeans are not part of so-called 'traditional culture', and therefore the professional gaze in our country, focussed on tradition, simply does not notice them, as a result of which the whole complex web of relationships between the global and the local ceases to be relevant to our costume specialists, as are the problems of authenticity and hybridity, and their works are founded on a thoroughly outmoded methodology of cross-cultural comparison, and have long ceased to promise any interesting discoveries.

evolution and to this end compares at least two periods, two states separated by time) lies precisely in the establishment of the boundaries of such disciplines embodying particular *technai*, in the attempt to discern real cognitive communities beyond the conglomerates with official designations created by institutions or by history. What does he see: a single object or many? And if they are many, is there any rational basis for uniting all these elements, establishing rules and drawing lines that will separate such and such a set of elements from others which are not subject to these rules and regularities? Or does it all boil down to institutional inertia, quirks of fate and the imposition of an administrative classification which has resulted in a 'table of ranks' according to which for defending a dissertation on contemporary phenomena one may receive a degree in history?

As for the changes that are taking place, there is every reason to assert (though this thesis, of course, requires proof which would need a much more extensive exposition than is possible within the format of a reply to a question in such a forum as this) that the fragmentation of the discipline, which was no longer unitary even in the last decades of the Soviet Union, has now become so profound that in reality we are dealing with a collection of weakly interconnected specialities and communities, some of which are now outside what might be considered socio-cultural anthropology or ethnology, others (the remains of the alliances with related disciplines which never formed organic elements of that (quasi-)unity that was called ethnography or ethnology: ethnopsychology, ethnoarchaeology, ethnodemography and ethnocartography) have practically ceased to exist, and yet others, which now form the nucleus, or rather two practically independent nuclei, are now ready, as in a nuclear reaction, to separate into two independent disciplines, neither of which, however, has a name canonised by tradition, and continue to present Russian anthropology as a separate and relatively independent subject. They are very likely, however, to undergo a painful metamorphosis in the near future, because their heuristic potential (if not their *raison d'être*) and, consequently, their intellectual attractiveness is almost exhausted. These fields are applied political anthropology (in its Russian variant) on the one hand and ethnographic folklore studies on the other. It is understandable that there is hardly any contact between them at the level of theory, or subject matter, or *socium* (insofar as they are carried on by different communities), so that it is hardly possible to speak of common trends of change within the discipline. There are nevertheless attempts to construct a socio-cultural anthropology on other foundations, which are, however, being made by relatively small groups of researchers who have not yet received much recognition or support from the community as a whole. On some of the reasons for this state of things, see below.

2

I have had occasion to write elsewhere about the catastrophic ageing process of the humanities in their academic context and the almost complete lack of a middle generation [Sokolovskiy 2011]. As the older generation gets older, not only do its members find it harder to go out into the field, but they are ever less *au fait* with what is going on outside their 'own' long-established set of problems, particularly with regard to new directions and conceptions. As a result conceptually obsolete approaches and topics are being forced on graduate students, and genuinely innovative subjects are not recognised as 'ethnographic' (or 'anthropological') and are supported only exceptionally or fortuitously. In this way the subjects for dissertations in the leading departments and institutes consist of endless variations on well known themes, and innovative research is marginalised, and its authors find themselves seeking work outside academia or their own discipline. This is why, for example, the anthropology of organisations (as also the anthropology of the media, modern art or sport) is being developed mostly by sociologists or by anthropologists working in sociological centres. One could say the same of STS and of research in general into the modern world not connected with ethnicity or folklore. For this reason most projects in applied anthropology are either assessments of proposed legislation and ministerial projects and programmes, or associated with topics which clearly respond to a political rather than a social requirement (tolerance and conflict, migration, territorial and party branding, youth groups etc.). The discipline is being modernised in homoeopathic doses and as a rule as a result of external stimuli (the department acquires a member who has spent time abroad and invites his younger colleagues to participate in joint projects). In short, we have been successful in ensuring the continuance of the discipline, perhaps excessively so, but as far as developing it is concerned, let alone incorporating new directions relevant to the times, the situation is far from rosy.

3

The stagnation resulting from the lack of a normal succession of generations has resulted in its provincialisation within the global knowledge economy and the degeneration of those applied aspects most in demand in society into servile expert opinions in the public sector. Judging by publications describing the situation in certain related disciplines in the humanities and social sciences, similar processes have also been taking place in them. Among the reasons why the humanities and social sciences in Russia have fallen behind and become provincialised we may name the following:

- swingeing funding cuts to scholarly/scientific disciplines in the 1990s and a reduction in personnel;
- the lack of any infrastructure for horizontal mobility of scholars within the country and a concentration of research centres and large universities in the capital a few major cities [Elfimov 2008; Sokolov 2008];

- the fall in the quality of the education of students and the level of the requirements demanded of dissertations, the gap between the contents of research and of teaching courses, the crisis in the system of academic certification and the rise of conflicting standards of scholarship [Bikbov 2009; Gudkov 2009; Sokolov 2010];
- the commercialisation of university education on the ‘grey’ and ‘black’ markets [Bikbov 2009];
- the lack of a middle generation, the joint effect of the flight of young people from the country, the reduction in the number of positions in the academy, the reduction in the real value of academic salaries and the fall in the quality of university education, which forms a barrier to an academic career for many young people from the provinces [Sokolovskiy 2011];
- a sharp fall in the funding of academic libraries (a quarter of a century ago they stopped receiving a range of leading journals essential to researchers, not to mention books from the leading publishers, and there is still no improvement in the situation) and a shortage of contemporary academic literature, which hinder Russian researchers from being normally integrated into the global exchange of knowledge and form a barrier to effective participation in the world-wide division of labour; remaining poorly informed about the details of the current policies of major learned publishers and journals abroad, our scholars have less chance of making their way onto their pages [Sokolovskiy 2009];
- the absence, even in central academic institutes (for lack of the appropriate funding) of any access to full-text electronic libraries of journal articles, which, given the ever-increasing prices of our own learned journals means in practice a new self-isolation of the humanities in Russia, a widening of the gap in the languages of research and analysis between Russian researchers and the rest of the scholarly world, and a further reduction in the readership of Russian learned journals;
- a lack of legal regulation of the finances for fieldwork and the purchase of equipment and software;
- insufficient financial support for ethnographic fieldwork and archival research, and excessive bureaucratisation of the reporting mechanism attendant upon them;
- the failure of the main body that finances research in the humanities (RGNF) to work in accordance with international standards (lack of anonymity in considering applications, lobbying, participation of members of the evaluation committees in the projects under consideration; no open publication of the

reports of grant recipients on their results; a lack of transparency in how the members of evaluation committees are recruited and rotated).

It seems to me that the very description of the problems indicates how they are to be solved: a reform of university education; another level of funding for scholarship and the infrastructure of access to scholarly information; the creation of a supplementary pension fund (possibly by making deductions from grants) to ensure a decent pension for retired scholars, which would allow a certain freedom in personnel decisions and permit a significant rejuvenation of academic staff; a liberalisation of the grant reporting system; a reform in the activity of funding bodies to make them more conformable to international standards (double blind review, thorough investigation of incidents of lobbying and infringement of anonymity, with sanctions against those guilty of them such as exclusion from competition for a specified period); dedicated work to create new academic posts and stimulate the demand for anthropological knowledge (including development of appropriate programmes); diversification of the sources of research funding; decentralisation of scholarship by the creation of a network of more powerful regional research centres with adequate funding; the creation of a new generation of textbooks, approved by specialists and not by officials from the Ministry of Education, who have already published a series of dunderheaded compilations in huge print-runs; propaganda of anthropological knowledge and work directed towards creating a positive image of the discipline, etc.

I feel that it is still too early to talk about the achievements of Russian socio-cultural anthropology, but one could enumerate some successes and positive tendencies which may have been observed over the last few years or have always been typical of our discipline. The strong points of ethnographical research remain its historicism and continuing tradition of interdisciplinary synthesis and the juxtaposition in the course of historical reconstructions of properly ethnographical data with information from linguistics, archaeology, history, geography and demography. Over the last decades such directions as juridical and political anthropology have been brought back to life, and new ones are coming into being — medical anthropology, the anthropology of organisations (see, for example, [Romanov 1999], the anthropology of science, and so on.

There are now several universities, academic institutes (or separate faculties, centres, departments and laboratories) where a significant number of the problems outlined above are being successfully solved. Among these are the European University (St Petersburg), the Smolny Institute of Liberal Arts and Sciences, attached to the Faculty of Literary Studies of St Petersburg University, the State

University-Higher School of Economics (the ‘Vyshka’),¹ the Moscow Higher School of Social and Economic Sciences (the ‘Shaninka’)² and certain faculties and centres of the Russian State Humanities University. Their experience should be generalised and extended over the whole system of academic and university learning in the field of research into the social sciences and humanities. Finally, there has been a significant increase in the number of journals that regularly publish work in the field of ethnology and socio-cultural anthropology, and of departments with corresponding interests (on which see [Romanov, Yarskaya-Smirnova 2011]), and programmes leading to undergraduate and master’s degrees in ‘Ethnology and Anthropology’ have been validated. These changes allow us to hope that despite the provincialisation noted above, and the slow pace at which new questions are being assimilated, the community in the discipline is still capable of further development.

4

Continuing to answer the question of whether anthropology in our country consists of one disciplinary community or many, I have reasons to claim that present-day Russian anthropology is determined by the activities of a dozen or so leaders and two large communities, which have solidified around two different sets of questions, one of them centred on the problems of power and the administration of a multicultural society, and the other on traditional culture and its modern transformations. Since I intend to develop this thesis in a separate article for *AF* and offer my argumentation therein, I shall refrain from discussing it at length in these short answers to the questions asked by the editors.

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¹ From its Russian title, Vysshaya shkola ekonomiki. [Eds.].

² From the name of its founder, Theodore Shanin. [Eds.].

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1. From dependence and the 'human dimension' to mattering in our own right

Arctic Anthropology has experienced a tremendous transformation in both the significance of its enquiries and its relations to other disciplines and society at large. This transformation coincided time-wise with a major reorientation and then consolidation of the academic track on which I myself move today. From the point of view of somebody who has worked in the Russian North out of a base in the West, there were three phases through which this transformation of Arctic Anthropology took place:

1) A phase of idealism and fascination with the newly opened-up field, where we saw the entire circumpolar Arctic beginning to fuse into one major field for comparative anthropological

enquiry. On the other hand, this fascination seemed to be meaningful mainly for us anthropologists, while the rest of the world still thought what we were doing was more *'l'art pour l'art'* without much meaning for anybody other than ourselves.

2) A period where those with decision-making power in academia and funding agencies seemed to have noticed that Arctic anthropologists might offer a lot in terms of providing answers to questions that had surfaced by now for general public debate: how does the Arctic function as an early warning system for the effects of global warming, natural science models of which have gradually become a subject of public debate? What can we learn from Arctic experience for the rest of the world? How can indigenous knowledge be used for understanding the processes of climate change?

3) The most recent period is still at its very beginning, I think. But there are signs that we are emancipating ourselves in Arctic Anthropology from a natural science-driven agenda to one where we can ask our own research questions and even invite colleagues from other disciplines, including natural sciences, to answer questions that are at the core of anthropology, namely the principles of human cultural and social diversity, evolution, and relation to the entire environment.

Let me elaborate in the following on these three periods through which I see Arctic Anthropology as transforming itself:

The years around the turn of the millennium saw the publication of a number of thorough ethnographic monographs written in a western anthropological tradition about particular peoples and field sites in the Russian Arctic all the way from Chukotka (e.g., [Kerttula 2000; Gray 2005]), Sakha / Yakutia (e.g., [Ventsel 2005; Vitebsky 2005; Willerslev 2007]), Taimyr (e.g., [Anderson 2000; Ziker 2002]), Yamal [Golovne & Osherenko 1999; Stammner 2005], Komi [Habeck 2005] to Murmansk Oblast (e.g., [Took 2004; Konstantinov 2005; Vladimirova 2006]). These works not only introduced the English language anthropological readership to specific Russian Arctic fieldsites and linked them to western anthropological debates, but also connected to the existing ethnographies in the Russian language that colleagues in Russia had been producing with reference to all these regions during the Soviet period.¹

By the year 2005 it seemed to me that now the time had come to move one step further and start comparative research that would be based on those thorough ethnographies, and link Russian Arctic

¹ By mentioning here English-language literature, I acknowledge that this contribution covers only a highly selected part of the publication record. By no means do I want to diminish the numerous works published by colleagues in Russian. I would hope somebody with better command of that literature can engage in a review-article of some sort in this journal.

cases topically, after which then we could reach out and establish further focal topics with ethnographic material that would serve as an anthropological 'brand' for the North to which the rest of anthropology could refer to. Just as anthropologists know that the Amazon stands for the development of the discipline of ethnobotany, the Arctic could become known for the anthropology of human-environment relations, among which also human-animal relations would figure prominently, as stated by Tim Ingold in a keynote speech at the 2004 congress of the International Association of Arctic Social Sciences [Ingold 2005]. These were fascinating prospects which some of us enthusiastically pursued, for example through edited volumes on reindeer livelihoods in the circumpolar North [Stammler, Beach 2006] and on the social significance of animals in the Arctic with comparative perspectives on Central Asia and some African examples [Stammler, Takakura 2010], or an examination of the notion of fieldwork partnerships in Siberia and India [Vitebsky 2012].

However, these efforts were dwarfed and almost made invisible in comparison to the magnitude of the transformation in the second period mentioned above: the publication of the encyclopedia of the Arctic (2005) and the Arctic Climate Impact Assessment (2005) were a first culmination point of multidisciplinary Arctic research publications that had political relevance where anthropological research was included in the study of the natural environment and climate change. Arctic anthropology now entered fully the global international research community, with millions of dollars and euros of funding for the study of climate change. Academic decision makers had realised that there was a 'human dimension' to all the natural phenomena that they study, and that this should somehow be considered and included in interdisciplinary research, which they postulated as the cutting edge.

Ten to five years ago it was becoming increasingly difficult to get any research idea funded without relating it — seriously or for the sake of plugging in to funding — to the big question of 'impact of climate change and responses to challenges'. As a then young post doc this development seemed to me a serious restriction in my own academic freedom, as if some 'big brother' were watching and saying to people 'unless your research addresses climate change, it is neither relevant nor worth funding'. In the mid 2000s the problem created by this development was not only the strong channeling of research efforts to this one topic, but also that the social and human sciences seemed to be at best half-wanted minor additions to the big natural science research agenda in the Arctic. Correspondingly, our colleagues became 'token social scientists' getting the breadcrumbs of research budgets that fall off the table when big funding-cakes got divided up among big actors in Arctic Science.

It is in this unequal partnership setting that the topic of indigenous traditional knowledge (TK) became pressingly relevant beyond the purely academic sphere. In principle, many of the Russian Arctic ethnographies mentioned above are strongly concerned with such ways of knowing, but TK was not flagged in the climate change science jargon. When big projects and politically relevant bodies such as the United Nations and the Arctic Council made traditional knowledge presentable to policy and law makers (e.g. article 8j, Convention on Biological Diversity [CAFF 2004]), this knowledge became codified according to a western decision making system, and it became, together with the anthropological research itself, cut off from the society it studied, a mere add-on to big natural science programmes (see [Forbes, Stammler 2009] for a critique).

However, gradually an increasing number of colleagues in Arctic anthropology started perceiving this situation as unsatisfactory, seeing themselves increasingly as handmaidens of big science agendas. I myself always felt in this respect great solidarity with Russian colleagues and partners who were in general much less susceptible to the big western media-machine surrounding climate change science. During many meetings after 2005 I found Russian colleagues — even those funded by climate change projects from the West — sharing their critical thoughts and views, though only in the coffee breaks, and when speaking in the Russian language, so their western colleagues would not understand.

Four key events to my view contributed greatly to the start of overcoming that ‘handmaiden’ position in our discipline. Almost at the same time, two landmark decisions strengthened our position in relation to colleagues from other disciplines and the funding agencies, and contributed to emancipating ourselves out of the shadow of the big science and policy decision makers. First was the strong human and social sciences component in the international polar year (IPY 2007/08 [Hovelsrud, Krupnik 2006]). Second, and even more important, was the emphasis on completely human-centric topics in a key major international funding effort (ESF BOREAS, 2006–2010 [Ziker, Stammler 2011]). These developments were crucial in paving that way for an important change in our perceived relevance. I remember well how it was at the very first BOREAS planning meeting in September 2004 at the Scott Polar Research Institute in Cambridge that Peter Schweitzer passed on greetings from Igor Krupnik, who had just succeeded in convincing the planning committee for IPY to include the human and social sciences research agendas in the Arctic as a separate big topic. In both the BOREAS and the IPY projects, our capacity to engage in research of a fundamental nature in partnership with our friends in the field was now recognised, and we were no longer assistants or add-ons to big natural science projects or research commissioned by political bodies.

This led in turn to a third development: a number of us spearheading what we called ‘Our movement to retire the term “human dimension” from the Arctic Science vocabulary’ [Stammler with colleagues 2010]. There we argued that the time had come to see all the changes happening in the environment as different aspects of an overall human research agenda, as ‘*natural* dimensions’, so to speak, and that we should acknowledge that actually all science that is done in the Arctic is for the sake of survival of the *human* species and its societies and cultures on our planet. So, rather than seeing humanities and social sciences as ‘dimensions’ of a natural science agenda, we suggested seeing those agendas as sub-aspects of advancing knowledge on how humans can live better on our planet.

A fourth important factor contributing to our emancipation was also that outside of academia the interest in the Arctic has become more general and less climate-change-obsessed. This is partially due to the hot debates on Arctic mineral and hydrocarbon resources. I remember how back in 2004 the EU framework programme funding agency justified the refusal of a funding application that I had submitted jointly with Piers Vitebsky on social and cultural impacts of Arctic hydrocarbon extraction in Russia by stating the topic had not enough relevance for the European Union! Nowadays the EU has applied for observer status in the Arctic Council and highlights issues of corporate social responsibility in the race for resources in the Arctic.

From these transformative processes follows that now in 2014 Arctic Anthropology has partially emancipated itself from being the handmaiden of natural science-driven climate change research and politics. The significance of the anthropological study of Arctic societies and cultures using participant observation has increased alongside the political significance of the region as a whole. It seems that now Arctic anthropologists can increasingly afford to follow their own research agendas and still be meaningful as well as visible in broader societal debates. We are ‘allowed’ to contribute to the heart of the anthropological project, namely by improving our general understanding of how human societies work, how they act and react to changes and continuity, and how they are similar and different from each other. If needed, we have even moved on to buying in natural science expertise for our projects if that helps us answering fundamental research questions. An example is a current anthropology-led interdisciplinary ERC project on the principles of domestication and human-animal relations (<<http://www.arcticdomus.org/>>).

This emancipation has led us to have a greater say than before in the ways the environment is seen as a research topic. It is too early to lie back and be proud of our achievements, and there are still plenty of

instances in our field where Arctic anthropologists still act as wallflowers in bigger agendas and projects. But definitely now we are in a much better position to make our research known as a strong brand for the advancement of general knowledge on the nature of human-environment relations, one of anthropology's most fascinating core topics.

2. From 'reindeerology' to industry impact assessment and subcultures studies: topical broadening and personal integration of the field

The first generation of western-style anthropologists that entered the Russian North during and after perestroika was a rather small group of scholars, most of whom knew each other, and built close research partnerships with colleagues in Russia (see [Gray, Vakhtin, Schweitzer 2004]). Many of these scholars focused topically either on the study of reindeer livelihoods or of the postsocialist transformation of indigenous societies. Borrowing a term used by Piers Vitebsky, I would call this the 'reindeerology' generation of Russian Arctic anthropologists. This orientation was topical and an important contribution to both circumpolar Arctic Anthropology and the anthropology of (nomadic or post-nomadic) pastoralism. Since reindeer herding is not significant in the North American or Greenlandic Arctic, only Fennoscandian studies were accessible for a western readership to contribute to general theory making, until that first generation of scholars published their studies in the early 2000s (for the references see above). The change that has happened in the last decade is that topics covered by that generation of researchers have widened, even under the umbrella of the bankrolling of climate change research that I have mentioned earlier. As those post-socialism and 'reindeerologist' Arctic Anthropologists moved on with their careers and their first generation of PhD students entered the field, Arctic Anthropology moved beyond Arctic indigenous studies. Topics now embrace the entire Arctic population including the urban non-indigenous majority [Thompson 2008; Stammler, Eilmsteiner-Saxinger 2010; Bolotova 2012], sub-cultures such as urban ethno-music scenes [Ventsel 2008], urban representations of culture [Donahoe, Habeck 2011] and new religious movements [Vallikivi 2009], to name just a few examples.

Furthermore, our field has greatly benefited from Russian-Western integration, mostly through the agency of Russian scholars trained at universities abroad or in Russia according to western academic practice, who have engaged in co-authored publications that widen the topical and comparative scope of our discipline (e.g. [Krupnik, Vakhtin 1997; Golovnev, Osherenko 1999; Cruikshank, Argounova 2000; Fondahl, Sirina 2006; Bolotova, Stammler 2010; Volzhanina,

Anderson 2010; Safonova, Sántha 2011; Liarskaya, Dudeck 2012; Brightman, Grotti, Ulturgasheva 2012; Schweitzer, Golovko, Vakhtin 2013]. It is thanks to those collaborations that the anthropology of the North has become more balanced in terms both of topics and of the scholars involved. This shows that, as of the mid-2000s, there is more to the Anthropology of the Russian North than climate change studies, industry impact assessments, and reindeer livelihoods. While those topics are still crucially relevant and ethnographies continue to be written about them, the last ten years have led to a level of integration that seems to me beneficial for the academic community as well as for the discipline.

3. Becoming significant: the relations between Arctic anthropology and society

Bearing in mind these changes, there is no doubt that the discipline of Arctic Anthropology has gained a lot more public attention and significance throughout the last decade. This is first and foremost due to the increase in political significance of the Arctic, first through public debates on climate change, and then through those on industrial development. But one should also acknowledge that scholars have not shied away from exposing their research in public or political circles. On the contrary, they have been active in producing publications specifically on request of, or targeted for, public and political institutions such as the Arctic Council and the Nordic Council of Ministers. Studies such as *Sustainable Reindeer Husbandry* (2002), the *Arctic Human Development Report* (AHDR 2004), and *Arctic Social Indicators* (2010) are today referred to by influential decision makers and undergraduate students alike.

Other scholars have chosen to reach out to broader society by partnering with film or TV companies working in different formats. While scholarly publications will hardly get more than some thousand readers, films like *Arctic Aviators* (BBC, advisor Vitebsky), *Code Breaker* (CBC, director Thompson), *Arctic* with Bruce Parry (BBC, advisor Stammler) reach a multi-million public by making a difficult compromise between the popular longing for action and clear 'stories' and the academic need for thorough research and subtle messages.

Increasingly, Arctic anthropologists also engage in dialogue with industry, particularly the extractive industries, out of a conviction that sub-surface resources on their field sites will be extracted whether they are involved or not. While some have opted to stay away from such conversations and criticise the devastating social impacts of industry-advances in the Arctic, others have actively engaged with corporate and political actors to improve practices on the ground as well as contributing to guidelines (e.g. [Ilebs declaration 2009;

Knizhnikov, Wilson 2010; Martynova, Novikova 2011)). Furthermore, there is much more anthropological input in policy-making documents, corporate guidelines, NGO strategies, and decision making processes that stays invisible either because documents have to remain unpublished or because input is provided in the form of consultations and meetings.

Another way of showing our relation to society is through engaging our research partners and collaborators in the field as active co-authors in publications. This is a direction that is much more common in Arctic anthropology publications coming from North America (e.g. [Krupnik, Jolly 2004]), where there is also a specific procedure of obtaining a community licence to conduct research. This tendency has not (yet) fully arrived on the Eurasian continent, and even less in the field in the Russian North. It seems to me that this is not because our research involves less collaboration or acknowledgement of the role of our research partners. Rather it shows that the focus of the collaboration is not necessarily English-language academic publications. Why would a reindeer herder or a gas worker be interested in co-authoring scholarly publications in a language that he/she would not understand nor be able meaningfully to show to other practitioners? However, as a new generation of activists also become academic scholars and is additionally fluent in English, the next years will see much more co-authorship between Arctic anthropologists and indigenous research partners.

While it seems sometimes that in the current political environment all Arctic Anthropology can be justified only when it is relevant for the broader society, be it politics, indigenous communities, or sustainable development, the studies of the last ten years mentioned above also show that fundamental research continues to be published even on topics where the only orientation is the contribution to the advancement of academic knowledge (e.g. [Willerslev 2011]), an orientation that will certainly survive any political, economic, social and community agendas.

4. Marrying social significance, community collaboration and scholarly interest in the advancement of academic knowledge

As scholars engaged in a lot of anthropological fieldwork, we know that none of our research would be possible without the immense hospitality of our partners on the ground and often also host-scholars. Sooner or later several of us start asking themselves the difficult question: what have we left behind to our afterworld in the field and in the academic community? In my own case these thoughts were stimulated additionally by conversations in the field I know best, the Yamal Peninsula in Northwest Siberia. In those conversations with reindeer herders and fishermen I heard suggestions, even direct

questions along two lines: ‘Could you tell the world about our life and how industrial development is crowding us out?’ (Sergei and Aleksandr Serotetto), and ‘Why don’t you record some more of those stories of our elders, we didn’t even know about them, and there are not many people who know them any more?’ (Valeria and Khasavomboi Okotetto, Yamal). These two direct suggestions translated for me as a scholar into a stimulus to do something meaningful along those lines, but also use such efforts to carry out fundamental research that would bear relevance beyond my immediate field site.

This got facilitated also by advances in academic standing that allow me to coordinate projects that are no longer restricted to a single field site. A first project spanned from Murmansk region to Yamal and was part of a circumpolar IPY project under the ESF BOREAS programme I mentioned earlier. A second current project spans from the North of Finland and Norway along the Arctic shore to East Siberia.¹

The industry-topic lent itself easily also to the idea of bridging the academic gap in Arctic anthropology between indigenous studies and urban industrial studies. It turns out that the borders are much less clear and black-and-white than they are sometimes portrayed in the media. In my experience, most practitioners in my field sites are not against industrial development in general, but would like to improve the ways in which that development happens, optimise their benefit from it, and have fruitful relations with those people engaged in industry, with whom they now share their land in the North. This orientation has led me to a focus on the encounter of incomer-industrialists with indigenous peoples, and more theoretically on developing a general approach that can be applied for studying social-cultural impacts of industrial advancement (see [Stammler 2011]). After all, it is time for us to realise that if we want to contribute to general anthropological theory, our arguments should be valid for both indigenous and non-indigenous people. This move against ethnic reification and towards generally applicable insights leads me to suggest the corporeality of the Arctic as an important component that unites the human-environment relations of groups as different as reindeer herders and industrial migrants alike [Bolotova, Stammler 2010; Stammler 2011b].

The topic of the stories with elders had to mature for more than half a decade, for funding reasons as well as my reservations in terms of theory: I had always thought that, inspired from North American literature, we tend to take the idea of elders too seriously, uncritically regarding them as the automatically authorised and institutionalised

¹ Both projects funded by the academy of Finland, BOREAS MOVE INNOCOM, decision number 118702, and ORHELIA oral history, decision number 251111.

group that speaks for entire ‘communities’. So suppose that in the Russian Arctic many are happy that there is no such institutionalised template of elders, and suppose our partners in the field do not always want us to focus on elders only? The way out was to focus on looking at the transmission of oral historical and cultural heritage comparatively. In my current research project, ‘Orhelia’, we therefore take groups of people sharing a similar environment (the Arctic coastal areas) and historical background (experience of an authoritarian empire in the twentieth century) to compare how knowledge travels down the generations in different settings. For this endeavour, we also integrate oral heritage research methods with those of social anthropology. This is especially important as we realise that in Arctic societies where non-verbal forms of communication are at least as important as words, we cannot assume that verbal life history interviews would give us a full picture of somebody’s role in the huge social transformation process of the twentieth century. We come much closer to that goal by, wherever possible, marrying interviews with long-term classical participant observation.

This latest focus and encouragement from the field has led me also to dream about less traditional and standardised processing of fieldwork insights. During 2012 and 2013 fieldwork for example we dreamt with Nenets friends about jointly producing an action-movie with local actors based on the Nenets *Yarabtsy*, a special form of Nenets song where oral mythical heritage of earlier generations and the distant spiritual past is performed by two people from different generations. This way of performing oral heritage is fading out with technological change and the appearance of DVD players and mobile phones in the remotest Arctic areas. We dreamt that the content could be preserved and kept alive if it could be performed in a contemporary way that incorporates some of the new technologies that the youth is nowadays so keen on. Along similar lines, the cultural heritage that we record in our oral history research could also be brought back to the people through streaming audiovisual content in an accessible format back to people’s personal communication devices (laptops, smartphones, tablets, ipads etc).

All these directions in the last decade contribute to my impression that Arctic anthropology is more visibly than before engaging in the advancement of fundamental theoretical approaches to the study of human societies, while also celebrating the wealth of social and cultural diversity on our planet.

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FROM THE EDITORS: TEN YEARS ON

Ten years ago the first issue of our journal opened with the inaugural Forum, 'Cultural Anthropology: The State of the Field'. This time, we have decided to discuss what has changed over that period in our discipline.

Allow us to remind you briefly of what was discussed ten years ago (*Forum for Anthropology and Culture*. 2004. No. 1). There was a question related to the shift in researchers' attention to subjects which had previously been considered marginal, and the consequent abrupt increase in the number of objects researched; of the shift of focus from the countryside to the town and from the past to the present. Suggested reasons for these changes were the crisis of 'grand theories', the transition from interpretative to descriptive strategies, the fragmentation of the field of research, and the change in attitude to former key concepts such as 'culture', 'people', etc. It was, in fact, a matter of radical changes in the landmarks of research.

From the perspective of the present day, many participants recall the beginning of this century as a time of hope and expectation, when the choice of topics and subjects was potentially

infinite (Konstantin Bogdanov). It seemed ‘that an anthropologist might, in principle, record everything’ (Levon Abrahamian). The euphoria which had particularly infected the Russian-speaking participants in the discussion, for obvious reasons (if not a new world, then so many new possibilities lay open before them) has given way to a more sober assessment of events. This was well put by Catriona Kelly: ‘the trailblazers have lost some of their enthusiasm and become more thoughtful’.

1

Judging by the answers, anthropology has continued to broaden the scope of its topics, to the point of disintegration. Moreover, in the opinion of Sergey Sokolovskiy, ‘the fragmentation of the discipline, which was not unitary even in the last decades of the Soviet Union, has now reached a point where in reality we are dealing with a collection of weakly interconnected specialities and communities’. It should be said that such a tendency is visible in the replies: the majority of participants prefer to comment on trends within their own field, without attempting wider generalisations.

It is quite possible that the blurring of the boundaries of the discipline is becoming more obvious for the reason mentioned by Alexander Panchenko: ‘The development of electronic networks and means of saving and searching for information has significantly transformed the “craft” of specialised research in the humanities. <...> It has become evident that the “old” methods of preserving and exploiting information were among the major factors in the formation and maintenance of the boundaries between disciplines’. In this author’s opinion, the best way out of the situation in which traditional specialities and institutions co-exist while the borders between them are constantly being blurred will be to recognise that they are arbitrary, which will allow us ‘to preserve and even to protect the old institutional “brands”, while giving them new research content’. Indeed, this is now the accepted way of facing this problem over which so many lances have been broken.

From the discussants’ point of view, it is not so much the theoretical vision that has changed as anthropological research practice. The basic definition of anthropology as the study of ‘other people’ has long been regarded as dubious. ‘Critics of anthropology suggested that the very project of studying others was epistemologically and ethically suspect, especially when those others are less powerful in a global political economy than scholars themselves’ (Mark Edele and Debra McDougall). The ‘other people’ appeared in ever-changing guises (the natives, the oppressed, the poor, etc.), but ‘if an earlier generation of scholars had to justify “studying up”, scholars now find it necessary to justify their interest in social forms like “the village”’ (Mark Edele and Debra McDougall). In other words, the tendency, noted in the discussion ten years ago, for the focus of

interest to move from the country to the town has become so much 'a fact' that now it is the return to 'the countryside' that demands an explanation. Anthropology, in the opinion of Ekaterina Melnikova, is more and more becoming the study of 'our own' — or probably more accurately of 'something else' within 'our own'.

The changes in the directions of anthropological research are due, among other things, to the politicisation of academic production, as Catriona Kelly points out: 'A more remarkable feature of the 2000s seems to me to have been the unmistakable resurfacing of a "grand narrative" that I hardly referred to back in 2004 — the grand narrative of national triumphalism'. As a rule this narrative coincides with a wave of patriotism. For the bureaucrats who deal with the sciences certain topics acquire a particular relevance, and these have priority in funding. This, of course, is perfectly natural for the bureaucrats. As she rightly remarks, 'considerably more worrying is the encouragement of analysis of a quite specific kind — the rise of an expectation that commentary on Russia's past and present should serve "patriotic" ends'.

Anthropologists have to adapt not only to radical transformations in informatics and politics, but to a historic swing from the national to the transnational, and from the local to the global. The discussants paid particular attention to the change in the status of the 'local' and its transformation into the 'glocal', since in this case it is not only a matter of having to assimilate theoretically a new construction of the world, but it goes to the heart of anthropological practice, which has always been based on a specific local field. In Nicholas Harney's opinion, 'anthropologists have been challenged to consider how to think about the "global inside the heart of the local", just as the intimacy of the local, a crucial site for anthropological knowledge production, has seemed to disappear'. From his point of view, one can attempt to understand 'the global in the local' by studying the dissemination and practice of modern corporate forms of administration as carried out by official bodies and NGOs, as he is doing in his project on migrants in Naples.

It is curious that in the previous discussion there was hardly any mention of the problems of fieldwork, whereas this time they are touched upon by many of the participants. Several answers discuss how the 'local population' has changed. In Vladimir Bogdanov's opinion, local people have become more open, which has allowed real penetration into topics which were previously closed to outsiders, insofar as 'the bearers of the tradition do not observe the prohibitions so strictly now, and are, if not pleased, at least not particularly embarrassed to talk about witchcraft, evil spirits and so on'. At the same time Joachim Otto Habeck has noticed an atmosphere of mistrust and suspicion of foreign researchers in Russia, which, in his

opinion, originates in 'a markedly conservative and inward-looking turn among wide circles of Russian society (exemplified by growing concerns about "proper" ways of living, loving, intimacy and education)'. At the same time he considers that the inhabitants of Siberia have grasped the new informational situation just as well as the researchers, which results in another substantial shift in fieldwork: it does not end when the researcher leaves, but often develops into continuing contacts. Against this background it seems quite natural that informants are more and more often becoming co-authors of publications, as Florian Stammler points out.

As I have already said, the authors of a number of answers have concentrated on their 'own' regions. Madeleine Reeves, describing the situation in Central Asian studies, notes that the countries in this region have become more active in publication over the last ten years and the conceptual content of the field has widened, so that there is no longer any reason to reproach the local researchers for neglecting the present day. At the same time, the growth in publications has not been equal in the different countries of the region. There is particular concern for 'the long-term prospects for research in the region, which has become less accessible to researchers than it was ten years ago, and which funding bodies regard as marginal to their strategic interests. For his part, Bruce Grant traces a similar picture in research on the Caucasus: 'Relative to most other world areas, we are seeing no flowering of anthropological work, especially in terms of monographs'. Here again, political factors are a central cause: 'In my own experience, despite ten years of visiting the same family in the same rural community, security police began shadowing almost every person with whom I spoke, closing down what I had once conceived as a long-term project'. Yet even so, 'Armenia, with its large diaspora and relatively more open universities, puzzles for its absence of new scholars entering the fold. Georgia, by contrast, is the one bright light: With its green landscapes, wine, khinkali, and visa-friendly setting, it remains the workhorse of the region'.

The anthropology of the Arctic has undergone changes of its own. In Florian Stammler's opinion, by now 'Arctic Anthropology has partially emancipated itself from being the handmaiden of natural science-driven climate change research and politics. The significance of the anthropological study of Arctic societies and cultures using participant observation has increased alongside the political significance of the region as a whole. It seems that now Arctic anthropologists can increasingly afford to follow their own research agendas and still be meaningful as well as visible in broader societal debates'. Joachim Otto Habeck notes a shift in his own projects from classical ethnographic research on the population of Siberia towards sociology, 'in three different ways: methodologically; in terms of research content; and also in terms of the body of literature that my colleagues

and I have drawn upon and the academic debates that we hope to contribute to. I think that anthropological research in Siberia can learn a great deal from sociological theory in order to extend its knowledge basis and its social relevance, notably in such fields as integration and exclusion, identity and inequality, individual aspirations and collective projects’.

The relationship between Russian and Western scholarship is a special question. It was not asked by the editors, but it is raised on both sides. In Ekaterina Melnikova’s opinion, ‘Russian anthropologists have become partly integrated into the international milieu. The Western milieu has merged with our native historiography’. Nevertheless, there is no question of total inclusion as yet. The boundary is still distinctly palpable. In this context Joachim Otto Habeck’s evaluation of the relationship between Western and Russian anthropologists is instructive: ‘While I do think that scholars from Russia and Western countries are now more willing and able to understand each other and make themselves mutually understood, I am worried about the disenchantment of Russia with the West and the no less disenchanted attitude of Western media and the wider public towards Russia’. One fears that after recent events in the Ukraine ‘mutual disenchantment’ can only become more ingrained.

2

The answers to the question about changes to the academic community deal mainly with changes in the relationships between different generations of scholars. According to Mark Edele and Debra McDougall, we are (at least superficially) living through a generational shift in the face of the social sciences and humanities. ‘The baby boomers (those born in the late 1940s and early 1950s) are on their way out, and several cohorts of younger scholars, with very different life-experiences, expectations, and sensibilities are poised to take their place’. In respect of anthropology in Russia (and the humanities in general), Sergey Sokolovskiy speaks of a catastrophic ageing process and an almost complete lack of a middle generation. ‘As a result conceptually obsolete approaches and topics are being forced on graduate students, and genuinely innovative subjects are not recognised as “ethnographic” (or “anthropological”) and are supported only exceptionally or fortuitously. In this way the subjects for dissertations in the leading departments and institutes consist of endless variations on well known themes, and innovative research is marginalised, and its authors find themselves seeking work outside academia or their own discipline’. There are, as ever, exceptions, one of which is the Faculty of Anthropology of the European University at St Petersburg, where new approaches and topics are welcome.

Furthermore, the younger generation is noticeably more pragmatic and, in Levon Abrahamian’s opinion, ‘on the whole chooses topics “that will fly” — particularly if they can get grants for them. The titles

are often more enticing than the contents. This, though, can be seen in international anthropological practice too — one has but to compare the headings of the articles in the leading journals with their content'. At the same time, despite the difference in their subject preferences and methodological procedures, one could hardly say that there is any tension in the relationship between the generations. 'If there are any clashes of principle in our field, they are rather ideological than generational in character' (Sergey Neklyudov).

Only Joachim Otto Habeck commented on the structure of the scholarly community, pointing out that the community of Arctic researchers 'continues to be made up of individuals who live in Russia, European countries, North America and Japan. Thus far, colleagues from India, China or Mexico are few and far between <...>. This reflects old colonial connections and voids in the asymmetric production of academic knowledge'.

3

The relationship between scholarship and the outside world is a serious worry for all our authors, which is natural, since the well-being of scholarship depends, in one way or another, on the 'outside world'. In Catriona Kelly's opinion, in the present set of circumstances 'an assault on academic autonomy for reasons of political expediency may become a serious possibility'. Russian researchers are particularly conscious of this in the discussions about the so-called reorganisation of the Academy of Sciences, which 'reveal mechanisms and motives designed to transform a learned society into a management group acting in the interests of state officials who speak in the name of the state, of society, of "social goods" and so on' (Konstantin Bogdanov).

From Sergey Neklyudov's point of view, 'as always happens at times of social and ideological crisis, society has become disillusioned with rational knowledge <...> This particularly concerns "pure", i.e. "fundamental" knowledge (the "purity" of which could be the subject of a separate discussion, as could its connexion with "practical" knowledge), the value of which has fallen almost to zero'. Yuri Berezkin seems to be in full solidarity with Sergey Neklyudov: 'In Russia neither society nor the élite have any need of scholarship. If our institutes and all their staff were suddenly to disappear, no one would take the slightest notice. It is indicative that the remarkable discoveries made in the last twenty years have passed unnoticed in society'. Sergey Sokolovskiy points out reasons why Russian anthropology is lagging behind and becoming provincialised, among them funding cuts, the lack of an infrastructure for horizontal mobility, a fall in the quality of education, etc.

Another serious problem is 'the bean-counting practice of impact factors and having to publish in specific prestigious or pseudo-prestigious journals' (Levon Abrahamian). Mark Edele and Debra McDougall write about the same thing: 'In our faculty, teaching

loads are calibrated to reflect publishing and grant-getting success (measured by what is called — without irony — the “Socratic Index”); more publications mean less teaching. At the bottom of the scale are scholars who are employed on contracts and completely dependent on the benevolence of grant-winning professors’. As they also point out, academic systems everywhere continue to produce more completed dissertations than academic posts, leading to a movement of young scholars outside the profession, and increasingly, to lack of stable prospects within it (the ‘adjunctisation’ of academia). Bruce Grant sums the situation up, referring to ‘a seemingly brave new world where the number of faculty hires steadily fall, while hiring of highly paid administrators, consultants, and public relations staff skyrockets’. In these conditions, any research without applied potential (‘impact’) — along with support for it, such as library collections and fieldwork — seems to the administrative caste like a waste of resources.

At the same time there are some positive developments. As Florian Stammler argues, Arctic anthropology, for instance, has achieved a great deal over the last ten years in terms of its profile and significance in society. This is due to the increase in the political importance of the Arctic, first and foremost thanks to the public discussion of climate change, and to industrial development in the region, but scholars themselves have been able to use the opportunity to demonstrate the value of their research to society and to the politicians, and to present a more rounded view of ‘traditional culture’ that shapes the work of policy-makers and the media.

And yet one cannot help fearing for the future. ‘Official intolerance of diverse explanations will certainly not lead to overall consensus, given the complicated societies we live in. But it may well change the academy in ways that will make the carefree debates of 2004 lose their topicality — not because we now take for granted the capacity to create our own narratives at will, as the questionnaire suggests, but because Realpolitik means that we are forced to mount narratives that directly repudiate, if they do not espouse, the “strategic priorities” that have been nominated by government departments’ (Catriona Kelly).

4

As to the answers to the question about who determines the vectors of development, schools or individual researchers, there is almost complete unanimity: the schools exist, but the vectors of development are set by individuals. As Alexander Panchenko rightly remarks, ‘modern scholarship is organised not as a hierarchy but as a network, that is, it does not consist of groups each headed by a charismatic leader, but of informal (usually international) communities which incline towards an egalitarian character. <...> It must be borne in mind that the concept of a “school” is connected not only with

notions of academic hierarchy, but also with the idea of epistemological monism, the orientation towards “single” and “universal” explanatory models. I think that this sort of approach is also becoming a thing of the past’. Sergey Sokolovskiy gives a detailed picture of the relations between leaders and communities in Russian anthropology in his article published in this issue.

The discussion has shown that ten years is too short a period to speak of significant shifts in anthropology. Each of the trends noted in the previous discussion had had its own development: the fragmentation of subject matter is only getting stronger; the transfer of attention from the ‘countryside’ to the ‘town’ is regarded as a fact that took place some time ago; the study of the present day is not even discussed, being that which anthropologists are primarily engaged in. Among new factors are the increasing dependence of anthropological research on political ‘objectives’ and on the transformations resulting from the information revolution. At a time of global shifts a concern for the condition of one’s own field comes to the fore (the stress on the local and regional is no accident). The attitude of society towards scholarship is worrying, particularly in Russia.

The editors are, as always, extremely grateful to everyone who participated in the discussion.

Albert Baiburin

*The contributions originally written in Russian
were translated into English by Ralph Cleminson.*