

Halle Studies in the Anthropology of Eurasia

Max Planck Institute for Social Anthropology

Exploring the Edge of Empire

Soviet Era Anthropology in the Caucasus and Central Asia

Edited by Florian Mühlfried and Sergey Sokolovskiy



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Florian Mühlfried and Sergey Sokolovskiy (eds.)

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Exploring the Edge of Empire

Soviet Era Anthropology in the Caucasus and Central Asia

All too often, Western scholars studying the Caucasus and Central Asia show a striking disinterest in, and occasionally even distaste for, knowledge generated locally. The anthropology of the Soviet era is often dismissed *in toto* as a fabrication of communist ideology and/or a purely descriptive and anti-theoretical endeavour. These assumptions greatly hinder communication between regional experts and between disciplinary specialists.

This book is an attempt to overcome this problem by re-evaluating Soviet anthropological work in the Caucasus and Central Asia. The contributors include scholars from these regions as well as others from Western countries. In addition to authors with first-hand experiences of Soviet era anthropology, the volume presents the voices of several younger scholars, as their reflections on the discipline's past will matter for its future in the regions.

The book is divided into five parts. The first part is devoted to the general framework of socialist anthropology in the Caucasus and Central Asia and scrutinizes relations between Soviet academic centres and peripheries. The second part deals with studies of collective farms and engagements with modernity in post-Stalinist Soviet anthropology. In the third part, two interviews with key "participant observers" and leading contributors to Soviet and post-Soviet anthropology bring in first-hand experiences and personal reflections. The final two parts of this book present the making of national schools of ethnography and related sciences in the Caucasus, followed by explorations of the contributions of some outstanding individuals and the institutional and/or political constraints within which they worked in Central Asia.

The topics range from discussion of the legacy of Soviet-era anthropology and application of theories of *ethnos* and so-called survivals to the impact of disciplinary traditions stemming from pre-Soviet times. The chapters bring out striking differences between the two large regions considered; they also draw attention to variation within these regions, and between different sub-disciplines of anthropology. The intricate histories of local research traditions subjected to strict controls contribute to a better understanding of the ways in which the social sciences interact with ideology. Chronologically, the book spans the whole epoch of the Soviet-style anthropology from its inception in the 1920s to its aftermath in a new century.

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Our workshop continued a series of meetings at the Max Planck Institute exploring the production of anthropological knowledge under socialist regimes. We are indebted to Chris Hann for his encouragement and support; to Aleksandar Bošković, Peter Finke, Svetlana Jacquesson, and António Eduardo Mendonca for their creative chairing of the workshop's sessions; and to Bettina Mann and Berit Westwood for logistical support. In selecting and soliciting contributions for this volume, we have benefited from the advice of Chris Hann, Svetlana Jacquesson and Nikolai Ssorin-Chaikov. Finally, we would like to express our gratitude to Berit Westwood and Anke Meyer for coordinating the production of this volume, to Benjamin White for the copy-editing and to Kerstin Klenke for harmonising the Russian transliterations and for painstakingly finalizing the manuscript.

Florian Mühlfried and Sergey Sokolovskiy

Notes on Transliteration

Transliteration of words and names in Russian follows a simplified version of the Library of Congress system, i.e. without ligatures. References to English publications of Russian authors keep the transliteration of their names used in these works. The same applies to transliterated Russian words and names in English titles or citations. The names of the book's authors are transliterated according to their own preferences throughout the text. Accordingly, the spelling of their names in bibliographical references may deviate from the Library of Congress system. Transliterations from Georgian, Armenian, Azeri and Arabic follow the system preferred by the individual author. Except for citations and references, names of cities, regions, rivers etc. are not transliterated, but rendered in standard modern English, if equivalent terms exist (e.g. Moscow instead of Moskva).

Armenian surnames are usually spelt with the suffix -yan in Latin script, according to a new passport transliteration system introduced in 1996. Throughout this volume, we follow this convention for Armenian authors who have published in Armenian from 1996 onwards. For earlier publications in Armenian -ian is used instead. The surnames of Armenian authors publishing in Russian are transliterated according to the Library of Congress transliteration system with -ian, regardless of the publication date. Names already used in publications are rendered as they appear.

Chapter 1
Introduction: Soviet Anthropology at the Empire's Edge

Florian Mühlfried and Sergey Sokolovskiy

Some general remarks

In March 2009, the Chicago-based sociologist and fervent advocate of world systems theory Giorgi Derluguian gave a talk at the Georgian State University on 'How not to Stay Provincial'. Although Derluguian referred to the 'state of advanced social science' in general, we consider it no pure coincidence that he chose this title in a place like Tbilisi and in a country like Georgia. It is hard to disagree that, whatever we do as social scientists, we should not be provincial but rather cosmopolitan, addressing the important, timely issues and conversing with dominant theoretical discourses in our works. This seems to be all the more true in the field of regional sciences, which, according to some, are in decline or even in crisis, not least because of their inherent demand for specialization.

So here we have the claim that if we want to know something about a region like the Caucasus or Central Asia, we should be guided by the dominant concerns of contemporary social science. This claim is put forward not only by social scientists like Derluguian but also by local students. At Tbilisi State University, for example, social anthropology students are eager to become acquainted with the main paradigms of social and cultural anthropology, and many of these students advocate their potential as explanatory tools for their own societies. They are thirsty for theory and hungry for methodology.

This appetite for theory and method is often accompanied by a striking disinterest in, and occasionally even distaste for, knowledge generated locally and more than twenty years ago. In the case of Western scholars studying the Caucasus and Central Asia, this disinterest seems to be based on the following assumptions: reading texts by locals is time-consuming, demands a great deal of language proficiency, and the content of these works is oftentimes completely out of fashion. In the case of local students and some younger scholars, Soviet academic traditions are often dismissed *in toto*. Soviet anthropology in particular is often regarded as being of no value.

Accusations along the following lines are the most popular: 'It is all tainted by communist ideology', or 'It is only descriptive and does not contain any theory'.

Obviously, this dismissive stance can have very real implications. In the former GDR, for example, literally every former professor of anthropology, ethnology, ethnography, and *Volkskunde* was expelled from his or her position after German reunification. They all lost their jobs. In contrast, the situation is substantially different in the Caucasus and Central Asia, where former Soviet anthropologists still walk the floors of their departments. Yet in most cases, there is a significant tension between generations and approaches, and severe disputes have arisen concerning the institutional position of anthropology. Apparently, it is all too easy to accuse and dismiss colleagues for being rooted in Soviet traditions or, even worse, caught up in a Soviet mentality.

Regional and national differences have to be taken into consideration, however. In post-Soviet Georgia, the older generation of Soviet-trained academicians has largely been replaced by young elites, many of whom have been trained in the West. Competition between the older and younger generation has caused tremendous conflicts and even upheavals in Georgian universities. In contrast, the academic situation in Armenia seems to be far more inclusive. Revolutions are not high on the agenda, and old and young generations actually talk to each other. Hence, it is important to be very specific about locality when talking about anthropology in socialist and postsocialist societies.

But in addition to space, time also matters. Anthropology underwent various shifts during the Soviet period. Thus when talking about 'Soviet anthropology', we should be sure to specify which time span we are referring to. Interestingly, we (in the West, at least) know far more about the early years of Soviet anthropology than about its development in the decades following World War II. This has to do with the remarkable works produced by Bertrand (2002, 2003), Cadiot (2007), Hirsch (2005), Slezkine (1991, 1994), and others, which have shed light on the making of Soviet anthropology. But this also has to do with Western sovietology's obsession with spectacular events and periods like the October revolution and Stalinism, and its relative ignorance of the less spectacular time spans like Khrushchev's thaw and the Brezhnev years.

With this book, we attempt to counter these blind spots, paying special attention to the post-World War II period. To this end, we explicitly address Soviet *kolkhoz* studies, which mainly developed in the 1950s and dealt with questions of social transformation and Soviet modernity (Bobrovnikov and Abashin, this volume). In so doing, our discussion is intended to contribute

to larger debates in our field and in neighbouring disciplines regarding the nature of both modernity in general and Soviet modernity in particular, development policies, social engineering, and so on.

Another strand of the debate on the Soviet state has centred on the topic of colonialism (Cadiot 2007; Hirsch 2005; Martin 2001; Slezkine 1994). It seems that the question of whether or not the Soviet Union was a colonial empire can be at least partially answered with regard to the making and workings of anthropology. What needs to be asked is the following: To what extent was the discipline of anthropology in the Caucasus and Central Asia coordinated and controlled by power centres situated in Russia proper? Who organized ethnographic excursions and interpreted the data collected in the two respective regions? What role did local anthropologists play in the development of Soviet anthropology in general? How did the tension between nation-building and Soviet state-making manifest itself in the discipline of anthropology? These questions play a key role in the fourth and fifth sections of this book, which are dedicated to national schools and research activities in the Caucasus and in Central Asia respectively.

What, then, about Soviet anthropology itself? Was it an imperial science *per se*, comparable to the early decades of Anglophone social and cultural anthropology? One way of dealing with Soviet anthropology is to place it among 'other anthropologies' and to try to identify 'deviant' patterns in the global development of anthropology (Bošković 2008). Alternatively, it is possible to treat Soviet anthropology as a continuation of the central anthropological tradition of the nineteenth century, with its stress on historically grounded research (advocated by Morgan and others). Both of these approaches, however, largely omit the question of power. In the first case, Soviet anthropology and its Russian predecessor are located among other anthropologies of former colonies (e.g. Kenya), anthropologies of former colonizers (e.g. the Netherlands), anthropologies of internal colonizers (e.g. UK and Brazil), and anthropologies of post-colonial global players (e.g. Brazil again). The only common feature of 'other anthropologies', then, seems to be that they are not direct descendants of the French, American, and British schools of anthropology. Soviet anthropology, however, shares much more of a past with the 'leading' schools of anthropology than with most 'other anthropologies'. Hence, there seems to be little heuristic value in such a binary model.

In the second case (treating Soviet anthropology as a continuation of the nineteenth century tradition), the role of anthropology in maintaining and expanding empire is of little relevance; yet, this is precisely what we are after in this volume. The question remains, after all, whether or not anthro-

pology is still so tailored to the Soviet empire's heritage that it cannot successfully exist without it — hence the success of anthropologies in imperial, post-imperial, and neo-imperial states. Failed imperialism seems to have developed weaker anthropological traditions, and prospering imperialism, stronger anthropological traditions. This is an uncomfortable thought; yet, it needs to be falsified in order to let it go.

Within this debate, centre-periphery relations have begun to appear on the agenda again. The normalizing discourses inherent to centre-periphery relations have not been specific to anthropology. The Soviet centres of Moscow and Leningrad were simultaneously places in which the infrastructure necessary for conducting social science and humanities research was located, including archives, large libraries equipped with the latest book and journal collections from all over the world, editing houses, the best and the largest universities, and academic research institutes with higher-grade facilities for research. The editorial boards of the leading academic journals were primarily comprised of scholars from central institutions, and until the 1990s, only the Moscow and Leningrad academic councils situated in the institutes of the Academy of Sciences had the prerogative of accepting doctoral dissertations for defence. Hence research standards elaborated in the centres were to be emulated if not blindly followed in the peripheries.

Colonialism as a catchword and its twin orientalism also play a role in current debates on the status of anthropology in and of postsocialist societies. The Polish anthropologist Buchowski (2007), for example, concludes that the scholarly productions of anthropologists based in postsocialist countries are not taken seriously enough by the Western academy and that the specific anthropological traditions of these countries are often ignored. Moreover, it is no great secret that if one wants to apply for international funding for anthropology in a postsocialist society, one has to express the will to emulate Western standards.

According to our own personal experience, Buchowski's criticism is well-founded, and it leaves a bitter taste to the experience of teaching social anthropology in Georgia on a payroll from the Open Society Foundation, as Mühlfried did in 2006-7. Yet, other personal experiences counter or at least subvert this criticism. When Mühlfried, for example, presented his research on the main street market in the Georgian capital Tbilisi to the ethnographic department of the state university in the late 1990s, there were plenty of interested students, eager to ask questions. The professors told the presenter, however, that what he was doing was not anthropology but sociology, and they discouraged their students from following this approach. One professor asked why a German anthropologist like Mühlfried was not following the

'German tradition', by which he meant the German school of cultural history, established in the nineteenth century. What worries us in this case is not so much the exhortation to conduct anthropology in a nineteenth century vein but rather the strong discouragement of students from caring for social anthropology in its Western shape. A lot has changed since the 1990s; in fact, a large percentage of the university staff in Tbilisi has been replaced, but this experience raises the question of how to proceed with anthropology in the former Soviet Union.

In this respect, the most pressing question is how to institutionalize anthropology in postsocialist societies. Should the discipline stay put among the historical sciences, or should it be reframed or perhaps even reinvented as a primarily social science? Again, spatial, temporal, political, and possibly social factors have to be taken into consideration, and we should refrain from a simplistic model to be readily applied everywhere. However, local anthropological traditions should be of pre-eminent importance for the academic framing of anthropology as a discipline, and in this respect, our book not only tackles the past but also the present and future of anthropology in postsocialist societies.

The last general question we would like to bring to the fore now is how anthropology in the West could possibly profit from engaging with Soviet anthropology. In other words, why should we (or they, depending on the perspective) care? One superficial but nevertheless consequential answer is that Soviet anthropology's fixation on history can serve as a reminder of one of the discipline's main roots and can help us to overcome the presentist bias that has dominated anthropological approaches in the aftermath of Malinowskian functionalism. History has begun to reappear on the scene, and an increasing number of anthropological studies have paid tribute to this trend. In the field of the anthropology of postsocialism, this trend is especially vital. One indication is the recent (2009) inauguration of a working group dedicated to historical anthropology at the department of 'Socialist and Postsocialist Eurasia' at the Max Planck Institute for Social Anthropology.

Another lesson to be learned is to be cautious in dispensing advice. As Dragadze reminds us in this volume, the last twenty years have witnessed a multitude of interventions in the academic fields of postsocialist societies. Within this context, catchwords like civil society and cosmopolitanism sometimes sound like a war cry. All too often, Soviet anthropology is dismissed not only for being wrong but for being essentially different from its Western twin. Abashin (this volume) reminds us, however, that Soviet academic circles also had their own 'writing culture' debate and lively discussions about the advantages and drawbacks of multi-sited fieldwork.

Furthermore, Kuznetsov highlights how the introduction of ethnos theory by Iulian Bromlei in the 1960s was accompanied by extensive and sometimes heated discussions concerning the role of ethnicity for social and historical sciences — not least in the empire's peripheries such as the North Caucasus. Perhaps much more than the fiction of 'objective' knowledge, these debates can serve as a reference point for the re-evaluation of the future of anthropology in postsocialist societies.

When we compare Soviet/Russian anthropology's preoccupations, both past and current, with research agendas in various other national anthropological traditions and world anthropology more generally, however, interesting gaps, omissions, and preoccupations begin to appear that are worthy of study in their own right. Efforts in the history of anthropology to identify past achievements of the discipline, however glorious they may be, belong more to propaganda than to history of science *per se*. This does not mean that historical gaps can only be filled by reliance on external resources; local resources remain as rich and variable as they do outlandish.

Finally, there may be something to discover in the workings of Soviet-era anthropology that is worthy of attention in its own right. One potential candidate we have in mind here is the 'complex-intensive' method advanced by the Georgian anthropologist Giorgi Chit'aia. This method utilizes a teamwork-based approach to complex historic, prehistoric, and contemporary community study. This method can easily be discredited as a remnant (or 'survival', as Soviet anthropology would call it) of nineteenth century holism, but according to former participants in Chit'aia's expeditions like Ketevan Khutshishvili, collective enthusiasm for identifying stable traits of a longue durée history sometimes produced remarkable sparks of interdisciplinary knowledge. Contemporarily, the collective organization, performance, and analysis of fieldwork would be a most relevant enterprise in an era still dominated by the rhetoric of inter-disciplinarity. Yet, such a methodology is hardly ever put into practice. Is there something here to take inspiration from and to experiment with? If the still comparatively well-financed West will not pursue this agenda, it is likely that this avenue will remain unpursued in the near future, as the practice of collective fieldwork expeditions in post-Soviet Georgia has come to a complete standstill, due to the absence of significant state funding for anthropological endeavours in general.

Attempts at identifying the pearls of Soviet anthropology, however, should not lead us to turn a blind eye to its flaws and faults. In his contribution to this volume, Ilkhamov outlines in great detail how the ideological construction of a common, 'good' past for the Soviet republic shaped the dating of the origin of the Uzbek nation beyond scientific rationality. Moreover, in one of the most touching chapters of this book, Nikulin describes

how empirical studies of economic realities in Central Asia were actively suppressed by Stalinist-era political authorities, with often fatal consequences for the most capable scholars in or of the region. Schoeberlein, in turn, makes it plainly clear that official ideology not only framed the work of Soviet anthropologists (e.g. the famous citations of Marx or Lenin at the beginning and the end of texts) but also limited choice of research topics and determined analytic concepts. In respect to Soviet-era anthropological studies of Central Asia, in his chapter in this volume, DeWeese argues that the notion of 'survivals' was purportedly applied as a research instrument but ultimately served to discredit contemporary religious practice and attachment. DeWeese does not find much value in these works, as their underlying assumptions are based on a misleading and ultimately incorrect vision of a 'pure' Islam.

This brings us to the last general topic to be tackled at this point: how to deal with the works of Soviet anthropologists in the present. Answers to this question appear throughout this volume and are rather heterogeneous. Mammadli argues that writings produced by Soviet anthropologists of the Caucasus and Central Asia contain interesting and relevant data but should be stripped of their ideological context. Schoeberlein, on the other hand, questions whether this is possible and notes that ideology has too deeply penetrated the workings of Soviet anthropology, at least in respect to its treatment of religion in Central Asia. Dragadze, in contrast, comes to the conclusion that the pervasiveness of Marx and Engels' doctrine affected the interpretation of data rather than the quality of data itself, ultimately concluding that there is much of value to be found in Soviet-era anthropologies of the Caucasus and Central Asia. In any case, we must know these works before we can judge them. Thus, returning to our starting point, let us be provincial for a while in order to be cosmopolitan.

Anthropology, Ethnography, or Ethnology?

For pragmatic reasons, in this volume we stick to a broad notion of anthropology that encompasses the disciplines of ethnology, ethnography, folklore studies, and occasionally even oriental studies. However, readers should keep in mind that throughout the Soviet period, by 'anthropology', Soviet scholars understood only physical anthropology. The terms 'social anthropology' and 'cultural anthropology' were reserved for British and American anthropological disciplines respectively. 'Ethnology' was used in the 1920s to cover theoretical and comparative approaches built upon ethnographic observation and could apply to research conducted both at home and abroad. The term came into vogue again in the early 1990s and has been incorporated into the official names of several university chairs throughout the

Russian Federation and the Moscow institute of Ethnography.[1] 'Ethnography' remained the single legitimate term for the designation of theoretical, comparative, and field anthropological research from 1929 to the 1980s.

All of the terms used throughout the Soviet period had very specific connotations, the meanings of which varied considerably at different periods in the broader 'Soviet anthropology epoch', a fact that invited commentary from both proponents/practitioners and opponents/critics of this terminology and related approaches. Iulian Bromlei, a leader of Soviet anthropology in the 1970s and 1980s and director of the central Soviet research institute from 1966 to 1988, has on several occasions commented on this terminology and specifically on the discipline's name. Bromlei's book, *Contemporary Problems of Ethnography*, features a chapter entitled 'Ethnography and Socio-Cultural Anthropology' that is exclusively devoted to the comparison of various national disciplinary traditions and terminologies (Bromlei 1981: 98-113). Citing definitions of anthropology by F. Boas, A. Hultkranz, M. Herskovits, E. Gellner, T. Parsons, and C. Lévi-Strauss, Bromlei suggested that Soviet *etnografiia* and Anglo-American anthropology were analogous but different disciplines with distinct research subjects (ibid.: 98, 110-111).

Sergey Abashin (this volume) prefers to use the term 'ethnography', which in his opinion better delineates the specificity of Soviet anthropological practice and theoretical considerations and better reflects relevant historical circumstances. After 1928, ethnology was treated as a 'bourgeois discipline' (Slezkine 1991), and the term 'anthropology' was used to refer exclusively to biological anthropology, which, as a matter of fact, was called 'physical anthropology'. A similar point was made earlier by E. Gellner, who in 'The Soviet and the Savage' wrote:

> Terms, definitions, and boundaries of subjects are not the same in the Soviet Union as in Britain. Here a 'social anthropologist' is, roughly speaking, a man who uses the methods and ideas of the tradition of Malinowski and Radcliffe-Brown to study any society to which he thinks those ideas and methods can be applied. This usually, but by no means always, means 'primitive' societies. By contrast, a Soviet 'ethnographer' (the term used in Russian instead of 'anthropologist') is, also roughly speaking, a student of pre-capitalist social structures. The consequent overlap is great, but far from complete. The most important difference perhaps is not in the exclusions implied by either definition, but in the fact that the Soviet ethnographer is not separated, either in his ideas or his subject-matter, from

[1] The Institute of Ethnography of the Academy of Sciences of USSR from 1943 till 1992 had a Leningrad (in the period of 1991 – June 1992 St. Petersburg) branch, so to refer to it as Moscow is correct only when specifically its Moscow headquarters are meant.

> the archaeologist or the historian. Someone trained in the British tradition of social anthropology who enters the world of Soviet anthropology undergoes a drastic, and no doubt salutary, culture shock. [...] They [the Soviet ethnographers] constitute a different world, and in many ways an impressive one, a world which stands in sharp contrast to at least British, if not to all Western, anthropology. Briefly and crudely, it is the contrast between the evolutionist-historical and the functionalist-static visions of man and society. Less crudely, it is the contrast between two traditions, each struggling with the inadequacies of its own ideas (1988, pp. 1-2).

Major Theoretical Trends in Soviet Anthropology

The evolutionist-historical vision that Gellner mentions has remained a pervasive trait of Soviet *etnografiia* since its Marxist reorientation in the late 1920s and is traceable even now in a multitude of practices and perceptions of Russian, Caucasian, and Central Asian anthropologists. Among these peculiar dispositions, we might mention a form of teleological presentism derived from the Hegelian (via Marx) conviction that contemporary forms of social life are superior to their historical antecedents. Another consequence of the evolutionary paradigm was a tendency to view social aggregates that anthropologists studied as organisms — endogamous, autonomous, self-perpetuating units of development, progressing through a number of stages from primitive tribes to modern nations. This tendency is also present in current preoccupations of post-Soviet anthropologists in such fields as the study of inter-ethnic relations and ethnic conflicts, in which so-called ethnoses[2] and not people compete for resources (hence the popularity of Lev Gumilev's views on ethnic interaction). To call such views *anthropology* would be an overextension of the term, but they were part of the Soviet *etnografiia*.

The reification of the units that were ostensibly observed and studied by Russian ethnographers (e.g. folk culture, people, class, and *etnos*) is evident in all periods of Soviet anthropology's history, but the naturalistic gaze was inherited from antecedent research traditions. However, in Russia the tendency towards reification of observed phenomena was reinforced by the advent of dogmatic Marxism in the late 1920s and early 1930s, which

[2] The spelling *etnos* is a transliteration from Russian and designates a specific concept of ethnicity prevalent in Soviet anthropology (see Skalník 1986 and Gellner 1980). For this reason, we use *etnos* in italics whenever we refer to this specific concept, and 'ethnos' in all other cases when we refer to the meaning shaped by its usage in Western anthropological literature. However, in the individually authored chapters that follow, we have retained the conventions of the respective authors.

downplayed everything 'subjective' as fortuitous and spurious and insisted on the study of 'objective forces' in social and economic life. This Marxist stance in Soviet social sciences led to the gradual obliteration of the differences between reality and its representation and the creation of a peculiar version of constructivism in which social reality itself became so malleable and pliable that it could be created from scratch by the Bolshevik government. This inimitable Soviet episteme made it impossible for ugly facts to destroy beautiful theory and reinforced in its turn dogmatic Marxism as the only scientific and objective theory ever to have existed. The glorification of scientific dogma contributed to the special status of theory in Soviet social thought and produced an unbridgeable chasm between 'theoreticians' and 'practitioners' in every social discipline, including anthropology. New theories ceased to be put to the test of practice, as such a test would appear futile and dangerously anti-Marxist.

Notwithstanding such tendencies towards the reification of social phenomena and objectification of social representations, in its initial and final phases, Soviet anthropology was preoccupied with the study of ethnic units, as exemplified in the works of Sergei Shirokogorov (1923) and Nikolai Mogilianskii (1908) from the 1900s to 1920s, then again in the works of Pavel Kushner, Sergei Tokarev, and Nikolai Cheboksarov from the 1940s to 1960s, and finally in the works of Viktor Kozlov, Sergey Arutyunov, Iulian Bromlei, and Lev Gumilev from the 1960s to 1980s.

After a twenty year hiatus, the revival of ethnic-centred studies could be attributed to the post-war preoccupation with the geographical distribution of ethnic groups in the territories of Central and South Europe that came under the control of the Russian army. During the war in 1943, after emerging victorious in the battle for Stalingrad, the Soviet government (in particular, the Ministries of Foreign and Internal Affairs and the Army Headquarters) requested from the Russian Academy and Moscow Institute of Ethnography ethnographic information on bordering states and foreign European states, and especially on Germany and the countries along the Danube (Austria, Bulgaria, Czechoslovakia, Hungary, Romania, and Yugoslavia). Sergei Tolstov, the director of the Moscow Institute at the time, had a meeting with the minister of foreign affairs, Maksim Litvinov, who asked Tolstov to provide relevant information on Yugoslavia, Albania, Transylvania and Banat, Transcarpathia, Poland, East Prussia, and Finland. Research into the ethnographic composition of European countries was distributed among three teams of scholars: one for Germany, another for the Danube region, and the third for Turkey headed by the newly-appointed director of the Moscow Institute, Sergei Tolstov. The 'contested' borders of Lithuania-Germany, Belorussia-Poland, Ukraine-Poland, and Moldova-Ukraine-

Romania were subject to special focus. With the gradual advance of the army towards the western borders of the USSR, leading ethnographers were dispatched to various regions and countries to map ethnic compositions. Nikolai Cheboksarov, the head of the Department of Europe at the Moscow Institute of Ethnography, worked in Latvia, Lithuania, and Estonia on the problems of ethnogenesis, ethnic history, and the ethnic composition of the population of these republics. Sergei Tokarev compiled information on Austria, Czechoslovakia, and Hungary and later visited several Balkan countries. Pavel Kushner studied the western border of the Lithuanian population. In 1945, in collaboration with the Ukrainian Academy of Sciences, the Moscow Institute of Ethnography organized ethnographic research in the western regions of Ukraine. A new department of ethnographic statistics and cartography was created at the Institute under the leadership of Pavel Kushner, an eminent scholar and specialist in ethnic geography and the author of the first post-war theory of *etnos*, elaborated in his famous book *Ethnic Territories and Ethnic Boundaries* (1951).[3] In March 1944, ethnographers were requested to compile an ethnographic map of Central and Eastern Europe (Kozlov 2004: 413; Tokarev 1995: 181-85). As part of the war effort, all cartographic works had been classified, and a special agreement between Army Headquarters and the Institute precluded publication results and even mentioning this work in lectures to students.[4] More than 30 detailed ethnographic maps were compiled, and a voluminous work, 'The study of the ethnic composition of Central and South-Eastern Europe' was written but never published, in accordance with an agreement with the Headquarters.

Another 'stronghold' of ethnic studies had been the field of ethnogenetic studies (i.e. studies of the origins and histories of various ethnic groups), which was intimately connected with then-contemporary political agenda and ideology. The history of this research field still awaits its chronicler. An influential figure in ethnogenetic reconstructions was Nikolai Marr, but Russian scholars became interested in the historical formation and genesis of various ethnic groups and linguistic families well before and long after

[3] Already by 1947, Kushner had formulated his ideas in his article 'Ethnic Boundary' (1947), which was published in the flagship journal of Russian anthropology, *Sovetskaia etnografiia*. Some of his ideas echoed those Fredrik Barth articulated in the introductory chapter to *Ethnic Groups and Boundaries* (1969). However, unlike Barth, as a geographer Kushner paid much more attention to the geographical distribution of ethnic groups and the fluid and unstable character of ethnic identity at the borders, deemphasizing the social factors underlying identity change.

[4] Archive of the Russian Academy of Sciences, fund 142, file 9, p.138 (cited in Alymov 2006: 42).

Marr's publications on the subject.[5] Long before Marr, Russian Indologist Vsevolod Miller conducted similar research on the Ossetians (1881-1887). Russian ethnogenesis had been the object of scholarly debates since the creation of the Russian Academy of Sciences. During the war, Slavic ethnogenetic studies were perceived as an ideological response to Nazi propaganda and gained new political significance. In December 1943, the Academy of Sciences organized a special session devoted entirely to the Slavic ethnogenesis. Soviet ethnology added a new dimension to this debate by applying Marxist evolutionary schemes to ethnic history.

With its unilineal treatment of history as inevitably progressing toward global communism, the Marxist version of evolutionism served as a prototype for similar conceptual constructions in various branches of the humanities and social sciences, especially in linguistics (e.g. Marrism's account of language evolution) and *etnografiia,* where several eminent theoreticians proposed competing schemes of the historical progression of *etnos* through a series of successive stages from primitive tribes to modern nations. The study of so-called early pre-capitalist social formations was the major preoccupation of Marxist ethnographers from the 1920s onward.[6] In the final period of Soviet academic scholasticism, there were several theories of ethnic evolution instead of a single theoretical schema, with Bromlei's and Gumilev's theories as the most popular. Bromlei and his colleagues made explicit attempts to merge ethnographic data on various societal stages with the doctrine of dialectical materialism by assigning to every historical stage (slavery, feudalism, capitalism, and socialism) a special type of ethnic community. Gumilev implicitly rejected historical materialism in favour of his own version of social Darwinism: Gumilev's *etnos* moved forward through a series of stages, progressing and regressing according to its 'age'.[7]

However, evolutionary transformations were the subject of more intensive focus in the initial stages of the development of Soviet *etnografiia*. From the 1960s to the 1980s, the stress was on classification of various ethnic group types and on peculiar forms of group transformation termed 'ethnic processes' (i.e. acculturation, assimilation, and various types of splitting and merging of ethnic groups).[8] This preoccupation with cultural

[5] Marr started a series of books and papers on ethnogenetic issues in 1924 by publishing his 'Indo-European Languages of Mediterranean', in which he attempted to combine archeological and linguistic perspectives in order to produce better ethnogenetic reconstructions.

[6] The subject of patrimonial system decay was especially popular during the first 25 years of the discipline's existence.

[7] He claimed that the average duration of an *etnos* was 1,500 years.

[8] E.g. Sergei Tokarev's ideas on the typology of ethnic groups (1964), which were later elaborated by Nikolai Cheboksarov (1967), Viktor Kozlov (1967), and Iulian Bromlei (1973).

and ethnic divisions was present throughout all periods of Soviet anthropology's history and was closely linked to the foreign and domestic policies of the state and the practical tasks of integrating various ethnic groups into a single Soviet nation. Anthropology was so centralized that when there was a change of director in its heading institute, the Institute of Ethnography in Moscow (which had a branch in Leningrad), an associated change in paradigm and research focus was sure to follow: ethnogenetic reconstructions at the time of Sergei Tolstov; ethnos theory and ethnic processes under the directorship of Iulian Bromlei; and ethnic conflicts and ethnic politics under Valerii Tishkov. However, perestroika brought the demise of this single hierarchical structure and the emergence of less powerful, new centres with their own research agendas: Yerevan, Tbilisi, and Tashkent were among the first to cease emulating Moscow fashions in anthropological research. Kazan', Novosibirsk, and Omsk followed suit by establishing their own academic councils, scholarly journals, and research agendas that deviated from the Moscow canon to such an extent that it became possible to identify a much more variegated and fragmented field with multiple centres and new peripheries, which would prove much more difficult to map. In present-day Russia, instead of the former 2 or 3 chairs specializing in anthropological research, there are now approximately 30.

Outline of the Book

The present volume is the third in a series initiated by Chris Hann and dedicated to the investigation of socialist-era anthropology. The first two volumes are both entitled *Studying Peoples in the People's Democracies* and focus on East-Central and South-East Europe, respectively (Hann, Sárkány, and Skalník 2005; Mihăilescu, Iliev, and Naumović 2008). In general, we follow the same agenda as outlined in the introductions to these prior volumes. We stick to the same broad notion of anthropology, which provides for a wide range of academic activities that nevertheless share a common intellectual history. We also share an interest in 'back door stories', that is, information on the workings of Soviet anthropology in the Caucasus and Central Asia that cannot be found in published articles and books. As Dragadze demonstrates in her chapter, her conversations with female colleagues in the restrooms and with male colleagues in the corridors of the old Institute of Ethnography in Moscow substantially informed her understanding of how anthropology in the Soviet Union really worked. Hence, we are strongly interested in first-hand accounts of practical and concrete aspects of Soviet anthropology in the Caucasus and Central Asia, such as the organization and conducting of fieldwork, collaboration between researchers, processes of data analysis, and publication processes. To this end, scholars with personal

experience in the workings of Soviet anthropology have played a central role in all three of the volumes thus far published in this series.

In contrast to the two preceding volumes, however, herein we have also invited contributions from young scholars. We think it is important that the past be reflected upon by those who are directly affected by it and who will shape the future of the discipline, *in situ*. We have also encouraged contributions by Western scholars with long-standing commitments to research on the Caucasus and Central Asia, in the hope that their contributions speak to the scholars from the regions, and that they ultimately speak to each other. Finally, we have actively searched for contributions from authors who are part of or familiar with the academic circles of Moscow as the former centre of Soviet academia. Without addressing the role of the Moscow academy, it is impossible to reflect on Soviet-era anthropology in the Caucasian and Central Asian republics.

The book is divided into five parts. The first part is dedicated to the general framework of socialist-era anthropology in the Caucasus and Central Asia. Based on her personal experiences in the Soviet republics of Georgia, Azerbaijan, Tajikistan, and Russia, Dragadze presents a profound anecdotal sketch of 'structure and sentiment' in Soviet anthropology. With regard to its underlying communist ideology, DeWeese carefully analyzes the social, political, and academic implications of the widespread and quasi-unavoidable usage of the concept of survivals in the investigation of religious beliefs and practices in Central Asia. In a similar vein, Schoeberlein investigates the question of how ideology-laden theory affected the representation and analysis of Islam in post-war Soviet ethnography of Central Asia.

The second section is devoted to studies of collective farms. Abashin investigates this topic in respect to Central Asia and highlights interesting parallels between collective farm studies and community and peasant culture studies conducted in Western anthropology around the same time. Bobrovnikov turns his eye to the North Caucasus, and more precisely to Dagestan, and highlights how the quest to identify mountain traditions ignored both actual living circumstances and the fact that many of these traditions had practically vanished — a trend that, according to the author, continues even now.

The third section is dedicated to interviews with two leading anthropologists of the Caucasus and Central Asia respectively, both of whom experienced the workings of Soviet anthropology first hand. Arutyunov has served as the head of the Caucasus department of the institute of ethnography in Moscow since 1985, has shaped several generations of Caucasus anthropologists, and has developed long-term fieldwork commitments in various places in this region. Khazanov is a leading expert in the study of

nomadism and a regional expert on Central Asia and Kazakhstan, in particular. After being expelled by Soviet authorities in the 1980s, Khazanov nevertheless maintained ties with his colleagues in Russia and Central Asia.

National schools of anthropology and the role of individual scholars in their making are at the centre of the fourth section. Dabaghyan investigates Armenian anthropology throughout the Soviet era, paying special attention to the role of the salvage paradigm. Mammadli focuses on Soviet-era ethnographic studies in Azerbaijan and brings to the fore hitherto barely addressed approaches to urban anthropology and ethno-sociology. Tuite analyzes the impact of the famous Soviet scholar Marr and the concept of Marrism on Georgian anthropology, providing nuanced and even intimate insight into the world of concepts, ideas, and paradigms circulating in Georgian academia in Soviet times and thus affording us a better understanding of contemporary academic controversies in the post-Soviet world. Finally, the Soviet past, precarious present, and insecure future of anthropology in the Northwest Caucasus are addressed in Kuznetsov's chapter. Scrutinizing dominant, essentialist, and nationalist assumptions stemming from Soviet times and perpetuated in the present, Kuznetsov emphatically demonstrates how anthropology may serve a political agenda based on xenophobia.

The final section of this book presents the making of national schools of ethnography and related sciences in Central Asia as well as individual contributions and the institutional and/or political constraints to their making. Ilkhamov uses the works of the Soviet historian Iakubovskii to illustrate how wishful thinking had more of an impact on the writing of Uzbek national history than hard facts. Naumova scrutinizes the works of the well-known anthropologist Sukhareva in the field of material culture and kinship studies and outlines her role in the spread of anthropology in Uzbekistan. Finally, Nikulin tells the sad story of Chaianov, a man obsessed with empirical studies of Soviet economic realities and eventually killed in exile in Central Asia because he dared to address the problematic effects of Soviet modernization. Chaianov's life history is a good reminder of how dangerous it was at times to conduct anthropology and related sciences in the Soviet Union and how, in order to simply stay alive, certain empirical realities had to be ignored, downplayed, or dismissed.

This is not the end of the book, however. First of all, an appendix is provided with data that we hope can serve to substantiate future research into the practices of Soviet anthropology. Dabaghyan has been so kind as to provide a catalogue of all the dissertations in ethnography in Soviet and post-Soviet Armenia. This list bears witness to the changing regional fashions in the discipline. Furthermore, we also include a list compiled by Sokolovskiy that documents and provides the history of ethnology and anthropol-

ogy chairs at Russian universities. This data reveals a clear spike in the total number of chairs dedicated to ethnography and/or ethnology, as well as the pluralization of topics attached and attributed to ethnography/anthropology. Finally, Levon Abrahamian thoroughly reflects on the main findings of this book in his epilogue and brilliantly contextualizes them in the workings of Soviet social and cultural sciences, calling upon his unrivalled, first-hand insights and experiences.

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THEORETICAL FRAMEWORKS AND THEIR PRACTICAL IMPLICATIONS

Chapter 2
Soviet Ethnography: Structure and Sentiment

Tamara Dragadze

I want to engage here in a kind of 'ethnography' of the Soviet communities of scholars I have worked with, as a participant observer, for the past 42 years. I will place this 'ethnography' into an analytic framework based on the power relations of the Soviet government, the CPSU (Communist Party of the Soviet Union), and Soviet Academia at large. Secondly, I presume that at least some of us wonder if there were some elements of merit in Soviet ethnography that should be preserved if not revered. And presumably, some also share my rage when fellow Western colleagues and their over eager disciples from the former Soviet Union belittle Soviet ethnography and impute an almost magical meaning to the use of Western terminology. So I also intend to hint at some of the power relations implied in such attitudes.

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Equality But Not in Bravery

The learned societies for ethnography were founded at about the same time in both London and in St. Petersburg, in the mid 19th century, with many of the same type of eccentric scholars whose ideas were hard to prove conclusively. To a certain extent, therefore, we in the West have always been among equals as far as both our ancestral, scholarly pedigrees are concerned.[1] However, even at this time there was already a contrast: it was the exile of some scholars to Siberia by the ruthless Russian Tsarist government of the day that provided some of the best field reports, whereas the British explorers had the full social backing expected by Victorian scholars. This kind of contrast in the degree of political intervention, to a greater or lesser

[1] The Geographical Society of St. Petersburg, founded in 1845, had a department of Ethnography right from the start. A similar society in Great Britain was founded in 1830 with the name Geographical Society of London for the advancement of geographical sciences, under the patronage of King William IV. The various societies that finally became the Royal Anthropological Institute followed suit.

extent, continues to this day. It has always meant that for many (but not all!) Russian and then Soviet scholars, a degree of bravery has marked their personalities in a way we in the West have never been challenged to show.

A further split from Western scholars in the intellectual climate occurred during the early Soviet period, when in Leningrad the Red Professors' Ethnography sector was moved from the Geography department to the History department, in order directly to politicize the subject so as to conform with the new ideology of the Marx-Engels doctrine. This affected the *interpretation* of data rather than the *quality* of the data itself, which, after all, remains the hallmark of good work everywhere. As a British scholar who attended university in the late 1960s, I lived through functionalism, Marxism, socio-structuralism, gallo-structuralism (as Meyer Fortes called Lévi-Strauss's ideas), and other schools of interpretation, but of course with the caveat that I would not be imprisoned if I challenged them!

Not prison, but more subtle punishments awaited colleagues who spoke their minds by the time I first visited the Soviet Union in 1968 as a second year undergraduate student from the University of Kent. I have fond memories of conversations in the many years that followed with female colleagues in the toilets and with males in the corridors of the old Institute of Ethnography on Dmitrii Ul'ianov Street in Moscow. Ethnographically speaking, I think that the lack of quasi-secret places in the new building to which the authorities moved the whole Institute, by Gagarin Square, would have seriously impeded frank exchanges! It took the creaky old building's nooks and crannies to foster the possibility of deconstructing formal declarations and to earn the trust of valued colleagues. I am so grateful that I was able thus to converse informally with ethnographers like Averkieva and Kabo who had been in Gulags for one reason or another and who were happier to talk discreetly.

However, for Soviet ethnographers, the important point is this: the result of Sovietization was not that they embraced Marxism *per se* but that the Sovietization of scholarship led them to view ethnography as a branch of history. As Gellner put it: Western anthropologists look at a society which has a history like a comet that has a tail; Soviet anthropologists were only interested in a society in the present day, insofar as it would help them understand that society's tail — its history, the main source of fascination and devotion.

For example, when the late Vladimir Basilov was my guest in London, we went to the Victoria and Albert Museum, and as we were walking through a long and splendid corridor with artefacts on either side from India, he suddenly started shouting. Embarrassed, I rushed up to him. Very agitatedly, he explained that no, he wasn't ill, nobody had pushed him, none of

that — he was jumping up and down in ecstasy because an ancient Indian engraving had just proved to him that the violin was Scythian in origin! He had always suspected it, but now he had the evidence. A happier person for the rest of the day was hard to imagine!

Organization of Soviet Science

The organization of scientific and academic life in the USSR followed strict patterns: the USSR Academy of Sciences was based in Moscow and everywhere else was subsumed to it. The censor's office was also in Moscow. It goes without saying that this pattern was simply a reflection of the power structures of the USSR, where the ruling Communist Party was also Moscow-based.

The myth propagated officially and believed by some of the Moscow scholars themselves was that the USSR Institute of Ethnography would somehow be the centre of Soviet excellence. In reality, from the time I became more closely involved in the early 1970s and onwards, it was a question of some scholars managing to get Moscow residency permits (severely restricted if you moved there from elsewhere) who became USSR Institute scholars and those who didn't and who therefore found themselves in the outlying republics with their own Institutes or Departments, and who would immediately have a subordinate position. In effect, what should have been a RSFSR Institute (serving the Russian Federation alone) doubled as the USSR Institute (with authority over all the other fourteen Soviet Union Republics).

The greatest impact of this arrangement is that ethnographers in the outlying Union republics were not allowed to study peoples other than those from their own regions.[2] If you wanted to study Japanese or even Siberian peoples, you had to be in Moscow or Leningrad. This deprived the others of the opportunity to debate comparative materials, let alone experience fieldwork in an alien culture. The inward looking focus of ethnographers in the outer Union Republics should never be viewed in isolation of the repressive framework of the organization of Soviet Science.

The scholars of the Republics and those of the USSR Academy of Sciences' Institute of Ethnography shared the edicts of the Communist Party of the Soviet Union (CPSU): theme-based five-year plans proclaimed at CPSU congresses. One of these, for example, was 'The Scientific Techno-

[2] The immediately adjacent territory, just outside your Union republic, in exceptional circumstances could be studied with official permission, such as some Georgian expeditions to the North Caucasus. Likewise, North Caucasian or Azerbaijani ethnography students occasionally studied in Tbilisi, albeit in application to their own local materials.

logical Revolution'. Whatever your fieldwork interest, you were obliged to publish at least something on that subject, however much you had to twist your data. This was not devotional adherence to Party Hack dogma; it was a survival strategy for every individual.

Central Authority and Patron-Client Relations

My participant observation of local scholars in the Union republics was mostly in Georgia but also in Azerbaijan, briefly in Armenia, and longer in Tajikistan, where I worked in 1979, and then also, of course, at length in Moscow and a short time in Leningrad. As you can perhaps imagine, as a third generation British citizen but with my name clearly indicating we had originally come from a place which was now in the Soviet Union, my own ambiguous identity was not lost on my colleagues. USSR Institute Director, Iulian V. Bromlei said as much to me directly: Soviet colleagues could speak their mind to me since I would never betray them because of the personal consequences disloyalty could have on my own Soviet relatives. However, I like to think that my professionalism in never passing value judgements and being an expert secret keeper gave me greater access to the expression of opinions on all sides than the facts of my mixed identity. So these experiences of listening are where I will draw my examples of the dynamics of subordination and authority among ethnographers.

Georgia had its own school of ethnography, and it was not brought there by Soviet power as in some of the other republics, nor was it brought there by Russians.

Thus Georgia contrasted with some of the other republics that were dependant on the good will of their superiors in Russia. In some of them, it was necessary to bring your doctoral thesis to be examined in Moscow, and this created its own dynamics of subtle patron-client relations.

I accompanied the preparation of *plov* (a rice dish) on its journey by air from Dushanbe to Moscow to be brought to the Moscow Institute's Central Asian Sector. The dish was always received and enjoyed by all, and the Central Asian scholar felt that this would help his doctorate to be accepted. Likewise, local scholars everywhere felt obliged to be in attendance when Moscow-based scholars came to do their fieldwork in the republics. Of course there were exceptions. Here, again I can cite Vladimir Basilov, who went year after year to remote areas of Central Asia on his own, and who integrated into local life, to the extent that when there were shortages throughout the Soviet Union — for example, black pepper, needed for making wedding *plov* — he arranged for it to be brought from Moscow with as much seriousness as if his own life depended on it.

It is also important to note that the Moscow scholars who returned each year on expeditions developed close personal relations with local scholars and villagers. As Tat'iana Zhdanko said in an interview with Valerii Tishkov in his book *Conversations with Ethnographers* (2008: 22): 'We knew these people, we watched their children grow and their children's children'.

These specialists in Moscow also found it very annoying when non-specialists from different academic Institutes in Moscow went down to the Republics on other business like 'Party Line Inspectorate' missions because they could undo much of the trust that was built up over the years by the fieldworkers. However, nobody could escape the edicts of the CPSU.

One cannot ever point the finger at any individual Moscow ethnographer and accuse him or her of deliberate arrogance towards colleagues in the Republics since it was the system itself that created unequal relationships, not the individual.

Everyone in the Soviet Union had to send a summary of their 'candidate' or 'professorial doctoral' thesis to Moscow to be approved before they were granted a degree, a source of deep resentment for scholars from the Republics. These summaries had to be approved even when they came from Republics where scholars' dissertations could be examined and approved locally. The positive side of this arrangement, however, was that by having their degrees finally approved or dismissed by complete outsiders, scholars from the outlying republics could avoid local intrigues. Interference in the contents, however, by local and Moscow censors alike was usually along Communist Party (CP) lines. For example, one might be asked to remove references to marriage before the age of 18 because the CP would not admit that this practice existed. Data about marriages of 15-year-olds were usually not falsified, but simply omitted. Incidentally, it is this kind of example which illustrates that, on the whole, one cannot say that data were altered and falsified, but that it was simply removed. The focus was on the interpretation of data and commentary, where some ideological point presented an impediment to the Party line of the day. Worse of course was when some petty dispute between Moscow scholars resulted in theses from the Republics being held up for ages. However, the blending of scholarship with CP monitoring resulted in the identity of these Moscow decision makers sometimes remaining unknown, which increased the local scholars' anxieties in the Union Republics. Their sense of powerlessness was acute at times, in a way only colonized peoples have experienced elsewhere.

There was, however, a specifically Soviet aspect in the dynamics between the centre and the periphery that did not have a parallel, or at least not much of one, in Western colonial or neo-colonial experience. It was this:

local scholars sometimes had a higher standard of living than their influential Moscow visitors, at least as far as their diet was concerned. String bags (*setka*) were often brought by these important visitors as standard expedition equipment, to carry back fruit to Moscow. Often the produce first had been presented as gifts by villagers to the local scholars who then felt obliged to hand these gifts over to their Russian guests.

Unfortunately, whether these scholars liked it or not, it was Moscow authorities who were dispatched wherever there was suspicion that a local Academy of Sciences was not being sufficiently vigilant in towing the Party line. For example, a young Georgian wrote a thesis that was accepted by the local History Institute, but it was about the independent Georgian (Menshevik) government's reforms of 1918-21. This resulted in a Moscow commission coming down to threaten the Institute's authorities, wanting it to remove from their jobs those academics who had been on the examining board.

One of the most hurtful aspects of the unequal privileges held by Moscow and non-Moscow scholars was that the former got their pick of opportunities to travel abroad to international congresses. This was of course decided by the CP apparatchiks. Indeed, on their one and only chance to travel abroad, Giorgi Chitaia and Vera Bardavelidze, among the most distinguished scholars the whole Soviet Union ever knew, were booked to travel to Japan to attend the International Congress of Ethnographic Science. They had flown from Tbilisi to Moscow and had actually boarded the plane to Japan, and — if you can believe it! — they were suddenly escorted off the plane with no explanation except that they later learned that a factory director had curried favour with the CP in order to attend the Congress in their place, even reading aloud Chitaia's lecture!

Importantly, once again, my purpose in relating this event is to demonstrate that it was the tyranny of the CP and the Soviet government and not individual Moscow Institute of Ethnography scholars who humiliated the ethnographers of the Union republics in the period preceding *perestroika*.

Theoretical developments

The rigidity of the historical principle (*printsip istorizma*) in the analysis of ethnographic data, which the CP demanded, was, in fact, readily embraced by scholars in the Republics. This was partly because in Central Asia and the Caucasus, Russia was officially presented as a benevolent elder brother with a superior culture from which much could be learned. In all the Caucasus Republics and to a lesser extent in Central Asia (in Uzbekistan and especially in Tajikistan), the emotive dimension of the use of analysis according to the 'historical principle' was that they believed it confirmed, as subtly as possible, that their own cultures were more ancient than that of the Russians.

At least this was the unspoken subtext, mostly missed by their Russian colleagues. Whereas my use of data on funeral rites in Georgia focussed on contemporary belief systems and kinship relations, my Georgian colleagues were keen to trace these rites back to Gilgamesh and beyond to demonstrate that Georgia fully participated in the civilizations of antiquity, a claim that few other traditions could make.

Kinship theory was subsumed under the Marxist stages of history schema devised by Engels, and here again, good data was interpreted accordingly. The interpretation of kinship data had such a long and varied history in the Soviet Union and in 19th century Russian scholarship that it cannot be addressed comprehensively here. However, the fieldwork data was not fudged data, although it was usually obtained with data-gathering techniques different than those that were characteristic of British practice based on participant observation. They often started with a list of vocabulary used in kinship relations, followed by questions about marriage customs and prohibitions. Then, there were questions about property, dowry, and bridewealth. And then, in writing up, these scholars compared their data to ancient notions of marriage, or seized on symbols of wedding rites or pieces of kinship vocabulary to reconstruct what might have been an evolution from matriarchal to patriarchal kinship systems as described by Engels. However, when I lectured on kinship in Georgia and did our usual kinship diagrams, they were readily understood! They just were not interested in using my data as I did for understanding the social dynamics of a village in the 'ethnographic present'.

Again, I have two publications on fieldwork in the USSR (Dragadze 1978, 1987), the latter of which focussed on the power of the historical principle in affecting local approaches to fieldwork. In Georgia, I led with Lia Melikishvili several summer expeditions where students did fieldwork to satisfy both the synchronic and historical approaches without much difficulty. Good fieldwork overrides theoretical approach.

After all, was it not left to none other than that excellent and highly experienced fieldworker Lia Melikishvili, head of the Department of Caucasian Ethnography at the Georgian Academy of Sciences, on the cusp of U.S. military involvement, to find out exactly what was going on in the Pankissi Pass at the height of the Chechen War. She was able to draw on local contacts she had developed over decades and her keen eye and leadership skills.

In their attitudes towards me personally, however, it amazed Russian scholars and disappointed the best of them, such as Sukhareva (see Naumova in this volume), that I was not immersed in the history of the peoples I studied (the Tajiks in particular in her case), and that without a more diachronic, holistic approach, I would miss the meaning of what I was studying,

namely, the impact of *kolkhoz* legal and organizational systems on rural families and on their size. Surely I was not trying to be a sociologist! How could I understand the meaning of childbirth to a Tajik without knowing their history? Analysis using an 'ethnographic present', even with my eye on social change, was nothing but partial, scrappy study if one did not first go back at least a millennium!

A further characteristic of Soviet ethnography was the prominence given to material culture, which included the study of artefacts and their evolution, such as ploughs used at high altitudes in Georgia, agricultural and building techniques and their symbolism, rituals that accompanied their use, and so on. These studies need to be understood as more than mere expressions of Marxist dogma or its avoidance. Instead, they should be understood as examples of the 'complex-intensive' method, to use a favourite term of Giorgi Chitaia.

The role of material culture and the 'complex-intensive method' had to be viewed in action! I encountered this different kind of holistic approach to fieldwork (i.e. different from how I had been taught in England) in my interactions with one of its greatest practitioners in the former Soviet Union, the late Atiga Izmailova, a little published but very accomplished ethnographer in Azerbaijan. Paul Stirling, on his visit to the Caucasus, accompanied her to a few villages and told me that he was deeply impressed by her mastery of fieldwork methods and her subtle, penetrating approach.

When I worked under Izmailova in the village of Charhan, Shemakha District, she insisted that we record all the dishes made by women from flour: bread, dough, noodles, *dushbara*, the whole lot. When I asked her if this was necessary, she replied that in a society where having or not having flour was vital to daily life, one had to know why! Without a deep knowledge of material culture, a study would be incomplete. Material culture studies could be used to provide a 'material base' on which we could build our knowledge of the religious, family, and political life of the village people we studied, but in a way that reflected their own thinking, not ours, and not Marx's. With Atiga Izmailova, until we had copied out every single bit of census data from the archives held by the village head, we could ask about neither household life, nor income, nor their rituals nor any other point of interest. Official Party line and Marxist teachings notwithstanding, in her view nothing could be skipped; for gathering data there was no such word as 'irrelevant'! Compared to keen observation, just asking questions was useless. And the historical principle? That we would handle when we got back to Baku! In fact, the very density of our data resulted in the obligation to include Marxist piety being virtually lost.

Ethnos theory

I do not want to leave out the impact of ethnos theory in my discussion of Soviet Ethnography (Dragadze 1980, 1990). In republics like Georgia, the perceived need for national survival in the face of Sovietization was a wholly encompassing task in itself; creating generalizing theory that would have to be approved by Moscow-based CP officials was not something for which these ethnographers had much interest. The importance of recording local phenomena that were rapidly disappearing was considered more important than creating theory acceptable to the CPSU.

This did not bother Muscovites at all. As Viktor Kozlov said so candidly and without being at all aware of the colonial implications — I knew him quite well — 'You in the republics send us the materials, and we will provide the theory'.

The idea of *Homo Sovieticus* was beginning to be thought about in the Politbureau towards the end of the 1960s and early 1970s. Bromlei (1969) wrote his seminal article on the role of endogamy in maintaining ethnic boundaries and identity and, he told me, only afterwards did he realize the enormity of what he had done. He expected that he might be expelled and was amazed that in fact he survived, possibly because the CP did not quite know what to make of it.

Some time after, however, a new bomb was dropped by the Moscow Institute of Ethnography: Iurii Vartanovich Arutiunian's edited collection *The Social and the National* (*Sotsial'noe i natsional'noe* 1972). It exposed for the first time the identity dynamics of Tatars and Russians in Tatarstan, their life chances, the role of Russian language skills in these life chances, and other factors.

I happened to follow a few weeks later in two different villages in Mingrelia in Georgia, after Arutiunian's team's previous visit to Georgia to do the same kind of research. If his finding had been accurate in Tatarstan, well and good, but the locals and their Tbilisi interpreters led Arutiunian's research workers astray.

A farmer told me: 'What? Would I mind if my daughter married a Russian, he asks? Tell the son of a bitch *mama dzaghli* whatever he wants to hear: say I'd love it!' I cannot stress enough the local lack of trust in the ulterior motives of this Moscow program.

Ethnos theory was raised (or 'godfathered'!) by Iulian Bromlei, in its most recent Soviet appearance. It was welcomed as a way of circumventing the restrictions of class analysis. The beauty of ethnos theory was that indeed one criteria of an ethnos was that it should encompass more than one class. In Moscow academic circles, this was much appreciated.

This was not the case in the Caucasus. Scholars there were fearful that ethnos theory would be used by Russians to 'divide and rule' by ascribing different ethnos affiliation to, say, Mingrelians and Georgians or Svans and Georgians, and this activity would certainly spell the death knell for any attempt at creating a Georgian notion of citizenship that could be embraced by Abkhazians and other ethnic minorities within the Republic. While pondering all this, perestroika arrived, national movements developed, and calls for independence in the Union Republics grew as a result. Here unfortunately, some Moscow ethnographers played a rather irresponsible role. Igor' Krupnik and Galina Staravoitova began pressing for 'cultural autonomies'. They envisaged around 200 cultural autonomies that would be more or less self-governing. However, when I directly asked Igor' Krupnik who would be the ultimate coordinator, where would the ultimate seat of that coordinating power be, he replied, 'Of course in Moscow, Tamara!' That for me gave the game away.

Galina Staravoitova descended into Azerbaijan to tell the Lezghins to create their 'cultural autonomy' as she did in Abkhazia and Nagorno Karabagh. These brave gestures might have been well intentioned, and we must not speak ill of the dead, but they went directly against the Union Republics' desire to become independent citizen-based sovereign countries. These attempts were interpreted locally as Russian sabotage of the independence movements, nothing less. This is sad but true. By the time of independence it is exactly what happened, when ethnic affiliation, combined with the legacy of Stalin's territorialization of ethnicity (see Dragadze 1993), was used to challenge and virtually break up the territorial integrity of Union Republics in Georgia and elsewhere.

During *perestroika*'s last days before the Soviet Union finally ended, one of the new sectors was set up 'for the study of ethnic processes' in the Moscow Institute of Ethnography (now re-named the Moscow Institute of Ethnology and Anthropology). Unfortunately, without informing their Georgian colleagues, a group from the Institute's new sector travelled to Georgia to visit the Dukhobor/Malakan community of Russian believers, originally exiled to the Caucasus by Catherine the Great, and who had lived there continuously from that time, to find out if they were being discriminated against and repressed by the Georgians locally and by the Soviet Georgian Republic's authorities, who were still Communist but believed by Moscow to be bowing to popular nationalist sentiment that 'Georgia should be for Georgians'. This clandestine visit was viewed as a political act by Tbilisi ethnographers. Interestingly, not a single member of the Caucasus sector of the Moscow Institute of Ethnography was invited to come on this mission by their colleagues from the same Institute. This was completely a mission

masked in theoretical clothes of the 'ethnos focus of study' (Dragadze, 1999). It happened in the wake of a massacre of Georgian independence demonstrators by the Soviet military in April 1989, and the researchers' claims that their mission was to study 'ethnic processes' fell on deaf ears, as much among the Caucasus specialists in the Moscow Institute as in the entire academic community in Georgia. Twenty years have passed, and this incident has probably been forgotten, but I have touched on it now as it demonstrates some of the dynamics, once again, as they existed all those years ago.

In fact, ethnographers from the 'Northern brotherly neighbour' appeared exclusively to focus on minorities in the South Caucasus, and rarely on the majority populations within these outlying republics, a practice maintained to this day. And given the military consequences of this focus, it is something that post-Soviet ethnographers should reflect on.

The Undermining of Ethnographers

A different set of problems that perestroika and the end of the Soviet Union brought about was that a host of Moscow scholars such as Arabist historians and linguists in the Oriental Institute and other institutes converted overnight into ethnographers and specialists on Chechens or other 'juicy' people. They sometimes had better access to the corridors of government power than the ethnographers and some gave dreadful advice. Viewing their work really does help one to appreciate the scholarship that most ethnographers produced in comparison with some of these converts. I believe the ethnographers at the present Institute of Ethnology in Moscow deserve as much support as possible to maintain their pre-eminence, compared to other institutions, in the study of the regions that interest us.

Power Relations between Western and Formerly Soviet Scholars

The fading away of Soviet power and the Soviet Union was accompanied by a fading away of funding for their scholars and, in our case, for anthropologists. It coincided with an opening of access to Western scholars to the most remote republics and regions to which previously only some of us had had access and only with great difficulty. So, enter the Western scholar and Western funding! Enter Soros, enter the 'peace industry', enter the helping hand from the West for introducing democracy, civil society, and value free anthropology. 'Value free anthropology', I ask myself, can we from the West really introduce it, however well-meaning we are?

We come in and change their vocabulary: ethnographer must become 'social anthropologist', and in Georgia, during the massive reforms of the educational and scholarly institutions under President Saakashvili, the 're-

formed' university jobs went to those who used the word 'anthropology' and not 'ethnography'. When you look at applications by formerly Soviet 'anthropologists' for funding from Western foundations, there is a fantastic use of 'politically correct' (in Western eyes) vocabulary that is almost touching. But it worries me.

And furthermore some of us in the West apply for travel and teaching grants from our own governments, going so far as to state that social anthropology never existed in the Soviet Union and that we must teach our science to this indoctrinated academic community. Such statements are more likely to secure funding than saying: 'I'd like some funding because although there is a strong school of ethnography I want to exchange my views and perhaps teach some courses so that they will have more choice'. Such sentiments won't get much funding![3]

The 'peace industry' needs Western anthropologists to look at minority dynamics, to conduct 'early conflict monitoring', and to do all the teaching of good citizenship activities. There are jobs out there for our girls and boys in the West that did not exist in the Soviet period. And it is always us Westerners teaching them in the former Soviet Union, us Westerners helping them and not us going to be trained by them in any single field. And these are terms the totally under-funded, formerly Soviet academic institutions, especially in the poorer outlying republics, are obliged to accept.[4] What we Western scholars do not seem to do is to empower our colleagues to find their own vocabulary in these worthy activities instead of parroting Western approaches.

Because we have more funding possibilities in the West for our colleagues in the former Soviet Union republics than they themselves have at home, are we slowly replacing Moscow? Are we as unconscious of our dominance as the Muscovites used to be? Can this publication on Soviet ethnography at least serve the purpose of making us Western scholars aware that perhaps some of our surviving colleagues in the former Soviet Union have sufficient scholarship and intellectual acumen for us to listen carefully to them and that we make space to allow our colleagues there to challenge our concepts and terminology? Can we let them do this before they die? Is no synergy possible between our two traditions (or more than two traditions, given the diversity inside Western anthropology in the US, France, Ger-

[3] See Michal Buchowski (unpublished) on how Eastern European anthropology either is ignored or requires improvement by Western anthropologists, although he does not seem to include former Soviet works as worthwhile Eastern European scholarship.

[4] Often individual scholars' incomes in the former Soviet Union depend on doing research chosen as relevant by their Western colleagues but which they themselves see as having 'missed the point' or of no significance.

many, the UK, and diversity also within Russia and other former Soviet Republics) when these traditions are understood by most Western scholars through an 'us against them' perspective.

Above all, can we Western specialists on the Caucasus and Central Asia become less lazy? Can we stop accepting Western works for publication in which hardly a single local scholar is cited? Can we learn the languages that are meaningful to the people we study and not get by with those tongues we happen to know? Can we start to apply the standards of anthropologists who are Africanists or Latin Americanists, for example, to our own community of scholars of Central Asia and the Caucasus? Can such standards include, as they do, a region-specific choice of terminology?

Can we perhaps, in this way, gain some respect from our colleagues in these regions rather than achieve uncritical adulation or else provoke partially justifiable resentment?

The answer is: 'Yes we can!' I live in hope!

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Chapter 3
Survival Strategies: Reflections on the Notion of Religious 'Survivals' in Soviet Ethnographic Studies of Muslim Religious Life in Central Asia*

Devin DeWeese

The following discussion reflects the considerations of a non-ethnographer who is nevertheless a frequent user and consumer of ethnographic literature, from a historian's perspective; it is drawn in part from a broader study of the many problematical assumptions, approaches, and entrenched misconceptions that continue to shape the study of Islam in Central Asia. The interpretative strategy based on the notion of religious survivals is just one of these, but I believe it is one of the most widespread, and one of the most insidious, in terms of its effects on our understanding of both historical and contemporary religious life. It is certainly the one most closely linked with the theme of Soviet-era ethnography.

The idea that contemporary religious life, as observed by ethnographers, should be interpreted in terms of externally imposed obligations of a narrowly understood 'Islam' competing with so-called 'survivals' (in Russian, *perezhitki*, a favourite term of Soviet writers on religion) — i.e. only partially submerged practices and beliefs (whether viewed as monolithic or as layered) originating in pre-Islamic religious systems that were shaped by earlier socioeconomic conditions — constitutes, I believe, one of the most pervasive, enduring, and yet insufficiently problematized aspects of Soviet ethnographic scholarship. Instead of being explicitly addressed as an interpretative problem, subject to either attack or defence, it was, as a rule, simply assumed; and yet the notion of survivals came to be especially deeply entrenched as a foundation of Soviet ethnography, in part because it fit well with the evolutionary model that underlay Soviet ideology, and in part, I

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believe, because of its usefulness in discrediting contemporary religious practice and attachment.

What is particularly problematic with this strategy, however, is that it has been generalized and has assumed the status of a sort of 'default' characterization of Islam in Central Asia. That is, the notion of religious survivals has shifted from being an assumption that was to be applied to specific instances of practices that supposedly do not belong in 'Islam', to become a blanket characterization of the quality of Islam in Central Asia; it is the preface, in effect, to the conclusion that Islam in Central Asia, as a mixture of Islam and pre-Islamic practices, is not really Islam at all, but something else. The notion of survivals thus became a simple shorthand for 'explaining' the defective, substandard, tainted, hybrid Islam determined to be characteristic of Central Asia, whether that characterization inspired disgust or hope.

In contrast, I would argue, first, that specific instances of rites or beliefs labelled 'survivals' from pre-Islamic times most often cannot be shown actually to be such survivals: claims that a particular rite or belief is a survival are most often poorly evidenced and poorly sourced, and are typically based more on the argued absence of a particular phenomenon from an essentialized 'normative' Islam than on any clear or even clearly arguable historical trajectory from pre-Islamic times to the present. Second, and more broadly, I would argue not that religious forms and meanings never 'survive' across religious boundaries — they unquestionably and all too obviously do — but that our analytical task cannot end with merely pointing out such survivals (and, by extension, that the 'analytical' motives for applying the notion of survivals all too often mask political and polemical motives that aim to discredit or delegitimate particular practices). Such survivals are to be expected; what we should marvel at is not their existence, but rather the remarkable flexibility and creative dynamism of those who make something out of them. This — the development of meaning attached to such 'survivals' — is what the survival strategy overlooks. The rhetoric of religious survivals must pass the 'so what?' test: assuming we can identify a belief or a practice as a survival from some earlier religious system — a proposition that is in fact much more difficult than most applications of the notion of survivals allow — then the question is, so what? What do we claim that this fact shows us? What do we do with it? All too often the notion of survivals is enlisted to argue broader conclusions (including polemical ones) that cannot be sustained in their own right, and cannot be strengthened through the faulty notion of 'survivals' either.

I believe, moreover, that addressing the notion of survivals directly is important, precisely because it has not been identified as a fundamental

problem within Soviet and Soviet-inspired ethnography. Indeed, even Western scholars, whether historians or social scientists, with an interest in Central Asia have fallen into the trap of using this notion, almost reflexively, since it sounds reasonable and, indeed, even scientific; some have been no more aware of the longstanding critiques of the theory, or of more recent, historically-oriented reminders of the notion's flaws that have emerged over the past 30 years, than were Soviet specialists trained in this theory.[1]

The present chapter cannot provide a full analysis or critique of the notion of survivals. What I hope to do in the space available is to offer some thoughts on the multiple routes by which the notion of survivals entered Soviet scholarship, and on the reasons why it emerged as a default position among the Soviet-era canons of ethnographic interpretation. I would argue that the preservation of this notion in Soviet ethnography, long after its abandonment elsewhere, was chiefly a result of its usefulness to the anti-religious policies of the Soviet government, and that its continuing survival reflects a still wider range of ideological perspectives in which the notion is analytically useful, rather than any scholarly merit or explanatory power it might have. A brief look at specific cases of 'survivals' in need of re-evaluation will then precede final comments on what I believe is a key problem with the notion of survivals: the false promise of historical grounding it seems to offer to those engaged in the ethnographic present.

The Roots of Survivals from Protestant Polemics to Anthropological Theory

The interpretative strategy based on the notion of religious survivals has yet to be subjected to a thorough historical study.[2] The closest thing we have to such a work is Margaret Hodgen's book from 1936, which primarily focused on the role of survivals in evolutionary anthropological theories emerging in the second half of the nineteenth century (Hodgen 1936; cf. Hodgen 1931). This was over 70 years ago; and it was 30 years ago that Jonathan Z. Smith, a scholar of religion, promised a special study of the notion of survivals (Smith 1978: 260-261), but as far as I am aware, despite Smith's many

[1] For example, in a 'post-Sovietological' work focused on policy issues, we are reminded that Sufi groups in Central Asia maintained practices that 'included old shamanic rites deriving from the nomadic tribes' (Roy 2000: 147), and that in 'tribal zones', 'Islam was late *in being imposed*' and came through 'Sufi brotherhoods such as the Yasawiyya', which, we are reminded, 'includes elements deriving from the shamanistic traditions of Turkic nomads' (143-144, my emphasis).

[2] A promising step in this direction, with direct relevance for Russian and Soviet scholarship, is the work of Eden 2011.

contributions, this promise has not yet been fulfilled. Smith has shown elsewhere, nevertheless, that the roots of the notion of survivals, as employed in scholarship on religion, go back much further, and may be traced — with particular relevance, I would argue, for the case of Islam in Central Asia — to Protestant critiques of Catholic tradition that decried the latter as a mixture of pagan rites of various origins with a mere veneer of Christianity (Smith 1987: 96-103). Smith's discussion is doubly important insofar as it highlights the ways in which the Protestant critiques, with their emphasis on analyzing Catholic tradition in terms of religious survivals, looked backwards as much as they looked around, at contemporary society. That is, the notion of survivals, as expressed in this context, and indeed, it may be suggested, in its essential logic, entails disgust with the present as well as a subjective valorisation of the past.

Smith discusses a work by a Cambridge theologian from the early eighteenth century, Conyers Middleton, the title of which should remind us of the many Soviet-era titles invoking the notion of survivals: *A Letter from Rome showing an Exact Conformity between Popery and Paganism: or, The Religion of the Present Romans to be derived entirely from that of their Heathen Ancestors* (London, 1729). As Smith notes, Middleton described his realization, upon witnessing the rites of 'Popery' in Rome, that he was seeing essentially the same rites (being performed in the same places) as he had seen described in classical texts that recorded the rites of 'Primitive Paganism' (Smith 1987: 97). Though it was by no means Smith's central argument, he drew attention to the dual underpinnings of Middleton's hostile reaction to what he saw. Middleton, to be sure, laid out a familiar Protestant critique of what he regarded as Catholicism's excessive devotion to 'empty' rituals, and he based his critique in large measure upon his claim that those rituals were 'copied' from pagan rites. But Middleton was also repelled by his certainty that he was seeing a fundamentally corrupted version of those ancient rites: while he had looked forward to finding genuine monuments of ancient, pagan Rome, and thus walking where 'Great Heroes of Antiquity' had walked, he was instead overcome by 'the Fopperies and ridiculous Ceremonies of the present Religion of the Place' (ibid.). As Smith writes, Middleton's 'beloved past was alive in contemporary Roman Catholic ritual, but distorted, as in a fun-house mirror'. While we may be sure that Middleton's aim was the polemic against Catholicism, the intimation that 'the present religion of the place' was effectively interfering with, in Smith's words, 'his communion with the antique past' is equally relevant for the case of Central Asia in Tsarist, Soviet, and even post-Soviet times; the antiquarian and archaeological focus of much scholarly interest in Central Asia (with the romance of the 'Silk Road' added as well), including even ethnographic

scholarship, yields an often barely-concealed frustration with contemporary religious practice, which is summarily declared to amount to survivals of ancient rites, but more precisely to distorted and corrupted survivals, tainted by 'the present religion of the place', Islam.[3]

We may find another foreshadowing of Soviet narratives of religious survivals, rooted in the 'imperfect' conversion of Central Asian peoples, in the anti-Catholic rhetoric of another eighteenth century Protestant author. In 1774, a Reverend Joseph Brand reminded his readers how Catholic monks had facilitated the preservation of such survivals, which he termed 'antiquities', as they lured pagans from their old ways by substituting a 'profusion of childish rites, pageants, and ceremonies' that reflected an effort 'to twist flowers' around the 'shackles' of Popery (cited in Stocking 1987: 54). These survivals, Brand explained further, were of three types: beliefs and rites linked to the yearly cycle of the calendar; life-cycle rites of rural folk, 'from christening to wake'; and beliefs about witches, charms, ghosts, etc. These three types overlap well with the categories of rites and beliefs typically 'explained' in terms of religious survivals in Central Asian contexts as well.

The widespread currency, in Europe, of such attitudes toward religion and ritual, rooted in the political and religious critique of Catholic tradition, helped ensure the adoption and expansion of the notion of survivals as an explanatory paradigm in Enlightenment-era critiques of religion, in the beginnings of scholarship on religion, and especially in the evolutionary models of religious and cultural development formulated in the second half of the nineteenth century. Here, the key voice was that of Edward Burnett Tylor, whose *Primitive Culture*, published in 1871, served as an influential formulation of the evolutionary approach that interpreted contemporary phenomena, observed today, in terms of the primitive cultures in which they had presumably originated.[4] According to Tylor's definition, survivals are 'processes, customs, opinions and so forth, which have been carried on by force of habit into a new state of society different from that in which they had their original home, and they thus remain as proofs and examples of an older condition of culture out of which a new one has been evolved' (Tylor 1871: 16). Tylor's work served to cement the term 'survivals' into anthropological theory. It also originated the notion of 'animism', setting its course

[3] The problematic 'interface' between contemporary life and a particular envisioning of history is indeed one of the key difficulties with the notion of survivals. Central Asia has often been approached, especially in Western antiquarian and archaeological scholarship, as a palimpsest to be examined for traces of earlier religious 'texts' that had been effaced, but only imperfectly, by Islam.

[4] It may be worth noting, by way of tracing the thread discussed earlier, that Tylor (d. 1917) was born into a Quaker family and was deeply anti-Catholic.

toward being reified as a primitive, if not always *the* original, stage of religion, the traces or 'survivals' of which may still be found today.

Tylor's notion of survivals was profoundly influential through the end of the nineteenth century and into the early twentieth; its impact is evident in James Frazer's *Golden Bough* (1890), and in a host of evolutionary theories of culture explored in George Stocking's survey of *Victorian Anthropology* (1987). Stocking also notes other views of survivals from the mid- to late nineteenth century that remind us of the range of contexts, some far from salubrious but nevertheless relevant for our times, in which the doctrine of survivals can be found useful. A case in point is the work of Henry Sumner Maine, who in effect limited Tylor's notion of survivals to particular 'historical' racial frameworks, in line with his view of the progress of the Aryan race. Survivals, according to Maine, must be traced on the basis of a historical continuity, and that continuity must be framed in ethnic and, indeed, virtually genealogical terms; hence one need not look to non-Aryan races for the origins of Aryan rites (Stocking 1987: 127-8). Such views recur less often in Soviet-era anthropology than they do in post-Soviet nationalist permutations of Soviet ethnographic traditions and 'habits of mind', where they yield, for example, claims about the tenacity of 'survivals' from old Turkic religion in the religious lives of contemporary Turkic peoples.

The chief irony of Tylor's doctrine of survivals, and indeed of its influence, lies, perhaps, in the historical claims it implied, and the wedding of such historical claims with contemporary ethnographic observation and description. For Tylor, the doctrine of survivals was intended primarily to provide a framework for deducing earlier conditions of mankind on the basis of phenomena presumed to survive from earlier periods; the criterion by which those phenomena deemed to be survivals were selected was the absurdity and uselessness of these practices or beliefs in the present, which signalled, for Tylor, that they must have originated at a time, and in conditions, in which they did make sense (Stocking 1987: 162-163).[5] The doctrine of survivals thus looked backwards, to a reconstruction of the past; that it simultaneously 'explained' phenomena observed in the present was, it seems, secondary for Tylor (who did not, in any case, focus exclusively on *religious* survivals), but it was certainly assumed to do so, by virtue of the criterion by which those phenomena were known to be survivals, namely, their meaninglessness. In effect, the reasoning was circular. The notion that such survivals might have meaning or function in the present was not entertained; this was one of the weakest aspects of the notion of survivals as elaborated by Tylor, but it is precisely this aspect that was necessarily pre-

[5] The notion of survivals is discussed, as one of the four or five key aspects of the ideas Tylor advanced in *Primitive Culture*, in Morris 1987: 98-102.

served and strengthened in Soviet applications of the notion (one could extend the critique back to Protestant assumptions that Catholic ritual was meaningless to its practitioners, reminding us of the polemical context linking so many applications of the notion of survivals).

The 'survivals' approach was abandoned long ago by most cultural anthropologists in the West; critiques of Tylor, and of evolutionary frameworks more generally, were the subject already of Hodgen's book, noted above, on the *Doctrine of Survivals* (1936), which appeared, however, even before the broader departure from Tylor and other evolutionary models reflected in the work of anthropologists who stressed meaning and function as explanatory focuses. To be sure, religious circles continued to find the notion of survivals useful. Protestant criticism of the empty ritualism and pagan survivals of the Catholic tradition continued, and still continues; more pertinent for present purposes, by the late nineteenth century, we find increasing criticism, among Islamic reformists and 'modernizers', of the 'superstitions' that had crept into Muslim life. Such criticisms, of course, have their own long trajectory, going back to the sort of attacks on innovations and 'popular' practices associated with Ibn Taymiyya[6], and flowering more recently, with 'Protestant' overtones, in Salafist ideologies that posit a golden age of pure Islam before it was spoiled by elements of pre- or extra-Islamic origin.

During the last 30 years, moreover, criticism of the notion of religious survivals in Western historical studies has caught up with its rejection in anthropological scholarship, and such historical critiques have been augmented by anthropologists of religion directly engaged with the Muslim world.[7] Of particular importance is the work of I. M. Lewis, who has directly addressed the problem of 'survivals' in Islam from pre-Islamic African religious traditions, and whose brief but potent article on African 'survivals' in Islam should be required reading for all students of Islam in Central Asia (Lewis 1983). With direct relevance to Central Asia, Bruce Privratsky forcefully argued against the value of the survivals theory with regard to understanding the practice of Islam in contemporary Kazakhstan (Privratsky 2001), rightly noting the link between the survival of the notion of survivals today and the broad application of the notion by the ironic hero of Kazakh national ethnography, Chokan Valikhanov (whose bitter antipathy toward Islam was matched only by his admiration for the Russian brand of European culture); the use of the notion of survivals by Valikhanov, who died in

[6] See Memon 1976. Above all, such 'innovations' include practices associated with saints and shrines, and a host of other magical, calendrical, and life-cycle rites.

[7] The seminal work was the study of Brown 1981; see also Schmitt 1983; Campany 1990; Karamustafa 1993, 1994; Atwood 1996; DeWeese 2000; and Stewart 2001.

1864, reminds us of the notion's currency well before its specific articulation by Tylor.

Survivals and the Soviet Critique of Religion

The major force of the critiques of Tylorean and other evolutionary approaches — including the doctrine of survivals and, obviously, the impact of more recent historically-based critiques — was felt well after these notions were embedded in Marxist doctrine, and in the nascent Soviet academic establishment (which arguably inherited, early on, as much from broader European frameworks of positivist history and evolutionary anthropology, appropriated by Russian academic circles in Tsarist times, as from specifically Marxist adaptations of these frameworks). Without suggesting the work as a model for religious studies, for example, one may readily note the suggestive coincidence of the date of the establishment of the Soviet state with that of the publication of Rudolf Otto's classic contribution to the phenomenological approach to religion, *Das Heilige* (1917), published in English six years later as *The Idea of the Holy* (Otto 1923). Soviet approaches to religion, and to culture more generally, were not entirely calcified at that point, but it is clear that Soviet approaches developed on a quite different trajectory, and largely in isolation, from those that yielded the field of religious studies in the West.

The development of Soviet treatments of religion was outlined a quarter of a century ago in the now-classic work by James Thrower, *Marxist-Leninist 'Scientific Atheism' and the Study of Religion and Atheism in the USSR* (1983), which surveyed the ideological and intellectual underpinnings of Soviet academic studies of religious life, beginning with Marx, Engels, and Lenin.[8] Thrower, to be sure, did not directly address the notion of religious survivals at length; rather, his analysis focused chiefly on philosophical and historical arguments for Soviet/Marxist rejections of the validity of religion, and did not go deeply into the 'genealogy' of Marxist conceptions of religion. For example, in his chapter on 'The Marxist-Leninist Understanding of "Religion"' (Thrower 1983: 335-358), he addressed the Soviet emphasis on the falsity of religion and its class character, with minimal discussion of the historical or ethnographic framework, or of the way in which the notion of survivals was itself employed to delegitimate religious life as an irrational hodgepodge assembled out of religious relics from multi-

[8] Thrower discussed Engels' dependence upon the views advanced by Tylor (pp. 47-48), providing yet another link in the survival of anti-Catholic views. Thrower also produced a phenomenological examination of the religious aspects of Soviet society (Thrower 1992), which stands, along with Tumarkin 1983, as one of the pioneering studies of the Soviet system's religious character.

ple ages. Indeed, one gets the impression that Thrower, too, accepted 'folk practices' as deviations from the preferred intellectual, modern, 'clean' religion (Thrower 1983: 337-339); this impression seems to be confirmed by his acceptance of the identification of certain Muslim practices as pre-Islamic in another of his works (Thrower 2004: 207).[9] His analysis of Soviet works down to the early 1980s, however, is invaluable not only for his extended discussion of the superficiality of Soviet-era 'historical' explanations, in line with scientific atheism, of the rise of historical religions (e.g. Buddhism, Christianity, and Islam)[10], but also for what he does *not* discuss: while he cites explicit defences of Soviet-style evolutionary models for the development of religion, he does not refer to explicit defences of the notion of survivals.

One indeed searches in vain for an actual defence of the notion of survivals, beyond the presentations charted by Thrower, in late Soviet or post-Soviet ethnographic literature; in such works, as in most earlier Soviet treatments, the 'survivals' approach is most often simply assumed, without defence, or else is 'justified' through citations of Marx or Lenin, without any actual engagement with the notion or its applicability in either general or particular contexts. The closest we come to a self-conscious defence of the notion of survivals, indeed, appears in an English article published by V.N. Basilov in 1980, already (it is not mentioned, oddly, by Thrower), and even here the notion is simply stated rather than argued (Basilov 1980). A more explicit defence, which, however, again simply assumes that diverse aspects of 'folk' religion are self-evidently to be understood as survivals from pre-Islamic times, appears in a recent article (also in English) by Dmitriy Mikulskiy, published in 2001.[11] Mikulskiy's article addresses the issue of ideological constraints, but concludes that the scholars he discusses should be recognized as having been 'honestly and objectively doing their jobs', inasmuch as they were 'unconnected with the official atheistic institutions', and that some of what he calls the 'deplorable drawbacks' in their works — which he does not identify — resulted merely from 'an attempt to reshape a traditional society' (a motivation that might in itself lead us to question the objectivity with which they did their jobs, if not perhaps the honesty), and

[9] Published posthumously, this work is unfortunately not in the same class as Thrower's earlier studies, relying as it does overwhelmingly on secondary literature, including Sovietological works and general histories, that could not do justice to the subject. One presumes that it would have been considerably improved but for his untimely death.

[10] See especially Thrower 1983: 278-288, discussing, among other works, S. A. Tokarev's *Religiia v istorii narodov mira* (1964), which Thrower identifies as the first Soviet attempt at a complete Marxist history of religion.

[11] I am indebted to Jeff Eden for a reference to this work.

not from 'their servility to ideological narrow-mindedness' (Mikulskiy 2001: 159).

I would in fact question this assessment, especially with regard to the matter and significance of a 'connection' with 'official' atheistic institutions; perhaps we should recognize the many Soviet ethnographers who contributed to antireligious literature as having been part of 'unofficial atheism'. But if Mikulskiy is correct, his praise for these scholars' 'recognition' of pre-Islamic survivals in the lives of contemporary Central Asians only underscores how deeply ingrained the notion of survivals became in Soviet ethnographic scholarship. Again, this notion did not require demonstration, in specific cases, or even defence as the basis for a reasonable and fruitful analytical approach, but was simply hailed as something Soviet ethnographers could see (despite the best efforts of those contemporary Central Asians, we may suppose, to pretend that these survivals were part of their lives as Muslims). It is, in the end, precisely the unconsciously, or at least uncritically, *assumed* character of the notion of survivals that Mikulskiy's brief comments reveal.

Although to some extent the ubiquity of the notion of survivals as an explanatory strategy in the works of Soviet ethnographers reflects the independent and largely isolated development of Soviet scholarship, it also reflects the ideologically charged status of religion, the intensity of Soviet efforts to undermine it (without properly understanding it), and the sheer organizational weight of the governmental apparatus developed for this end, with institutes, museums, publications, and other structures and personnel for 'outreach' expressly designed to further antireligious work. The notion of survivals furthered this work materially, and I believe this must be acknowledged as one of the chief reasons for the maintenance of the idea throughout the Soviet era. To be sure, once embedded as part of Soviet ideology, the notion of survivals could not be easily jettisoned, and its basic compatibility with the Soviet approach to religion, culture, and history should not be lost sight of. But simple intellectual inertia is not sufficient to explain its persistence or its strength. We may accept, in the language of the notion of survivals, that Marxist thought preserved fossilized forms of the intellectual views, including the doctrine of survivals, that were prevalent in the era in which Marxist thought was first formulated, but we must also be careful to recognize that the notion of survivals did not merely survive as an irrational holdover from the nineteenth-century environment in which Marxist thought developed; rather, it retained a specific functional value for the Soviet regime in its antireligious efforts.

That functional value lay in the fragmentation of religious life made possible through the notion of survivals, which served the goal of demon-

strating not the irrationality of the survival in its present environment, but the irrationality of the religious system that preserved the survival. Through the notion of survivals, and the analysis of religious life it facilitated, Soviet ethnographers and historians could and did supplement the work of anti-religious activists in showing that the religious life of Muslims was filled with non-Muslim practices, and that Central Asian Muslims were more attached to those 'non-Muslim', indeed 'pre-Islamic', practices than to the rites and doctrines of 'orthodox' Islam. The value of this faulty analysis for Soviet policy is clear: 'proving' that elements of popular religious practice had origins outside Islam was believed to help weaken Islam by fragmenting the religious life of Muslims, defining it as a collection of 'relics' and 'survivals', and ignoring the identity of the practitioners as Muslims.

It might be suggested, we should note, that Soviet-era ethnographers were, as a rule, respectful of and sympathetic toward the religious practices of the people they studied, and the people themselves, and were in fact seeking to 'protect' them from the authorities by labelling their practices as something other than Islamic rites. Knowing that the Soviet government might take harsh measures against unauthorized participation in Muslim religious practices, this view suggests, Soviet ethnographers took the pressure off their subjects by identifying what they observed as survivals of pre-Islamic religions, retained as part of the 'national' culture of the people. In this view, the notion of survivals formed an essential part of what we may think of as an ethnographic 'dodge', an interpretative strategy designed by sympathetic researchers as a way to provide 'cover' for their subjects and to save them from harassment by the authorities. Such a claim is, I believe, ultimately unsustainable, but it is also hardly a basis for an admiring endorsement of Soviet ethnography.

It is unsustainable, I would argue, in the face of the frequent participation of ethnographers, noted already, in the antireligious efforts of the Soviet state, and in the face of the palpable condescension and derision directed in some ethnographic literature toward the superstition and irrationality of the gullible practitioners of any and all religious rites, whatever their origin.[12] Moreover, although there is no doubt that some practices identified as national rather than Islamic were indeed rescued, by such an identification, from ideological pressures, debates about such practices, in the Central Asian republics, were more often the province of writers than of ethnogra-

[12] One may reasonably question the respect of an ethnographer for his subjects' religious attitudes when, for instance, he calls for the desecration of their shrines and the excavation of their graves to test a historical interpretation (see Poliakov 1973: 78).

phers.[13] The idea that most ethnographers understood that 'survivals' were part of their subjects' lives as Muslims, but that they consciously sought to conceal this reality out of suspicion or opposition toward the Soviet government, is belied also by the sheer earnestness of the ethnographic rhetoric of survivals. This view is also unsustainable, finally, on practical grounds: it requires us to divine the individual political stance of particular Soviet ethnographers (one can readily think of some for whom a presumption of opposition to Soviet policies might be true, and of others for whom it was most certainly not), or, failing such knowledge, it requires us to assume that any and all evocations of the notion of survivals signal a political stance hostile to at least one important policy of the Soviet government. The former is impossible, practically; the latter, which in turn requires us to envision academic and institutional structures in the Soviet Union filled with closet dissidents, is absurd.

As for the consequences of such a view with regard to the character of Soviet ethnography, the inescapable conclusion is that such a view in and of itself would instantly render the entire Soviet ethnographic enterprise regarding religious practices among the Muslim peoples of the USSR useless in terms of both data and interpretation — it introduces, after all, an element of conscious and wilful obfuscation and distortion into the collection and analysis of ethnographic material, however laudable the putative motives might have been. In the end, then, it must be kept in mind that Soviet ethnographic literature is now part of the historical and intellectual record, so to speak, available to be read and digested by students now and in the future who have no direct knowledge of the political framework in which Soviet ethnographers worked, and who have even less access to the 'back-story' of a particular researcher that might cast his or her evocation of the notion of survivals in a more favourable moral light (if not in a more favourable interpretative light). Given also the persistence of the notion of survivals in the post-Soviet era, there is every reason to assess the claims of Soviet ethnographic literature on their merits, whatever the underlying motivation of the scholars making such claims and offering such interpretations might have been. If the notion of religious survivals is explained away, in whole or in part, simply as one more consequence of an ideology that no one believed (against the evidence of what was written), or as a consequence of an academic alliance with the state to which no one was actually committed

[13] This exculpatory view of ethnographers' concern for filling 'national' culture with 'refugee' artefacts, fleeing their association with Islam, also presumes that the structures and ideologies of national cultures, as they developed in the 1950s and 1960s, were in place already at the dawn of Soviet ethnography, when the notion of survivals was applied, clearly, simply as an inherited interpretative strategy, not as an evasive tactic.

(against the evidence of actual careers), we risk moving ever further away from a proper assessment of the use of the notion of survivals, and of Soviet ethnography more generally.

In any case, whether for its functional value or its intellectual appeal, the sheer weight of the notion of survivals is evidenced simply by the litany of Soviet ethnographic works framed in terms of 'revealing' the origins of particular practices and beliefs as holdovers from pre-Islamic times[14]; this was an enterprise in which the leading lights of Soviet ethnography participated. One of the earliest was Sergei Pavlovich Tolstov, active since the 1930s; others most active in Central Asian contexts included Ol'ga Aleksandrovna Sukhareva (Sukhareva 1960, 1975)[15]; Gleb Pavlovich Snesarev, one of the most prolific with regard to the doctrine of survivals (Snesarev 1957, 1960, 1969, 1976-77, 1983, 1990); Sergei Mikhailovich Demidov, active in Turkmenistan (Demidov 1962, 1963, 1976, 1977, 1978, 1990, 1997); the late Vladimir Nikolaevich Basilov, who was central to the study of shamanism as a religious survival (Basilov 1968, 1980, 1992; Basilov and Kubakov 1983)[16]; and a host of others, with even a list that is far from exhaustive growing quite long (inter alia, Baialieva 1972, 1980; Baiarov 1968; Begmedov 1968; Kisliakov 1960; Knorozov 1949; Mirkhasilov 1972; Mukhiddinov 1989; Mustafina 1992; Vostrov 1959). The list expands still further if we include the genre of antireligious literature, even allowing for considerable overlap between the categories of 'ethnographer' and '*antireligioznik*' (e.g. Amanaliev 1967, 1969; Amanturlin 1977; Demidov 1977; Petrash 1963). Western scholars as well, especially those closely focused on the Soviet impact or on communities defined in ethnic or linguistic (rather than religious) terms, and those whose training was primarily historical or philological (rather than in anthropology or religious studies), often tapped into the notion of religious survivals (e.g. Carrère d'Encausse 1961; Cirtautas 1970).[17]

One notable, if partial, exception to the interpretative strategy focused on pre-Islamic survivals came from the author of a work often hailed as the

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[14] This is by far the most common use of the term *perezhitki* and its equivalents and is the focus of the present discussion; we may note here, however, that Soviet antireligious literature often spoke of Islam itself, simply as a religion still adhered to in the socialist age, as a 'survival' (see, for example, Diusenbin 1968; Kadyrov 1966; Kuliev 1976; Sadykov 1972).

[15] See Naumova, this volume.

[16] Basilov's dissertation (1967) studied the 'survivals of pre-Islamic beliefs in the Muslim cult of saints' on the basis of material collected in Turkmenistan.

[17] The use of the notion of survivals remained strong in the works of some French scholars, especially those associated with the study of Turkic peoples (such as the late Irène Mélikoff and her student, Thierry Zarcone); it also remains common in Turkish scholarship, where it facilitates the cultivation of a Turkish identity separate from Islam.

first sophisticated sociological discussion of Islam in the Soviet context (judged, to be sure, on the basis of a trajectory from the dismissive screeds of L. Klimovich[18] to the 'generic' antireligious tracts of the 1960s and 1970s). In *Islam and Society*, Talib Sarymsakovich Saidbaev addressed the question of survivals primarily in his historical chapter on the spread of Islam into the territory of the USSR, but he departed somewhat from the typical emphasis on analyzing Muslim practice in terms of its 'orthodox' or pre-Islamic origins. While never evincing doubt that such an analysis was basically sound, Saidbaev argued against concluding that pre-Islamic beliefs constituted a 'popular religion', or that Islam should be regarded as fundamentally different in the various regions into which it spread, on the basis of locally prominent pre-Islamic traditions (Saidbaev 1978). Saidbaev's views, to be sure, were no less ideologically driven (or at least ideologically conformable) than those of the ethnographers who stressed the notion of survivals; his point was to lump together the entire complex of practices that others would analyze as pre-Islamic survivals and identify them all as potentially harmful. In this way, his views fit smoothly into the antireligious motivations, or obligations, that were never far from the surface even in a 'sophisticated' work of the Soviet academy, insofar as stressing that Islam was Islam, wherever it was, underscored the need to fight against Islam; yet in acknowledging that even those practices identified by some as pre-Islamic survivals were harmful, *because* they were Islamic, Saidbaev implicitly acknowledged that they held meaning, as Muslim practices, for the people who maintained them. It appears, however, that Saidbaev's views were not taken seriously by most ethnographers.

The upshot of all of this is that virtually without exception, all Soviet ethnographic publications must be read with a keen eye for the subtle and less than subtle intrusions not only of blatant ideologically-based evaluations, but also of assumptions, classifications, and analytical approaches shaped by less obvious, but no less ideologically driven, habits of mind; among these, the analytical approach focused upon the tracing of religious survivals is only the most pernicious. To some extent this means that we must utilize the data collected by Soviet scholars, but discard, or at least suspend, their interpretations and evaluations; we must acknowledge, however, that even this degree of caution leaves us uncertain about the extent to which the assumptions and ways of thinking inculcated by the ideological atmosphere of the Soviet academic establishment shaped even the collection and recording of the data itself.

[18] On this prominent champion of antireligious work with regard to Islam, see Kemper 2009a, 2009b: 28-34, 40-46.

Confronting Survivals in Soviet Ethnography: Some Examples

Given the critique and abandonment of the interpretative strategy based on the notion of survivals in historical and anthropological scholarship outside the Soviet-shaped academic world, the continued vitality of this notion in the Soviet and post-Soviet environments may strike Western scholars as baffling or quaintly old-fashioned; on the other hand, the decline of the notion of survivals in the West means, among other things, that critical discussion of the notion must proceed through direct engagement with its ongoing application in Soviet and post-Soviet scholarship. Such engagement must inevitably operate on two fronts: there is a need for a general, systematic, and partly theoretical critique of the notion of survivals itself; and specific cases advanced in the literature as instances of religious survivals must be examined individually. Unfortunately, there is not sufficient space herein for even an outline of such a systematic critique, for a full analysis of the notion of survivals as it was applied by Soviet ethnographers to the study of Muslim peoples in Central Asia, or for an analysis of the key issues underlying the notion's impact (e.g. the influence of Soviet constructions of 'national' and ethnic identity, the specific areas of religious life to which the notion of survivals was typically applied, or the multiple ancillary assumptions that supported the interpretative framework centred on survivals); we may defer discussion of these issues to another venue and consider a few illustrations of the mainstream Soviet application of the notion of survivals.

One Soviet ethnographer, for instance, highlighted a story about a saint popular among the Turkmens as indicative of the survival of pre-Islamic beliefs about snakes. The story describes the saint, Shikh Sheref, riding a wall and holding a snake that he uses as a whip (in one version, both the saint's mount and his whip take the form of snakes). The ethnographer affirmed that in the image of the snake-whip 'clearly appear the traces of ancient pre-Islamic beliefs, among the peoples of Central Asia, connected with snakes', and insisted that this further underscores 'the connection, in popular tradition, between Central Asian Sufism and local pre-Islamic beliefs' (Demidov 1976: 73; cf. Demidov 1997: 33). The motif of a saint wielding a snake as a whip, however, is extremely widespread in Sufi hagiology and is attested at least as early as the twelfth century. As an image, it may indeed be rooted initially outside Muslim contexts, but if it has been attached to saints in many parts of the Muslim world for many centuries, what can it mean to label it a 'pre-Islamic survival' in this particular Turkmen manifestation?[19] More to the point, what can it tell us at all about any

[19] On the motif, its possible origins, and its wide diffusion, see the studies of Van Bruinessen 1991 and Digby 1994.

specifically Turkmen cultural phenomenon, aside from the Turkmens' engagement with Muslim and Sufi hagiology?

Another example with multiple ethnographic 'layers' involves a supposedly pre-Islamic fertility rite for which we find nearly exact parallels in Sufi hagiographical accounts from the seventeenth century (about a rite performed by a thirteenth-century Sufi). The saint is portrayed aiding his community by sacrificing a bull by a river, thereby causing it to rain. The account immediately recalls a sacrifice in Khorezm, described in the 1950s by G. P. Snesarev, who declared the rite to be 'genetically connected with an ancient cult of fertility' (Snesarev 1960). Snesarev's account has been cited by other researchers, who have often added their own descriptive material; one such researcher added the key detail, omitted by the others, that this rite was referred to as a *dzhar*, immediately linking it not to a pre-Islamic context, but to the complex of practices surrounding the communal performance of the vocal form (*jahr*) of the Sufi *dhikr* (Sazonova 1978: 86-87, citing Snesarev, but also her own material, collected in 1947).

Snesarev, however, had claimed that this rite simply marked the survival of ancient religious beliefs and practices associated with a 'fertility cult' and with totemistic ideas in connection with the bull, insisting that such rites were 'typical sacrificial offerings minimally linked with Islam'; only in cases when the rite ends with the prayers of the 'clergy' without the sacrifice did Snesarev refer to them as 'exceptionally Muslimized [*sic*] variants' (Snesarev 1960: 199). As with most Soviet discussions of religious survivals, the fact that the people performing these rites were Muslims seems not to matter, and the potentially fascinating process and ramifications of the rite's Islamization, its integration into Muslim life, and its conceptualization in Muslim terms — assuming that its 'origin' might indeed be shown to be pre-Islamic, which is highly questionable — are thus ignored. Moreover, if we find a parallel rite performed by Muslims for Muslims in the thirteenth century, or simply described in the seventeenth century, and infused with meaning in a Muslim and Sufi context, then labelling the same rite in the twentieth century as 'pre-Islamic' becomes patently absurd. In this regard Snesarev's own comment is perhaps revealing, as he praises one of his informants who '*understood well the aim of our ethnographic investigations*, and, perhaps despite his religious conceptions and precepts, introduced us into the world of the ancient pre-Muslim beliefs of Khorezm' (Snesarev 1960: 200, my emphasis).

We might note many other such examples. We have, for instance, descriptions from the seventeenth century of Sufi healing rites that, if observed in the twentieth century, would have been latched onto as 'shamanic' survivals, but were, in the seventeenth century, already thoroughly absorbed into a

Muslim framework, whatever their 'origins'. Likewise, from the same period we might note accounts of Muslims dealing, again in contexts of healing, with spirits that are typically identified as being of pre-Islamic origin (the famous *albasti*), and we might ask, if, in the sixteenth century, we find evidence of this spirit's interpretation in a wholly Muslim framework, with its warding-off linked to the insightful knowledge of a pious Sufi woman, what can it possibly mean to keep harping on ideas and practices involving this spirit as survivals of 'pre-Islamic' religion? Such examples suggest what may be learned from medieval or early modern sources about a host of practices labelled now as survivals from pre-Islamic times; again and again, they show that such practices were subsumed into an Islamic framework long ago, and that identifying a practice observed in an Islamic framework in the sixteenth century and again in the twentieth century as 'pre-Islamic' is both historically inaccurate and hermeneutically empty. Highlighting such examples, on a case-by-case basis, however, will be important for chipping away at the edifice built by those invested in the interpretative strategy based on the notion of survivals.

Conclusion: Survivals and the 'Interface' of History and Ethnography

Far from receding in the post-Soviet environment, the notion of survivals survives today, despite the disappearance of the major ideological superstructure that helped maintain its application in scholarly environments; in some regards, indeed, it has become even more entrenched in recent years, through the expansion of general literature on Central Asia that adopts and applies the notion of survivals to accounts of the region's religious past and present, and simply through the impact of generations of Soviet education. Why the notion of survivals has not been abandoned as quickly as other aspects of Marxist ideology is a reasonable and important question. I would suggest that the reluctance to abandon this notion must be understood against the obvious institutional momentum linking the training of ethnographers with the views and methods of the Soviet era — entailing intellectual continuities, of course, but also personal and institutional continuities — and against the array of historical and contemporary constituencies that have found the notion of survivals useful and have had a stake in its continued application. These constituencies include nineteenth-century Tsarist scholars, local Muslim reformers intent on criticism of a debased Muslim society, Soviet-era nationalists eager to separate a distinctly national component to cultural life from any association with Islam, the Soviet-era religious establishment, Western scholars interested chiefly in the ancient or contemporary

history of Central and Inner Asia, post-Soviet governments seeking to distinguish 'local' versions of Islam from outside Islamist agendas, internal and external Salafist and Islamist groups claiming to offer a purer Islam, and Western Sovietologists, as well as Sovietological interpreters of Islam in post-Soviet Central Asia, equipped with neither ethnographic nor historical skills, and thus dependent upon former Soviet 'experts' on Islam in Central Asia (who, just over two decades ago, staffed the antireligious campaigns but now seem to be the only voices heeded in Western governmental circles regarding Islam in Central Asia).

What these constituencies share, in addition to an investment in the 'survivals' strategy for assessing contemporary religious life, is a neglect — sometimes accidental but sometimes quite wilful — of the historical development of Muslim religious life in Central Asia, especially in the centuries immediately preceding the Russian conquest. This historical neglect, evident also in the specific examples of so-called 'survivals' noted above, leads us back, finally, to the essentially hollow character of what is often touted as the particular merit of the notion of survivals: the notion is advanced as an interpretative strategy that takes stock of historical development, but in most cases its putatively historical component turns out to be spurious.[20] The survival of the interpretative framework based on the notion of religious survivals is to some extent rooted in the veneer of interdisciplinary synthesis, even syncretism, with which it is presented. Whether in its Protestant manifestation, its Tylorean articulation, or its Soviet formulation, the doctrine of survivals looks backwards and sideways, claiming to provide a historical foundation for anthropological observation, as well as an anthropological confirmation for the interpretation of history (not to mention a 'use' for historical investigation with contemporary 'relevance').

The notion of survivals, in other words, claims, for the historian, to validate his or her inquiry by finding that history can illuminate contemporary behaviour; it claims, for the ethnographer, to infuse the ethnographic present with historical groundedness. I would argue, however, that what the notion of survivals in fact enshrines is only a pretence of historicity, and an ethnography that is actually at odds with itself. The ahistorical character of this approach builds upon the essentialism that accompanies the notion of survivals, by situating the origin of those survivals in the timelessness of 'popular religion' and ignoring actual historical development; that historical development is often unknown or invisible because it took place precisely in the 'missing middle' that marks the study of Central Asia, dominated as it is

[20] Smith noted that the notion of survivals as framed by Tylor already depended upon an 'illegitimate' combination of an evolutionary framework with an ahistorical methodology (Smith 1978: 260-261).

by the antiquarian focus of 'Silk Road studies', on the one hand, and the legacy of Sovietology, on the other. The Islamic middle is, of course, not missing but is simply inaccessible to those who ignore the sources produced in pre-modern times. These sources and the living bearers of Central Asia's Islamic heritage are studied by a remarkably small number of specialists on the religious history and religious present of the region. They are far outnumbered by those who were taught, either by the Soviet experiment or by the Sovietological reaction to it, to analyze Islam in Central Asia on the basis of outmoded interpretative strategies such as the notion of survivals. The 'survivals' theory thus distorts history, but its real impact is to drain meaning — and understanding — from the present.

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Chapter 4
Heroes of Theory: Central Asian Islam in Post-War Soviet Ethnography

John Schoeberlein

A World of Subtext Theory?

One of the ways that people talk about the function of literature during Soviet times is as a medium in which good authors sought to say what they wished within the constraints of what was acceptable. If they wanted to say something that the authorities would not accept, then they would create in their texts an acceptable surface reading and under this bury the more problematic message. The hidden message would hopefully get past the censors, obscured by formal conformity, symbolism, and encoding. Among other things, this way of talking about Soviet literature is an affirmation of the ability to create good literature under conditions of tight state control of artistic expression, as well as a defence of writers who might otherwise be criticized for having made excessive compromises of their artistic integrity while working under a morally compromising regime. In this way, the realm of literature is figured as a domain of heroic struggle of the creative individual against the repressive state, providing artistic redemption for his society.

By analogy, one can ask whether it was possible for ethnographers of Central Asia to write with scholarly integrity, despite the political constraints of the Soviet system. We can posit that ethnographers were working against an agenda imposed upon them by the state, risking the state's sanctions for the sake of producing the worthy scholarly product to which they aspired but that the state would not allow. In this chapter, I ask whether Soviet-era ethnographic writings allow a reading that reveals an aspiration to intellectual integrity that runs counter to the necessity for formal ideological compliance and to surface level meanings fraught with the state's ideological agenda. More precisely, I ask whether it is appropriate to view Soviet ethnography as a realm in which ethnographers struggled to maintain the high scholarly values of truth and creativity, in the face of a state that had a much less noble agenda for scholars, as mere tools for promoting state policy and legitimating the regime. Is ethnography a realm in which we can find 'intel-

lectual resistance' through which ideas and analysis were developed that were not part of the official ideology?

In this vein, one often hears that ethnographers — like scholars in other realms that were closely related to the official ideology — formulated their texts such that formal ideological compliance was clearly stated in introductions and conclusions, with the intervening pages having little to do with this ideological agenda, and instead exhibiting the author's more sincere and noble intentions. By citing Marx, Lenin, or Brezhnev and referring to decisions of the latest party congress at the beginning of texts, it was possible to achieve formal compliance while gaining cover to proceed with a study that was minimally encumbered by ideology — or so it is argued. This amounts to a claim that, even in an environment pervaded by state ideological agendas, it was possible to produce ethnographic descriptions of Central Asian culture that were not exclusively governed by these agendas but that instead represented a realm in which useful theory and true observations could be developed. Does it make sense to look for theory being used in the analysis of Central Asian culture that was not in strict conformity with the state's ideological agenda?

In this chapter, I examine Soviet ethnography of Central Asia from the post-war period onwards. I have chosen the post-war period because, in many ways, the Soviet system had become normalized by this point. By the late 1950s and 1960s, scholars operated in a system that had relatively well-established procedures and norms. In writings from before this time, there are different possible ways to interpret the way ethnographers wrote. They could be understood as writing in a tradition more characteristic of pre-Soviet scholarship that had simply not yet been effectively eradicated. They could be seen as avoiding expressions of integrity due to the pervasive caution or fear that resulted from the tumultuous period of the Stalinist purges when scholars were forced to focus on simple survival. Ideological conformity in their writings could reflect the revolutionary enthusiasm that was characteristic of some early-Soviet scholars who were truly taken by Marxism and thus for whom the tension between scholarly integrity and official ideology would not be much of an issue, as long as the official line did not change or narrow so much as to make their position become unorthodox. By the post-war period, a normalized mode of Soviet academia had been established, allowing scholars to have a good idea of what compliance entailed, and potentially also a good idea of what possibilities there were for doing 'true' scholarship while demonstrating formal compliance, if that was indeed what a scholar was inclined to do.

To explore these matters, I draw examples from ethnographic work on Islam in Central Asia. By choosing to focus on religion, I am addressing a

realm in which ideology had a particularly strong presence, and so for this reason, the problem of achieving formal compliance with ideological requirements was especially challenging. Like in the case of other aspects of 'traditional culture' such as marriage practices, healing, group identity, and the social division of labour, the state undertook to identify, on the one hand, those social phenomena that could be acceptable components of national culture and that were compatible with socialism and, on the other, those that should be viewed as negative or reactionary survivals of the pre-socialist social order that must be condemned and eradicated.

Ethnographers were tasked with several projects that were intended directly to contribute to the state program of cultural transformation, eradicating 'harmful' cultural phenomena and fostering progressive socialist culture. The problem was:

> In class society, religion becomes a tool of social control, defending the interests of the ruling class. This task was also fulfilled by religious ritual [*obriadnost'*], constituting an integral element of traditional rules of people's behaviour, their celebrations and rituals [*ritualy*].
>
> Traditional customs, on the strength of their persistence and conservatism, long preserve remnants of the social and ideological order inherited from previous epochs; these survivals in new conditions can turn out to be a brake on further development and can negatively influence people's consciousness and behaviour (Lobacheva 1975: 4).

It was the job of ethnographers to identify good culture, distinguishing it from bad. They were expected to highlight and praise the achievements of the Soviet project of cultural transformation, and sometimes this entailed whitewashing its failures. They were to play a central role in finding cases in which 'harmful' cultural and social phenomena continued to exist, to analyze the causes of this persistence, and to propose strategies for more successful eradication of bad beliefs and customs. And they were to play a direct role in discouraging bad culture and promoting socialist cultural transformation through their own participation in ideological work among the masses.

In the case of Islam in particular and religion in general, the regime's position was rather definite. From World War II onwards, the state supported the establishment of Islamic institutions under the auspices of the Spiritual Administration of Muslims of Central Asia and Kazakhstan that were accepted as playing a role in sustaining the religious life of Muslims. However, outside of this narrow institutional sphere, the state apparatus of ideology and control was overwhelmingly aimed at discouraging and suppressing religion. And though anti-religious campaigns were not always pursued with

equal determination or efficiency, the ideological apparatus, which included ethnographic work, always treated Islam as a negative phenomenon. It was understood as an ideology that was originally created and invariably functioned as a means for maintaining the patriarchal-feudal order. Ethnographers contributed greatly to elaborating this understanding by explicating the patriarchal-feudal origins of various beliefs and practices. It was normal — indeed obligatory — for ethnographers to express their antipathy toward religion and their sympathy for the state's program of eradication of religious survivals.

This chapter considers how ethnographers played a part in Soviet social production of knowledge. The query posed at the start of this chapter assumes that ethnographers constituted a part of an intellectual milieu, occupying a position like what we imagine to be characteristic for creative intelligentsia under conditions of totalitarianism. They are figured as substantially independent of the system in which they operate, and their activities are understood as acts of resistance against this system. Similar assumptions are often held in relation to a wide range of others besides artists; indeed, Western scholars have often operated under the premise that the key to understanding the behaviour of the population in general is that they see themselves as coping with the constraints of a restrictive state. Where Western citizens are assumed go about their lives only occasionally seeing themselves as being pitted against the state, Soviet citizens are imagined to be constantly running up against unwelcome state constraints. By examining how Soviet ethnographers undertook ethnography, we can gain a better picture of their relationship to state ideologies and agendas, and how this affected their particular activity of knowledge production.

Ultimately, this chapter also attempts to define the relationship between theory, ideology, and what might be called untheorized 'notions' entertained by ethnographers. In the epistemology of social science, we tend to think of theory as a coherent, definable system that is usually explicitly formulated in the statement of the research problem and applied systematically in the research methodology. In Soviet ethnography, research agendas and the interpretive frameworks applied to gathered information were generally situated in a conceptual nexus between ideology, theory, and prevailing notions that may bear no systematic relationship to one another, and that may even be contradictory due to the environment of constrained discourse.

There is also a moral discourse that underlies any such investigation into the relationship between state ideology and knowledge production. Put bluntly, one could potentially accuse Soviet ethnographers of fabricating falsehoods about the world that they were representing in order to further their scholarly careers and to help a powerful and insidious state destroy the

cultures and lives of people in Central Asia. In Leninist terms, one could say that Soviet ethnographers were creating an image of Central Asian society — whatever the truth of it — that was socially useful in helping to create a better society. As part of the Communist system, ethnography would help bring an end to exploitative social relations, emancipate women, promote scientific medicine to replace superstition, and loosen the stultifying grip of religion on society and people's minds. Or alternatively, one can imagine ethnographers as playing a heroic role by providing a critical perspective to counterbalance state power and striving to defend those they study from the state's arbitrary interventions (in the same heroic mode whereby many Western anthropologists see themselves as aligned with the people they study against the dominant states under which they live). My goal is not to arrive at a moral judgment of Soviet ethnographers, though in the current Western paradigm of anthropological scholarship, it is inevitable to engage moral issues that underlie the scholarly endeavour. In my own anthropological context, it is unthinkable not to support the principles that one should strive for truthfulness no matter how inconvenient that may be, that one should oppose the actions of institutions of power that violate the will of those whom we study, and that one should 'give voice' to those people who are not otherwise heard, especially when that voice contradicts the view of the powerful. These were not characteristics of Soviet Marxist approaches to ethnography, though some individual ethnographers may have been motivated by such concerns. Ultimately, in fact, it may have been virtually unthinkable for Soviet ethnographers not to support the state's ideology and agenda for social progress, and to hold them accountable to a different set of standards is simply unrealistic.

Theory, Ideology, and Soviet Ethnography of Islam

While other topics received more attention than religion in the Soviet ethnography of Central Asia, religion was nevertheless a recurrent theme. Much more literature was devoted to such topics as material culture, social organization, and funeral customs. But in these contexts, religious questions frequently appeared: ritual objects were a part of material culture; religion was often what accounted for conservatism in social organization; and funeral customs had ideological underpinnings in religion. Religion, or 'spiritual culture' [*dukhovnaia kul'tura*] was usually included as one of the analytical rubrics of the comprehensive treatment of traditional culture that appeared in larger ethnographic works. Soviet ethnography was categorical regarding the problem of religion in Central Asia:

> The socialist transformation of the life of the peoples of Central Asia encountered particular difficulties. In addition to the economic, so-

> cial and cultural backwardness characteristic of the pre-revolutionary past of the peoples of Central Asia, Islam and the reactionary Muslim clergy severely interfered with the formation of a new way of life. The precepts of Shariat, in the course of many centuries, regulated the social life and family relations and set the norms of behaviour of every Muslim (Lobacheva 1975: 8).

One central Soviet ethnographic genre was comprised of works that exposed negative cultural practices and described how the program of Soviet socialist transformation was succeeding in overcoming them. This genre addressed a variety of themes, such as patriarchal-feudal relations, but because of the particular ideological significance of religion, one of the most common themes in this genre — and a substantial part of the ethnographic literature on religion — was concerned with overcoming religious survivals.

Toktobiubiu Dzhunushakunovna Baialieva's book, *Religious Survivals among the Kyrgyz and How they Are Overcome* (1981) provides a typical example of Soviet ethnography on religion in Central Asia. In its colophon, the book is described as follows:

> In the monograph, on the basis of new ethnographic and documentary materials, light is shed on the struggle by the society of the republic under the supervision of the Party-Soviet organs to overcome harmful religious survivals in the contemporary daily life of the Kyrgyz population.
>
> The work is intended for specialists: ethnographers and historians, agitators and propagandists, and a wide range of readers (Baialieva 1981: 2).

In explaining her purpose, Baialieva writes:

> A comprehensive acquaintance with the beliefs, both in the past and in the present, and also with the environment in which they existed and in part still exist, a clarification of the reasons for the persistence of survivals, and finally, an exposition of their character and territorial peculiarities will make it possible to fight more effectively religious survivals (p. 4).

The ten illustrations included in the book provide an interesting pictorial narrative of the book's message. They include six pictures that represent pre-Islamic and Islamic tradition in Kyrgyzstan (art and architecture), and four that represent the bright present of the Soviet republic, including a photo of the atheism section of a library, two pictures of the 'modern' style of wedding, and a picture of the Soviet Santa Claus figure, captioned 'Father Frost [*ded moroz*] among the herdsmen'. Negative religious traditions were to be replaced with atheism, Soviet-devised rituals, and a common Soviet mass culture sanitized of religious content.

Lest there be any doubt that Baialieva was a bona fide ethnographer and not just a propagandist, I would note that in 1969 she defended her Kandidat degree dissertation (Baialieva 1969) at the Leningrad branch of the Institute of Ethnography, Academy of Sciences of the USSR. This led to her first major monograph entitled, *Pre-Islamic Beliefs and Their Survivals among the Kyrgyz* (1972). And lest there be any suspicion that Baialieva's work was marginal within ethnography, I would also note that she was one of only a handful of Kyrgyz ethnographers during Soviet times working in the Kyrgyz SSR (complemented by a somewhat larger but nevertheless also small number of ethnographers working on Kyrgyz ethnography in central Soviet institutions).

Baialieva's book begins with an overview of the history of religion in Kyrgyzstan prior to the revolution. The major themes in this overview are that Islam was assimilated by the Kyrgyz only quite recently, with full Islamization only being achieved by the force of the Kokand Khans from the eighteenth to twentieth centuries, that Islam among the Kyrgyz was not of a pure type but rather was mixed with pre-Islamic religions, and that the strength of pre-revolutionary Islam can be attributed to its use by the feudal elite as an ideological tool of domination. The remainder of the book is devoted to a description of how the Soviet government and Communist Party worked to eliminate the influence of religion — largely successfully, but with some work remaining to be done by the institutions charged with conducting atheistic propaganda.

It is impossible to discern any references to explicitly theoretical propositions in this work other than the formulaic treatment of religion as an ideology of feudal exploitation and the evolutionist concept of cultural-religious survivals [*perezhitki*], which portrays religion as inherently reinforcing the persistence of social relations that are incompatible with socialism (dominance of elders, subordination of women, etc.) and that persist in spite of the successes of socialism. According to Soviet Marxist theory, the economic base was determinant, with changes in the economic infrastructure leading, sometimes with some delay, to changes in social structure and ideological superstructure. The standard argument for why religion persists in the face of economic and social transformation is that the 'social consciousness is more conservative than social being, and that religion is the most conservative form of ideology' (Basilov 1980: 234). Baialieva's analysis does not ask how economic and social change accompanying the construction of socialism should lead to ideological change and the decline of religion. Rather, she discusses exclusively the realm of ideas, in which, for example, Soviet propagandists were working to counter the faulty logic of

religion, and religious leaders in Kyrgyzstan were modifying their religious message in order to better compete against Soviet ideology.

A number of themes common to Soviet ethnography of Central Asian Islam in general appear in Baialieva's analysis: that, as nomads, the Kyrgyz were not very susceptible to Islam and were never thoroughly Islamized; that their devotion to Islam was weakened by its syncretistic character; that Sufism is not a true form of Islam; that Kyrgyz religious figures have a poor knowledge of Islam; and that the Kyrgyz quickly abandoned religion during Soviet times, leaving barely a quarter of the population as believers by 1977 (ibid.: 72-3). It is worth noting that the overall viewpoint of Baialieva's analysis has become a dominant contemporary discourse about the Kyrgyz and their relation to Islam. Whether or not there is any truth to these claims, which should be doubted, ethnographers like Baialieva seem to have been highly effective in contributing to a narrative about the defective character of traditional Kyrgyz Islam that reappears far beyond the realm of Soviet ethnography.

One can identify a number of theoretical propositions in the works of Baialieva and other ethnographers of Central Asia for which one could hardly find a basis in Soviet Marxist theory: that nomads are less likely to be strong believers in Islam; that a putative late Islamization of the Kyrgyz led to weaker contemporary adherence to Islam; that syncretistic, 'impure' Islam is a weaker form of Islam; that pre-Islamic religious practices are more resilient than properly Islamic ones; that women's life offered a preserve for conservatism that sustains religion, etc. While these propositions are not articulated as grounded in Marxist theory, they must be understood as being part of the ideational framework that characterized Soviet ethnography of Central Asia. These themes dominated the discussion of religion in Soviet ethnography of Central Asia. Meanwhile, these themes were not well articulated in theoretical terms, and their weak and contradictory status as theory is apparent. For example, two of the previously mentioned propositions — that the persistence of pre-Islamic customs weakens Islam and that pre-Islamic customs are more resilient in the face of socialist transformation — seem to directly contradict one another. Never mind that this literature never attempts to provide an explanation for why, for example, pre-Islamic customs should be more resilient than Islamic ones. These themes also dominate the way that the Soviet intellectual elite came to think about Islam, so they are very strong as assumptions but very weak as theory.

This is not the place to elaborate how these propositions were ill suited to representing the reality of Central Asian Islam. However, it is obvious that many of them related to the project of Soviet transformation and could be deployed to support it. One could argue, for example, that, in

Soviet ethnography, the appeal of the idea that syncretistic Islam is weak Islam was that the idea offered the promise that this kind of Islam would quickly disappear. The appeal of the claim that Central Asian Islam is mixed and therefore not true Islam could be used to bolster the argument that Central Asians should abandon their inauthentic Islam. While these arguments were largely implicit in the Soviet ethnographic literature, they were quite explicit in the ways that Soviet officials and intellectuals talked about Islam, and indeed they are often repeated today by Kyrgyz and Kazakhs — and by others talking about Kyrgyz and Kazakhs — when they wish to argue that Islam is not an inherent part of the national culture and heritage of these peoples.

Another major theme in the ethnography of Islam in Central Asia focused on the state project of introducing new rituals to replace 'harmful', chiefly religious rituals like those that Baialieva's work was dedicated to overcoming. Nina Petrovna Lobacheva's work on the formation of new rituals in Uzbekistan was a characteristic contribution to this literature (Lobacheva 1967, 1975). Lobacheva describes the process of establishing new rituals as essentially completed. References to the harmful traditions that the new rituals were meant to replace appear, as a rule, in the past tense. However, Lobacheva devotes considerable attention to initiatives undertaken in the very recent past by the state and the Party to end harmful rituals, so from references to these initiatives in Lobacheva's work, one gets the impression of a process that was completed very quickly and recently — certainly within a timeframe that would allow the author to investigate these changes in her ethnographic fieldwork. Yet, there is no sense of field research that emerges from the narrative, which describes pre-socialist customs as belonging to an ethnographic past and the new rituals as simply having come into being and now being fully operational without any description or analysis of the process by which this change took place.

Lobacheva's account begins by laying out a Marxist account of why pre-socialist practices endured despite the rapid transformation of the economy and social relations, a transformation that was treated as a *fait accomplit*. According to the familiar argument, customs survived because they were more conservative than the social order, though they should nevertheless be eradicated because they could slow progress. She makes the programmatic claim that only negative customs needed to be changed, and the rest, which represented the wisdom and moral heritage of the population, were therefore to be left intact. She discusses state initiatives to introduce new customs, but these are described only in terms of mentioning the decisions of various government and Party entities and meetings held to determine what form new rituals should take. Though she mentions that it was

important for new rituals to be acceptable to the people, there is no discussion of resistance to new rituals or how such resistance was to be overcome. On this score, the only key that we are given to understand how the Soviet government was able to bring about change so fast is Lobacheva's assurance that the new rituals were well designed and that ethnographers had played a crucial role in identifying good traditions that could be drawn upon in creating new rituals. She describes the introduction of many new rituals that were in no obvious way replacements of the 'harmful' ones. For instance, celebratory registration of births held at the new *bakht öyi* [houses of happiness] did not replace any specific former function of ritual, but they obviously introduced a new function by involving the state in an arena that had previously not been its concern. The book contains a truly impressive account of a large number of the new ritualized activities that were introduced in all aspects of life in different parts of Uzbekistan.

Lobacheva's discussion of the introduction of celebratory birth registrations is revealing. She notes that the authorities encountered a problem that required them to adjust their approach, because according to tradition, one should not show a newborn child to anyone during its first forty days, but the celebratory registration was to be carried out within one month of birth. It is only from this that we learn that these rituals were imposed on the population as a requirement, as opposed to something that people immediately and eagerly took up. She notes that, though all sorts of harmful magical practices were employed by Uzbeks during this time period, there was a degree of wisdom in these practices, as they protected the infant from exposure to disease through public contact. After reading this, one might expect Lobacheva to inform us that the Soviet government adjusted the time period to allow people to do the registration after forty days, yet this was not the case. The state instead adjusted its approach by ramping up propaganda on hygiene. Lobacheva tells us that the revised approach helped to eliminate superstitions and prejudice hindering the introduction of new rituals. She affirms the importance of the ethnographer in this process: 'The process of forming and introducing the ritual of registering newborns into the way of life is not finished. In our view, the period when it is carried out and the ritual itself, for example, are not quite in agreement with the demands of medicine' (1975: 84). One might read into this that Lobacheva was trying to convince the authorities that they should not force people to conduct this ritual before the end of the forty-day seclusion period, but it is interesting to note that she grounds her position in an appeal to medical concerns. It was not an appeal to consider the sensibilities of people who, as she already acknowledged, probably had good reasons to fear for the well being of their children.

For me, it is impossible to read Lobacheva's account without being conscious of the fact that, to put it bluntly, she was fabricating. When I first began to spend extended periods in Uzbekistan in 1988, more than a decade after her account, vastly less progress had been made in supplanting old customs with new ones than Lobacheva's account would have led one to believe. For example, Lobacheva describes several new forms of wedding, two of which were so-called 'red weddings' [*qizil töy*] with almost nothing in common with traditional weddings, and one of which she refers to as 'traditional'. Even in the 'traditional wedding', the young couple supposedly chose their own partner (1975: 41). In fact, in most parts of Soviet Uzbekistan, even by the end of the Soviet period, arranged marriages were the predominant pattern. What is more, there are even Soviet sources that offer a very different picture from Lobacheva's: for instance, the literature on religious survivals usually took pains to minimize the prevalence of the negative survivals it dealt with while nevertheless depicting attempts at eradication as less than successful. Soviet press accounts and sociological literature also provide a picture of much less thoroughgoing success. Clearly, Lobacheva was describing what the Soviet regime preferred to be true rather than what she actually observed.

Theory Unshackled?

One way to assess what Soviet ethnographers were trying to do beyond what was dictated by the official ideology is to compare the positions expressed in their contributions to Soviet ethnography with what they wrote and said in the period following the introduction of glasnost and the demise of Communist state ideology. Of course, this is only possible in the case of ethnographers who left something in the 'ethnographic record' both during and after the Soviet period, and there are relatively few such ethnographers of Central Asia. This is in part due to the fact that the ranks of what had been the community of Soviet ethnographers of Central Asia were decimated after the collapse of the Soviet Union, which was accompanied by a dramatic drop in state support for scholarship. Comparison of Soviet-era and post-Soviet writings from the same authors is also made problematic by the fact that it is often difficult to determine to what extent differences in post-perestroika writings reflect what the scholar would have wanted to write in Soviet times but could not and to what extent these differences resulted from the experience of glasnost and all that accompanied it. During that tumultuous period, some Soviet ethnographers — along with their compatriots generally — adopted dramatically new perspectives due to the prevailing ethos of critique of the Soviet experience, and greatly increased exposure to the approaches of non-Soviet scholars.

Keeping in mind these difficulties, I would like to ask whether there is evidence that the lifting of Soviet ideological constraints revealed any previously hidden tendencies among ethnographers to develop ideas counter to Soviet ideology. First, I will look at some of the writings of Vladimir Nikolaevich Basilov (1937-1998), who received his Kandidat degree in 1967 with a dissertation on the theme of pre-Islamic survivals in the 'cult of saints' among the Turkmen. Basilov continued to publish on the theme of pre-Islamic survivals, particularly 'shamanism' in Central Asia, until his accidental death in 1998. His dissertation and other early works exhibit many of the key characteristics of Soviet-era ideological compliance in the ethnography of religion that I have already outlined. He describes the scholarly contribution of his dissertation, as consisting of the following claims: 1) The study of pre-Islamic features of Islamic religion reveals the character of local Islamic traditions such as the cult of saints and thus contributes to a better understanding of the Islamic religion as a whole (1967: 3). This is important because the role of the cult of saints is central to the continued practice of Islam among Turkmen, and this understanding is thus crucial for effectively combating religion with atheistic propaganda (ibid.: 2). 2) The study of pre-Islamic survivals reveals the extent to which Islam in the region deviates from Islamic norms, as it involves practices and beliefs that are not truly Islamic but that have been borrowed from the religions that the advent of Islam in Central Asia had supposedly displaced. Basilov's analysis of pre-Islamic elements analytically separates these beliefs and practices from Islam itself (which for Basilov is defined by the Quran and by Islamic norms observed among early Arab Muslims), though this is not the way that the people he studied understood Islam. Thus, Basilov's contribution lies in demonstrating that certain practices are misunderstood by their practitioners as belonging to Islam when they are in fact inauthentic Islamic practices. 3) Basilov aims to demonstrate the inherent contradictions within Islam. Islam claims to be monotheistic and depends on the acceptance of Allah's will, but in fact, according to Basilov, it contains polytheistic elements such as the cult of saints and entails appeals to the intercession of saints rather than submission to divine will (ibid.: 18).

In this project, Basilov closely follows the work of his predecessor, Gleb Pavlovich Snesarev (1910-1989), who published on pre-Islamic survivals in the 1950s and 1960s, and a comparison of the two authors helps to highlight the normal discourse of Soviet ethnography. In a 1957 article entitled 'On certain causes of the retention of survivals of religious and daily-life practices among the Uzbeks of Khorezm', Snesarev expresses his concern that 'in recent years attention to the overcoming of religious survivals has weakened [...] [The opinion has emerged] that in our days religious

survivals have lost their former significance' and have turned into 'habits that are quickly dying out on their own', but this has resulted in a complacency that 'slows the ultimate reconstruction of the consciousness of the Soviet people' (1957: 60). Snesarev's article has a strongly political character, appealing to ethnographers to become more actively involved in identifying practices that should be eradicated, and 'once the researcher has established [their] causes, he must draw practical conclusions and mobilize society's attention toward the struggle to eradicate them' (ibid.: 72). Like Basilov, Snesarev affirms that practices associated with the cult of saints and their graves 'have little in common, in the conditions of Central Asia, with Islam' (ibid.: 62). Like Basilov, Snesarev also associates such practices with women's activities and considers that the continued observance of such practices was an obstacle to the transformation of gender relations and the elimination of the patriarchal order of traditional Central Asian society (ibid.: 64-67).

Apart from the practical matter of identifying and eradicating harmful practices, the main analytical project of both Basilov and Snesarev in these writings is to produce a genealogy of customs and beliefs that traces their origins to pre-Islamic religions. While both authors describe Islam and pre-Islamic survivals as contradicting one another, they acknowledge that their informants do not refer to these contradictions and do not recognize the pre-Islamic survivals as anything other than Islamic practices. Both refer to shamanism, for example, as a definable set of practices analytically distinct from Islam, which allows them to speak about shamanism as a religion that persisted into contemporary times among the Uzbek people, though Uzbeks were unaware of their being shamanists and simply considered themselves as Muslims. In so doing, Basilov and Snesarev engage in a project that has much in common with evolutionist anthropology of the turn of the twentieth century, which was concerned with origins and with the ways that contemporary practices represent survivals of past evolutionary stages of societal development. In this way, of course, they also shared something in common with Engels' work on *The Origin of the Family, Private Property and the State* (1942 [1884]), which remained a key work in Soviet theoretical orthodoxy. However, for the most part, Basilov and Snesarev's tracing of origins does not focus on the ways that particular practices were functionally connected with particular forms of social organization or stages of evolution; instead, their accounts have more in common with those of 'bourgeois' evolutionists who viewed survivals as having the ability to endure entirely outside of a social context in which they might have originally functioned. As survivals, both Islamic and pre-Islamic practices are analyzed as carry-overs from feudal-patriarchal times, and the two authors' analysis does not

address in Marxist evolutionary terms the question of why pre-Islamic practices endured in the face of the adoption of Islam. Instead, they both appeal to explanations that, in Marxist terms, operate exclusively in the realm of superstructural ideology such as conceptual weaknesses or contradictions in Islam. Basilov claims, for example, that Islam could not satisfy people's need for mediation between the individual and the remote deity (hence the need for mediating saints). He also claimed that monotheism could not out-compete the previously familiar forms of polytheism, which were simply transformed into the cult of saints (1967: 18). I would note that while this stance does not amount to Marxist evolutionism, neither does it amount to a coherent psychological theory of religion, and, in any case, it should be viewed as purely speculative since it is based on no information other than suppositions about how medieval Central Asians might have responded to the idea of a unitary or distant god.

This is in contrast to the claim that Basilov made in a programmatic article on the ethnography of religion:

> The research of Soviet ethnographers into religious phenomena is based on Marxist methodology. This means, above all, that we approach religion from a materialist position, i.e. we seek the 'earthly roots' of religious beliefs. Furthermore, Marxism defines with precision the place of religion among other phenomena connected with human activity: religion is a form of social consciousness. Such a conception of the nature of religion underlies the more elaborate definitions of it, and is shared by all Soviet scholars. It explains the character of the interrelations between religion and other phenomena of social life. It also shows that religion belongs to superstructural phenomena determined by the basis, i.e. the system of relations of production of people within an economic system. […] Changes in religious beliefs and customs are at best reflections of changes in economic, social or family relations (1980: 231).

This is no doubt a good description of how Soviet Marxism treated religion on a theoretical level, but in his analysis of Central Asian religion, Basilov himself does not seem to have been interested in this sort of analysis of change. Though changing circumstances are reflected in Basilov's work — for instance, he mentions that the practices he describes are dying out — the actual analysis of this change does not go beyond a general assumption that conditions in Soviet society would ensure the disappearance of such practices. For the most part, Basilov does not specifically address how such changes are taking place, and he seldom refers to changes in specific social conditions as the causes of specific changes. Basilov was first and foremost interested in changes occurring in remote times past (inaccessible to ethno-

graphic observation); he was interested in the transformation of pre-Islamic customs in an Islamic context, but here too, his analysis does not focus on the material causes of change but rather on the ideational framework in which change is assumed to have taken place.

Indeed, in Basilov's programmatic article cited above, a curious statement appears that is noteworthy in relation to the questions guiding my investigation into the possibility of ethnographers developing theoretical frames that go beyond official ideology: After a paragraph in which Basilov allows that Western anthropologists may have made some contributions to the study of religion, he writes, 'we nevertheless think that their account of the social roots of religion adds nothing new to Marxism. Moreover, the problem of the social nature of religion is accounted for in Marxism with greater accuracy and, in essence, exhaustively' (ibid.: 232). Such a view leaves no room for the development of theory by Soviet ethnographers. This claim is perhaps best read as an assertion of the perfected nature of Soviet ethnographic theory in a text that was written for a foreign audience, and that can be assumed to have undergone strict scrutiny for ideological compliance, but that is not all it is. Basilov devotes the first half of his article to elaborating the previously mentioned claim that Soviet ethnography is devoted to analyzing the basis of religion in social organization. Only in the latter part of the article does Basilov turn his attention to issues that are the focus of his own work, prefaced by the assertion that 'Historicism in the study of religion is a prerequisite of Marxist-Leninist methodology' (ibid.: 238). In this, he describes an approach to ethnographic work that has no ambition to develop theory and that undertakes to reconstruct the origins of rituals with only the broadest allusions to the theoretical basis for such a project.

In Snesarev and Basilov's works, 'survivals' are of two types: on the one hand, elements of pre-Islamic religion that, centuries ago, survived the domination of Islam; on the other hand, elements of a religion, Islamic or pre-Islamic, that survived the advent of socialism. Though their mutual interest in the first type of survivals did not extend to an interest in the social conditions that allowed these survivals to persist, their interest in the latter type of survivals was based on the functions of this kind of survival, which are analyzed both as being caused by enduring aspects of an antiquated social order and as providing the reactionary ideological underpinnings that could allow anti-socialist social order to persist. Hence, the practical solution to the problem of survivals is seen to lie in alternately promoting social change through administrative measures and attacking the ideas that perpetuate harmful social practices. Thus, Snesarev writes: 'The serious attention of scholars, the Party, and Soviet society should be directed to the dark aspects of family life, since the remnants of patriarchal-feudal relations in

the family create that closure of women's daily life that turns it into a sort of preserve for religious survivals' (1957: 66). At the same time, in a rather un-Marxist admission, Snesarev notes that despite the many Soviet successes in changing the relations of production — specifically, women taking an active role in the rural workforce — expected changes to social organization had not occurred (ibid.).

Both Snesarev and Basilov seem to be struggling between the ideological imperatives of affirming the successes of the Soviet program of social transformation, and of pointing out areas where this transformation has not succeeded. Basilov writes, 'The current condition of the cult of saints reflects the general process of religion dying out in our country. However, one must not present the matter as if now there remained only weak remnants of the worship of saints. Despite all the indisputable evidence of the decline of the cult, belief in saints still occupies a solid position in the worldview of a large proportion of believers' (1967: 19). In 1969, Snesarev wrote, 'Shamanism has disappeared and the officiators of that cult have long ago ceased to exist as a profession' (1969: 336). Yet, Basilov wrote, 'In the 1970s, I was present on several occasions at séances of Uzbek shamanesses' (1989 [1984]: 29). These contradictory assessments of the success of attempts to eradicate religious practices must be understood, not as meaningful statements about how the ethnographers observed the phenomena, but rather as variable responses to a changing ideological environment and the contradictory imperatives imposed on ethnographers to support various aspects of the ideological agenda.

In the case of Snesarev, we have not been afforded an opportunity to see how his writing might have changed as a result of the lifting of ideological strictures, as he died in 1989 and had ceased his ethnographic work much earlier. In contrast, Basilov did publish a major monograph on the topic of religious survivals with a noticeable post-perestroika imprint. Published in 1992, his book *Shamanism among the Peoples of Central Asia and Kazakhstan* marked the culmination of Basilov's long-standing interest in this topic. The book in no way declares itself to constitute a radical rethinking of the topic, possible only after perestroika reforms, though it does make reference to the changes taking place in the Soviet Union as a result of perestroika: In the conclusion, he describes the activities of a 'shamaness' [*shamanka*] who became famous in the Ferghana Valley during the glasnost period and notes that there has been a longstanding decline in the significance of shamanism, 'though the changes evoked by perestroika in our country have created fertile conditions for the activities of shamans and similar kinds of specialists' (1992: 307). Thus, it is probable that Basilov either wrote or revised this book to reflect the new ideological conditions of the time. It is noteworthy

that the book lacks the advocacy of social change that was prominent in Basilov's earlier works. It even contains some language that would have been unthinkable in the context of his earlier work. For example, in the final paragraph of the book, Basilov writes, 'The reader may note that in this book I avoided speaking of any special characteristics of the shaman connected with his heightened sensitivity and influence on patients. This does not mean that I deny the possibility of someone having such qualities (which we call extrasensory)' (ibid.: 312). This way of writing is consistent with the tremendous interest in extrasensory phenomena that was prevalent in the glasnost period. Meanwhile, it is not clear whether these differences from his earlier work are indicative of how Basilov would always have preferred to treat his material or whether they are indicative of changes that took place in Basilov's thinking as a result his experience of liberalization. Perestroika, of course, transformed the way that many people viewed the world, and it must be noted that in the years leading up to the publication of *Shamanism among the Peoples of Central Asia and Kazakhstan*, Basilov was actively involved in international activities that may well also have affected his outlook.

In spite of all of this, however, the overall character of the analysis presented in this text differs little from Basilov's earlier works: *Shamanism among the Peoples of Central Asia and Kazakhstan* is a study of the survival of shamanism in the Islamic context of Central Asia, and Basilov's analysis, which traces the pre-Islamic origins of beliefs and practices, hinges on evolutionist theory and a conceptual opposition between shamanism, Islam, and contemporary (socialist) order whereby survivals are treated as vestigial ideas and practices with no functional connection to the social order in which they exist. In this book, Basilov gives fuller exposition to themes first raised in Soviet-era work, including the questions of how shamans choose to become shamans and how ecstatic states form a part of shamanic ritual. Basilov treats these themes in a descriptive manner focused on individual experience rather than analyzing them as part of broader social processes. One might view Basilov's increased attention to individualistic and experiential themes as signs of new intellectual directions fostered by glasnost. This does not, however, reveal any systemic treatment of theoretical issues that Basilov might earlier have been working on quietly under the constraints of the Soviet system.

As with most pre-glasnost Soviet ethnographic scholarship, Basilov's post-Soviet work did not raise or address the theoretical challenges to the evolutionist framework that led to abandonment of such approaches in Western anthropology and the rejection of the concept of survivals that was at the centre of all of Basilov's work. Even writing in the early 1990s, after Basilov would have had ample opportunity for exposure to Western scholar-

ship and after it was no longer compulsory to address Western literature exclusively in negative terms, Basilov did not address problems that had been raised in Western anthropology concerning the concept of shamanism as a coherent religion. There is no evidence that Basilov used liberalization brought on by perestroika to develop a more theoretically informed set of concepts for his study, much less to reveal one that he had developed earlier.

Ideology, Theory, Notion

To conclude, I would like to offer some reflections on the system of ideas that formed the research agendas and interpretive frameworks of the Soviet scholars who wrote the ethnography of Central Asian Islam. We would be a mistaken to expect the picture to look much like scholarship in the form that we are accustomed to thinking about it in a Western social science context.

In Soviet ethnography, there was a relatively well-formulated grand theory (the Marxist framework of evolutionary historical materialism), which was often programmatically referred to in defining research problems, though this typically had little to do with the project that was actually pursued in the main body of Soviet-era ethnographies. In addition, mid-level historical materialist theoretical propositions also got some play in Soviet ethnographic works — for example, the proposition that features of religious ideology such as wedding rituals served the interests of the exploiting classes, or that aspects of the social order such as women's subordination were rooted in the economic system. We might also distinguish an analytical realm of ideology that pertained to the political agenda of the Soviet state and that is hardly reducible to Marxist theory. This includes such things as the Soviet state's insistence that ethnographers should praise its achievements, as well as the more variable strictures set on scholarship. The latter include the requirement of referring to Russia's colonial expansion as something Central Asian peoples welcomed, or the obligatory citing of recent Party Congress decisions. Some aspects of this realm of ideology focused on issues of interpretation, such as the post-World War II imperative to specifically refer to Russian culture as the source of modernization in Central Asia rather than using less ethnocentric terms like modern or European culture. Aspects of this ideology played a major role in the research agendas of ethnographers, such as when ethnographers were called upon to study the lives of workers or focus their attention on collective farms. In this case, also, the state's ideological agenda dictated certain conclusions in the ethnography, such as showing that collective farms were flourishing or that socialist construction had led to the development of a vibrant working class.

At the same time, other notions crop up in Soviet ethnography that are neither Marxist theory nor bear any obvious relation to the state ideological

program. There is a notion, for example, that Islam is inherently foreign to the formerly nomadic populations due to the putative incompatibility between Islam and a nomadic lifestyle. While this can be used to bolster the claim that Islam is foreign to Kyrgyz and Kazakhs, it does not obviously fit within the framework of Soviet state ideology, and indeed post-Soviet intellectual elites have readily incorporated the same claim in their nationalist ideology. Other notions may stem from ethnographers' attempts to rationalize their ideational frameworks. For example, a Soviet ethnographer might have called upon the idea that pre-Islamic survivals are more resilient than Islam in order to justify not focusing on Islamic practices, which would be viewed as more ideologically problematic. However, it is probably impossible to identify the sources of and reasons for many of these notions, as they are not explicitly connected with broader theoretical or ideological frameworks.

Soviet academia was characterized by intensive critique, but this critique was oriented toward formal conformity with official ideological norms. Novelty in scholarship was to be produced by applying an accepted framework to a new field of observation rather than by proposing innovations to the framework itself, which would have been risky at best. There were even constraints on the extent to which one could coherently apply theory and ideology because in cases in which theory and ideology were at odds, conformity to ideology took precedence. Ultimately, it was impossible to apply a truly Marxist perspective to the study of social processes taking place in the Soviet Union. One could hardly take a truly Marxist approach without taking seriously domination and power in social relations, but the domination and power of the Soviet state and elite in the transformation of Central Asian Islam was a taboo topic. Theoretical inconsistency, therefore, was not something that would earn criticism but rather something that would achieve ideological compliance.

From this perspective, it is perhaps unsurprising that little coherent theory emerged from Soviet-era ethnography of Islam in Central Asia and that, instead, this field of scholarship was characterized more by ideological compliance. In Soviet ethnography of Islam, the use of theory was largely mechanical and uninspired. Between the need to comply with the state's ideological agenda and the need to formalistically appeal to theory, also as a form of ideological compliance, there was little space left for the kind of engagement with theory that is considered essential in Western social science. And it is hard to find evidence either that Soviet ethnographers of Central Asian Islam had any aspiration to pursue such an engagement, or that they did not share the state's negative view of Islam. Soviet ethnography created a scholarly environment in which there was almost no serious theo-

retical treatment of the analytical concepts that scholars employed, and this enabled the flourishing of unexamined and unfounded notions that dominated — and continue to dominate — thinking about Islam in the region.

Arguably, in thinking about Soviet ethnographers of Central Asia as an intellectual group, it is inappropriate to expect that they must be either heroic opponents of official ideology or else cowardly collaborators. Perhaps it would be better to think of Soviet ethnographers more in the way that we tend to think of Soviet school teachers — that is, as people who in most cases had rather limited ambitions to develop new ideas, who were first and foremost loyal to the system from which they emerged, and whose understanding of their role largely coincided with how the Soviet regime understood it. Their role was not to critically examine prevailing conceptual frameworks or to produce new ways of thinking but simply to implement approved ideas in a given realm, whether it be in a schoolroom or in the study of religious practices. While these ethnographers may have been quite erudite, this did not necessarily mean that they were also theoretically ambitious or that they were opposed to state ideology.

To impugn Soviet ethnographers for having failed in a presumed intellectual or moral imperative they had to resist the state's ideological agenda is surely to non-reflexively reproduce Western biases that assume that it was natural and morally correct to resist the Soviet state, that any Soviet intellectual with integrity would have shared the theoretical ambitions and moral commitments of Western anthropology, and that ethnographers really did not in fact share the Soviet state's negative view of Islam and other 'backward traditions'. Indeed it is odd that we should hold Soviet ethnographers to such heroic standards when we consider that Soviet ways thinking about Islam have often been replicated in Western Sovietology as well as post-Soviet scholarship on Islam — for instance, the focus on the 'non-Islamic' character of Central Asian religious practices, the 'superficiality' of Islam among historically nomadic groups, and the anti-modern character of Islam's influence on social development all can be found as unexamined in assumptions in Western and post-Soviet scholarship. One does not have to be under the coercive pressure of the Soviet ideological regime to consider it unnecessary to develop more adequate theoretical frameworks for the study of Central Asian Islam.

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COLLECTIVE FARM STUDIES

Chapter 5
Ethnographic Views of Socialist Reforms in Soviet Central Asia: Collective Farm (*Kolkhoz*) Monographs

Sergey Abashin

Disputes over the Soviet period usually develop around the question of to what extent ideology and the political system defined and oppressed people living in the Soviet society, to what degree people were independent, and to what degree they could resist, lead their own lives, or even use ideology for their own purposes. With reference to Soviet ethnographers, the following questions might be posed: Within the imposed limits of censorship and self-censorship, which certainly restricted research possibilities, to what extent were Soviet ethnographers free from ideological dictatorship to pose problems, raise and discuss questions, and search for answers? To what degree did published research results correspond to what these researchers actually saw and heard in their fieldwork? Was there any development in science in general? I would like to highlight some issues that are of interest in this respect.

It is important to understand the place ethnography and ethnographers held within the Soviet system of power and knowledge. Many contemporary researchers in the West who study the Soviet epoch regard ethnography as the main site in which the Soviet language of nationalities policy and national narratives was formed, only to be subsequently assimilated and mastered by regional elites (Skalník 1986: 157-166; Slezkine 1994; Hirsch 2005). In my opinion, this assessment requires updating: in fact, the total number of ethnographers in the Soviet Union was always rather small, and authorities often distrusted them. After all, ethnographers had their own agenda, often opposed official rhetoric, and enjoyed a degree of autonomy. Relations within the ethnographic community were complex as well. One should take into consideration the interaction of ethnographers with representatives of other disciplines, relations between various traditions and schools (including central and republican), interactions between ethnographers and various institutes of power, and, lastly, interactions between Soviet scholars and their Western colleagues. We should also take into considera-

tion how the situation changed over time, under Stalin's rule, then under Khrushchev, Brezhnev, and Gorbachev.

For a Western readership, it is important to understand the experience and knowledge that accumulated during the Soviet rule. It seems to me to be both unproductive and incorrect to renounce this experience in order to study the region exclusively on the basis of concepts worked out by Western scientists (Verdery 2006), as if turning over a new leaf. During the Soviet decades, extensive materials were collected, selected, classified, and interpreted, and this heritage could and should be integrated into contemporary knowledge. But I would like to pay attention to another aspect, namely, how the language created by Soviet ethnographers became a part of the Soviet language of self-description and representation. Therefore, in order to gain the right perspective on Soviet identity and culture, it is necessary to undertake a genealogy of Soviet ethnography.

Soviet Ethnography from the 1930s to the 1960s

The period I will specifically look into here in this chapter extends from 1948 to 1968. But to better grasp the main trends of this given period, I would like to say a few words on the development of ethnography from the late 1920s onward.[1]

To some degree following the example of their German colleagues, in the 1920s many Russian ethnographers considered the discipline to be a science that studied various aspects of people's lives and that offered universal concepts of cultural history. In the late twenties, this view began to be opposed by a great number of young researchers who had received a Marxist education and were loyal to the Soviet state. Many scholars from the older generation suffered personal attacks and persecutions. There were appeals to completely renounce ethnography as a discipline because of its alleged uselessness. Eventually supporters of a less extreme approach got the upper hand, and ethnography received its modest new mission of studying preclass and preliterate societies. With its new ways of organizing economy and society, the present was expunged from the list of ethnography's priorities. As Nikolai Matorin, the formal leader of Soviet ethnography at the time, wrote, '[T]here is nothing specifically "ethnographic" in studying any collective farm or state farm with advanced techniques' (Matorin 1931: 21).[2]

[1] The 1920s are discussed in detail by Slezkine 1994. See also, Solovei 1998 and Bertrand 2002.

[2] The *kolkhoz* (collective farm) and *sovkhoz* (state farm) were the main types of socialist agricultural farms, established in the 1930s after total collectivization.

In turn, in the late thirties, both radical and moderate Marxists (and among them Matorin) were persecuted and harassed, and Soviet ethnography found itself almost on the verge of extinction.

During World War II, the regime became interested in ethnography and, in particular, the ethnographic study of Eastern Europe, which the Soviet army entered in 1944 (for further details, see Alymov 2006). Here, it is important to note that it was at this time that the Institute of Ethnography was founded in Moscow. Sergei Tolstov, a rather prudent man of the Marxist persuasion, became its head. He managed to be both moderate and radical and at the same time deeply immersed in research, all of which allowed him to avoid the troubles of the 1930s. Tolstov's specialization was archaeology and ancient history of Central Asia. He understood well that the main objective and, of course, the reason for financing the Institute and ethnography in general was that the state and power apparatus found this to be in its own interest. The study of primitive society, however, was hardly of interest to the state, and therefore soon after the war, Tolstov announced the return of ethnographers to concerns with modernity. Now the focus was on exploring USSR peoples' 'national specificities', a subject that allowed scholars to connect studies of the pre-Soviet past and Soviet present. Yuri Slezkine named this process 'the ethnisation of ethnography' (Slezkine 1994: 309-310). But I would contend that this ethnization actually took place much later, only after Tolstov's death, when Iulian Bromlei was appointed as the head of Institute of Ethnography in 1966. Bromlei was a historian specializing in the history of the Balkans, and so-called ethnos theory was created under his directorship and with his active participation. Though he advocated the study of national specificities, Tolstov was very cautious in developing ethnic narratives, which in the late 1920s and the 1930s were critiqued for being non-Marxist. Tolstov's cautiousness was necessitated by his research interests and his former sympathy for Marr's concepts.[3] From the late 1940s to the first half of 1960s, during which time Tolstov remained the official head of Soviet ethnography, ethnic and national problems had an ambiguous status; studies of ethnicity and nationality were the primary stated objective, but many individual and collective research topics had no relation to these themes whatsoever. References to national specificities were an important means of legitimating ethnography, but under the cover of this universal formula different research agendas were hidden.

[3] On N. Ia. Marr and Marrism, see Tuite this volume. See also, Slezkine 1996: 826-862; Alpatov 2004.

Studies of Collective Farm Peasantry

The study of the culture and life of collective farm peasantry in different republics of the USSR became one of the basic projects of the Institute of Ethnography, which received support during Sergei Tolstov's term as director. This project started in 1948 and most employees of the Institute were involved in it. I should mention here that it was only in 1950 that Stalin's critique of Marr in his well-known article 'Marxism and Problems of Linguistics' was published and ethnic studies were officially approved. Two years earlier, in 1948, such a turn had been completely unforeseen, so in all probability, Tolstov had considered the idea of collective farm peasantry research as a primary means of legitimating ethnography.

Central Asian collective farms were perhaps the most researched. One of the first articles on this topic was written by Tat'iana Zhdanko, who wrote a series of publications on Karakalpak collective farm life in the late 1940s and early 1950s (Zhdanko 1949, 1958).

In the 1950s, in addition to numerous articles and dissertations on the topic, the four so-called collective farm monographs were published, describing life on collective farms in Tajikistan (Ershov et al. 1954), Turkmenistan (Vinnikov 1954), Uzbekistan (Sukhareva and Bikzhanova 1955), and Kyrgyzstan (Abramzon et al. 1958). In the 1960s, two more monographs appeared: one on collective farms in Kazakhstan (Suleimanov et al. 1967), and one on collective farms in Uzbekistan (Rassudova et al. 1969). Though the last two books were printed when Iulian Bromlei was heading up Moscow and Leningrad ethnography, the materials were collected and the texts were prepared during Tolstov's term. I will briefly describe some characteristic features and the history of these monographs.

Not only Moscow and Leningrad scholars took part in the research that went into these works; Central Asian ethnographers also played a key role, primarily as co-authors and, in the case of post-graduate students, as assistants. Two books — one about Uzbekistan (published in Tashkent) and another on Kazakhstan (published in Alma-Ata) — were written exclusively by a small group of Central Asian scholars who were working within their own regional programs. Still, it is worth noting that at the time, researchers from the Centre and from the republics coordinated their work with the Institute of Ethnography, which served as the leading organization for researchers from the respective republics.

It is also important to note that for many researchers at that time, the collective farm monograph was not a personal research priority. For example, Ol'ga Sukhareva (see Naumova, this volume), the author of the book on Uzbek collective farms, was not very interested in the present; the overwhelming majority of her works were devoted to historical reconstructions.

She was especially known for her studies of Central Asian urban history (mainly, Bukhara). The book on Uzbek collective farms in Ferghana was obviously quite abreast of her primary research interests. It is possible to claim the same about many other ethnographers.

Notwithstanding variations and differing degrees of thoroughness, the structure of these books was more or less standard: 1) history of the settlement and of the collective farm; 2) economic activities of the collective farm (agriculture and water use, production branches, and collective farm statistics, including farmers' earnings); 3) home and family life (religious survivals, dwellings, food, clothes, and rituals); and 4) public life and culture (Soviet institutions and party organizations, educational institutes, clubs, etc.).

In the 1950s, the language of books was determined by a rigid ideological framework, with the works of party leaders being frequently quoted and with numerous phrases glorifying the achievements of the Soviet power in circulation. For example, in the Uzbek monograph written by ethnographers Ol'ga Sukhareva and Madina Bikzhanova, we find:

> [T]he history of Aykyran settlement and its collective farm named after Stalin reflects the victory of the Leninist-Stalinist nationalities policy of our Party. It shows in detail the immense reorganization of the Uzbek people's life, caused by the Great October socialist revolution [...]

and further:

> The elicitation of everyday life characteristics and changes in the culture of the modern agricultural population, generated by the socialist epoch, was the main goal of the authors of the present work. We tried to demonstrate how these new progressive features develop in the struggle against survivals of the feudal past, which are especially resilient in the field of ideology (Sukhareva and Bikzhanova 1955: 3).

It goes without saying that to fully grasp the context of the time, one must take into account censorship, which defined the limits of the permitted. Censorship was multivalent. At a session of the Institute, Tolstov remarked, 'Not everything that we collect is possible to publish in its entirety', thereby implying that certain findings might be used by the enemies of Soviet power. Tolstov diligently protected both himself and the Institute from ideological charges, at times being overcautious in his attempts to preserve the ideological purity of research. Censorship was also exercised through and in publishing houses; in preparing books for publication, publishing houses checked and corrected what seemed to them to be superfluous or negative.

Changes are evident in the 1969 volume on Uzbekistan (Rassudova et al. 1969). In it appeared a special chapter on ethnic processes that reflected new priorities for ethnographic research (ibid.: 17-49). The book also contained a chapter on religion (entitled 'The Spiritual Life of the Uzbek Peasantry'), which would not have been possible to include in a book in the 1950s. In a rather long chapter, Leningrad ethnographer Roza Rassudova discussed the historical emergence of collective farms and analyzed in a fresh manner the links between collective farms and traditional structures of Uzbek society. This last collective farm monograph was written right at the peak of Khrushchev's liberalization campaign, which certainly influenced the text. In those years, it became easier to openly discuss various problems, and the style and tone of the 1969 volume was therefore, in general, less orthodox.

Collective farm monographs were saturated with ideology, and Tolstov and the ethnographers' guild used this format to demonstrate their political loyalty. Nevertheless, research on collective farms was accompanied by heated debates, accusations, and self-criticism. These debates not only centred on ideological concerns and the legitimacy of ethnography, which were nevertheless obligatory and ritual elements of such debates; crucially, the methodology of ethnographic work was discussed in a manner that to some degree resembled similar debates taking place in world anthropology.

Debates on Methodology

Soviet scholars discussed how to study collective farms in a variety of forums and in relation to a number of audiences. In particular, these discussions took place at the first pan-Soviet Union ethnographic meeting in 1951. From 1952 to 1954, the debates inaugurated at this meeting subsequently continued in the pages of *Soviet Ethnography*.[4] These conversations were catalyzed by ethnographers who had been disappointed with the preliminary results of the first collective farm peasantry study. In the early 1950s, pre-existing ethnographic publications on collective farm peasantry looked hackneyed, shallow, and uninteresting. It was still commonly supposed that ethnographers should study times past and not contemporary topics.

Moscow ethnographer Pavel Kushner, who formally supervised collective farm peasantry studies at the Institute of Ethnography, published an article (Kushner 1952b) in which he explained the failure or impasse presented by the imperfect methodologies utilized in fieldwork. According to Kushner, collective farm studies did not constitute full-scale research; rather,

[4] Stalin died in 1953.

they were mere reconnaissance. Data were collected via questionnaires over the course of as little as a few days and up to two or three weeks. Kushner proposed that more stationary methods of research should be used, with long-term fieldwork allowing deeper observation. As it was presumed that research would be conducted by a group of scholars rather than a single individual, the fieldwork period could be reduced, but all the same it was necessary to observe the life of a people during different seasons and to coordinate teamwork. According to Kushner, a single research project would ideally last two to three years.

The second object of Kushner's criticism was the fact that ethnographers overemphasized the history of collective farms, the organization of agricultural production, and various statistics on the collective farm economy, while omitting the 'national specificity' of culture and everyday life. Kushner suggested that such economic questions should be put aside. Instead, he contended that ethnographers should study people's lives in their households as they undertake everyday activities. He raised polemical questions: 'Is the socialist consciousness of collective farmers formed only under the influence of the collective farm economy? Is it not the case that the economy is subordinated by the human mind?' (Kushner 1952b: 140).[5] Kushner, in particular, proposed refocusing research away from the collective farm to the broader settlement, on the grounds that not all countrymen were members of collective farms and people's lives were irreducible to collective farm life only.

Kushner's call for stationary field research was supported by many other ethnographers. An expert on Tajikistan, Leningrad scientist Nikolai Kisliakov wrote that ethnographic work consists of two periods: the first period is comprised of interviews undertaken within the framework of some particular programme, which requires somewhere between several weeks and three months to complete; the second period involves observation, which allows researchers to plunge deeper into family life and its inner relationships. The latter requires long-term participation in the life of families and local society (*kollektif*) (Kisliakov 1952: 147). An expert on Kyrgyzstan, Leningrad ethnographer Saul Abramzon also criticized the 'job-hopper' research method but noted that for various reasons it could be difficult for Moscow and Leningrad scientists to stay in the field for long periods of time, therefore for prospective monographs it was better to have ethnographers from the republics conduct stationary research (Abramzon 1952: 146-147). Besides, as Abramzon contended, in undertaking monographic research exclusively centred on one place, there was a danger of only shallowly

[5] Kushner would later reconsider this statement, ultimately pronouncing it to be mistaken.

penetrating what were ultimately trans-local problems; therefore, it was necessary to develop thematically driven research programmes. For stationary research conducted in a single location, there was the question of whether or not the investigated phenomena were typical. Stationary research thus did not allow one to make deeper theoretical conclusions.

An expert on Uzbekistan, Tashkent ethnographer Ol'ga Sukhareva supported the idea of stationary research. She wrote:

> The wide-ranging research thematic, which comprises many objects and is held by means of tours across a territory of considerable size, should rest on strong and reliable conclusions that result from long-term stationary research of a single object [...] [Stationary research allows us] to discover and comprehend the most characteristic and typical features of a people's life [...] [whereas en-route research provides] only superficial explanation of the facts ... (Sukhareva 1954: 114).

Moscow ethnographer Zhdanko in her turn insisted that monographs on collective farms 'are somewhat introspective and narrow; collective farm settlements were studied separately from collective farms of other areas and regions of republics', so 'local phenomena [...] could be misrecognized as common for the republic and its people' (Zhdanko 1956: 218). It was therefore necessary to compare the data from a single collective farm with materials covering a wider territory; in other words, stationary research should be combined with en route research: 'However typical a chosen collective farm could be, it should not be dealt with separately' (Zhdanko 1956: 219). Kushner rather reasonably replied that 'the same object could be typical for one research topic and quite atypical for another' (Kushner 1952a).

Kushner's renunciation of the study of collective farm production was met by objections. Abramzon criticised Kushner as an adept of 'everyday life studies' (*bytovizm*) and insisted that ethnographic study of the economy should be obligatory, as all aspects of life were closely interconnected (Abramzon 1952: 150). Kisliakov agreed that the economy was always in the sphere of ethnographic interest, however he contended that the contemporary, complex economy required special knowledge. Thus economists and agriculturists were ideally to be involved in monographic research. Ethnographers would contribute by studying everyday life and managing the research project. Kisliakov was also against contrasting the collective farm with the village: he described how the collective farm 'merged' with the village, maintaining that collective farm life was simultaneously the life of a village, and that the collective farm was not only a production collective, but also a social and political organism (Kisliakov 1952: 147). Kisliakov also noticed that elderly informants could not give exact information beyond the

framework of their everyday lives; therefore in order to understand different phenomena, it was necessary to use statistical methods, archival data, questionnaires, and other written sources.

In the same discussion, Abramzon reasserted that small settlements united into one big collective farm, which formed 'one whole organism' (Abramzon 1952: 150). He remarked that 'the ethnographer deals with actual persons first of all' (ibid.: 148). It was thus necessary to study individual persons and their biographies instead of providing generalized characteristics that only weakly related to actual individuals. By working with actual persons and their folkways, it would be possible to study national consciousness and national character.

Criticism of 'Collective farm Monographs'

In the mid-1950s, the first collective farm monographs were published, causing a new wave of debates. At a meeting on archaeology and ethnography of Central Asia in 1955, Sukhareva presented her analysis of a monograph of an Uzbek collective farm (she was its author) and another about a Tajik collective farm (Sukhareva 1955). She briefly stated the aim of this research: 'We study the problem of the transition of Central Asian peoples from feudalism and survivals of patriarchal society directly to socialism *per se*, omitting the stage of capitalist development'; 'The task of ethnographers is the analysis of the process of the backward pre-revolutionary peasantry's (*dekhkanstvo*) transformation into the advanced collective farm peasantry', and 'to find out national specificities that are reflected in the household life of collective farmers'; 'The ethnographer should give a full and colourful portrayal of peasant life in its entire national and local singularity' (Sukhareva 1955: 31).

However the implementation of this general outline was met with difficulties. The first question brought up by Sukhareva was in what way and how much the history of a given settlement in pre-revolutionary and Soviet periods should be presented in modern collective farms studies. Ethnography was considered to be a historical discipline, so at the time, ethnographers received a basic historical education. Thus ethnographers were generally interested in the past and the search for historical roots. They did not wish to become historians of the Soviet period, as Abramzon wrote, not only due to their educational background but also because of the fact that the study of the Soviet period imposed on the researcher a set of ideological restrictions.

The Uzbek monograph, as Sukhareva self-critically admitted, did not solve the problem of analyzing the period of the October revolution and the events of early Soviet history. As to the Tajik monograph, it showed the

Soviet period in greater detail but ultimately insufficiently attended to the pre-revolutionary period.

The second question under consideration was the study of economy and collective farm business, which Kushner had previously discussed. Sukhareva contended that it was difficult and even excessive for ethnographers to research economic activities and practices of collective farms with ethnographic methods and frankly admitted that the authors of the Uzbek monograph had left the study of the economy to the economists (Sukhareva 1955: 35). The authors of the Tajik monograph chose a different approach and decided to describe the local economy. However, the result was rather sharply (according to Sukhareva even 'severely') criticized at the Institute of History and Archaeology of the Uzbek Soviet Socialist Republic (ibid.: 36). Opponents claimed that the authors relied on 'old, obsolete methods' of management that were sometimes 'praised to high heaven' (ibid.). They had thus forgotten and underestimated the role of the new techniques and organization of production. On the whole, Sukhareva would contend that 'it is necessary to recognize that the problem of socialist relations in a given collective farm reality is solved neither in Tajik nor in Uzbek monographs' (ibid.: 35).

In Sukhareva's opinion, the chapters on family were more successful. But she also noted that the authors of the Tajik monograph had 'paid too much attention to the old forms of everyday life' (ibid.: 37). In particular, she mentioned a selection of drawings and photos that appeared in the book, which mainly illustrated former, traditional patterns of economy and everyday life. 'The reader does not see the new houses of collective farmers', Sukhareva reproached her colleagues and competitors. Ethnographers, of course, should expose 'backward features' but not protect them, and those features 'should not receive approval'. As a case in point, Sukhareva cited an article from Saul Abramzon, who, instead of criticizing 'patriarchal survivals' allegedly wrote positively about how, in the Kyrgyz family, women did not work (Sukhareva 1955: 38).

To defend Abramzon, I would note that this peculiar justification of old customs could be found in practically all ethnographic works of the time. Many of these works made similar statements: 'customs, the use of magic, retreated into the past' (for example, birth rituals), and furthermore, 'now, as our informants say, only people who do not wish to displease their old mothers adhere to these customs' (Rassudova et al. 1969: 216). Since, clearly, no one wishes to distress their old mother, it must then be the case that 'the customs, which retreated into the past' are observed everywhere! Another example: in 1969, Moscow ethnographer Gleb Snesarev wrote that '*gashtaks* [men's meetings] have almost disappeared' and that 'if we find

here and there something recalling them in substance, the similarity appears to be purely formal' (ibid.: 273). Snesarev even offered an original theoretical confirmation for the continued existence of survivals: 'Any survival phenomenon is a historical category and is not something frozen, static. In every such phenomenon, especially in conditions of modern reality, something dies, something new appears; sometimes there remains only a traditional form' (ibid.: 287). Still, Snesarev did not explain how one could distinguish form from content.

Let us move backwards from the 1960s to the 1950s. Critics of collective farm monographs had noted that the authors of the Tajik monograph did research in a village in which traditions had strong roots, but the situation in this settlement did not reflect the general situation in Tajik society, where survivals no longer played a significant role. Answering these charges in a speech given at the same meeting, Kisliakov remarked that it was 'more interesting to trace changes than to study a new collective farm on newly irrigated lands where there are no traditions' (Kisliakov 1957).

By the end of the 1950s, the criticism of collective farm monographs had changed in character. If Sukhareva thought that the authors of the Tajik monograph exaggerated the value of survivals, in respect to the Uzbek monograph, a reverse reproach was made that its authors had concealed negative phenomena by embellishing the life in their field site, presenting it as devoid of any problems, and failing to disclose social contradictions. Leningrad ethnographer Roza Rassudova, an expert on Uzbekistan, voiced such charges during the 1959 meeting of the Academic Council at the Institute of Ethnography. She said that local residents and scientists considered these books to be 'superficial', 'uninteresting', and 'biased' and that the books that did not help society in any way (Alymov 2006: 203-204).

Discussing both sets of criticism, Zhdanko made a similar statement. On the one hand, economists and historians of Soviet society condemned these books for their 'excessive tribute to the past'; on the other hand, ethnographers were reproached for paying 'insufficient attention to the survivals that pass forever, [for] insufficient display of the role of the old in the new, [for] stretching the truth, [and for] devotion to "well-known" sides of collective farm life'. Zhdanko continued to insist that the ethnographer should not become absorbed exclusively in 'times past and break away from contemporary life of people'; rather, 'he should observe and study ongoing [...] changes in the culture and outlook of the peoples, that testify to the process of gradual transition from socialism to communism' (Zhdanko 1956: 218).

Soviet Ethnography in Central Asia

If I am permitted to assess the value of collective farm monographs and the direction of such research in general, I would like to contend that these works belong to the mainstream of anthropological research of the time. At the same time in Western anthropology, a shift from the study of 'primitives' to concerns with the 'peasantry', the 'nation', and 'urban' and 'migration' studies was taking place.[6] Admittedly not an intellectual centre or a reference point for theoretical originality, Soviet ethnography belonged with all its particularity to that common anthropological tradition and developed under the same laws. From an insider's perspective, irrespective of the current political agenda, Soviet ethnography managed to create a locus of independent thought. Though ethnographic research on collective farms in the 1950s and 1960s served an ideological function, inside the ethnographic community there were nevertheless serious disputes surrounding the question of what to study and how; there was discontent with research results and a desire to go further in order to correct errors.

I would like to specify some of the contradictions that spurred further reflection, discussion, and even conflicts, and which demonstrate the complex internal life and dynamics of the ethnographic community. Collective farm research was undertaken at the same time that Stalin's epoch of suspiciousness and fear was coming to an end and Khrushchev's rule was starting. This was a thawing out period that considerably expanded the limits of what was permitted, generating new views and opening up possibilities for critical reassessment of the discipline's activities. Furthermore, the transition did not pass simultaneously and evenly everywhere. Such conditions made fertile soil for the collision of different research preferences. Moscow and Leningrad ethnographers keenly felt the contradictions between the ideological dogma they received during their upbringing and education, the living standards in the capitals, and the absolutely different reality that they observed in the field. For many of these ethnographers, the fieldwork experience was both a cultural and ideological shock.

There existed an obvious contradiction between historical education and the historical conceptual framework of Soviet ethnography, with its determination to study primitiveness and ancient exotics, on the one hand, and the necessity or obligation of analyzing the present (the subject to which Soviet ethnographers grew unaccustomed), on the other. Even when observing contemporary life, Soviet ethnographers consciously or involuntarily looked backward and exclusively reconstructed the survivals from the past.

[6] See, for example, works from the same period on similar topics such as Redfield 1955, 1956.

Finally, there was an obvious contradiction between the 'centre' and national republics. Critics from Central Asia reproached Moscow and Leningrad scientists for both embellishing and negatively presenting the reality of peasant life. Such criticism was rather contradictory, but its essence in any case consisted in a defence of the right of Central Asian scholars to speak on behalf of 'their own' nations or republics. The 'centre', of course, enjoyed many advantages in this debate, but it did not possess a monopoly on truth, and we might claim that the increasingly strong local academic elites took over the initiative in regional studies.

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Chapter 6
From Collective Farm to Islamic Museum? Deconstructing the Narrative of Highlander Traditions in Dagestan

Vladimir Bobrovnikov

Portrait of an Oriental Village

Let me start my account of the ethnographic tradition in studies of Dagestani mountaineers with a personal note, if not reminiscence. I first encountered mountaineers' 'traditions' at the turn of the post-Soviet era, in September 1992. In spring or summer of that year, Tamara Sivertseva, a scholar from the Moscow Institute for Oriental Studies, specializing in gender sociology of the Middle East, proposed that I join her expedition to a remote Muslim village in the Tsumada district in the mountains of north-western Dagestan, at the Dagestani border with Chechnya and Georgia. For this project, I was charged with investigating the micro-history of the village community under Russian rule.

A greenhorn Arabist from the same institute, I had recently finished writing a dissertation on custom and modernity among Berber-speaking mountain peoples in nineteenth and mid-twentieth century Maghreb. My work centred mainly on French colonial anthropology, the immense body and conceptual constructs of which were by that time frustrating me. I was eager to discover real, living mountaineers still following — I expected — a traditional way of life, far from the dull existence of modern civilization. Consequently, I gladly accepted Tamara's invitation.

The expedition turned out to be a downright adventure, and its results had a quite unexpected influence on my subsequent research career and interests. After this trip, I plunged into Caucasian studies and social anthropology and continued working on the North Caucasus.

The setting for my first case study (1992) seemed to have been well chosen. Khushtada, a mountain settlement in the Tsumada district, had a classical traditional appearance, embedded in a primordial mountain landscape, at least in the eyes of romantically minded Moscow scholars. The settlement was what one member of the expedition, Evgenii Rashkovskii, would later call a 'village fortress' (selo-krepost') (1999: 328), and it looked

as if it had come down from one of the panoramic colonial pictures of Franz Roubaud, who painted battles of the Russian Caucasus war.[1] The dead walls of the settlement houses, built of grey and brown river stones, blended in with the landscape, resembling a beehive stuck to the vertical cliffs of Mount Angaraz, which towered over Khushtada. The flat roofs of the lower dwellings served as courtyards for the upper ones. In the centre of the village was an ancient two-story mosque that had been erected in the year of Hijra 1009 (corresponding to 1600 or 1601 AD), according to an Arabic inscription on the building wall. It was thus the oldest mosque still in use in the whole Tsumada district. Until 2003, at its side stood a round tower, the upper level of which had been destroyed by the Russians after the defeat of the famous mountain hero Shamil in the mid-nineteenth century, according to the oral histories of Khushtada. The stones of the village witnessed the advent of Islam in the sixteenth century as well as the turbulent years and civil wars of the nineteenth and early twentieth centuries. They survived Communist modernizing experiments and made me and my friends feel that the modern life we had left in Makhachkala only one day earlier was but unreal. Their history and age even preceded the mosque's, as is indicated by the solar petroglyphs on the walls of the building. Archaeologists date them back to the pre-Islamic times, from the tenth to the fifteenth centuries.

The local folk appeared to fit the ostensibly perfectly traditional landscape of a mountain village. We met mainly elderly men and women. Later I learned that in mid-September, when we first visited Khushtada, women and teenagers from the village were harvesting onions in lowland state farms or *sovkhozes*, the lands of which they, since the late 1970s, had rented each year. The Dagestani elders were really very impressive, especially the ones we found sitting at the entrance of the mosque in a tiny central place called *godekan* by Dagestani mountaineers. Though the weather was bright and sunny, village elders wore sheepskin coats with false sleeves. Later, I learned to appreciate the convenience of this traditional clothing at a cold dawn in the mountains.

In the morning, married women queued for water with heavy tin carrier cans or *kumgany* at the fountain near the lower mosque of Khushtada, the place of their regular gatherings to practise the female *zikr* (Sufi rite of remembrance of God). These scenes evoked an image of the continuity with old local traditions in present-day life. Most of my visits to Dagestan occurred in late autumn, when the mountaineers would store beef. This helped me to sample most of the traditional dishes of their scanty kitchen, though I am not a great admirer of mountain cooking. I also had the opportunity to

[1] These paintings are displayed in the Museum of Fine Arts in Makhachkala, the capital of Dagestan.

see the interiors of several traditional houses as well as to observe mountaineers in their everyday public and family lives. Alongside the modern facilities, the mountaineers maintained a number of traditional utensils like wooden granaries called *hek'osh* by the locals.

This expedition was initiated by Tamara Sivertseva. She received funding from a number of Western foundations that started working in Moscow in the early 1990s.[2] The project's theoretical approach was elaborated by the political scientist Mikhail Sivertsev. The fieldwork location was chosen by another Moscow Orientalist, Mikhail Roshchin, who directed our group of four researchers, which worked in Dagestan in the early autumn of 1992. The project resulted in two village monographs. The first book about Khushtada appeared in Moscow in 1995. In 1999, it was followed by a monograph dedicated to the 'traditional identity' of Rutul villages (Sivertseva, Roshchin, and Bobrovnikov 1995; Sivertseva 1999a). Both books claimed to present a new sociological approach to the contemporary Muslim societies in Russia's borderlands, based on first-hand field data. As such, they were supposed to mark the beginning of a series called 'Portrait of an Oriental Village' (*Portret vostochnogo sela*).

It so happened that I took part in writing and editing both books, and their history is very familiar to me. The project was very ambitious but poorly prepared. Almost all its participants, including the project leaders, lacked substantial fieldwork experience. To compensate, we reflected on highly sophisticated theoretical patterns and compared the North Caucasus countryside to the modern Arab Middle East, which we knew about from sociological literature, not from field research.[3] For this reason, Sivertseva's condescending advice to anthropologists to 'observe, suggest a research hypothesis, and perform an experiment' (Sivertseva 1995: 4) seems quite inept if not ridiculous.

In fact, the whole introductory chapter of the book provided readers with nothing but a confused list of banalities and a conglomeration of well-known modern research methods taken from different disciplines, often incompatible with each other. Of course, oral histories (Sivertseva 1995: 6; 1999c: 283-285), for instance, might be an important means of investigation, but none of the authors of either volume based their account of the village on

[2] There was a bundle of ambitious projects, including grants received from the MacArthur Foundation in 1993 and 1998. These later awards allowed Sivertseva to publish two village monographs. At the end of the 1990s, Tamara Sivertseva moved to the United States. With the support of IREX, she is conducting fieldwork in Azerbaijan that focuses on the role of 'tribal identities' in rural-to-urban migration.

[3] See, for instance, other monographs published from the 1970s – 1990s by the main contributors to this project: Sivertsev 1975; Sivertseva 1985, 1997; Roshchin 1992; Bobrovnikov 1998.

them. Genealogical tables reprinted from recent works on Rutul ethnography and a sample from a family oral history lacked substantial comments (Sivertseva 1999a: 286-305, 311-314). Why re-invent the wheel concerning oral tradition at the turn of a new millennium? Nonetheless, the book had some merits: separate case studies dedicated to gender, economic issues, folklore, and the ritual practices of Muslims in the former Soviet Union were incorporated in both of Sivertseva's monographs (Bobrovnikov 1995: 47-62, 1999: 98-112; Bobrovnikov and Roshchin 1995: 115-131; Khan-Magomedov 1999: 143-156; Konstantinova 1995: 145-192; Ramazanov 1999: 215-239; Ramazanov and Bobrovnikov 1995: 132-144).[4]

Seeking Ethnic Traditions in Mountainous Dagestan

The 'Portrait of an Oriental Village' series was not continued. Its first two issues were its last publications. One third of the papers presented in the first volume (five of fourteen) and eight of the twenty-two articles of the second volume were either inappropriate to the topic of the monographs or incomplete, requiring serious revision and comments.[5] Some chapters seemed like they could have been written in the days of early colonial rule. They were full of misleading Orientalist allusions and even factual mistakes.[6] In short, the program failed. Despite Sivertseva's numerous assurances about 'long-term contacts with respondents' and full-time involvement of the project participants in fieldwork, most of the authors from Moscow and Makhachkala confined their visits to the mountaineers to less than a week. Only Mikhail Roshchin and myself continued the case studies of the Khushtada community from 1995 to 1997, though we carried out our subsequent study independently, working with different programs having nothing in common with the concept and aims of Sivertseva's project.[7]

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[4] Some of the monograph chapters were drawn from earlier publications by project participants. The others were seriously revised and later republished under new titles. See, for instance, Bobrovnikov 2005: 157-183; Bobrovnikov and Roshchin 2009: 230-247; Khan-Magomedov 1998; Musaev 1997.

[5] Some of studies published as annexes to the first volume did not at all relate to the village of Khushtada or Dagestan. For instance, under the pen name Konstantinova, Moscow Orientalist Tat'iana Savel'eva published a case study of gender issues among Russian and Uzbek women in the town of Tashkent. There was even a strange nationalist essay by Viktor Nikitin on the rebirth of Great Russia in the national ecological movement (Nikitin 1995: 225-236).

[6] For instance, Rashkovskii (1995: 12-13, 25-27, 1999: 328-331) stated that economic autarky of traditional village fortresses survived up through modernity. Sivertsev (1995: 112) mistakenly contended that the introduction of Russian troops into the mountains of Dagestan threatened to incur jihad warfare.

[7] In my case, I did fieldwork in Khushtada from 1995 to 1997, with the financial support of the Wenner Gren Foundation for Anthropological Research and the Soros Foundation. My

I consider this project to be a strange but somehow characteristic historiographical episode.[8] The two monographs that resulted from it mark a certain stage in the development of Dagestani historiography of mountain customs. Though published in a small number of copies and never actually sold in the bookstores of Makhachkala and Moscow, these books — and especially the first volume — were noticed by anthropologists and historians doing fieldwork in the Caucasus.[9] They drew the attention of Dagestani scholars and were reviewed and cited in bibliographies in the region.[10]

On a programmatic level, the 'Portrait of an Oriental Village' project continued the search by Soviet scholars for local mountain traditions. Following this example, the authors of the monographs edited by Tamara Sivertseva divided the mountain societies along the lines of local custom vs. global modernity.[11] They were eager, at any cost, to discover the mountaineers' traditions (even if they did not exist at that moment at all). This intention is clear from Sivertseva's statement about the initial goals of the study (1995: 10-12).

Tradition played an important role in the project's conceptual framework. According to Sivertseva (1995: 10), 'the village chosen for the study should have developed and well-preserved spiritual traditions, such as a permanently functioning church, mosque, or prayer house'. The main selection criterion in the Khushtada case was a 'degree of integrity of living Islamic traditions' (ibid.). Deploring the corruption of the 'militaristic patriarchal culture of the village-fortress', as she put it, the editor argued that 'traditional links including clan (*tukhum*), guest-to-host, and communal relations as well as respect for elders […] are able to deter growing radical

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aim was to study land tenure regimes in a village community as a sort of local answer to socialist and postsocialist changes in Dagestani society.

[8] I happened upon Mountainous Dagestan by chance. Nevertheless, it should be noted that in the early 1990s separate specialists in African and Asian studies from the Moscow Institute for Oriental Studies launched a number of case studies based on sociological fieldwork in ex-Soviet societies. See, for instance, Kosmarskaia 2006: 11, and the work by Tat'iana Savel'eva mentioned in footnote 5.

[9] Several years ago, the Khushtada monograph was republished online on the website of the Tsumada district of the Republic of Dagestan. It is still available at http://www.tsumada.ru/?f=book&p1=histor&p2=book&id=hushtada.

[10] For a critical review of Sivertseva's project analyzed from the point of view of the Soviet ethnographical school, see Karpov 2010: 168-172.

[11] It is noteworthy that this division was also powerful in Western anthropological literature. The second volume edited by Tamara Sivertseva in 1999 was an attempt to combine the merits of Western and Soviet anthropology. Besides the Moscow Orientalists, who claimed to apply the research methods of Western scholarship, a number of Dagestani anthropologists belonging to the Soviet ethnographic school such as the late Professor Angara Bulatova and Gamzat Musaev contributed to the volume.

movements like that of the Wahhabis, guaranteeing social economic stability and subsistence' (1999b: 6). Following this approach, the authors of both monographs presented their data on the social organization, economics, material culture, manners, and customs of mountaineers in terms of tradition and modernity. For instance, the Moscow-based economist Andrei Sud'in (1995: 31-36) examined settlement and habitation practices of the mountain-dwellers in Khushtada by dividing them into two distinct categories of analysis: contemporary outskirts and the ancient core of the village. According to the author, custom reigns in the centre of Khushtada, making way for modernity in the modern quarters at its outskirts. In my own writings (Bobrovnikov 1995: 49, 52, 58), I too exaggerated the traditional character of land tenure in the mountains and the influence of customary law norms on the social behaviour of contemporary mountaineers.

Continuing my own case studies in Mountainous Dagestan, I had to plunge again into the omnipresent narrative of mountaineer customs. Tradition and modernity used to be seen as eternal natural antagonists in the literature of Soviet anthropologists (e.g. Islammagomedov and Sergeeva 1988: 7, 9-11). Most (though not all) anthropologists who wrote about the area (as well as about other regions of the North Caucasus) portrayed it as a sort of historical ethnographic open-air museum. Chronologically, they would begin their accounts of mountaineer traditions with the introduction of Russian imperial rule in the eighteenth and the nineteenth centuries, concluding at the time of the establishment of Soviet power. In so doing, they permanently referred to pre-Soviet times. The threshold between custom and modernity was October 1917, as some authors clearly stated (e.g. Seferbekov, Gashimov, and Bulatov 2004: 5). Only in the very ends of these works would there be a short epilogue listing the benefits that appeared under Soviet rule, in order to show the growth of culture and the well-being of the Soviet people at the end of the twentieth century. This attitude is evident also in the predominant anthropological descriptions of contemporary Dagestan. As a rule, accounts are narrated in the simple past in order to stress the link of the ethnographic data to ancient times.

What did 'tradition' mean for late Soviet (and post-Soviet) anthropologists in Dagestan? In the *Encyclopaedia of Russia's Peoples*, a leading Soviet anthropologist specializing in prehistory, Abram Pershits (1994: 462) defined tradition as a 'process of non-biological transmission of established patterns of behaviour from one generation to another, as well as established patterns of behaviour as such'. He distinguished different forms of traditions according to their content (ideas, norms, etc.), social functions (rituals and customs), political evaluation (positive, neutral, and harmful), and social background (elite and people's traditions). 'Traditions present the most

stable, stereotypical element of culture, contrary to innovations' (ibid.). According to Pershits, the importance of custom diminished in modern times.

Most Dagestani and Russian anthropologists still accept such definitions. Their studies of tradition are first of all concerned with material culture. As a rule, investigation of ethnic culture starts with a detailed description of the ethnicity in question (e.g. Islammagomedov 2002; Musaeva 1995). In addition, traditions are perceived to include social organization with an emphasis made on clan structure and village community. In the last quarter of the twentieth century, a fixed pattern of portraying mountaineer traditions emerged. Its successive components were: household; settlement; dwelling; clothing; food; rites of passage concerning marriage, birth and death; community governance; clan-*tukhum*; hospitality; mutual aid; and entertainment. All of the ethnographic accounts of the manners and customs of mountaineers follow this repertoire of research topics. In these works, traditions are studied at the level of village and ethnic communities.

Given the ethnocentrism of late Soviet anthropology, the study of traditional culture was closely related to the investigation of ethnogenesis and the historical evolution of titular ethnic groups and minorities. Soviet anthropology had a planned character, and these topics were included in the individual and collective projects of research fellows at the Institute of History, Archaeology, and Ethnography in Makhachkala. Each case study of an ethnic group, once completed and published, became a doctoral (*kandidatskaia*) dissertation. In the whole North Caucasus, as in the other Soviet 'peripheries', it became accepted as a rule that anthropologists should belong to the people that they were going to study. Such an unspoken rule seemed to have emerged in Soviet anthropology in the first decades after World War II (Longuet-Marx 1990: 370-375). Thus, anthropologists were supposed to seek after their own ethnic traditions and write them down in order to preserve the cultural heritage of their people in the body of a singular and indivisible Soviet people in Russia. This is an implicit task for every anthropologist, in the opinion of most contemporary Dagestani scholars.

In their turn, their Moscow and Leningrad colleagues did similar work. In 1944, an expedition to Dagestan was carried out by the Moscow Institute of Ethnography under the direction of Evgenii Shilling (Volkova 1992: 29-30, 199-200). Similar expeditions were regularly carried out until the early 1990s, resulting in numerous publications. One of the first was a collection of articles devoted to the peoples of Dagestan (*Narody Dagestana* 1955). Later, separate monographs on titular peoples were published, beginning with the works of Evgenii Shilling (1949) on the Kubachis and Sakinat

Gadzhieva (1961) on the Kumyks.[12] In the 1990s, a series of monographs on Dagestani minorities without titular status was initiated, following the same pattern based on local ethnic traditions. Contrary to the monographs edited by Sivertseva, this series described mountaineer customs in an anonymous manner, omitting people's life stories and concrete historical context.

Attempts at Escaping from History

Overall, the two centuries of Russian rule in North Caucasus were marked by rapid and significant changes in the society and culture of local populations. Mountaineers gave up many local customs, adopted Russian-style clothing and houses, learned Russian, and were involved in extensive village-to-town migration throughout the region and beyond its borders. Nevertheless, post-World War II anthropology was more concerned with the traditions of mountain peoples. Of course, there were other important topics in social anthropology like enthnogenesis and socialist nation-building. But tradition remained a topos in anthropological research. Tradition became the key notion defining the frame of and approaches to institutions and peoples, which Soviet anthropologists worked with and took for cold realities (Volkova 1992: 37-45, 54-55). Researchers of ethnic traditions drew considerably on ethnographic data gathered in Mountainous Dagestan under tsarist rule. It is no coincidence that their works on the ethnic traditions of the mountaineers took the eighteenth century advent of the Russians in the pre-modern Caucasus as a starting point.

Written by Russian military officers with the help of local Muslim scholars in the Russian service, the first ethnographic descriptions of mountaineers appeared in the times of the Russian conquest, from the eighteenth to the mid-nineteenth century. A systematic ethnographic study of Mountainous Dagestan was initiated by the Caucasus branch of the Imperial Russian Geographic Society, established by the Russian viceroy to the Caucasus in Tbilisi (capital of present-day Georgia) in 1851 (Bobrovnikov 2002: 147-150). For more than half a century, numerous observations were carried out, and dozens if not hundreds of research works were published either as separate issues or in historical ethnographic journals sponsored by the regional imperial power. Amongst the most important of these periodicals were *Collection of Information about the Caucasus Mountaineers (Sbornik svedenii o kavkazskikh gortsakh), Collection of Information for the Description of Territories and Tribes in the Caucasus (Sbornik svedenii dlia*

[12] Gadzhieva's monograph is the first example of a systematic description of ethnic tradition, and its pattern became a generally adopted cliché in most subsequent monographs about Dagestani peoples from the 1970s to the 2000s.

opisaniia mestnostei i plemen na Kavkaze), and *Dagestani Collection (Dagestanskii sbornik)*. The importance of the imperial 'pre-history' of Soviet field ethnography in the area under study is still underestimated (Bobrovnikov 2002: 148-152; Jersild 2002: 65-71).

Dagestani anthropologists of the Soviet (and post-Soviet) school often reproach their pre-revolutionary colleagues for ignoring the local pre-imperial past while focusing exclusively on the tsarist present (e.g. Musaeva 1995: 4). Actually though, imperial and Soviet Caucasian studies greatly differed in their respective approaches. Nineteenth century anthropology had not yet separated from the other social sciences and humanities. Under tsarist rule, ethnographic work was conducted jointly by anthropologists, linguists, and Orientalists. This was the heyday of the so-called *kraevedy* school, consisting of amateur folklorists and amateur historians specializing in regional (*krai*) studies, following the late Russian imperial scholarly tradition. Though continuing the German ethnographic tradition of Volkskunde that dominated pre-revolutionary ethnography in Russia, anthropologists of the nineteenth and the early twentieth centuries were much more oriented towards modernity than their Soviet successors. Contrary to their predecessors, post-World War II Soviet ethnographers were much more concerned with the pre-modern past of mountaineers. From the nineteenth to the early twentieth century, anthropology was closely related to sociology, for instance in the works of Maksim Kovalevskii (1890) and his disciple Bashir Dalgat (1934). In Soviet scholarship, ethnography became the 'servant' of archaeology, helping to reconstruct the prehistoric past of mankind in the manner of positivist German *Urgeschichte*.

Soviet notions of 'ethnography' and the 'ethnographic past' date back to the classical positions of positivist anthropologists like Edward B. Tylor and especially Lewis H. Morgan, which were appropriated and modified by Soviet anthropologists.[13] In its late Soviet meaning, the notion of tradition was based on the famous yet mostly forgotten (except in the Caucasus) notion of 'survival', introduced to anthropological usage by Tylor at the end of the nineteenth century (see DeWeese, this volume). Tylor defined survivals as 'processes, customs, opinions, and so forth, which have been carried on by force of habit into a new state of society different from that in which they had their original home, and they thus remain as proofs and examples of an older condition of culture out of which a newer has been evolved' (Tylor 1903: 16). In the Soviet era, Tylor's works entered the canon of anthropology, which was studied and commented on by each and every Soviet anthropologist, including those from the Caucasus, of course.

[13] On the influence of Morgan's views of the primitive clan on Soviet anthropological and historical scholarship, see Christian Dettmering's (2005: 469-489) excellent paper.

In the interwar period, the notion of survivals was reinterpreted in political terms. All remnants of the old regime, such as religion and religious courts and rituals, were to be destroyed and were labelled as harmful survivals of the past. In 1928, a special bill 'On crimes being survivals of the tribal past' passed in some autonomous republics of the Russian Socialist Soviet Federation in the North Caucasus, including Dagestan. This law formed the tenth chapter of the Russian Criminal Code and was in force up until 1996.

A number of local village traditions were legalized in the second half of the 1940s. Nevertheless, anthropologists looked for ethnic traditions outside of modern history. They also essentialized this concept by relying on the out of date, Tylorian notion of survivals and Morgan's notion of savage society, both of which were constructed on the example of Iroquois and other nineteenth century stateless societies, but which never existed in reality anywhere (Dettmering 2005: 489). Both models were considerably rethought in the late Soviet era, becoming a sort of generally accepted official cliché. It is amazing that post-World War II scholars turned their attention to the already vanished pre-Soviet past while almost completely ignoring the socialist present in their narratives of local ethnic traditions. Their fieldwork was done in mountain collectives and state farms set up all over Dagestan in the 1930s and early 1940s. Nevertheless, even the names of local *kolkhoz*es were omitted in these accounts.[14]

To some extent, this peculiarity can also be found in the monographs edited by Tamara Sivertseva in the 1990s. In the first monograph, we referred to the Chapaev collective farm as the framework of local traditions. But in the second book, there was no mention of the Mirzabekov Kolkhoz in Shinaz or the Rutul Sovkhoz in Rutul, even though they continued functioning from 1995 to 1998, when we conducted fieldwork in these villages.[15]

The Return of Pre-Modern Islamic Traditions?

The end of Soviet rule in 1991 marks another turning point in the anthropology of contemporary Dagestani mountaineers. Many scholars prefer not to transgress this date when describing local ethnic traditions. For instance, Ruslan Seferbekov clearly omits the post-Soviet decades in his work on the traditional way of life among the Tabasaranis, although the work was published in Makhachkala thirteen years after the disintegration of the Soviet

[14] There were but a few important exceptions to this rule such as Ikhilov 1963 and Nikol'skaia 1950. See also Abashin's survey of *kolkhoz* monographs in this volume.

[15] It is also noteworthy that A. F. Panfilova completely forgot about the existence of the *sovkhoz* and did not even inquire about its name in a published field diary where she often referred to so-called household records (*khoziaistvennye knigi*) as belonging to the Rutul state farm (Panfilova 1999: 323).

Union (Seferbekov, Gashimov, and Bulatov 2004: 5). This is rather typical for contemporary Dagestani anthropologists engaged in the study of ethnic traditions.

Furthermore, many of them fall ill at ease when dealing with Islam. The disintegration of the socialist polity was accompanied by a stormy re-Islamization in the North Caucasus. The Muslim spiritual elite recovered and even expanded its influence on Dagestani society and has been trying to revive religious culture, which was outlawed in the USSR. They enjoy the support of former Communist Party officials who have retained their leading positions in the republic's government. Islam has once again become a political trump card, and every politician hastens to assure believers that he loves and protects the religion. This is illustrated in a typical statement by the first president of Dagestan, Mukhu Aliev: 'Without religion, our history means nothing, and thus we will help our traditional religious movements, traditional religion… and strengthen the Spiritual Board in this matter' (Shikhsaidov 1999: 65).

Today, once again, views on customs and religion are changing. What the early Soviet leader of Dagestan Nadzhmuddin Samurskii called a 'religious narcotic' (Samurskii 1925: 3) is once again being exalted as the glorious traditions of the highlanders, depicted in a romantic manner à la Walter Scott's novels. Measured by the number of Islamic institutions, Dagestan is one of the most Islamized regions of Russia. From 1990 onwards, the number of registered mosques grew rapidly, as did the number of new congregations (*jama'at*). The number of congregations increased from 27 in 1988 to 1,557 in January 1998, and to 1,910 (including 1,107 Sunni congregations and 20 Shi'i religious organizations) by January 2007.[16] To date there have been no more considerable changes in these figures. The building of new mosques and prayer-houses has become the most common act of piety in postsocialist Dagestan.

Remarkably, most mosque congregations in the countryside re-emerged in the framework of small mountain collective farms, which continued operating in Dagestan until the beginning of the 2000s. For example, the Chapaev Kolkhoz gave birth to the Khushtada *jama'at*, and the Mirzabekov Kolkhoz turned into the Shinaz *jama'at*. In northern Dagestan, numerous private prayer-houses called *qurma* (or *qulla* after the Arabic word *qul'a*, meaning 'ablution room') appeared, consisting of a small prayer hall with a room for ablutions. Several qurma were rebuilt in seasonal cattle

[16] I am thankful to Kaflan Khanbabaev for kindly providing this statistical data from the *Archives of the Direction for Religious Affairs by the Government of the Republic of Dagestan*, kept in Makhachkala.

hamlets on the Chapaev collective farm. Although this is more than really needed, villagers consider the prayer halls a symbol of their devotion.

Competing visions of Muslim mountaineer custom emerged in post-Soviet Dagestan. The traditional narrative is challenged not only by secular scholars but also by the two rival Muslim factions that are pushing for the cause of re-Islamization in Dagestan: on the one hand, the officially recognized Muslim elite controlling the Dagestani Spiritual Board set up in Makhachkala in the early 1990s; on the other, the dissidents generally referred to as 'Wahhabis'. The two factions are bitterly opposed to one other. Congregations of Wahhabis emerged in the North Caucasus in the beginning of the 1990s. Their imams appealed for the 'purification of Islam' from 'illicit innovations' (*bida*'), including the Sufi ritual of remembrance of God (*zikr*), visits to holy places, celebrations of Muhammad's birthday, recitations of the Qur'an in cemeteries (*talqin*), and the use of protective charms (*sabab*). The Wahhabis regarded the Qur'an and the Sunna as the only sources for an authentic Islamic tradition, and they thus did not consider their traditionalist opponents to be faithful Muslims, referring to them instead as 'heathens' (*mushrikun*). Severely defeated in 1999 at the beginning of the second Russian-Chechen war, the Wahhabi movement went underground. Nevertheless, its defeat did not mean the reconciliation of competing visions of Muslim mountaineer customs.

One should take into account that re-Islamization emerged in a context of and in reaction to the Soviet legacy. It presents a specific modern answer to challenges originating from the collapse of Soviet rule in the Caucasus. Those who claim that Islamic customs still persist in the mountains forget that about two thirds of so-called Muslim mountaineers live in the plains nowadays. There is no need to search for local customs where they do not exist. Constructing myths about local Islamic traditions is characteristic for scholarship and politics in post-Soviet Dagestan. The aforementioned visions are more or less detached from reality. The 'highland Muslim traditionalism' advocated by Moscow and Makhachkala anthropologists and politicians as the key to the nature of North Caucasian society does not take into account the radical social and legal changes that have resulted from the continuing series of drastic state reforms that have transpired in this region for more than a century (from the 1860s onward); nor do these accounts factor in the mass deportations in 1944 and the subsequent planned resettlement of Muslim mountaineers to the lowlands from the 1950s to the 1970s.

In lieu of a Conclusion: Building an Islamic Museum in the Mountains

In conclusion, I want to dot the 'i' in the history of the Soviet narrative of tradition in Dagestan. With the beginning of the new millennium, the Dagestani alpine villages abandoned by the majority of their populations were becoming something like archaeological and ethnographic museums of local material culture. Many mountain villages (with the exception of district centres) still retain their traditional appearance, although they do not function as before. Their permanent population mostly consists of retired elderly men and women. As soon as the elder generations die, the traditional houses are abandoned, gradually falling into ruin. The traditional centre of Khushtada with its ancient Friday mosque was completely destroyed in a big fire on the 19th of January 2007. Hidden unemployment makes the majority of youth leave their native villages as soon as they graduate from secondary schools, or even before. Most of the Dagestanis from the mountains who have settled in the lowlands return to their home villages from time to time for short visits in summer or late autumn. Since the 1920s the republic's politicians and politics have left the Dagestani mountains for large contemporary towns. Even the controversies over mountaineer custom between the Wahhabis and their traditionalist opponents have taken place in the lowlands.

Nevertheless, mountaineer tradition remains the key term in late Soviet and post-Soviet anthropology of the North Caucasus. While studying contemporary villages, Soviet anthropologists and their successors modelled their representations of society and culture according to a presumed pattern of tradition, which was derived from colonial sources. Though it emerged in a secular form under late Soviet rule, the narrative of mountaineer tradition still plays an important role in representing and recognizing the contemporary religious boom. Some scholars clearly refer to and reify the narrative of Dagestani custom (e.g. Magomedov 2001: 36-38). They claim that the growth of Islam is nothing but a return to pre-Soviet and pre-modern local traditions, which managed to survive the anti-religious repressions of the twentieth century. Similar views were also expressed in the two previously mentioned monographs of the 'Portrait of Oriental Village' series.[17]

The narrative of Dagestani tradition also has continued in a new genre of village monographs, which first appeared throughout the North Caucasus in the late 1960s and early 1970s, and which quickly gained popularity

[17] To some extent, I also contributed to propagating such a perception in the early 1990s. Over the course of fifteen years of fieldwork, however, I changed my views on Dagestani Muslim society. Nevertheless, my position is not representative.

throughout the region in the post-Soviet decades.[18] This genre represents a strange mixture of Soviet scholarship and pre-modern village chronicles based on local oral tradition. Muslim village monographs are authored by amateur folklorists (*kraevedy*), who are supposed to find and read all the ethnographic writings on their ethnic group, and then retell this content to their fellow countrymen. In the 2000s, this narrative began to appear on the Internet. It is noteworthy that village histories and custom figure centrally in websites dedicated to mountain districts and villages (such as www.odnoselchane.ru and http://www.sogratl.net, amongst others). Their contributors focus on local Islamic traditions taken in the framework of a separate village or district and presented as pieces from an Islamic museum.

For their part, mountaineers have contributed to transforming post-Soviet ethnic traditions into custom based on Islam. In this respect it is noteworthy that the office of mullah in Khushtada was held for almost ten years by a moderate young Muslim scholar from the village of Khvarshi, near the Wahhabi village of Santlada.[19] Nevertheless this recent election has not meant the return of the Wahhabis, who attempted to destroy the mountain museum of their traditionalist opponents and to build their own Islamic museum in its place. Wahhabism was defeated and has had to remain underground. Most former Wahhabi activists left Khushtada and the other villages in the Tsumada district. At the same time, a third faction has appeared in the village, headed by secular-minded village resettlers from Makhachkala and Khasavyurt. This faction is centred around the Cultural Association of Khushtada, established in Makhachkala on June 4, 1995. In the 1990s it held its gatherings in Khushtada every August, when Village Day (*den' sela*) was celebrated. Among its activists are the head of the Chair of Arabic language at the Oriental studies department of Dagestan's state university and the former dean of the Faculty of History at Dagestan's pedagogic university, who heads the Association.

A poignant symbolic event happened in Khushtada in 2003. Disciples of the local mullah dismantled the fifteenth century military tower in the traditional centre of the village. They used its stones to build a new huge three-story mosque at the outskirts of Khushtada. The signboard announcing that the tower monument forms part of traditional culture under the guardi-

[18] The first example of this genre in Dagestan was a monograph by *kraeved* Ramazan Ramazanov, who later worked on Tamara Sivertseva's project (Ramazanov 1990). In the northwest Caucasus, village monographs appeared earlier (Batsuev 1967; Kardanov 1993; Mambetov 1968; Stupak 1970; Ulakov 1972; Zhemukhov and Musukaev 1998). I am grateful to Moscow ethnologist Irina Babich for these references.

[19] Mullah Makka-Sharip from Santlada has since been replaced by a graduate of Imam Ashari Islamic University in Khasavyurt. Abubakar is the son of Magomed-Zahid, the former accountant of the Chapaev Kolkhoz from Khushtada.

anship of the state did not prevent villagers from this act of vandalism. The mosque was built in this way, although a *madrasah* school attached to the newly built mosque still is not functioning due to a lack of students. This event evoked a serious conflict within the Khushtada community, which was already split between traditionalist and Wahhabi factions. It is noteworthy that even after the Wahhabis were beaten, their traditionalist opponents could not maintain power in Khushtada. Their leader, who ordered the destruction of the tower, was soon dismissed by the *jama'at*. Attempts to rebuild these competing narratives of mountaineer traditions in anthropology, society, and politics continue on.

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INTERVIEWS

Chapter 7
Interview with Sergey Aleksandrovich Arutyunov

Interview with Sergey Aleksandrovich Arutyunov (hereafter, '**A.**'), corresponding member of the Russian Academy of Sciences and chief of the department of Caucasian peoples at the Institute of Ethnology and Anthropology of the Russian Academy of Sciences, Moscow.

Conducted by Sergey Sokolovskiy (hereafter, '**S.**') on August 24, 2010 at the department of Caucasian peoples at the Institute of Ethnology and Anthropology of the Russian Academy of Sciences, Moscow.

S.: Sergey Aleksandrovich, thank you for your gracious consent to give an interview for our book about Soviet ethnography in the Caucasus and Central Asia. The focus of the book is the approximately 70-year-long period of the development of ethnography from the 1920s to the beginning of the 1990s. My first question is about the chronology of this scientific subject. Is it possible, in your opinion, to subdivide the history of Soviet ethnography into stages? And if so, could you please elaborate on your version of the chronological aspect of this subject as well as the foundations of this chronology (change of leading figures, emergence of new methods, centres and institutes, changes in political situation in the country as a whole, etc.)?

A.: It is precisely that — the history of Soviet ethnography — that can be subdivided into several stages. The first stage — the initial stage of revolutionary enthusiasm — came to an end by time of the decimation of the ethnographic science around 1930. There was a large variety of discussions at that time, the results of which you are aware: ethnography was left to play second fiddle to Marxist historical science.

The beginning of the second period can be said to be marked by the year 1943, when the Institute of Ethnography was established in Moscow, which also became the headquarters. The tradition of the Petersburg ethnographic school was still kept alive, and I *mean* the Petersburg school, as it is all about the Anuchin tradition and the tradition stemming from V. G. Bo-

goras, who started their activity long before the revolution. This school, with the arrival of S. P. Tolstov, basically ceased to exist, for he was the one who set the pace in ethnography from then on. So up to 1964, a period continued which can be called the 'Tolstov period', but which can be further subdivided into three lesser periods, which were associated with 'forced changes' in the stance of Tolstov himself. He was a prominent scientist and a strong person, but he showed distinct authoritarian inclinations. So whenever his stance and his views changed, the views of the Institute of Ethnography and its leading research scientists had to change accordingly.

Before J. Stalin's paper 'Marxism and the Problems of Linguistics' was published in 1950, Tolstov was an ardent follower of Marr. The VASKhNIL session of 1949 — and this was an obscure phenomenon that turned biology upside down, but it paradoxically went hand in hand with Stalin's debunking of Marr's theories in this work of his, which returned linguistics and ethnography back to where they belonged. The change of position did not come easily to Tolstov, and I well remember this (I entered the Moscow Institute of Oriental studies in 1950, and in 1954, having graduated, I started postgraduate studies in the Institute of Ethnography, having commenced to work at this institute in 1957). In 1956, the twentieth convention of the KPSS [the Communist Party of the Soviet Union] took place and the debunking of Stalin's personality cult. Tolstov was quite distraught because he himself was a kind of 'little Stalin', and he worshipped Stalin. This was, in a way, the debunking of the personality cult of Tolstov himself within the framework of Soviet ethnographic science.

In 1964 the sixth International Congress of anthropological and ethnological sciences took place in Moscow, which was the most significant and large-scale as well as the most widely published (if I remember right, nine volumes of papers from the Congress were released) of all those congresses. This was the first mass encounter of Soviet ethnographers with Western anthropologists. Prior to this, only two of them — D. A. Olderogge and G. F. Debets — were at a congress in Philadelphia in 1956; following that there was a more imposing delegation of Soviet ethnographers in 1960, comprised of about 15 persons, who attended a congress in Paris. And now, the Moscow congress. Tolstov suffered a stroke right before the congress, and he ceased exerting any influence on science henceforth. In biology at that time, there was a renunciation of the Lysenko doctrine (our physical anthropologists had never accepted Lysenko's ideas).

From 1965 onwards, a period arose that can be labelled the 'Bromlean period', with its emphasis on studying ethnic communities and ethnic processes, their typologization, the use of sociology and sociological methods, and the emergence of ethno-sociology. This period lasted up until 1990.

Starting in 1990, the transition from Soviet style to the post-Soviet style got under way; even the papers from 1990-91, though written still in the USSR, represented this transitional style. This can be seen even when leafing through the journal *Soviet Ethnography* issued during those years. In 1990-91, the specifically Soviet Bromlean style or attitude of our native ethnographic science fades away, and the post-Soviet approach establishes itself, which, by chance or otherwise, coincided with the transfer of the Institute to a new building, with the re-naming of the journal *Ethnographic Review*, with the democratic re-election of the institute's director (V. A. Tishkov became the director), and with the appointment of a new editor-in-chief (the post went to M. V. Kriukov). These two were rivals during the elections and advocated alternative programs, and, as the journal's editor, Kriukov frequently opposed Tishkov.

S.: Did the chronology of the development of Caucasian ethnography differ from the periodization you have just given us for Soviet ethnography as a whole?

A.: Yes, it did differ. Caucasian ethnography had its chronological peculiarities. In Armenia and Georgia there were local ethnographic schools (a poorer one in Armenia), but the main representatives [S. D. Lisitsian and Y. A. Lalayan] of these schools left the arena in the 1920s and 1930s. They laid the foundation for the main traditions of twentieth century Armenian ethnography and, gradually dwindling in intensity and profundity, continued to be active [for the entire Soviet period] in Yerevan, where a life of its own was happening, which was only very marginally affected by the events in Moscow, Leningrad, and in Soviet ethnography at large. This is all the more true for the Georgian school, where such rival figures as G. S. Chit'aia and A. I. Robakidze were active. Nevertheless, their ethnographer wives were also accredited: Vera Bardavelidze and Rusudan Kharadze.

Georgian ethnography exerted significant influence on the development of ethnographic science in Dagestan and in North Caucasus.

During the Marxist epoch, the close ties between ethnography and linguistics that were obvious before 1950 ceased to exist later in Soviet ethnography. In Soviet ethnography there existed, besides the Moscow and Leningrad schools, only a variety of 'sub-schools' in Kazan, Omsk, Tomsk, and Novosibirsk. Right now I am not taking into account Central Asia, where S. P. Tolstov's influence was substantial, and I am not talking about the Ukrainian (the Belarusian school was generally very under-represented and can be bypassed here) and the Baltic ethnographic schools, which in many aspects developed on their own, but which, in contrast with the Georgian

school, were more provincial in their nature. So in Georgia, this association between linguistic and ethnographic studies was preserved, especially in research regarding non-literate languages and languages that had only recently acquired a written form. Caucasus ethnography had its own chronology: a more gradual evolutionary development without distinct watershed moments.

The orientation towards Tbilisi and the Georgian school in that respect was stronger in North Caucasus, and especially in Dagestan, than orientation towards either Moscow or Leningrad.

S.: In this connection I would like to ask how you would characterize 'centre vs. periphery' relations based on the example of Caucasian ethnography?

A.: From the moment of the USSR's creation and into the present, in various forms, and not only in ethnography but also throughout intellectual and political life, in economic and societal visions, in science in general, in particular in humanitarian science, and especially in historical science as well as in ethnography (here I have presented a certain kind of *matryoshka* — the Russian stacking doll), a certain rivalry between the Russian and the Georgian models took shape and continues today, and the Georgian model is characterized by a greater orientation towards Europe, by the absence of antagonism towards Europe, whereas, unfortunately, the Russian model has this inherent feature.

As far back as the 1950s, a popular joke was in circulation: 'The Tbilisi–Soviet Union Express Train leaves from platform five'. It is just a short joke, but it reflects the economic reality. Nowhere in the USSR except in Georgia could one buy party membership (it was known that there it could be purchased for two to three thousand roubles). Perhaps something similar took place in Central Asia (the party membership card was dubbed a 'bread passport' there). The party membership card was a prerequisite for a successful career, but nobody in Georgia had any faith in arbitrary socialist values and ideals. In 1950 when I was 18, I came to Moscow and was very surprised that the Russians seemed to believe in all of this. In Georgia the 'shadow economy', the tsehoviks [shadow capitalists], as it were, working underground, existed under the tacit control of party organs and functioned as a parallel economic system. They did not hide and actually paraded their wealth. This was the case in Armenia and Azerbaijan as well, but in Georgia it was most apparent. During a banquet in the mid-1970s, one big-shot shadow capitalist complained that he could buy everything but respect. He could not buy respect and felt piqued accordingly. They received little respect; as little as our oligarchs and *nouveaux riches* are respected today. But

in Georgia and Armenia, multi-millionaires existed way back when, which was not the case in Central Russia.

So this antagonism between Russia and Georgia is of a very broad and pervasive nature, and it also reverberated in the tenor of ethnographic science. It manifested itself strongly in cinematography, and less so in literature (the Russians did not read Georgian, but they still knew Nodar Dumbadze and Chabua Amiredjibi). This antagonism holds sway even nowadays.

S.: Were there any special features characteristic of various ethnographic traditions within the framework of Caucasian ethnography — Armenian, Georgian, Azerbaijani? Can one speak here about some specific approaches, perspectives, methods, or independent ethnographic schools?

A.: Armenian, Georgian, and Azerbaijani ethnographic traditions are similar, but each searches for the 'navel of the world' within its own precincts. Of the three, Azerbaijani ethnography is the weakest, as its ambitions are poorly substantiated. Georgia and Armenia had already accumulated their own historical 'ammunition': from the fifth century onwards, they had their own literary tradition and historiography, including ethnographic historiography. Of course, historiographic contentions within the framework of nationalism (the first such contention took place as far back as the 1890s between Il'ia Chavchavadze and Khristofor Vermishev) around the cultural heritage, architectural monuments, and historical events were attempts of each of the national elites to announce its monopoly. This is a meaningless occupation, as there existed a common South Caucasian culture with Christian and Muslim elements. This common South Caucasus cultural heritage (i.e. the ethnographic reality) is also inseparable into national segments, and it is to be studied in the totality of its constituent pragmemes (and isopragms), and, if I may put it this way, behavioremes, with their cross-national and cross-ethnical distributions, and not to be looked at from a narrow national viewpoint, which, alas, makes for the current distinguishing characteristic of these schools today.[1]

[1] 'Pragmemes', 'isopragms', and 'behavioremes' are situational coinages, designating patterned distributions of practices (with 'isopragms' denoting similar practices) and behaviour traits, created by analogy with terminology from dialect studies (comment by **S.**)

S.: This is a very interesting research program, but it is not apparent upon which institutional foundation it can be carried out.

A.: There is no such foundation, and it is not clear when it will emerge.

Chapter 8
Interview with Anatoly Mikhailovich Khazanov

This interview was conducted in Madison, Wisconsin, in the Social Science Building at the University of Wisconsin on Tuesday, October 12th, 2010 by Yoshiko Herrera (**YH**) and Anatoly Khazanov (**AK**). The initial questions were prepared by Azim Malikov and Florian Mühlfried; interview transcript edited by Sergey Sokolovskiy; the final revisions were made by Anatoly Khazanov in Halle (Saale) in May 2011; final editing by Chris Hann.

YH: You studied at Moscow State University during a period often referred to as Khrushchev's Thaw. Could you describe the atmosphere at the University at this period?

AK: It's quite a difficult question for many reasons. First of all, when I was a student, I was young and my perception of people and the situation was quite different from my perception in the Brezhnev period. I was admitted to the University in 1955 and graduated in 1960. Actually, in spite of many positive political changes in the country, I cannot say that university education was much affected by these changes directly. The same professors were teaching in the same way. This is quite understandable. These people were shaped by their experience during numerous purges of the faculty and in the country in general, in the 1920s and early 1930s, then by the Great Purge of the late 1930s, and after the war, by the anti-cosmopolitan campaign and other related events of the late 1940s and the early 1950s. They were afraid to deviate from any Marxist dogmas and the official Party line whatever they might have thought about them privately.

The same situation prevailed among the majority of students, but there were quite a number of students who were eager to extend their horizons, who had new optimism for the future. Still, we were a minority. We talked about political developments in the country, and we shared books with each other that were, if not forbidden, then not advertised. Some of them were scholarly books; other books were novels and volumes of poems. We were

especially fond of the poems by Pasternak and Akhmatova. As far as I remember, a little later we discovered such poets as Zvetaeva and Mandelstam. At that time, a very few of us managed for the first time to read some books smuggled from the West, which were forbidden in the Soviet Union. I still remember how impressed I was by reading Zamiatin and Orwell.

Sometimes our professors were too dogmatic, even for the taste of the majority of students. But it was quite possible simply not to listen to them and to read books or just to chat with each other during the lectures that we were obliged to attend. When one is 18, or 20, all girls look pretty; it was more exiting to flirt with them than to pay attention to boring lectures. It was so easy to pass the exams because there were textbooks and, practically, that was all that was required. And with only a few exceptions, seminars were not very different. Many professors required mostly the knowledge of their own course of lectures and just a few publications.

So it was extremely easy to be a student at the Moscow State. One third of the whole time was devoted to such 'exciting' subjects as dialectical materialism, historical materialism, the so-called political economy. 'Scientific atheism' was not introduced in university curricula until after my graduation. During all the years of my study at the University I was always an A student. The only B that I got was for a course in dialectical materialism. I deserved this grade. To this very day dialectics, whether of Hegel or Marx, is completely incomprehensible to me. I don't understand why black should be perceived as white and slavery as freedom.

YH: Were the professors mostly the same? Did any stand out?

AK: Some of them were better than others; some of them were simply ignorant, others were knowledgeable, but almost nobody dared to deviate from the Marxist-Leninist dogmas and the Party line.

YH: Even during the thaw?

AK: They didn't really follow the thaw until they got instructions. A few historians who attempted to say something new, especially about the Soviet period, were severely criticized. In any case, they were not teaching at the Moscow State. There were a couple of faculty of the younger generation, not anthropologists by the way, who in one way or another indicated that they thought differently, and would have liked to teach and do things differently, but they were in a very small minority. I remember, for example, one archaeologist who is now a full member of the Academy of Sciences, Valentin Ianin. At that time he was an assistant professor. He even liked to exchange

anti-Soviet jokes with a few students whom he trusted. He is really a nice man, a good scholar and, as far as I remember, he has never been a Party member. It is surprising that he could become an Academician in the Soviet era.

I started my career not as a social or cultural anthropologist, but as an archaeologist. I was already not happy with the situation in the Soviet Union. I had some doubts, many doubts actually. Why did I choose archaeology? Mainly because I wanted to be as distant as possible from all contemporary ideological issues. At the university I was very lucky to have an unusual and wonderful teacher. Here I have in mind the traditional Russian word '*uchitel"*, meaning not 'adviser' but a real 'teacher'. He actually was my teacher, Professor Boris Grakov, an archaeologist. He was very different from the rest of the faculty. He belonged by family ties to the best representatives of the pre-revolutionary Russian intelligentsia. He had had a very difficult life in the 1920s and 1930s. Several times he was fired, but, contrary to many other scholars of his generation, he never compromised his position. He maintained his friendship with those who were persecuted. He never uttered a single word or signed any letter condemning them. He waited for arrest many times. We liked each other. Very soon he somehow guessed that my political views were far from being orthodox. He trusted me and told me quite a lot about what he really thought and knew about Soviet power and communism, and about his conformist colleagues. He told me a lot how the things really were in the Soviet Union in the 1920s and in the 1930s. As a scholar he was a brilliant classicist and, at that time, the best expert on the Scythians, ancient nomadic people of the Eurasian steppes. So he inspired in me a life-long interest in the Scythians. After graduation, I became interested in nomadic peoples in general, my main field of research. I have studied other subjects: ethnicity and nationalism, transitions from totalitarian rule, collective memory, culture representation to name but few. I have published a book on ethnicity, nationalism, and politics in the ex-Soviet countries. But the pastoralists have always remained the main focus of my scholarly interests. And during all my scholarly life, time and again, I have returned to studying the Scythians.

YH: Why you had become interested in the nomads in the first hand?

AK: Illusion, romanticism of the youth. The nomads, at any rate, the ancient ones, seemed to be free people to me.

YH: What was so remarkable and specific in your teacher?

AK: He taught us to think independently and never take anything for granted. Personally, I have learned from him that one can preserve one's integrity and decency even in the most difficult times; that a betrayal is always disgusting, regardless of the circumstances, and that it can never be justified. He could forget and forgive many things but not a betrayal. This is what he taught me in human terms. And he loved his students. We reciprocated. Almost all his students were very loyal to him. I am trying to follow his example. I love my students, and I am very loyal to them. And to my great surprise almost all my students remain loyal to me as well, even after graduation. This is not so common in the US. Some of my colleagues told me that they envied me in this regard. My Israeli students are very loyal to me as well. Some of them have become excellent scholars who have their own brilliant students. That makes me very happy. Recently, they asked me to teach a course as a visiting professor at the Hebrew University, and the students who took this course joked that I was their scholarly grandfather. I was very moved indeed.

YH: What about the Brezhnev period?

AK: By then I was working at the Institute of Ethnography of the Soviet Academy of Sciences and, though I was acquainted with some people who were teaching at the Moscow State and some other universities, my knowledge of the situation there was rather indirect. In general, it was the peak of stagnation. The hopes for a better future that I and many other people still had at that time evaporated completely after the Soviet invasion of Czechoslovakia and the suppression of the Prague Spring. The selection of faculty became even stricter. For example, the few Jewish professors were by that time already retired and no new ones were recruited, although some of them were excellent scholars. Anthropology was very weak at the Moscow State University. Even when I was a student, the Department of Ethnography consisted only of five or six people. Compare this with more than one hundred scholars who worked at the Institute of Ethnography. One of the university staff, Professor Tokarev, was very erudite; the rest were much weaker. Anthropologists preferred to work at the Institute of Ethnography, and eventually Tokarev did the same. But, as a rule, those, who worked at the Institute were not involved in regular teaching, with the exception of supervising the graduate students there. There was much more free time at the Institute and less ideological pressure. Some of the scholars there worked hard, but there were also those who did not publish anything for many years. In the Brezhnev period, the situation at the University was quite different. In

order to be able to teach at the University one had to be a member of the Communist Party.

YH: But not at the Institute of Ethnography? At the Institute you didn't have to be member of the Party?

AK: No, especially if you didn't strive to make administrative career. At one time, I was the youngest Doctor of Science at the Institute of Ethnography (that was the highest degree in the Soviet Union). I got this degree at the age of 39. But I was never a member of the Communist Party. Moreover, I got my degree despite the fact that at that time I had already had many troubles with the KGB. I was on their blacklist and was never allowed to travel abroad. Few people at the Institute had any illusions in respect of my 'devotion' to the Soviet regime. Nevertheless, I got my degree, though not without some difficulties, which were not connected with the quality of my thesis.

YH: So your first job was at the Institute of Ethnography?

AK: The first and the last one in the Soviet Union.

YH: Did you do any fieldwork expeditions while you were a student?

AK: When I was a student I worked as archaeologist in the field every summer.

YH: And who organized the expeditions?

AK: Archaeologists who were teaching in the Department of Archaeology. But occasionally the students worked in expeditions organized by members of the Institute of Archaeology of the Academy of Sciences. I did this once too.

YH: Who participated? Who analyzed the data? Other students?

AK: Usually the faculty, sometimes graduate students, a few undergraduate students and hired workers participated in expeditions. The university expeditions were rather small. All materials belonged to the leader of the expedition.

YH: And the leader was typically a faculty member from Moscow State?

AK: Of course. Students were not allowed, and rightly not allowed, to do excavations themselves. At that time all of us were undergraduate students. Sometimes professors would say: 'OK, maybe it is worth choosing such and such a subject for your thesis, in which case you can use my materials'. Often that was what they were doing. But, in principle, they could deny access to the excavated materials to everybody.

YH: And how about your shift from archaeology to socio-cultural anthropology: when did that happen? And why did you switch?

AK: The door of the Institute of Archaeology was absolutely closed to me for two reasons: the director of the Institute, Academician Rybakov, hated my teacher and disliked me personally. Besides, he was quite an anti-Semite and never bothered to hide this.

YH: The director of the Institute of Archaeology?

AK: Archaeology. Nowadays, more and more archaeologists in contemporary Russia and Ukraine complain that Rybakov caused great damage to Soviet archaeology through his ignorance, intolerance, and despotism. My teacher, Professor Grakov, was very much concerned with my employment. But he was on good terms with the director of Institute of Ethnography, Sergei Tolstov. Actually, Tolstov simply was afraid of the tongue of my teacher. Because he really had a sharp tongue. Tolstov had a permanent archaeological expedition at his Institute that excavated Khorezm, a region in the Lower Amu Darya and Syr Darya rivers. Grakov recommended me to him and Tolstov hired me.

Alas, I instantly started to dislike Tolstov, because he was an arrogant and rude man who liked everybody to work for him and for him only. Besides, I became less and less interested in empirical archaeology for two main reasons: I always hated getting up at 4:30 in the morning! I am a night bird. More important, I became more interested in social regularities than in pots and arrows. But I didn't have any other option. However, just at that time Tolstov became very ill, and Iulian Bromlei was appointed as the new Director of the Institute. He was more liberal and open-minded, as long as this did not hurt his career. He also was more polite to people; he did not like to antagonize them. As a scholar, he was quite mediocre. He published many works on a theory of ethnicity ('*etnos*' in the Soviet scholarly parlance). All of them were extremely primordialist. But Bromlei introduced some positive changes, and one new and peculiar department, the Department for the Study of the History of Primitive Society [*Sektor po izucheniiu istorii pervobyt-*

nogo obshchestva]. I was invited to join it, and I have never regretted this. The head of the Department was a very peculiar man. Publicly and in his publications he followed the Marxist line. Privately he explained to me: 'I don't have any other income. If I deviate from this line, I will be fired. And you too, by the way'. Nevertheless, he did not mind if I tried to extend the borders of the permissible in my publications, and when I was periodically accused of revisionism, he always did his best to defend me.

YH: What was the name of the head of your Department?

AK: Abram Pershits. In other conditions he might have been an excellent scholar. He was a very knowledgeable man. He had a disciplined and very logical mind. He published a very good book on the nomads in Arabia in the 19th – early 20th centuries. It's a pity that this book has never been translated into English.

YH: What year was this?

AK: Well, I became a member of the Institute in 1960, joined the new Department in 1966, and worked there until I was fired, when I applied for emigration to Israel. Officially I was fired for 'behavior incompatible with the dignity and honor of a Soviet scholar'. By the way, most of my colleagues at the Institute, people of quite different political views, in one way or another indicated their sympathy with my flight. One of the few exceptions was my former graduate student, who was even afraid to say 'Hello' to me. During the perestroika period, when I already lived in the West and could be useful to him, he tried to restore our personal relations. I rejected his attempts, but still, I am very glad that he has become a good scholar who is successfully pursuing his own agenda.

YH: When was this?

AK: I applied for emigration in January 1980, two weeks after the Soviet invasion of Afghanistan, but was allowed to leave Soviet Union only five and a half years later. However, this is another story.

YH: To what extent did the centres of Soviet Anthropology in Moscow and Leningrad control developments in the rest of the Soviet Union? Was it very centralized?

AK: To a very large extent. But I want to be fair. A lot of efforts and material resources were invested into the development of anthropology in Central Asia, the Caucasus, and, to a lesser degree, in some autonomous republics of the Russian Federation. The same holds for archaeology, and to a certain degree for history and oriental studies. New institutes of history, archaeology, and ethnography (*etnografiia*) were created, and two generations of Central Asian anthropologists or ethnographers (I prefer to call them ethnographers because nobody was devoted to theoretical aspects) were trained. The first, and some of the second generation were educated at Moscow or at Leningrad Universities. Even afterwards, many came to Moscow or Leningrad, to the Institute of Ethnography, or to its branch in Leningrad, where they wrote their theses and got the Candidate of Science degree, sometimes even the Doctor of Science degree. But with regard to ethnography, they were educated mainly by scholars, who, in my opinion, hardly met western standards of scholarship. Besides, people who worked in Central Asian research institutions or universities were constantly under a double pressure. First of all, the local party authorities were even more afraid of being accused of deviations than the people in Moscow. And second, the final word of approval or disapproval lay with their Moscow colleagues. Moscow was in this regard an ultimate centre of authority.

YH: Was it coming mostly from the University or the Academy of Sciences in Moscow?

AK: I would say that it came mainly from the Academy of Sciences, because most of those people worked at research institutions of the republican Academies of Sciences. That pressure was not formal in principle. In fact, the situation was more contradictory. Sometimes, when Central Asian scholars were criticized by their Moscow and Leningrad colleagues, local party authorities somehow tried to protect them because of ethnic solidarity and local patriotism. This happened quite a few times. So it is not surprising what subjects they usually chose for studying and publications.

YH: What kind of subjects did they choose?

AK: For example, the subjects of many theses of Candidates of Sciences (more or less equal to American Ph. D. theses) were: 'The happy life of the *kolkhozniks* in such and such a *kolkhoz*', or 'Traditional Culture of the Uzbeks of Zeravshan Valley in the XIXth century', or in some other region. Sometimes it was simply ridiculous. Thus, there were quite a lot of theses and publications written on marriage and wedding rituals in various areas,

but the crucial questions, for example that bridewealth still existed in Central Asian republics, or the persistent gender inequality, were never mentioned. There was almost nobody amongst the Central Asian ethnographers that I would consider really on a par with the best Moscow or Leningrad colleagues, with one exception in Kazakhastan. There were some good Kazakh ethnographers, and archaeology, ethnography, history, and oriental studies in Kazakhstan always did a lot better than in all the other Central Asian republics.

YH: Why do you think this was?

AK: More westernized, or, if you wish, more Russified. After the disintegration of the Soviet Union Kazakhstan has become much more open to the West than other Central Asian countries. Since the mid-1990s I have often visited Kazakhstan again. I go there every year, sometimes even twice a year to do research and to participate in other scholarly activities. Recently they celebrated 50 years of my research in Kazakhstan and on Kazakhstan. This was quite an unexpected and very emotional event for me. In the post-Soviet period, anthropology, and scholarship in general, in Kazakhstan became independent with regard to Moscow. It's amazing how many new works in history, oriental studies, and other disciplines are published there, including research by the younger generation on previously forbidden subjects. Kazakhstan's government, although quite authoritarian, spends a lot of money on higher education, science and scholarship. Every year many Kazakh students are sent to study at Western, mainly American universities. Recently, a new national university was opened in Astana. It is organized on the American model: B.A., M.A., Ph.D. The education is supposed to be only in English, and most of the faculty is composed of Western scholars. It is oil, of course, but oil could be used for many other things. Compare this with the other Central Asian republics, where these fields are in complete decay. Sometimes I wonder whether this policy will eventually result in a democratization of the country. It is hard to predict, at the moment. Democracy requires many preconditions. Only American presidents believe that it can be established everywhere and immediately. However, for better or worse (personally, I think for the better) the future is in principle unpredictable.

YH: What about anthropology as a discipline in the Soviet days? Did the government use….

AK: Anthropology, that discipline simply didn't exist in the Soviet Union. There was ethnography.

YH: OK, was this ethnography useful to the state?

AK: Not to a great extent. Much less than it could have been. Soviet rulers never wanted to know the truth, how things really were. And if they wanted to, they resorted to other sources like the KGB etc. From time to time anthropologists were ordered to write a report on a particular issue. Few people who participated in writing such reports would privately admit that the conclusions had been dictated in advance. Some were asked to write formal reports on foreign countries, though nobody knew what for. An ethnographer might study one country or another for many years, without ever being able to visit it, not to mention being able to do serious fieldwork there. In a few cases, ethnographers volunteered, without any request, to write reports or letters to the government, for example documenting the miserable conditions of the small peoples of Siberia. But as a rule, such documents were ignored.

YH: Let's move on to your contributions, in terms of your publications. Who were the Soviet scholars that supported your new approach to nomads and their relations with the sedentary world?

AK: Nobody, or almost nobody. A few of my colleagues at the Institute were intrigued and quite supportive, but they were studying other issues, not pastoral nomadism.

YH: Did you keep in touch with your former teacher?

AK: Oh yes, for the rest of his life. Unfortunately he passed away long, long ago. I am regularly in touch with many of his other students.

YH: Did they support your approach to nomads?

AK: Some were very interested in it and supportive, but they were archaeologists. They told me that they were learning from me, an anthropologist with an archaeological background. In the beginning I became interested in the social organization of the Scythians, and the only two sources of information on them were ancient Greek and Latin authors and the archaeological materials. So, it was a kind of reconstruction. But during that work I decided it might be useful to compare ancient nomads with medieval and early modern ones. And then I came to the conclusion that actually they had very

much in common. I have to remind you that at that time, in the 1970s, the most common view on nomadic societies was based on Marx's theory of universal socio-economic formations. Therefore, it was assumed that in ancient nomadic societies we should find slavery and in the medieval societies — feudalism. There was a long and fruitless discussion about whether it was mature feudalism, or the so-called patriarchal feudalism. There was a discussion at a special conference in 1956 on the developmental level of pastoral nomadic societies. When two Kazakhstan scholars (ironically they were not ethnographers, one of them was even a teacher of Marxism-Leninism) insisted that nomadic societies were never feudal, they were severely criticized by Moscow and Leningrad ethnographers.

Anyway, pastoral nomadism had ceased to exist in the Soviet Union in the late 1920s and the early 1930s during the bloody collectivization and sedentarization campaigns. Nomadism was declared a backward way of life and the campaign was accompanied by many atrocities. One third of the Kazakhs simply perished.

YH: In the early 1930s?

AK: Yes, in those few years. And their herds were decimated. In the Soviet Union, nobody was ever able to write about the Kazakhstan tragedy. A million and a half people literally perished of starvation and persecution. Instead, there were quite a lot of theses and books on the 'glorious' collectivization of the Kazakhs, how happy they were, etc. This was absolutely disgusting. Scholars not always could write the truth, but they should abstain from writing blatant lies. There are still people in the West, even scholars, who claim that, despite mistakes and crimes, the Soviet achievements were remarkable and undeniable. I wonder whether these achievements were worth 35 million people perishing during the 70 plus years of Soviet rule (this is a conservative estimate).

During my field research in Central Asia, especially in Kazakhstan and Turkmenistan, I talked with people, and when they began to trust me, they told me quite a lot about collectivization. In the 1960s and in the first half of the 1970s, the older generation still remembered these traumatic events quite well. I wrote down some of their stories, of course taking precautions for the protection of my informants. For many years they were kept in my personal archives at home. Unfortunately, in 1984 they were confiscated by KGB officers during a search of my apartment. Nowadays, some Kazakh scholars praise me for having been the first scholar who publicly raised this question in 1986. But, you see, I don't deserve this praise, because raising this question in the West was much easier and safer than doing

it in the Soviet Union. Besides, I had only limited information at my disposal. Now, of course, Kazakh scholars are exploring this subject much better than I did, in greater detail with precise statistics, etc. There are many excellent articles and books published on this issue. It is a pity that they are not known well enough or largely ignored by the Western scholars.

YH: Taking into account the Iron Curtain in the Soviet Union, how did you establish contacts with Western scholars like Ernest Gellner?

AK: Well, in the Brezhnev period the Iron Curtain already had a few holes. Like some other scholars I maintained contacts by correspondence. This was not encouraged and raised suspicion, but it was not altogether forbidden. A few Western scholars visited Moscow. In the 1970s a Dutch anthropologist, Henri Claessen, with whom I had corresponded for several years and who had published some of my papers, came to Moscow ostensibly as a tourist, but actually just to meet with me in person, when he had understood that, despite his efforts, I would not be allowed to participate in any conference outside the Soviet Union. We became and remain very good friends.

But you know, the main Moscow libraries were not so bad in respect of the Western literature. Those who wanted to educate themselves were able to read Western anthropological books and journals.

YH: In the 1960s and 1970s?

AK: Yes. I remember in the beginning, just like a few other anthropologists, I became very much impressed by neo-evolutionism and ecological anthropology. That was the beginning of my education in the Western anthropology.

And now about Ernest Gellner. He once visited Moscow, came to the Institute of Ethnography, and we chatted for a while. He asked me what the Soviet Marxists thought about such and such issues. I replied that I was not a Marxist at all. He became curious. I told him: 'Listen, this is not the best place to talk. If you want to talk, may I invite you to my home'. 'I would be honoured', he replied. Much later he told me that I was the first and the last person in the Soviet Union, who ever invited him to his home, because it was not encouraged. To say the least, it was not encouraged. On such occasions one was supposed to get official permission, and to invite other Soviet scholars as well, in order not to be alone with the foreign visitor, etc. But I did not give a damn. When I was privately reprimanded by the Institute's administration, I replied: 'I obey official laws, but not unofficial ones and confidential instructions'. Usually, the people in the administration preferred

not to pursue this issue further. I was just warned time and again: 'If you have troubles on account of your disobedience, just blame yourself'. That was fair. Anyway, when Ernest visited me the first time, we talked for many hours and immediately felt some affinity between us.

YH: You liked each other?

AK: I don't know about Ernest's first impressions, but I instantly liked him very much. Yes, he used to come to Moscow very often at that time, and later. He was interested. His hope was that gradually the Soviet Union would be reformed from within by revisionist communists, and that that would prevent its disintegration. After the Soviet Union collapsed, Ernest with his exemplary intellectual honesty publicly admitted that he was wrong.

YH: What years was it that he was visiting?

AK: I don't remember. He visited Moscow so many times. I don't remember the exact year of my first encounter with him. It was in the early 1970s. And then we became friends, and we remained close friends until his premature death. I would say that no other scholar influenced me as much as Ernest did, not only via his publications, but also during our numerous meetings and conversations, often till the morning hours. He always encouraged my interest in the pastoral nomads and later in nationalism. When I needed one publication or another, not available at the Soviet libraries, I wrote a letter, asking Gellner to send me books, or copies, or off prints, and he always did this. After I had applied to emigrate and was refused, Ernest Gellner and Henri Claessen constantly campaigned on my behalf in the West. We had conversations, discussions, and sometimes disputes: especially with regard to the Soviet Union, its future, and later, when I was already living in the West, about the consequences of its collapse. Ernest Gellner was a great scholar, a great man, and a great friend. Although already 14 years have passed since he left us, I still miss him very, very much.

By the way, after Gellner's death, obituaries and papers about him were published in many countries, but not in Russia. This is despite the fact that no single western scholar assisted Soviet anthropologists and made their publications known in the West as much as Gellner did. Such was their gratitude. The only exception was my own paper, written at request of Sergei Panarin, the editor of the journal Acta Eurasia (*Vestnik Evrazii*) and published in his journal in 1998.

YH: Back to your book 'Nomads and the Outside World'. Ten years have passed since the second publication.

AK: No, already sixteen. There is already a fourth edition, recently published in Russia by the St. Petersburg University Press. It contains many updates, revisions, and some additional sections and chapters that reflect my most recent views and interests.

YH: OK, is the second English edition connected to this fourth Russian edition?

AK: No, as I have told you, the second was published much earlier. The third was published in Kazakhstan, in Russian.

YH: So there are two English editions, then a Kazakh edition and then a Russian edition.

AK: And a Korean; and a Chinese edition is in preparation.

YH: But the Korean edition is no different from the second one, or you don't know?

AK I don't know. It was a pirate edition. I was told about it and got one copy thanks to the courtesy of one Korean scholar many years later. As for the Chinese edition, let us wait and see what will come out of it.

YH: What are your most recent interests?

AK: One is the role of the nomads in the World history. A second is the situation of traditional pastoralists in our brave new world. In this regard I have changed my previous views because I have become less optimistic about their future. But I still think that my main thesis is correct. Nomadic societies of the Eurasian steppes for almost two thousand years were just oscillating between pre-state and state forms. This depended not so much on their internal development but on their interrelations with the outside, mainly sedentary and urban world. In this regard I have not changed my views. As far as I know, they are now accepted by the majority of Western scholars and by more and more scholars in the former Soviet Union.

YH: But what do you think are the main reasons for the formation of the nomadic state?

AK: Oh, very simple. The nomadic societies were never autarchic. They needed to be supplemented by the agricultural products and crafts they procured from the sedentary societies. They traded, they used other measures, but the best way from their point of view was just to exploit or even to conquer those societies, when the chance occurred. Remarkably, they were often capable of doing this just because of their relative backwardness, undeveloped social division of labour, and the wide social participation. With but few exceptions, in the sedentary states the military business was always specialized and professionalized, while every nomad, just because of his way of life, was a natural warrior, moreover, a mounted warrior, and a very skilled one. In Mongol society children started to learn archery from the age of five. In Mongolia, I myself saw children of the age of six or seven who were better riders than many Western adult riders. I was amazed how skilled they are, even today.

YH: So, military advantage is the main reason for state formation?

AK: In a nutshell, military advantage sometimes allowed the nomads to exploit sedentary societies. And, in turn, in particular historical situations this exploitation led to an emergence of nomadic states. In other words, their relative backwardness was a leverage for political domination. In theoretical respects, this is a very interesting case.

YH: And the basis for their state formation was conquest.

AK: Conquests were not always necessary and feasible. Sometimes it was enough to establish tribute relations.

YH: During the IUAES Congress in China, in 2009, you have received an award from the Commission on nomadic peoples for your life-time outstanding contribution to the study of pastoral nomads, right?

AK: Yes. When I accepted this award, I joked that it implied that my researches were already in the past; from that moment on I should stop working and go to the afterworld! Finita la comedia!… The name of this award is really funny. But I am still enjoying and doing research as best as I can at my age.

YH: What about nomadic civilization? Do you think this term, which has recently become popular in Central Asian states, is appropriate?

AK: I don't like this term in general. It is too imprecise. Even in different Western countries it is applied to quite different societies and levels of development. Take French scholars. They write about Paleolithic civilization, Neolithic civilization… In the Anglo-Saxon tradition a definition of civilization that goes back to the famous archaeologist Vere Gordon Childe is still quite popular. In his opinion, civilization implies mainly statehood plus urbanization plus literacy. This is indeed a great simplification of the problems connected with the term and its history and I prefer to eschew it in my works. But in principle I can use it in a not very professional way, for example, 'Western Civilization'.

Why did it become popular? Even the best Kazakh anthropologist, Nurbulat Masanov (unfortunately he died at the age of 54 in 2006; I liked him very much as a scholar and as an individual), called his very interesting book 'The Kazakh nomadic civilization'. I can only speculate why this term, as you correctly noticed, became relatively popular in Central Asia. Because throughout the Soviet period, and even in the tsarist period, people there were told that nomads were backward, that the nomadic way of life was backward, and that it should be abolished as soon as possible. But many of them are the direct descendants of nomads, they are proud of their heritage, they are searching for their roots, and they don't want to have an inferiority complex… This is why the term 'civilization' with regard to the nomads became relatively popular in Central Asia. It means that the nomads were just as civilized as their sedentary counterparts. It is not accidental that nowadays there is a special research Institute for the Study of Nomadic Cultural Heritage in Kazakhstan. I maintain close ties with it and advise its many projects. But even in Kazakhstan not everybody accepts the term 'civilization'.

YH: But you think it is OK?

AK: No, no. It is not OK, but there are understandable reasons why it became rather popular.

YH Is this popularity connected to nationalism?

AK: To a certain degree, it is. And this is not an extreme case. Nationalism may become quite funny when it is promoted not so much by the professional scholars, but by enthusiastic volunteers. Sometimes I laugh, when I read that Kazakhs discovered America — I am not kidding — and that Kazakh civilization exists from the Upper Palaeolithic times. Not so long ago Kazakh scholars published a very interesting volume devoted to decon-

struction and criticism of this rubbish. But all new nations are doing the same, and all nationalist ideologies have myths as their integral part. Remember that not so long ago all French children started their history from 'our ancestors are the Gauls'. This is not so terrible, if nationalism does not acquire militant and xenophobic forms. Myths cease to be myths and become social reality only when people are ready to kill and to be killed for them.

YH: All right, do you see any relationship between mobile pastoralism and labour migration?

AK: It all depends on what one means by labour migration. If one means that the contemporary industrial labour mobility...

YH: Well, let's say agricultural workers.

AK: Labour mobility has quite different causes. Take, for example, Canada. Miners there go to the north, work in the mines for 15 days, and then return to their families for another two weeks. A similar situation exists with some Mexican agricultural laborers employed in seasonal work in California or even here, in Wisconsin. They come without their families. Let's say they are temporary workers. Nomadic migrations were caused by different economic reasons, by the necessity to maintain the stock all year round on the seasonal, natural pastures. Everyone, whole families, moved with the stock. (I am not talking about migrations connected with conquests or other political, or ecological reasons. The latter may be defined as a one way ticket.) Contrary to the nomads, Mexican workers who come to work in agribusinesses in California or other places do not take all their property and families with them. They are laborers, hired seasonal labourers.

YH: What is the main difference between Soviet and Western study of nomads?

AK: Very few Soviet anthropologists, except those who studied contemporary reindeer pastoralists in the North, worked on this subject at all. The most interesting Soviet (and even post-Soviet) publications stem not from anthropologists but from historians and orientalists. Studies of ancient, medieval, and early modern nomads were much more advanced and safer than studies of nomads of the 20th century. By contrast, in the West both anthropologists and historians studied those nomads. But western scholarship, to my mind, has or had one deficiency. Not infrequently, anthropologists don't read the works of historians and historians don't read anthropo-

logical works. Of course, there are many exceptions, especially amongst the historians, far fewer among anthropologists. And this is still, in my opinion, the main deficiency of western anthropology, which in general is much more advanced with regard to nomadic studies than the Soviet and post-Soviet studies.

Anthropological studies of nomads in the West are quite pluralist in theoretical respects, while in Soviet Union these studies were put in the straitjacket of Marxism-Leninism. I am talking mainly about Anglo-Saxon anthropology. In France the situation is, or was different, since, in my opinion, many French anthropological studies of the nomads are excessively influenced by dogmatic Marxism. The Soviet anthropologists were much better educated in historical issues. This is not surprising because, as you know, ethnography was considered a part of history, was taught at the Faculties of History, etc. And *volens nolens*, you took those courses in history. Personally, I never regretted this. The Western situation is different; in my opinion, the western anthropologists should know more history. Nowadays, the Russian anthropologists should pay more attention to contemporary issues and should become better acquainted with Western anthropological thought. There are some young anthropologists, quite promising in this regard, but they're few in number.

YH: So, who in your opinion were the most outstanding Soviet scholars of nomads?

AK: It is difficult to answer this question. There was a generation of outstanding orientalists and historians, who have made a great contribution to the studies of the ancient and medieval nomads. Some contemporary scholars are very good too, but I would prefer not to rank them. Still, even nowadays it is easier to mention good, excellent historians of the medieval nomads than anthropologists studying contemporary pastoralists.

YH: What do you think were the main stages in the development of the study of nomads in the Soviet Union?

AK: I described them in the foreword of my book. And there is nothing I can add except that hopefully, in the post-Soviet period, a new stage of the study of nomads has begun. Scholars can deal with these subjects much better, more easily. But so far there is no drastic change in the paradigm. By the way, in the early 1990s (I do not remember the exact year), Valerii Tishkov, the director of the Institute of Ethnology and Anthropology (the former Institute of Ethnology) made a remarkable statement in his paper published

by Current Anthropology. He lamented that Western anthropologists had an advantage in their field research in the non-Russian parts of the Russian Federation because they had been trained to treat those peoples like equals. I doubt very much that Tishkov would repeat this complaint today. Moreover, I think that it was unfair to many Soviet ethnographers. For example, those who studied reindeer pastoralists in the 1990s, Nikolai Vakhtin, the late Aleksandr Pika, and several others, they published very interesting works criticizing the policy of the Soviet and post-Soviet authorities with regard to these peoples. Unfortunately most of those critical works were published in the 1990s, and there has been much less during the last decade.

YH: So it is easier now for people in Central Asia to study their own pastoralists.

AK: It has become easier in Kazakhstan and Kyrgyzstan. Unfortunately, though understandably, most ethnographers in those countries are still following the Soviet tradition. They prefer to study the traditional culture, rites, rituals, cuisine, etc. In Kazakhstan, historians are faring much better than ethnographers. And it is not surprising that in Kazakhstan a lot of good research was published documenting the Kazakhstan tragedy. This is how collectivization is perceived in independent Kazakhstan. These events were really horrible. They even exceeded the horrors of collectivization in Ukraine.

YH: But less known.

AK: Unfortunately, less known outside Kazakhstan. In the early 1990s, I suggested to the famous Kazakh poet, Olzhas Suleimenov, who at that time was a leader of the main opposition party, to make a documentary film about the Kazakhstan tragedy. He became very excited and enthusiastic, and asked me immediately to write a short proposal. He assured me than in the next few days he would send his collaborators to search for relevant photographs in the archives of Moscow, Almaty, and other cities. But I suspect that soon afterwards he became distracted by other matters. By the way, nowadays Suleimenov is Kazakhstan's ambassador to Italy.

YH: Let us return to Russian anthropologists. What about their current studiers of pastoralists?

AK: At the moment, very few anthropologists in Russia, especially in Moscow and St. Petersburg, are interested in pastoralists. There are two reasons.

First, the traditional pastoral nomads ceased to exist after the collectivization. Second, to study contemporary pastoralists in Mongolia and Central Asian countries is extremely difficult for Russian anthropologists for financial reasons, and sometimes for political reasons as well. In the communist period, some Soviet anthropologists and archaeologists regularly did their field research in Mongolia. There was a joint Soviet-Mongol historical-cultural expedition that worked in the country for many years and achieved very impressive results. My friend Natal'ia Zhukovskaia, the best Russian anthropologist of Mongolia, regularly participated in this expedition. But in 1990, the Mongol side suddenly terminated collaboration with all Soviet scholars and scientists, except geologists. Six or seven years later they changed their mind again, but at that time the Russian scholars lacked money to do any field research, even in their own country. It is a pity, because the Russian/Soviet school of the Mongol studies was traditionally very strong. Nowadays, one can meet in Mongolia mainly with German, American, British, Japanese, even Israeli scholars. However, apparently, the situation is gradually changing for better. Sampildondov Chuluun, the new, young and energetic director of the Institute of History of the Mongolian Academy of Sciences, is all in favor of collaborative projects with anthropologists from Russia. So far he has established various contacts with scholarly centres in Tuva, Yakutia and Kalmykia, as well as with some other centres in Siberia. Buryat anthropologists, archaeologists and linguists have been doing research in Mongolia for ten years already. This is understandable, since the Buryats are the closest ethnic relatives of the Mongols. The main obstacle is that the Russian side is expected to provide grants for all collaborative projects.

The situation concerning research in Central Asian countries is hardly better. In 2005, the Department of Central Asia and Kazakhstan was abolished at the Institute of Ethnology and Anthropology. At the moment, about 110 researches are working at this Institute, but only five of them are studying Central Asian peoples, and only two are interested in the pastoralists of the region. Approximately the same number of anthropologists is employed at the *kunstkamera* (the Museum of Anthropology and Ethnography) in St. Petersburg, but again very few of them are interested in pastoralists. In the second half of the 1990s I was a co-principle researcher in a huge project to study contemporary pastoralism in different Central Asian countries, sponsored by USAID. From time to time I had to deal with the local authorities. Americans were welcomed, local scholars were welcomed too, but sometimes I had to argue when I invited this or that Russian scholar to take part in our project. I always succeeded in the end, but I had to argue. This is a rather sensitive issue in some Central Asian countries.

In short, even those who want to study contemporary pastoralists in the ex-Soviet countries don't have much of an opportunity. For example, there is one ethnographer of Kazakh pastoralists in Moscow, whom I invited to participate in my project. Later she told me: 'I am so grateful because you provided me with the rare opportunity to go to Kazakhstan and to do fieldwork there. Now I don't have this opportunity anymore'. It's not just a matter of the permit. First of all, it is a matter of research funds. But sometimes it is a matter of attitudes too. On a personal level, some Central Asian anthropologists and historians maintain very good relations with many Russian scholars. But at the institutional and administrative level the general attitude is 'Thank you very much, but we won't be your junior brother anymore'.

The result is that Russian anthropologists are nowadays mainly confined to studying pastoralists in their own country. Actually, many of these are not real pastoralists at all, but rather people involved in some limited pastoralist activities. However, there are very few, if any, anthropologists in Moscow and St. Petersburg, who study the Kalmyk, Tuvinian, and Buryat, pastoralists. Some local scholars privately grumble that they can study their own people themselves, but they are not studying the pastoralists either. There is a special Department of Siberia in the Institute of Ethnology. But it consists of only seven researchers, and during the last 10-12 years hardly more than three or four of them were engaged in occasional fieldwork amongst the reindeer pastoralists. One may add a few more scholars working in St. Petersburg, and regional centres such as Novosibirsk, but this does not change the general picture. It is not surprising, then, that in the post-Soviet period most important works on reindeer pastoralists in Russia have been published by Western anthropologists. But I am by no means sure that this situation will continue in the future. It is becoming more and more difficult for Western anthropologists to do field research in any part of Russia, because of the changing political climate in the country and growing bureaucratic obstacles.

All in all, nowadays studies in Russia of contemporary pastoralists are in decay. During the last five years or more, not one substantial paper on pastoralists has been published by the main Russian anthropological journal, *Etnograficheskoe Obozrenie*. Recently, the editor-in-chief of this journal invited me to be a guest editor of a special issue devoted to pastoralists in the ex-communist countries. It would be much better if he could invite a Russian anthropologist, but this is how things are at the moment.

YH: Thank you.

THE MAKING AND FUNCTIONING OF NATIONAL SCHOOLS

Chapter 9
Salvage versus Knowledge: Armenian Ethnography Across Soviet Times

Artak Dabaghyan

> Our destiny is to collect the materials now because their scarcity makes scientific studies impossible, and beyond this, there would be no lack of time for investigations. We will collect the stones and will trust to those who come next to create the building of our national identity and to draw the lines of human evolution.
>
> Yervand Lalayan, '*From Publisher*'

The many deficiencies of Soviet Armenian ethnography have various explanations, and most of them are beyond the purview of this chapter.[1] In my understanding, they would be better treated not as partialities brought about by the 'sovietness' of their historical context — eventually such an approach leaves us uncertain about the achievements of local ethnographic and cultural study. On the other hand, there is no need to either veil the imperfections of Armenian ethnography in the past or to omit the expressions of self-constraint, partially produced by the historical context of the discipline's development before and after the sovietization of Armenia.

Both externally- and self-imposed restrictions on Armenian ethnography suggest the persistence of an innate opposition between paradigms of 'Knowledge' and 'Salvage'. These words are probably too known and outworn, but I have not found a better term to describe the corollary relations

[1] This chapter was first presented at the 'Socialist Era Anthropology in the Caucasus and Central Asia' workshop at the Max Planck Institute for Social Anthropology, Halle/Saale, April 23-24, 2009. My thanks go to Florian Mühlfried for inviting me to attend the workshop, to Suren Hobosyan for providing me the archive of Deren Vardumian, to everyone who participated in discussions in Halle/Saale, and to my colleagues at the Institute of Archaeology and Ethnography, National Academy of Sciences of Armenia for their continuous support in collecting materials. I am especially grateful to Sergey Sokolovskiy, Ara Sanjian, Levon Abrahamian, and Benjamin White for their patience, assistance in improving the English text, and many insightful comments on the earlier drafts of this chapter. All remaining errors herein are my responsibility.

between two most powerful intellectual currents in the history of Armenian ethnography, which gained striking importance in Soviet years. The paradigmatic value given to them in this chapter means that, in my opinion, neither 'knowledge' nor 'salvage' are 'conceptually separable from our relationships and group memberships, the social dimensions of our lives' (Barth 2002: 2). 'Knowledge' in this context is only 'ethnographic', but worked to a transmittable format as a dataset with multiple interpretations, many of which may be at variance with others. Taken as a paradigm, 'knowledge' may embrace the whole discipline and the adjacent sciences, as well as 'the characteristic media and representations in which it is cast and communicated, and the social organization within which its activities take place' (Barth 2002: 9).[2] So, because there are no context-free sciences, 'knowledge' as a paradigm varies in certain circumstances. Some of these circumstances, like those that have brought 'salvage' or 'rescue' ethnography and archaeology to the fore, explicated general values of certain societies. In the ethnographic context, the 'salvage' paradigm is nested in the 'chronic problem of ethnography, namely, the impossibility of being omnipresent in the field-study community' (Hanchett 1978: 618), and this makes the parity of 'salvage' and 'knowledge' observable in at least the individual experiences of ethnographers, in the 'habitus' of ethnographic work.

Ethnography in the Early Soviet, Late Imperial, and Early Imperial Periods

More than two decades after the sovietization of Armenia, Stepan Lisitsian (1865-1947) wrote in a letter addressed to the higher authorities of the then-Armenian SSR: 'It is ridiculous to say that in the whole of Armenia, only me, a 78-year-old man, and one or two young associates [in the State History Museum of Armenia in Yerevan] are engaged in ethnography. With all our devotion it is impossible to embrace the entirety of current problems to be tackled by the ethnographers of Armenia' (Vardanian 2005: 407-409).

Although Stepan Lisitsian was not literally alone in the field through the first decades of Soviet rule, the state of ethnography described in his letter is in sharp contrast to the fact that the Armenian Ethnographical Association (founded in 1906), with its headquarters in Tiflis (Tbilisi) and with

[2] These references to Fredrik Barth's famous lecture on anthropology of knowledge do not mean that I accept the author's idea about the coherent transformability of the elements of folk knowledge to scientific accounts. I use the word 'knowledge' without any novel appeals or concessions to 'lore' and as a synonym for scientific (multi-disciplinary) data and findings, with all related forms of extracting them (2002: 8-10).

branches in many other towns of Transcaucasia, had 174 members in 1914.[3] In a brief article (1936), published long before his letter, Lisitsian thoroughly described the activities and plans of Soviet Armenian ethnography, specifically mentioning the study of the cultural traits of the refugee population from western Armenia. Although he expressed a casual interest in the improving living conditions of Armenian *kolkhoz* peasantry, Lisitsian was ultimately preoccupied with rescuing a vanishing past in difficult circumstances.[4]

In this chapter, I analyze data from a series of Armenian-language ethnographic research programs and questionnaires in order to trace the changing agenda in Armenian social and cultural studies. These print instructions, especially the two most detailed of them, penned by Khalatian (1887) and Lisitsian (1946), were intended by their authors as guides to standardize already gathered ethnographic materials (cf. Petrosyan 2008). Unlike one of their western analogues, the British Notes and Queries, the 1843-1951 issues of which reflect 'an increasing sophistication of the ethnographic process, [...] linked with its increasing professionalization' (Stocking 2001: 202), the Armenian questionnaires, published over a seventy year period spanning the late imperial and early Soviet years, consistently referred to a limited scope of ethnographic topics. Lalayan's (1896b, 1907) and Zelinski's (1898a, 1898b) additions to Khalatian's research program only tempted further extension of rigorous ethnographic recording to more elements of traditional culture. But owing to the disastrous effects of World War I and the great systemic changes of the next two decades, the expansion of ethnographic works, so expected and solicited by the authors of these questionnaires, did not occur in their lifetime. The last Armenian language ethnographic questionnaire, which Lisitsian saw published when he was 80 years old, recuperated roughly the same range of topics prioritized in the questionnaires from 1887-1906. In other words, by the mid-Soviet period, the agenda of Armenian ethnography, which was formulated over the last quarter of nineteenth century, was still awaiting answers. Therefore, trying to

[3] Yervand Lalayan (1867-1931) and Khachik Samuelian (1873-1940), both of whom served on the editorial board of *Azgagrakan handes*, were also active, although Lalayan was mainly engaged in museum work, trying to arrange the collections of the Armenian Ethnographic Society, which he managed to move from Tbilisi to Yerevan, and to recruit a new set of enthusiasts in the provinces. From 1925 to 1930, Lalayan headed the voluntary organization of *kraevedy* (regional history researchers). Samuelian, who was prosecuted by the Tsarist Government for being a member of the Social Democratic Party in the early 1900s, became the head of the State Archive of Armenia and lectured in Armenian cultural studies and atheism at the Yerevan State University.

[4] Lisitsian's most extensive monograph (1969) was provoked by a 1931 earthquake in the highland Zangezur (Syunik) province.

summarize the legacy of the preceding pre-Soviet ethnographic research on Armenians in his questionnaire, Lisitsian did not seen any need to modify the evolutionist standpoint of his predecessors.[5]

One of these predecessors was Grigor Khalatian, who was more a classical orientalist than an ethnographer. His research program of 1887 appeared after his return from Germany, where he most likely would have encountered the *völkerkundliche* theses during his two years studying in German universities (Nazinian 1985: 16-20).[6] Similar permeations of certain elements of Western human sciences into Armenian studies are easily discernable in the course of detachment of ethnographic science from the broad corpus of oriental studies throughout the eighteenth and nineteenth centuries. As the orderly and practically arranged registration of new Russian imperial domains was superseding the earlier travelogues (which were mostly written by foreigners) and chronicles (recorded by locals), emerging ethnographic knowledge about the Armenians had to reproduce the approaches invented by the early German academicians employed by the Russian Tsarist government for description of illiterate Siberian peoples (Vermeulen 2008: 13-26, 99-159, 271-85). The coarseness of further penetration of such 'ethnography' has been observed by many recent historians of imperial Russian/Soviet ethnography and anthropology (Clay 1989; Hirsch 2005; Khachaturian 2009; Knight 2000; Marshall 2006: 120-130; Martin 2000). Armenians, Georgians, and several other peoples of European Russia, with their own literacy and ensuing possibilities for earlier development of local ethnographic traditions, had to adopt and heavily lean on this metropolitan paradigm. The latter was not void of attractive features: it could serve for Linnaean type classifications of statistical, geographical, linguistic, ethnographic, and folklore materials first collected in Armenian-populated villages and towns of the empire in the second quarter of nineteenth century. It secured the preservation of the multidisciplinary, complex nature of the first systematized data and introduced the first broad perspectives on data collected within the orientalist spectrum of human sciences: universalist aspirations 'to draw the lines of human evolution' (Lalayan 1896a), or in Boasian terms, 'to establish the laws governing the growth of culture' (Boas, cited in Rodseth 2002) were to appear again in the late nineteenth century.

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[5] To be precise, the structure and even the sequence of chapters of Lisitsian's questionnaire are obviously influenced by an earlier work written by Grigorii Chursin (1929), with whom Lisitsian worked for many years at the Caucasus Historical-Archaeological Institute (Vardanian 2005: 103-109).

[6] This interest of one of the Deans of the Lazarian School of Oriental Languages resulted in the publication of nine volumes of *Eminean Azgagrakan Zhoghovatzu* (*Eminian Ethnographical Collection*, 1901-1911), the second Armenian language academic periodical in the Caucasus.

This chapter does not explore all the various ways in which imperial interests have transformed the 'pure' inquisitiveness of local or visiting scholars and ethnographers in the 19th and 20th century. Setting out from Marshall Sahlins' observation that in '[a]nthropology there are some things that are better left unsaid' (2002: 35), I only describe how, before committing to any particular theory, the first generation of Armenian lay intellectuals had to accept some viewpoints of the European scholars, who were employed as administrators in newly acquired imperial domains (cf. Sokolovskiy 2009: 54-55), as their precursors in researching the Armenians 'ethnographically'. The German civil servants Hermann Abich (1806-1886), August Haxthausen (1792-1866), Johann-Friedrich Parrot (1791-1841), and Moritz Wagner (1813-1887) were accompanied in their studies in Armenia by Derpt (Tartu) University graduate Khachatur Abovian (1806-1848), who served as an interpreter. This fact would seem to indicate that as one of 'Great Powers' of the time, Russia was present in Armenian ethnography from its very origin, if we put this origin point in the second half of the nineteenth century.[7]

This tendency was hardly acknowledged in the most recent textbook on ethnography (Bdoyan 1974), and it appears at variance with the consensus surrounding the origins of Armenian ethnography that links the discipline's origins in Armenia to Srvandztian's and Khalatian's research programs (Khalatian 1908; Petrosyan 2008). Nevertheless, Vard Bdoyan, as a founder of the narrowly localist genealogy of ethnographic science in Armenia, did not suppress his desire to move the origins of Armenian ethnography further back to late eighteenth and early nineteenth centuries, linking them with the formation of European-style historiography, linguistics, and folklore studies among the members of the Catholic Armenian Mkhitarist congregation of Venice (Bdoyan 1974: 8-13).[8] In fact, the problem of choice across

[7] Lisa Khachaturian has also reflected on the origins of Armenian ethnography, noting that 'The loosening restrictions of the reform era, which gave the Russian generations of the fifties and sixties [of the nineteenth century] the professional support to embark upon demographic, ethnographic and statistical expeditions, also encouraged their non-Russian peers to organize similar information gathering projects' (2009: 191). See also Chapter 6 of her book for an account of the gradually emerging presence of an ethnographic agenda within Armenian studies from the 1860s to the 1880s. Also, note that Stepanos Nazar'yantz, the central figure of Khachaturian's book, also graduated from Derpt University.

[8] Through one of its former monks — Serovbe Karnetsi, who reverted from Catholicism to the Armenian Apostolic faith in 1813 — the Mkhitarists also made contributions to the Russian Oriental school. Karnetsi was a learned expert on several European and Middle Eastern languages. He was appointed as Director of the Moscow Lazarian School of Oriental Languages in 1817, where he taught for the first decade of the school's existence (Khachatrian and Khacherian 1968). At around the same time, another Mkhitarist, Minas Bzhshkian,

multiple intellectual currents, including those linked with local pre-scientific literary traditions, predated the emergence of ethnography as a discipline. For centuries, Armenians have described and documented their neighbours, conquerors, and hosts in an 'objective' manner, which has encouraged them to believe in their own ability to study themselves.[9] But here it should be said that even if Armenian language sources can successfully compete with the Russian language materials in terms of quantity and thoroughness, one misguided approach of Armenian ethnography was its reliance on Russian statehood, in at least two ways: as an interested player in the formation of knowledge about peoples, and as an empire, which conquered and protected the East or Caucasian Armenia. The first has substantially supported and shaped the course of professionalization and maturation of the discipline. The second solely applies to the different fates of Armenians residing as indigenous people on the eastern and western sides of the Russian-Ottoman border.[10]

In this initial period of Armenian ethnography, the boundaries between 'salvage' and 'knowledge' were unstable, with the two folding into one another in the first decades of the discipline's formation both before and after the sovietization of Armenia. By introducing this pair of paradigms I hope to show how ethnographic knowledge about the Armenians was solidified, as the greatest part of this process occurred in the Soviet period (see the appendix). The active resistance of professionalizing Armenian ethnographers to both mercantile and ideologically determined knowledge-seeking practices of empires has a background in the earlier Armenian literary tradition. It was also an ecclesiastical tradition, though such an attribution might seem old-fashioned today (but cf. Barth 2005: 7-15; Robbins 2006). Several Armenian intellectuals of the nineteenth century, who participated in the first ethnographic enquiries, were either clergymen or from priest families. For

published his splendid travelogues on Armenians in Poland, Crimea, and north-eastern Turkey.

[9] The best-known example from the Armenian literary tradition of neighbouring peoples being described comes from seventh-century Armenian geography: The *Ashkharatsuyts* manuscript provides modern researchers with approximately two dozen ancient ethnonyms for the Caucasian peoples. Another key source for scholars of the early Mongol period is Patkanov 1873. Both sources were translated into Russian in the 1870s by Kerovbe P. Patkanov (1833-1889), a professor of the Faculty of Oriental Studies at Saint Petersburg University. Patkanov tutored N. I. Marr and was the father of the prominent researcher of western Siberia, Serafim/Serovbe K. Patkanov (1861-1918).

[10] As this volume explores anthropology in Soviet Caucasus and Central Asia, my chapter skips over many details of the ethnography of western Armenians, who have fallen victim or survived the genocide in the Ottoman empire from 1915-1921. This disaster has had a profound impact on Armenian ethnography of the Soviet period as well, and I hope to reflect more substantially on this in a future paper.

instance, in addition to serving as an officer in the Russian army and writing *Bibliographica Caucasica et Transcaucasica* (1874-76), Mikhail Miansarov (1830-1880) was the nephew of an Armenian Catholicos Nerses Ashtaraketsi and one of the founders of Armenian folklore studies (Vardanian 2002). Khachatur Abovian, a young priest who left St. Ejmiatzin to help professor Parrot climb mount Ararat and then became his student in Derpt (Tartu), was employed afterwards as a schoolmaster in Russian administration of the Armenian province. Abovian has been lauded as an ethnographer for his vivid representations of Armenian village life under Persian rule, the Muslim festival in Tiflis, and Kurdish wedding ceremonies. Members of the Patkanov family were descendants of a priest who ran a school for Circassian Armenians and other converted Christians in Armavir, North Caucasus. Ultimately, nearly all of the forefathers of the various branches of Armenian studies were connected to the Armenian Church.

In the last quarter of the nineteenth century, Armenian society undertook capitalist development, which wrought remarkable changes, and the salvage paradigm was secularized enough to fill in the veins of nationalism. But from the vantage point of the institutional changes of the period, or as an explanation of the composition of the Armenian Ethnographical Society, both paradigms were equally influential in the ongoing formation of ethnographic scholarship. As one of the largest and most dispersed nations in the region Armenians were widely employed in the Russian civil administration of the Caucasus. At the same time, the Armenian Church continued to provide a well-established parallel network for the dozens or even hundreds of amateur ethnographers and folklorists. One such amateur was Yervand Lalayan, who used his savings from five years of teaching in a parish school to attend courses in Geneva and Paris, before subsequently founding *Azgagrakan handes* (*Ethnographical Journal*, 1896-1916) and *Armenian Ethnographical Association* (1906).

Late Soviet Armenian Ethnography: Institutional Changes

After the establishment of an academic department in the Institute of Archaeology and Ethnography at Academy of Sciences of the Armenian SSR (IAE) in 1959, Lisitsian's disciples reinitiated regular fieldwork trips to areas inside and beyond Armenia, which were selected as insufficiently studied at the moment (Vardumian 1959).[11] Already in 1960, the geographical scope

[11] The Ethnography group at the Institute of History of the Academy of Sciences of Armenia was established earlier, in 1953, and was comprised of a small number of ethnographers who were previously employed in the Ethnography Department of the State Museum of History, which was at that point also part of the Academy of Sciences. In a 1960 manuscript, Deren Vardumian linked the organization of this group to the Pan-Union ethnographic conference in

of their studies spanned to the Armenian-populated Nagorno-Karabakh and Javakheti regions, the Russian coast of the Black Sea, and Rostov-on-the-Don. Thus the practice of ethnographic rescue expeditions to distinct Armenian groups was restored. The interest in Soviet peasant and labour studies, which can be observed in the publications of some of the members of these expeditions in the late years of Stalin's rule (e.g. Vardumian 1956), evaporated as soon as the official status of Armenian ethnography improved with the establishment of the Institute of Ethnography in Moscow. The latter was intended as a centre of Soviet ethnography, replicating the centralized structure of the Soviet state. The first actual collaboration between the Caucasus department of the Moscow Institute and their Armenian colleagues occurred in the course of writing the second volume of 'Narody Kavkaza' (*Peoples of the Caucasus*, Gardanov 1962), in which some hundred pages were dedicated to traditional Armenian culture. As the senior members of the Ethnography department at IAE told me, Mark Kosven (1885-1967), then the head of the Caucasus department of IE, actually encouraged the institute's personnel to narrow down and deepen their focus on research of traditional culture.

The 'Moscow Centre' was always present in the Armenian academy, although not to the extent indicated in certain contemporary studies of totalitarianism (cf. opinions about the relative unimportance and thus the relative safety of ethnographic studies in Soviet Union and Eastern European countries from 1960 through the 1980s: Buchowski 2007: 7; Slezkin 1993: 114). From the peripheral Armenian point of view, the enormity of the tasks discussed and decided by the Centre at the time that Russian/Soviet ethnography was being restructured fell under the paradigm of 'civil service' or 'knowledge for sale'.[12] Trained in older, traditional ethnography, which was mastered not long before by Lisitsian in his prominent Ethnographic Questionnaire (1946), the next generation was neither willing nor able to shift from 'cultural studies' to 'community studies' in a broad sense.[13] Their traditionalist leanings were about to be outweighed by the need to be in tune with 'progressive' Soviet trends in developing ethnographic science, to be

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Moscow 'which suggested combining ethnographic scientific research with works on building Communism' (Vardumian 1960).

[12] In comparison with the situation in Western societies described by Willner (1973) in Weberian terminology, the Soviet sciences had a unique relation with society as the main consumer of scholarship. Soviet humanities scholars and society were walled off one from one another by the state, which served as censor, sponsor, and employer, which is not the case in liberal Western societies and university systems.

[13] Here, I accept Wolfe's provisional distinction between the Soviet socio-cultural research, which was more inclined toward studying 'cultures', and socio-cultural research in Eastern and Central Europe, which was more inclined toward studying 'communities' (2000: 198).

part of relations between the governments and the society to earn a living, as well as funds for ethnographic education and research. But with upwards of only nine scholars working at the Armenian Department of Ethnography in the early 1960s, this group would have been unable to fulfil tasks or recommendations appearing in the pages of *Sovetskaia etnografiia*, even if they had so desired. Furthermore, certain research trends from the (from the standpoint of scholars in the peripheries) fluctuating 'centre of ethnographic thought' (Moscow) were finding rich soil in Armenian reality and were fostering paths out of mainstream, traditional 'salvage ethnography'.

While the impact of Western anthropological thought on Moscow- and Leningrad/St. Petersburg-based ethnographers has often been overlooked in postsocialist reflections on the formation of the ethnographic agenda in the Soviet Union from the 1950s to the 1980s, the influence of ethnographic works initially prioritized in Moscow is easily observable in provincial Armenia. Vardumian's and Karapetian's study on *kolkhoz* peasants (1963) described the developments in 'thriving' socialist Armenian society, utilizing a historical-comparative perspective (albeit without an elaborated methodology), and simply juxtaposing traditional culture and modernity as two analytic categories (Mkrtumian 1979; Vardumian 1956; Vardumian and Karapetian 1963).[14] Karl Seghbosian's works even more strongly attended to socio-cultural changes and patterns of urbanization in Gyumri, Armenia's second largest city (referred to as Leninakan before 1991 and Alexandropol before 1924) and its hinterlands (Seghbosian 1974, 1980). Alla Panian's works (Ter-Sarkisiants 1989, 1998) also played a key role in initiating intra-community comparative studies of Armenians in Armenia and in Nagorno-Karabakh and the North Caucasus.

There is no need to go deeper into the findings of this earliest variant of Soviet Armenian ethnographic 'contemporary' or 'applied' studies: taken together, all such writings from the 1950s to the 1970s, both published and unpublished, comprise no more than a few thousand pages. The most important contribution of these studies was that they irreversibly though only partially overcame the 'historicist' or 'traditionalist' theory of popular culture. These efforts validated the study of cultural change as valuable in its own right. Of course, this should not be thought of as completely novel, especially when we consider the previously mentioned statistical and economic surveys of Russian Armenian civil servants in the late nineteenth century as an antecedent. But, unfortunately, this novel intellectual trend preserved close ties with the socialist vision of Armenian modernization and posed itself as an antithesis to earlier cultural studies. Aiming to move even

[14] Consult Abashin's chapter in this volume.

further from earlier traditions of scholarly inquiry, this trend arrived at a new methodological foundation by merging ethnography and sociology (and distancing the two as much as possible from the discipline of history).[15] The convergence of ethnography and sociology was administratively secured with the establishment of the new ethno-sociology department at the Institute of Archaeology and Ethnography in the 1970s. And, as might be expected, this radical departure was orchestrated from Moscow.

Meanwhile, the small community of Armenian ethnographers could not come to an agreement regarding the urgency of studying social change. They continued to devote their limited time and resources to collecting data on traditional cultural artefacts and lore, as their respondents steadily grew old and died. From the 1950s onward, the bulk of ethnographies written either in Armenian or Russian amounted to historical-ethnographical studies, devoting roughly equal attention to 'material' and 'spiritual' culture as research subjects. Comparing the titles of dissertations written during this period (see the appendix) with the structure of Lisitsian's *Ethnographic Questionnaire*, the unrestricted dominance of traditional themes becomes apparent, although the first passionate excursions into the field of social and cultural changes associated with socialism appeared shortly after publication of the questionnaire. Only a few of the 50 chapters of *Ethnographic Questionnaire* deal with questions related to Soviet-era changes, although Lisitsian did make some general remarks about the importance of studying such changes in the book's introduction (Lisitsian 1946: 6-9; Vardumian 1963). Long after his death, Lisitsian's questionnaire continued to play a key role in shaping the agenda of Armenian ethnography. Customarily, ethnography students at Yerevan State University consulted *Ethnographic Questionnaire* before assembling their own questionnaires for summer fieldwork. In the course of their training, Yuri Mkrtumian and a number of colleagues would work with the novices to help them develop their fieldwork skills (Arutyunov 2006).

A quick look at the appended list of dissertations makes apparent the recurring chronological frame of Armenian ethnographic studies: the titles of 45 of the 73 works contain phraseology like 'a historical-ethnographical study of the late nineteenth and early twentieth century'. Even when not explicitly referenced in titles, this time period figures prominently in the introductory chapters of these works.

[15] A similar merger of social sciences was observed in the US in the 1930s, inspired by the increased influence of the British social-anthropological theories and by the more mundane forces described by Goldschmidt as the 'recognition that we were also a part of the world's cultures, as a deficiency adjustment to the lack of funds for overseas research, and the concern with the poor and underprivileged in our society in those depressive years' (2000: 796).

I cannot provide here an exhaustive explanation for this phenomenon. Moreover, this 'classical' period is likely not a uniquely Armenian experience. Sergey Sokolovskiy detects the same developments over a much wider Russian/Soviet milieu and refers to it as 'ethnographical time' (Sokolovskiy 2009: 51). Whatever the reasons for this ethnographic 'diet', the second half of the twentieth century finally witnessed the realization of an agenda for Armenian ethnography that was set at least two generations earlier.

Of course, there are many clues that allow us to speak about the attractiveness or higher value of historical ethnography over applied ethnography, as the latter term was familiar to Vardumian, for whom applied ethnography roughly meant the study of social change. This evidence not only proves the reliance of Lisitsian's disciples and their own subsequent disciples on the 'salvage' paradigm but also testifies to the decay of this paradigm in the course of relations between the expanding community of professional ethnographers and the local bureaucracy. Thus the 'project' of rescuing disappearing elements of Armenian culture that began in the 1960s was spurred by the events of 1965 and the subsequent decision of the government to build an Armenian National Museum of Ethnography.[16] As the Sardarapat Museum was being built on the periphery of Yerevan between 1968 and 1978, further changes were made to the curricula and training received by future ethnographers at Yerevan State University. Ethnographic education was continuously improved throughout the 1970s and 1980s with the contributions of Yuri I. Mkrtumian, who led basic courses and trained students.[17] In 1970, Mkrtumian began to introduce new courses to Yerevan State University in an attempt to bring the institution's standards to the level of his alma mater, Moscow State University.[18]

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[16] On April 24, 1965, the 50th anniversary of the Armenian genocide was commemorated with mass riots by Armenian students and youth in Yerevan, which finally led to further liberalization of the government's cultural policy. The measures that followed were aimed at reconciling the people with the government and included decisions to build the Genocide Memorial and the Ethnographic Museum in Sardarapat (for more details, see Gispert 2007; Marutyan 2009; Suny 1993: 180-186, 205-210).

[17] Although the independent Chair of Ethnography came into being at Yerevan State University in 1989 (and re-united with the chair of archaeology in 2004), a limited number of courses on ethnography were taught by YSU's history and geography departments at various points. The first such courses were taught by Lisitsian in the 1930s and 1940s (Vardanian 2005: 220-250). In the 1950s and 1960s, ethnography was taught to history students by Karapet Melik-Pashayan and Deren Vardumian, with the support of their younger (Anushavan Petrosyan and Rafik Nahapetyan) and elder (Vard Bdoyan) colleagues.

[18] Sergei A. Tokarev, a leading Russian ethnographer, was invited to teach a special course in 1970. From the late 1970s onward, Sergey A. Arutyunov began to give annual lectures on pre-selected topics. Nahapetyan and Petrosyan both taught special courses on Armenian ethnography. Petrosyan also taught a course entitled Ethnography of the Slavs and Urban

Summarizing my review of the institutional changes in Armenian ethnography, we can first note the rapid numerical increase both of state-funded institutions and professional ethnographers over a relatively short period of time. This boom strikingly contrasts with the depletion of the field in the early Soviet decades. The Ethnography Department at the State Museum of History was supplemented by two departments at the Institute of Archaeology and Ethnography (the ethnography department in 1959 and the ethnosociology department in 1981) and the State Museum of Ethnography (in 1978). Graduates from the ethnography program were employed by other institutes of the Academy of Sciences (the Institutes of History, Arts, and Oriental Studies) and by museums with ethnography departments. The establishment of stable relations between Armenian ethnographers and their counterparts in the Moscow Institute was another important institutional development, enhancing the links between the centre and the periphery and with the various developing academic schools in other peripheries.

Changed Perspectives on Armenian Culture and Community

The widespread assumption that the main practice of Soviet ethnographers in the late Soviet decades was the adaptation of available freedom of thought to an existing corpus of folk knowledge may be seen in late soviet Armenian ethnography with the investment of a 'culturological apparatus' into a field dominated by calm salvage ethnography.[19] Limitations on freedom of thought backed the unexpected penetration of sociological methods into research projects targeting innovations (vs. traditions) or social and cultural changes. The results of these projects were published in the mid-1980s (*Kul'tura zhizneobespecheniia* 1983; *Naselenie Erevana* 1986), but the fieldwork leading to these collective monographs was originally planned in and had been conducted since the mid-1970s.[20]

Ethnography. Several other special courses were taught by leading scholars from the Institute of Archaeology and Ethnography: D. Vardumian taught fieldwork methods; Sargis Harutiunian taught folklore and epics; and Gevorg Tiratsian taught Armenian ethnogenesis. With the arrival of the Moscow-trained generation of scholars like Levon Abrahamian, Zaven Kharatian, Harutyun Marutyan in the early 1980s, the number of special courses increased: An introductory course entitled History of Ethnographic Thought provided students with an overview of the various anthropological and ethnological schools up through French structuralism. Regional studies were expanded to include Africa and the South Pacific.

[19] Although the late construction of the Ethnographic Museum extended the period of material collection (over the course of the 1980s, the museum purchased and acquired donations of more than 30,000 artefacts from all over Armenia, the adjacent Armenian populated regions, and from the diaspora), the era of salvage ethnography was fading away (at least temporarily).

[20] The contributors and assistant researchers were mainly local scholars, many of them recent graduates of the Moscow *aspirantura* (the graduate school at the Institute of Ethnography). In

The two volumes that resulted from this decade long collective effort deserve much more attention than the limits of this chapter allow. It suffices to note that *Kul'tura zhizneobespecheniia* and *Naselenie Erevana* are the most extensively cited monographs in post-Soviet Armenian ethnographic literature. These works are interesting insofar as they reveal top-down efforts to modernize local ethnography. The presence of two prominent Armenian scholars — Sergey A. Arutyunov and Iurii V. Arutiunian — from the Moscow institute confirms this supposition. Both teams of authors utilized specially designed quantitative methods, and although the ethno-sociological study of Yerevan (*Naselenie Erevana*) is based on statistical and other standardized materials much more than the volume on subsistence culture of the late soviet Armenian peasantry (*Kul'tura zhizneobespecheniia*), both are substantially removed from the traditional ethnographic questionnaire.[21]

The evolving views of local principal investigators add some interesting nuances to the previous discussion. As a contributor to *Kul'tura zhizneobespecheniia* and also a co-author of the four-part working theory of ethnic culture (Arutyunov 1989: 200-229; Mkrtumian 1978), early in his career Yuri Mkrtumian was one of the first to revisit the pre-Soviet statistical materials.[22] He successfully recuperated the implicit ethnographic value of those sources in his dissertation (Mkrtumian 1974; cf. Volkova 1984). In later decades, this trove of socio-economic information began to feed the younger generation of Armenian ethnographers, and it became obligatory to reference these sources in dissertations on culture and community relations in the nineteenth and early twentieth centuries. Before she edited *Naselenie Erevana*, Emma Karapetian was long involved with research into social

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addition to the ethnographers from IAE and Yerevan State University, a group of philosophers from the corresponding Institute of the Armenian Academy of Sciences, headed by Eduard Markarian, contributed to the collection of materials and to writing *Kul'tura zhizneobespecheniia i etnos* (1983); for a retrospective account by one of the contributors, see Barsegian 2000.

[21] *Naselenie Erevana* was realized as a part of the pan-Union research project known by the acronym 'OCY', which stood for *Optimizatsiia sotsial'no-kul'turnykh uslovii razvitiia i sblizhenia natsii v SSSR* (Optimization of socio-cultural standards of development and imminence). 'Acculturation' is the Western concept closest to this notion, which was developed and overseen by the Department of Ethno-sociology of the IE. It would be difficult to overestimate the importance of the rich materials from this project (for a critical overview, consult Shanin 1989). Modern scholars of the Caucasus may find the works of its Dagestani and North Ossetian groups summarized in *Traditsionnoe i novoe* (1988) and *Sovremennye sotsial'nye* 1987.

[22] With his four part model of culture, Yu. Mkrtumyin sought to avoid dichotomizing the notions of 'material' and 'spiritual'. He suggested the following four part analytic distinction: 1. Culture of primary production; 2. Subsistence culture; 3. Socio-normative culture; and 4. Humanitarian culture.

relations in the Armenian peasant community, focusing on clans and communities (Karapetian 1950, 1966, 1971).

There were heterodox tendencies in the works of these authors, who pushed for more sociological and verifiable methods of extracting ethnographically relevant information on peasant culture and urban communities. This time, efforts at rapprochement with the 'knowledge' paradigm were substantially backed by the resources of maturing Armenian ethnography departments — both surveys (*Kul'tura zhizneobespecheniia* and *Naselenie Erevana*) were nationwide and rather laborious endeavours. The constant ties with Moscow colleagues and the Russian language of publications had rendered these heterodoxies unique alternatives to the apparently receding traditional ethnography.

Today it is difficult to appreciate the real potency of these experiments in soviet Armenian ethnography, as the socialist model of modernization vanished with perestroika, the subsequent disintegration of Soviet society, the rise of nationalism, and other broad systemic changes. Studies of the 'late nineteenth — early twentieth century' dominated Armenian ethnography of the 1980s, like in previous and subsequent decades. The word 'culture', which had recently risen in status and gained a categorical meaning, was continuously and consciously decategorized in historical ethnography. We find this word attached mainly to the branches of material culture (for instance, as the appendix demonstrates, 'embroidery culture' and 'oil extraction culture' appear in the titles of the dissertations of Karine Bazeyan and Suren Hobosyan). In fact, the concept of culture was downgraded to a simple word, an extension, useless without the preceding noun or the adjective 'Armenian'. The works of ethnography students and numerous Moscow-educated scholars from this period actually constituted a detailed elaboration of the issues briefly formulated in Lisitsian's earlier questionnaire. Now they were striving to do more than merely note the existence of variants in folk culture, although such variants were not neglected. From a certain perspective, Armenian ethnography in the late Soviet decades was reconstructivist, emphasizing customs and rituals that were overlooked or simplified in the previous Soviet decades. The following brief assertion from Armenian ethnographer Levon Abrahamian (2004: 14) discloses the essential characteristics of this period, which in many ways constituted a continuation of the salvage paradigm:

> Those researchers who were governed by nonconformist professional and moral principles consciously or unconsciously preferred to reconstruct the archaic past, because here they enjoyed comparatively greater creative freedom. This does not mean, of course, that the search for roots was an entirely danger-free, apolitical occupa-

> tion: reconstructions of primeval society could be as fantastic as they liked, but they were not permitted to contradict the Marxist version of the beginnings of human history.[23]

The fervour for salvage in this period also involved fervour for invention and/or reinvention. Already in 1984, when Natal'ia Volkova was writing about the ethnographic value of statistical, economic, and other materials collected by late nineteenth-century Caucasian Russian administrators, these materials were already being extensively cited in works of Armenian ethnographers (Akopian 1988; Arakelian 1984; Dolzhenko 1984; Mkrtchian 2010; Mkrtumian 1974; Sarkisian 2002). Thus, since the late 1960s, citation of the publications of former imperial civil servants has been obligatory in bibliographies of Armenian ethnographic studies and especially in Russian language dissertations in the 1980s. Another practice from this period was the collection of 'rural histories', which eventually developed into an extensive archival project. During their fieldwork, which was organized into month-long 'complex ethnographic expeditions' (Gromov 1966), local amateurs (most of whom were teachers) were provided with a copy of Lisitsian's Questionnaire and were asked to write down their own observations, which were later purchased by the Institute of Archaeology and Ethnography. Since the 1970s and into the present, this collection of data has been extensively cited in dissertations.[24] It is worth noting that many of the individual files that comprise this collection are quite informative about Soviet realities both from the standpoint of folklore studies and from an economic anthropological perspective.

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[23] L. Abrahamian and H. Pikichian have provided one of the most picturesque ethnographic reviews of late-Soviet Yerevan (Abrahamian and Pikichian 1989, 1990). Descriptions of many of the traits of Armenian folk culture, originally studied and reconstructed in the 1970s and 1980s, are now available in English (Abrahamian and Sweezy 2001). L. Abrahamian's latest work (2006) includes an account of his interpretive approach to late Soviet Armenia as seen from the ensuing postsocialist era.

[24] The Archive of the Ethnographic Department (AED) of IAE also houses materials from the annual expeditions of IAE ethnographers, over 20,000 photographs, 600 drawings, over 200 genealogical trees, and 160 personal files from village correspondents. The former terminology — 'village correspondent' — is not always correct, as many of these correspondents actually lived in towns or cities, and about thirty were refugees from western Armenia or were repatriated members of the diaspora. In 2006, in accordance with Yuri I. Mkrtumian's will, the archive received Mkrtumian's collection of field materials and a portion of the HAG (*Hamalsarani azgagrakan gitarshav* [University Ethnographic Expedition]) materials collected in the Tavush and Vayots Dzor provinces of Armenia.

Conclusion

Agreeing that Iulian Bromley and his colleagues' leanings toward Soviet ethnography were predicated on Marxist cum evolutionist presuppositions and an irrefutable vision of socialism as the most advanced form of human society, and acknowledging that these leanings toward Moscow have impacted the peripheries since the 1970s, I must also state that it is hard to search for immediate realizations of an envisioned common Soviet agenda in Armenian ethnography. The following commentary from Teodor Shanin summarizes the features of late Soviet scholarship:

> Soviet perceptions of ethnicity and their expression within the social sciences differ in emphasis and in angle of vision from their Western counterparts. They follow a different tradition, which has led to a different reading so far and stimulated different patterns of data-gathering and analysis. While rejecting racialist ahistoricity, they did not accept as its alternative a fully relativist treatment of ethnicity. They accorded ethnic phenomena greater substance, consistency, and autonomous casual power and focussed attention on the ethnicity of majorities as well as minorities. Compared to mainstream Western studies, theirs have been more historical in the way they treated ethnic data, yet these analytical achievements have not produced effective ethnic policies for the close of our century. To see why, one must turn to the way ethnic images intersected with Soviet political strategies and the country's history (Shanin 1989: 415-416).

This commentary is one of the mildest I have encountered in post-Soviet reflections on 'Soviet' ethnology. The majority of other assessments of the historical role or the positivist value of 'Soviet' ethnography (I use the quotes to weaken the archetypical unity of this term) take on the form of inter-generational breach. Even the more substantial critique hardly goes beyond relations between scholars and the state, on one side, and the issue of the responsibility of ethnographers for the rise of national sentiments/self-consciousness, on the other.[25] This situation is at least strange: though we see the need for and some temptations to treat 'Soviet' and contemporary 'Western' anthropologies as equal counterparts (Buchowski 2006, 2007; Danilov 1992; Dragadze 1996; Hann 2005; Hann et al. 2007), there are very few comparative studies on the ways that both scholarly communities sought to move away from the salvage paradigm. Reading in Gupta and Ferguson (1997: 21) that American cultural anthropology 'outgrew its "salvage" phase' in the 1930s and 1940s (or even later, in the 1950s, according to

[25] Given that the state was governed by communist internationalist ideology, the combination of these two forceful currents in post-Soviet self-critique seems logically inconsistent.

Bunzl 2004: 437-440 and Goldschmidt 2000), and noting some similarities between acculturation studies in the United States (Gupta and Ferguson 1997: 21-22) and the 'optimization' mega-project (OCY) in the Soviet Union (Danilov 1992; Shanin 1989), I have not thus far encountered any analytical works that discuss the diffusion of ideas across the Iron Curtain. Instead, 'self-criticism' in post-Soviet Russian ethnography, initiated by Valerii Tishkov, the Director of the Moscow Institute of Anthropology and Ethnology, has pressed the disfavoured elders to defend their positions against faint accusations, generally voiced in political and, more rarely, in moral terms. A recent volume (Tishkov 2008) of interviews with leading Soviet ethnographers may be a sign of improvement, as it provides us the chance to hear the voices of individuals involved in the development of ethnographic theorizing and field research on the eastern side of the Iron Curtain.

One of my hopes in separating the not very complicated ethnographic practices of Soviet Armenia into 'salvage' and 'knowledge' and attributing them paradigmatic characteristics was to display that, under totalitarian rule, the nature of the discipline did not change to the extent depicted in post-Soviet reflections on cases in which local ethnographers took part in the Soviet manner of resolving the so-called 'nationalities issue' in the Soviet Union (Hirsch 2005; Marshall 2006). On the other hand, acknowledging some of the milestones achieved in Armenian ethnography both before and after sovietization, it is important to note those characteristics of the discipline that are pan-imperial or pan-Soviet in nature. In interaction with local traditions built on the stones collected for 'those who will come next' (Lalayan 1896a), these characteristics contributed to the formation of Soviet-era Armenian ethnography as a traditionalist discipline.

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Chapter 10
Soviet-Era Anthropology by Azerbaijani Scholars

Aliagha Mammadli

Early anthropological studies by Azerbaijani scholars date back to 1923, when the Society for Research and Studies of Azerbaijan (SRSA) was established, with the Historical Ethnography Department and the Department of Turkism as its constituent parts. In 1929, the Society was renamed the Azerbaijani State Scientific Research Institute (SSRI), with ethnographic studies being conducted by its Department of History. Studies by early Azerbaijani ethnographers — Alesker Alekperov, Mamedgasan Bakharly, and Davud Sharifov — primarily focused on traditional ways of living and the economic activity of the population of the country (Veliev 1921).

In the 1920s in the Soviet nations, studies of so-called survivals were typical (see DeWeese, this volume), and Azerbaijani scholars carried out ethnographic research in line with this trend. It is noteworthy that the study of survivals was stipulated by ideological directives, which were explained in terms of the necessity of destroying the 'old world' with all its cultural peculiarities and peculiarities of social life in the various nations. This circumstance was reflected in an article by Alekperov and Vartapetov entitled 'Tasks of Azerbaijan Ethnography', which was published in the journal *Soviet Ethnography* in 1932 (Alekperov and Vartapetov 1932).

Early Azerbaijani professional ethnographers actively joined the Bolshevik Party's struggle against religion. In particular, in his articles Alekperov referred to religious persons as 'true agents of imperialism' (Alekperov 1960), contending that it was necessary to fight against religious persons in the cruellest manner possible. In his view, it was naive to believe that religion would die off on its own. Therefore, it was essential to decisively confront it. In addition, Alekperov thought that *aksaggal* (Azerbaijani clan heads) and tribal leaders were also class enemies and called for an uncompromising struggle against the remaining survivals of the tribal system (Alekperov 1960). Notwithstanding the strict ideological undertones of these works on survivals, the authors of these studies collected large amounts of valuable data and observations on the pre-religious beliefs of the Azerbaijani

population. In his anti-religious articles, Alekperov gave detailed descriptions of the ancient religious views of the Azerbaijani nation. In particular, he noted the existence of different forms of stone cults. In his opinion, stone cults divided into two groups: rock mountain cults and river stone cults. He thought that these cults originated from animistic beliefs. In addition, in his research Alekperov described other forms of animistic beliefs such as the tree cult, wood cult, and so on. Describing the survivals of animistic views amongst the Azerbaijan population, Alekperov was the first Azerbaijani ethnographer to mention the mystical personas of *qulybani*, *ərdov*, and *halanasi* (ibid.).[1]

It should be noted that many early Azerbaijani ethnographers, even those who took an active part in the pursuit of Soviet policy in the country, were themselves later repressed under a variety of charges. Alekperov's fate was rather typical in this regard. Soviet power was established in Azerbaijan on April 28, 1920. One day later, Alekperov was arrested for anti-revolutionary activity. He was arrested again for a second time in 1929 on charges of anti-Soviet activity. But the true misdeed of this ethnographer was that he maintained relations with and aided the family of a relative, Akhmed Gadzhinskii, who was a famous representative of the Musavat Party and who was in prison at the time. Alekperov was arrested a third and final time in 1937. According to the allegations, Alekperov was an active member of an anti-revolutionary organization. He died in a solitary confinement camp in 1939 (Asadli 2008: 11).

Although a group of ethnographers already had been in existence in Azerbaijan since the 1920s, ethnographic science was not fully established in the 1930s. In 1932, within the framework of the Azerbaijani State Scientific Research Institute, the Azerbaijani Office of the Trans-Caucasus Branch of the USSR Academy of Sciences was established, with the Historical Ethnography Department continuing to operate as part of this institution. However, in 1935 the Office was renamed the Azerbaijani Branch of the USSR Academy of Sciences and new research institutes were established, including the Institute of Ethnography and Archaeology. In 1945, the Branch was renamed the Academy of Sciences of Azerbaijan, and ethnographic studies were conducted by the Institute of History's Archaeology and Ethnography Department. The Ethnography and Archaeology Department later was transformed into an independent Institute of Ethnography and Archaeology.

While the institutional beginnings of Azerbaijani ethnography date back to the mid-1920s, systematic ethnographic studies only started in the second half of the twentieth century. Azerbaijani scholars typically did not

[1] *Qulybani*, *ərdov*, and *halanasi* are characters from Azerbaijani mythology.

choose research topics outside the frames established by the Institute of Ethnography of the USSR Academy of Sciences. In the early 1950s, the scope of ethnographic research in Azerbaijan was thematically quite rich, with special focus on the study of traditional culture and economy. It should also be noted that studies of traditional economy were limited to the period extending from the nineteenth to the beginning of the twentieth century. It is noteworthy that this chronological frame was typical of all Soviet ethnography. It is no coincidence that Valerii Tishkov stated that 'the beginning of professional Russian ethnography was so impressive that even the very historical period, which covered a large-scale collection of materials and study of ethnical cultures, more precisely, the end of the nineteenth century to the beginning of the twentieth century, was represented as a period in which traditional culture norms existed' (Tishkov 2003: 24). According to Tishkov, throughout the twentieth century and into the present, many dissertations and publications focused on this period (Tishkov 2003). This tendency was also exemplified in Azerbaijani ethnography, and it continues to dominate the field even today.

The short history of ethnographic research in Azerbaijan has been the subject of many publications (Aliyev 2007; Aliyeva 2001; Bunyadov 1967; Guliev 1961; Dzhafarzade 1946, 1948). These publications summarized existing scholarship on traditional economic activity and material culture. But given the fact that practically all such research was descriptive, those scholars studying the history of Azerbaijani ethnography found it difficult to break with the descriptive method. Moreover, the majority of these reviews of the history of Azerbaijani ethnography mention but few pieces of scholarship dealing with contemporary issues. This is somewhat surprising, as Soviet-era anthropology was not a common object of study until the mid-1970s.

Archaeological and anthropological research conducted in Azerbaijan was first analyzed by Isaak Dzhafarzade in his articles 'Ethnographic work in the Azerbaijani Soviet Republic from 1940-47' and 'Ethnographic work in the Azerbaijani Soviet Republic' (Dzhafarzade 1946, 1948). In his articles, Dzhafarzade noted the fact that ethnographic activity in Azerbaijan in the 1940s was centred in the Institute of History, at the Museum of History of Azerbaijan and at the Academy of Sciences' research centre based in the city of Kirovabad (now Ganja) (Dzhafarzade 1946, 1948). Hasan Guliev's 1961 article, 'The Ethnography of Azerbaijan over 40 years' published in the journal *Soviet Ethnography*, should also be mentioned. In the 1960s and 1970s, Azerbaijani ethnographers — in particular, Emil' Kerimov — published several works on the history of ethnographic research in Azerbaijan during the period of the Russian Empire (Kerimov 1985). These studies

analyzed voluminous references to the Caucasus region in Russian publications from the turn of the nineteenth and twentieth centuries and also included ethnographic information on Azerbaijan. Teimur Bunyadov, who repeatedly wrote about Azerbaijani ethnography, noted in his article 'Fifty Years of Azerbaijani Ethnography' (Bunyadov 1967) that although a certain revival could be observed in post-World War II Azerbaijani ethnography, no substantial achievements had been made. In his opinion, the state was paying insufficient attention to ethnographic research.

From the 1920s to the 1960s, the scope and content of ethnographic studies in Azerbaijan were primarily determined by the character of Soviet ethnic (national) policy. Thus, the birth and subsequent development of Azerbaijani ethnography in 1920 was related, to a significant extent, to the Soviet policy of 'nativization' (*korenizatsiia*), which involved more than just the occupation of senior Party and governmental posts by 'natives'. Nativization also strove for complete equality between non-Russian languages and the Russian language and for reconciliation between the constituent nationalities and Soviet power. By supporting the development of various national identities, nativization aimed at legitimating the revolution, which had originated in cities, in primarily agrarian, multi-ethnic areas (Liber 1991; Swietochowski 1955). In a speech given at the twelfth Party Congress, Stalin said

> The first thing to be done is that extensive measures should be undertaken to make Soviet power in the republics understandable, native, to make the Soviet power either a Russian or international one. For this to happen, either schools or the entirety of institutes and agencies must be nationalized step by step, use the vernacular language, which is clear to the masses, and operate in conditions that correspond to the living standards of this nation. Only in such an event will we have the opportunity to transform Soviet power from being Russian to being international, which will be native, clear and understandable for the working masses of the entire republics, especially those that lag behind in terms of economy and culture (Stalin 1952: 263).

It is no coincidence that the termination of nativization policy and the tightening of ethnic policy in the 1930s led to a sharp decrease in the number of ethnographic studies. Soviet Science was in a period of 'a great breakthrough' when there emerged instructive, condemnatory articles that incorporated Marxist ideology into scholarly research (Bykovskii 1931; Matorin 1929, 1931). This policy continued until the middle of the 1950s and found its reflection in a related though quantitatively insignificant and qualitatively poor literature.

In the 1930s, efforts were made to declare anthropology 'a bourgeois surrogate of social science' (Solovei 1990) and to reduce it to an auxiliary historical subject. This narrowing of ethnography negatively affected the development of the discipline, and this is why ethnography was reborn as an independent discipline only in the 1950s (Solovei 1990). The gradual changes to the public, political, and scientific climate, which had been occurring since the second half of 1950s, provided an opportunity to take a more objective look at the development of ethnography. In the second half of the 1950s, following the Twentieth Congress of the Communist Party of the Soviet Union (CPSU), party control of literature and science in Azerbaijan and throughout the Soviet Union weakened; in particular, historical interpretations of the nineteenth century changed. A boom of ethnographic studies dates from this period.

The weakening of strict restraints on social science research in the USSR was reflected in the republics, including Azerbaijan. The search for ethnic roots and aspirations to make the histories of the titular ethnic groups more ancient and heroic, which were observed in historical studies at the time, were a kind of latent protest against Communist ideology, especially in the context of Soviet ethnic policy. The question of whether or not Azerbaijan voluntary joined Russia or was conquered by it was debated in the historical sciences for the first time. This question became particularly sharp after 1962, when celebrations were held to commemorate the 150th anniversary of Azerbaijan's unification with Russia. Azerbaijani historians, including Ziya Buniiatov and Makhmud Ismailov, openly disputed the idea that Azerbaijan had voluntary joined Russia, and they were punished for their views.

Azerbaijani scholars began to be concerned with ethnogenesis in the mid-1970s and early 1980s. The Azerbaijani Academy of Sciences' Institute of History held a series of debates on the issue. A workshop entitled 'The Ethnogenesis of the Azerbaijani Nation', conducted by the Institute of History in 1983, generated a significant public response. At the workshop, a number of historians openly advocated a theory that until this point had been only whispered in the lobbies of scientific institutes and universities. These historians contended that Azerbaijan was ethnically Turkic. Despite being based on relatively weak grounds, this theory was nevertheless widespread among Azerbaijani scientists of the day. In the political context of the time, the appearance of such theories not only among Azerbaijanis but also among other nations of the USSR related, to a significant extent, to a search for ethnical-historical and cultural identity that emerged in the second half of the twentieth century. On this basis, Viktor Shnirel'man remarked, 'there emerged, unexpectedly to many, a phenomenon, which could be called an

alternative understanding of ancient history. The matter at issue was ethnocentric versions of the far past, which were designed to glorify ancestors as glorious heroes, who made an invaluable contribution to the formation of human culture and civilization, and ennobled all the rest of the peoples. It is quite appropriate to interpret those versions as modern myths, with all the relevant characteristics' (Shnirel'man 2000: 12). Such an outbreak of ethnonationalism could be interpreted differently; however, this represented, to an extent, a reaction to the Soviet reality. By indirectly opposing Soviet Russification, which was presented by Soviet officials as a process of forming a 'new historical community — the Soviet nation', and by appealing to history, intellectual elites in national republics attempted to substantiate their nations' respective claims to indigeneity, uniqueness, and civility in comparison both with neighbours and the 'elder brother' of the Russian ethnos.

It is not necessary to extensively discuss the fact that, contrary to its stated aims, Bolshevik ethnic policy was strictly assimilationist. It is no coincidence that the number of ethnic communities comprising the USSR decreased each year in official documents. In a report given by Stalin to the eighth All-Union Congress of the Soviets, the total number of nations, nationalities, and national groups comprising the Soviet Union was put at 60 (Guboglo 1989). The CPSU Program adopted in 1961 stated that 'the extensive development of Communism means a new stage in the development of national relations in the USSR characterized by a further rapprochement of nations and achievement of their full unity [...] Borders between Soviet republics within the USSR lose more and more of their past significance' (Program of CPSU 1962). The Program also commented on the status of the Russian language in the USSR: 'The Russian language actually has become a language of contact and co-operation among the entirety of the nations of the USSR' (ibid.). Moreover, at this time the possibility of dissolving the federation of union republics was being actively explored, which was reflected in commentary on the CPSU Program published in the journal *Sovetskoe gosudarstvo i pravo* (Soviet State and Law). One author wrote:

> Currently, the issue of interrelations between nations in the USSR has only a purely communist aim — the achievement of a comprehensive unity of Soviet nations with the final goal of their full merging [...] In comparison with the past when the degree of integration with the federation, the character of national statehood, and the legal content of national-state borders mattered as guarantors of national freedom, now all of these, in fact, do not matter [...] Right now, we might confidently say that in this respect the national statehood and federation *in toto* have fulfilled their historical mission (Semenov 1961: 17).

So, the courage acquired by Azerbaijani historians and ethnographers in this period was supported by the softening of the regime in the late 1950s and early 1960s and the then-current policy of the Azerbaijani leadership, which allowed for a greater independence in matters related to language, culture, and science. In particular, in those years strong efforts were made to give the Azerbaijani language the status of a state language. In August 1956, the Bureau of the Central Committee of Azerbaijan's Communist Party made a decision that envisioned the teaching of the Azerbaijani language in Azerbaijan's Russian, Armenian, and Georgian schools starting in the third grade (Gasanly 2008: 124). This resolution absolutely contradicted an earlier directive by the chairman of the USSR Council of Ministers, N. Bulganin, who stipulated that schoolchildren from those primary, seven-year, and secondary schools in which teaching was not in the native language, were freed from learning it (ibid.: 118). The Azerbaijani leadership's firmness on this issue resulted in a constitutional amendment being passed by the Supreme Council of the Azerbaijan Soviet Republic in August 1956 whereby Azerbaijani was recognized as the republic's official language. This move upset officials in Moscow and ultimately led to the resignation of the Azerbaijani leadership, which was accused of nationalism (ibid.: 157, 523).

In the 1960s, one priority for Soviet ethnography was the study of 'Soviet modern living standards', the 'formation and subsequent introduction of new civic rites and customs', and 'modern ethnical and cultural-domestic processes' (Bromlei 1973). To a large extent, this development was brought about by the aforementioned tendencies of Soviet ethnic policy, which found their reflection in the official documents of the CPSU. During this exact period, Azerbaijani ethnographers began to focus on the ethnic aspects of the Azerbaijani peculiarity of the Soviet reality. Works began to appear that were devoted to the study of modern living standards and the culture of the working class and *kolkhoz* workers of Azerbaijan (Abbasov 1980; Chyragzade 1966; Guliev 1953; Kulieva 1979; Salimov 1983; Trofimova 1954; Umudov 1966). These works, which were conducted in line with typical clichés characteristic of this genre of study, contained descriptions of family, public, and working life of urban and *kolkhoz* workers. In accordance with the ideological clichés of Soviet propaganda, these studies very poorly corresponded to the actual situation in their respective fields of study, asserting a permanent improvement of the living and working standards of the Soviet people and the overcoming of so-called survivals of the past. The latter, as a rule, referred to separate elements of traditional culture. It is noteworthy that many Azerbaijani ethnographers thought of ethnography as a science designed to propagate the culture of the Azerbaijani nation and to help to increase its material wealth and culture. One of first to voice this

idea, Hasan Guliev, wrote, 'The process of formation and affirmation of new all-Soviet customs, rites, and holidays occurs in the domestic life of workers of our republic and also in the life of all Soviet people' (Guliev 1985). Many of these studies touched on matters such as public and political activity of workers (public life) and labour conditions (work life). Apart from failing to capture the real situation of the Azerbaijani population, these studies actually did not reflect ethnical peculiarities. This in turn led to the erosion of the subject of ethnography. It is no coincidence that it was relatively difficult in those years to determine the boundaries between historical, ethnographical, and sociological studies devoted to the vital activities of those working in different fields of production. On top of this, practically all such studies, like the overwhelming majority of Azerbaijani ethnographic publications, were purely empirical and descriptive, making no generalizations.

Irrespective of such shortcomings, these studies contained a lot of valuable information. For example, Arif Abbasov's research was one of the earliest attempts by an Azerbaijani scholar to study contemporary public problems through the prism of ethnic peculiarities (Abbasov 1980, 1987; Abbasov and Salimzade 1987). Under Abbasov's guidance, ethnographic and subsequently ethno-social studies of different aspects of the household life and culture of the urban population strongly developed. In his works, Abbasov used an approach whereby adaptation of 'the process of the new international elements' adaptation to the system of national cultures (but not substitution of the latter by the former) starts with the appearance of national cultures' adaptation to new social and historical conditions and then is continued by the appearance of a real international basis to make the cultures closer to one another in essential parameters, but not quite denationalization' (Abbasov 1987: 14). If we take no notice of the term 'international' here, which was typical for the period, we have to admit that this approach is quite interesting and reflects, to a considerable extent, the social-cultural processes among the population of Azerbaijan.

With this approach in mind, Abbasov tracked how urban culture emerged in a small town as a result of the interaction between 'international examples of a modern urban way of living and specific ethnical examples of the life of residents of Azerbaijan's old towns' (Abbasov 1987: 21). The author chose new towns (Ali-Bayramli, Sumgait, Mingechavir, and Dashkesan) as the subject of his study because they had received official 'town' status at around the same time in the 1940s and 1950s. At the same time, these towns strongly differed in terms of 'either their size and specialization or the history of the appearance and characteristics of the social environment in which they had appeared' (ibid.: 42). We should add that the establishment of such industrial centres in Soviet republics in which a significant

Russian population had settled, was, to a great extent, part of the Soviet government's policy of assimilation. Fully in line with ideological directives, in his works Abbasov nonetheless tried to track the complex interactions between ethnic groups in these towns. For instance, he stated that the formation of the way of life and culture of the town of Mingechavir was characterized by 'active interaction between the Azerbaijani traditional rural ways of living and distinctive types of living' (ibid.: 46). In this context, it was evident for an attentive reader that there existed a conflictual situation, which manifested itself in the concrete actions of people and inter-personal relations.

One of the typical peculiarities of Azerbaijani ethnographic studies of the period was the near complete absence of any comparative analysis of the ethno-cultural characteristics of the ethnic groups of a particular republic. In studies on traditional economic activity, separate elements of material culture, and rites and customs, in keeping with a strictly descriptive methodology, most authors practically took no notice of the ethnic belonging of the people that carried on these cultural traditions. Ethnographers had always been aware of the fact that the elements of traditional ethnic culture under study varied depending on the concrete region of the country in which research was conducted. In studies of the culture and domestic life of different social groups of the population, which had become widespread since the 1960s, one compulsory postulate was the formula of a further internationalization of the domestic life and culture of the Soviet people. To this end, the ethnic diversity of towns under study was demonstrated only in the context of this postulate. Apparently, neither ethnic peculiarities in adaptation to modern conditions, nor the real relations between representatives of different ethnic groups, nor conflicts in inter-ethnic relations were reflected in these studies. Thus, numerous ethnicity-related issues, which emerged for either natural-historical or subjective reasons, were ignored, which increased the potential for conflict in the community.

However, it would be unfair to deny the existence of Soviet-era studies of separate ethnic groups in Azerbaijan at the time. For example, there were studies of the population of the Lenkoran-Astara zone, which was populated largely by Talysh. Such studies became intensive in the 1960s. A case in point is Alexandra Trofimova's paper 'The Talyshs'. In this article, published in the collection *Narody Kavkaza: etnograficheskie ocherki* (Peoples of the Caucasus: Ethnographic Essays), the author presented the results of her study of domestic and family life and the economic activities of the Talysh and described the entire region's ethnographic features (Trofimova 1962). Atiga Izmailova's (1962, 1964) studies of the Lenkoran-Astara region and peoples (in particular, the Talysh) should also be mentioned here. In her

works, Izmailova paid great attention to changes in the region's economic life and spiritual and material culture. *The Talyshs* by Gamarshakh Cavadov (2004) was published in the 1990s. Having examined in detail the origin, economic life, material and spiritual culture, and domestic life of the Talysh and having noted characteristics common to both Talysh and Azerbaijani culture, Cavadov concluded that 'the Talysh, together with the Azeris, created a common material and spiritual culture' (Cavadov 2004: 533). However, all of these works devoted to the ethnographic study of the Lenkoran-Astara zone were based on the method of historical-ethnographic description and contained materials on local lore, history, and economy and data on dialects. Cavadov's monograph, which was no exception, was written on the basis of traditional, descriptive studies of ethno-cultural processes and change.

Along with ethnographic studies of the Talysh, Soviet-era ethnographers paid attention to other ethnic groups residing in Azerbaijan. In different years, ethnographic studies devoted to the Kurds (Aristova 1962; Chursin 1925), the Lezghins (Efendiev 1987; Ikhilov 1967), the ethnic groups of the Sheki-Zagatala region (Ibragimov 1990; Dzhangidze 1978; Nikol'skaia 1955), and the Shahdag group (Desheriev 1959; Guliev 1962; Piriev 1994) were conducted. However, as has already been noted, these works primarily relied on data collected in the nineteenth and early twentieth centuries.

Ethno-sociological studies in Azerbaijan took off in the early 1980s. This coincided with a more general upsurge in ethno-sociological studies throughout the USSR. In comparison to the studies undertaken in Uzbekistan, Georgia, and Estonia, there were no such large-scale ethno-sociological studies conducted by the central Institute of Ethnography. Nevertheless, a pilot study for such research in Azerbaijan was conducted in the early 1980s. Employees of the Department of Ethno-Sociological studies, the Institute of Philosophy and Law of the National Academy of Sciences of Azerbaijan, and the Institute of Ethnography of the USSR Academy of Sciences, and the Ethnography Chair of the University of Moscow took part in preparing a questionnaire entitled 'Peculiarities of ethno-social and ethno-cultural processes in Azerbaijan'.[2] Although due to complex socio-political processes in the late 1980s no full-scale ethno-sociological study was conducted in Azerbaijan, the results of the pilot study were nevertheless later used by employees of the Department of Ethno-Sociological Studies in subsequent research (Balaev 1988; Fatullaev 1992; Mamedov 1988; Sailov 1989; Shukurova 1991; Tagieva 1988).

[2] The Institute of Archaeology and Ethnography was established later.

Azerbaijan's urban population was the focus of early ethno-sociological studies of the country. It is worth noting that from the mid-1970s onward urban studies were rather popular amongst Soviet ethnographers (including Azerbaijani ethnographers). Like in other Soviet republics, the subject of ethnographic studies in Azerbaijan covered, as previously noted, the domestic life and culture of separate groups in the urban population, and processes of interaction between different ethnic groups in multi-ethnic cities and towns (Abbasov 1980, 1987; Abbasov and Salimzade 1987). This interest in urban populations was primarily brought about by urbanization, which was rapidly developing at the time, and increased migration within and between Soviet republics. These processes made visible changes in the traditional cultures of ethnic groups and changes in everyday interpersonal relations. On the basis of either traditional ethnographic or ethnic-sociological materials, in their works, Aydin Salimzade, Tevekkul Salimov, Iagut Shukurova, Mina Tagieva, Bakhtiiar Sailov, El'mir Fatullaev, and a number of other scholars revealed processes characteristic of the modern ethnic culture of the Azeris. On the example of Azerbaijan's historical and modern towns, these researchers studied how social and cultural peculiarities influenced inter-ethnic contacts; how migration was influencing the ethno-cultural composition of emerging towns; and the ethnic peculiarities of different professional groups comprising the urban population. It should be noted that unlike studies of the modern domestic life and culture of Azerbaijan conducted in the 1970s and 1980s, these works tended toward an analytical methodology based on data from public opinion polls.

Soviet-era ideological dogmas affected these studies. Like the overwhelming majority of social studies in the USSR, these studies presupposed the internationalization of all human activities, the gradual loss of ethnicity, and the formation of a new Soviet community that had nothing in common with reality. These publications relied on questionable statistical data. For example, USSR-wide census data on the ethnic composition of the population always raised many questions. Indeed, there seem to have been large discrepancies between official statistics on ethnic groups and actual numbers. Statistics on the development of culture and separate fields of production also deserved no credit. Nevertheless, in these studies, ethnographers and sociologists gathered data on the ethnic differences of vital activities of the Azerbaijan urban population that partially compensated for these drawbacks caused by ideology.

Research on the urban population demonstrated the central role of ethnicity in spiritual culture, economic behaviour, interpersonal relations, and general behavioural norms. For example, one work was based on data from an ethno-sociological study of the lifestyle of Azerbaijan's urban

intelligentsia and made conclusions about the ambivalent role of the intelligentsia in the formation of the modern way of life (Shukurova 1991). Shukurova stated that, on the one hand, the intelligentsia was the carrier of the community's most progressive features, but, on the other hand, in modern conditions, the intelligentsia was the most active instigator of 'conservative', nationalistic orientations. In principle having disengaged himself from Soviet-era ideological clichés, the author concluded that the socio-cultural orientations of the intelligentsia were of dual nature.

It must be admitted that in the 1980s Azerbaijani scholars could not demonstrate the real socio-political situation of the intelligentsia. It is no secret that national consciousness was undergoing a revitalization among this group at the time. The strict ideological limitations of scientific research in the 1980s prohibited researchers from extensively analyzing the attitudes of the Azerbaijani intelligentsia toward notions such as 'democracy', 'the nation', 'national ideology', and 'national unity'.

The intelligentsia's role and place in the multiethnic structure of the population of Azerbaijan was off limits to ethnographic studies in the 1980s. But it is now evident that in precisely this context it had become possible to track the rebirth and subsequent development of ethno-national ideologies and the formation of separatist ideas, which later grew into ethnic movements. Unfortunately, the issue of ideological and political pluralism among the Azerbaijani intelligentsia remained beyond the frame of Soviet-era studies. The issue of succession as a precondition of establishing an Azerbaijani intelligentsia establishment was also not explored. To pursue this line of inquiry, authors would have needed to state the irrefutable fact that succession was tragically prevented by the Communist regime, which had undertaken repressive measures against the Azerbaijani intelligentsia more than once in the twentieth century.

Azerbaijani researchers often examined the formation of urban populations, especially in Azerbaijan's new towns. These issues were analyzed by Arif Abbasov (1987), Aydin Salimzade (1986), Tevekkul Salimov (1983), and Iusif Ibragimov (1994). These studies were based on statistical and ethno-sociological data and revealed complex processes of internal and external migration amongst different groups in towns. The authors concluded that two parallel systems of social norms — an urban one and a traditional, rural one — were emerging as a result of rural-to-urban migration. This circumstance substantiates the peculiarities of the Azerbaijani urban population's living standards, which are displayed in both public and private life. According to these authors, rural-to-urban migration caused many problems for towns and the urban population. With such migration, the established system of urban social culture was dramatically affected. A

marginal normative culture was emerging that was comprised of rural cultural elements that were unadapted to the urban milieu. After entering urban areas in which socio-cultural conditions strongly differed from what they were used to, rural migrants entered a period of adaptation. Azerbaijani ethno-sociologists noted that rural-to-urban migration contributed greatly to the formation of the country's urban population, especially in new towns such as Sumgait, Mingechavir, and Ali-Bayramli.

These works contain interesting conclusions about the nature of social life in the above-mentioned towns and, in particular, the peculiarities of systems of social control in these places. It was practically the first time that Azerbaijani ethnographers examined issues of clan organization and kinship relations. Ibragimov (1994) noted that migrants arriving from villages to Ali-Bayramli maintained traditional norms of rural behaviour. In Ali-Bayramli, migrants had to change and become acclimated to a great number of social habits with which they were not already familiar. According to Ibragimov, the traditional, rural normative system whereby clan and kin exercised control over migrants was relatively strong in this town. In particular, there was a relatively efficient public control by kindred groups, which discouraged deviation from the behavioural norms while simultaneously encouraging adaptation to new living conditions.

Having examined the ethno-cultural peculiarities of social networks in Azerbaijan's new towns, researchers identified the social significance and dominance of kinship, neighbour, and countrymen links in processes of social control. At the same time, the birth of other types of social links based respectively on friendship and professional and other sorts of relations was also observed.

Azerbaijani ethno-sociologists who had studied the populations of new towns paid special attention to people who had lived in town for less than two years and who could thus only nominally be considered urban residents. The researchers reasonably concluded that such people comprised a marginal group that was, on the one hand, already alienated from its rural roots and, on the other hand, had not yet fully acclimated to the urban way of life. For precisely this reason, the authors of these studies thought that recent migrants to urban areas were potentially a destabilizing force.

Azerbaijani researchers who conducted ethnographic and ethno-sociological studies of the urban population could not help but pay attention to the interactions between ethnic groups. With the domination of the so-called ideology of proletarian internationalism, it was apparently impossible to highlight the evident problems in inter-ethnic relations. However, the conclusions contained in these studies reflected quite objective processes and were important for the assessment of the real situation in the 1970 and

1980s. For example, after having examined inter-ethnic contact in Sumgait, Salimzade stated that the interpersonal contacts between ethnic groups influenced the ethno-cultural image of the town's residents (Salimzade 1986). According to Salimzade, many Russians that had Azerbaijani friends or neighbours borrowed certain elements of Azerbaijani spiritual culture; on Armenian national holidays, Azerbaijani dancing music was used; and representatives of the non-Azerbaijani population used local proverbs and sayings in everyday conversation.

The studies by these Azerbaijani scholars in the mid-1980s were indicative of the relatively broad expansion of the subject of ethno-sociology. These studies focused on issues like the peculiarities of ethno-social and ethno-cultural changes in separate strata of the community; the ethnic peculiarities of urban populations; ethno-cultural processes in Azerbaijani villages; the socio-linguistic norms of different ethnic groups; and processes of national identity formation. The researchers focused on the role of traditionalism in group behaviour and on processes of modernization, as well as on the role of modern and traditional elements in the culture of urban and rural residents.

Apart from conducting ethno-sociological studies among the country's urban population, in the mid-1980s Azerbaijani ethno-sociologists began to consider bi- and multi-lingualism and ethno-cultural processes in villages, national coconsciousness, and historical memory (Balaev 2005; Fatullaev 1992; Gadzhieva 1998; Mamedov 1988).

One study of contemporary ethno-cultural processes in an Azerbaijani village revealed that substantial changes brought about by urbanization and migration were affecting the village and the rural population's culture (Mamedov 1988). Elements of urban culture were penetrating into rural areas. As to the field of territorial organization, changes were induced by the concentration of agricultural production and increased regional economic specialization. These processes were accompanied by the formation of rural agglomerations around large centres of local importance with close economic, social, and cultural ties. Rural settlements were on the decline for a number of reasons: the disappearance of most temporary agricultural settlements; the 'self-liquidation' of a number of tiny villages, especially those in the country's mountainous area; the resettlement of residents from small and/or remote villages to primary *sovkhoz* and *kolkhoz* settlements; and the absorption of some rural settlements by nearby towns.

Studies of ethno-cultural processes in rural areas demonstrated that the impact of urban processes on ethnic culture varied. On the one hand, these processes contributed to the consolidation of a unified, urban Azerbaijani ethnic culture, erasing differences between the regions that comprised Azer-

baijan. On the other hand, such processes destroyed the traditional way of life, which served as a repository of pre-industrial ethnic norms (Mamedov 1988).

Aydin Balaev (2005) analyzed contemporary ethno-linguistic processes in Azerbaijan. His research drew on an extensive assortment of materials collected primarily through interviews in order to describe the principal factors in and tendencies of two centuries of linguistic change in Azerbaijan. Balaev examined how the social functions of the Azerbaijani language were changing in different fields of public life, and the influence of political, social, and demographic factors on these changes. He paid special attention to the history of Soviet-era linguistic policy in Azerbaijan. Balaev contended that language policy in Azerbaijan in the 1920s and 1930s was comprised of three distinct phases. In the first phase, which went through the 1920s, the Latin alphabet was superseded by Arabic script. During this phase, there were three primary languages in use in the schools and the press: Azeri, Russian, and Armenian. The late 1920s marked the beginning of the second phase, in which the languages of Azerbaijan's ethnic minorities became the focus of language planning efforts, and Latin orthography was adopted for these minority languages. Finally, the third phase began in the late 1930s when the Latin alphabet was supplanted by Cyrillic. During this phase, the number of languages used in education and in the press in Azerbaijan was reduced again to the three majority languages. Azerbaijani and Russian were the primary languages in schools and the press.

Balaev claimed that the social prestige of the Azerbaijani language in the Soviet era was low. By the time of the dissolution of the USSR, the prestige of the Azerbaijani language was not in accordance with the guarantees set out in the 1977 Constitution of the Azerbaijan Soviet Socialist Republic. According to Balaev, this was a matter of great concern. By the late 1980s, editorials were appearing in Azerbaijani newspapers about the state of the Azerbaijani language. These discussions eventually coalesced into a powerful movement calling for support for and the development of the national language. Informal organizations, which were first appearing in the country at this time, began to push for enforcement of the 1977 constitution's provisions for the Azerbaijani language. According to Balaev, language planning was politicized due to the unequal position of Azerbaijan's various ethnic groups and, hence, the differing statuses and functions of their respective languages in the Soviet socio-political system. As one of the most important elements of ethnic identification, language policy was a suitable anchor for inculcating interethnic solidarity. According to Balaev, in the late 1980s and early 1990s, these factors helped the Azeri community when

national language revival became the key point of ethnic mobilization and a forerunner of political demands.

The Azerbaijani scientific community exhibited a great interest in ethno-sociological studies of national consciousness and historical memory that were conducted on the eve of the USSR's demise (Fatullaev 1992; Gadzhieva 1998). The fact that these studies were conducted at this particular moment was not accidental, as it was at this time that the Azerbaijani community, agitated by the escalating Karabakh conflict and the struggle for independence, began to actively seek out its own national identity. Who are we, where are we from, what is our place in this world? Azerbaijani scholars, journalists, and ordinary citizens attempted to answer these questions. In the late 1980s and early 1990s, the assumptions, hypotheses, and guesses that people arrived at in answering these questions were not always scientific. Regretfully, among the numerous articles devoted to the problem of national consciousness, there was an impermissibly small number of true scientific research works. Among the latter, an article entitled 'Historical memory as an element of national self-consciousness' by Suada Gadzhieva should be mentioned. Gadzhieva approached historical memory as a peculiar substratum of national consciousness (Gadzhieva 1998: 12), inquiring into the causes of human behaviour. Setting out from entrenched Marxist-Leninist theoretical presuppositions, many of us concluded that human actions, including ethnic and political ones, were economically determined. This theoretical commitment, however, made it impossible to explain ethnic difference in certain conditions and the diverse temperaments that determined the modes and results of those actions. For precisely this reason, Gadzhieva contended that the everyday life and practical activity of a people are no less socially constructed than their national stereotypes, traditions, customs, and myths. Taken together, this set of elements constitutes the distinctiveness of any individual ethnic group. According to Gadzhieva, historical memory functions as the principal guardian of all these peculiarities. She concluded that 'historical memory constitutes the deepest "ancient" layer of ethnic self-consciousness' (Gadzhieva 1998: 12), performing important social functions such as maintaining intergenerational links in socialization, education, and the formation of national identity. Gadzhieva studied the role of ethnic identification in individual conceptions of self-respect and national identification, and the function of ritual as an instrument of identification. She paid particular attention to centuries-old rituals like rivalry and self-restraint as well as to perceptions of group distinctions.

Gadzhieva's analysis of the concept of ethnic consciousness deserves attention. It is hard to disagree with the author that 'common ethnic traits belong to those aggregates of people who regard self-consciousness as the

compulsory component, without which common traits simply do not exist' (Gadzhieva 1998: 10). She stressed that national consciousness is informed by historical knowledge, which substantiates the primary role of historical memory in this process. She concluded that 'national self-consciousness is formed on the basis of reflection (in human consciousness) of objective phenomena such as the historical past of the nation, settlement area, and the system of socioeconomic, political, and cultural factors' (ibid.: 15). Furthermore, sentiments connected to national historical memory could be put to positive or negative ends, and Gadzhieva cites the history of the Armenian people as an example of the latter. At the same time, Gadzhieva analyzes the causes of 'the full loss of roots and a feeling of nativization' amongst the Azeris (ibid.: 16). Gadzhieva's contention that this was a 'full loss' is undoubtedly disputable, given that a full loss brought on by a wholly successful assimilation policy would have threatened the very existence of the Azerbaijani ethnos.

Fatullaev's paper entitled 'Social-cultural factors in the development of national consciousness of young workers of Azerbaijan' (1992) merits special attention as a key ethno-sociological study of the issue of national identity. Fatullaev analyzed 'stable and situational factors in the formation and change of national consciousness' (ibid.: 8-9). According to Fatullaev, the socio-political context in which youth are socialized influences the type of national consciousness that is formed. National socialization continues to be centrally influenced by the degree of traditionality of the social environment and daily communication. Basing his conclusions on a large collection of data, Fatullaev noted that entrenched inter-ethnic tensions deeply impact national identification and, as a result, lead to the formation of a specific type of national consciousness that differs from the one established in comparatively more stable national contexts (ibid.: 17). For example, after conducting a survey in the mono-ethnic town Ali-Bayramli, Fatullayev discovered that there were more favourable conditions for the formation of a stable type of consciousness there than in Baku with its politicized environment and its multi-ethnic population (ibid.: 19).

The late 1980s was marked by increased migration in the majority of USSR republics, which eventually contributed to the downfall of the USSR. The restoration of Azerbaijani independence, which had long been the hope of several generations of Azeris, did not ultimately live up to the widespread expectations of widespread and rapid prosperity. The deep economic and political crisis in the former Soviet republics undoubtedly affected the general state of science and, in particular, anthropology. Research initiated in Soviet times was halted. Thus, the early 1990s marked the end of both Soviet Azerbaijan and Soviet-era ethnography of Azerbaijan. Soviet ethnography

had been largely institutionalized in Azerbaijan and had matured when new research directions emerged in the early 1990s. However, Soviet-era research priorities were largely set by state administrators and determined by concerns of ideological expediency, and thus Soviet-era approaches were poorly suited for studying the socio-political and ethno-cultural processes transpiring in Azerbaijan in the 1990s. Nevertheless, these Soviet-era studies produced a wealth of valuable information that contemporary researchers can utilize to objectively study the history of that period and the psychological stereotypes of Soviet scholars, who worked under the unfavourable conditions of a totalitarian regime.

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Chapter 11
The Reception of Marr and Marrism in the Soviet Georgian Academy

Kevin Tuite

On the last day of October 1985, a month and a half after I arrived for nine months of doctoral research in what was then the Soviet republic of Georgia, I attended a daylong conference at Tbilisi State University in honour of the 120th anniversary of the Soviet linguist Nikolai Marr. One after another, legendary figures from Caucasian studies presented their personal recollections of Marr and spoke about his contributions to Georgian and Caucasian linguistics, archaeology, folklore, and literary studies. The first speaker was the venerable grammarian Akaki Shanidze, amazingly spry and energetic for a man who was already 30 years old by the time that the Soviet Union was born. He was to be followed by the linguist Arnold Chikobava, who was gravely ill (and was to die only six days later). The ailing Chikobava was replaced by a scholar only two years his junior, the Ossetian philologist and folklorist Vasilii Abaev (1900-2001). In the course of the next few hours, I also heard Shota Dzidziguri, Aleksandre Baramidze (1902-1994), Ketevan Lomtatidze, and Ioseb Megrelidze, one of Marr's last students.[1]

At the time, I had heard enough about Marr to find the jubilee event in Tbilisi surprising. Was it not Marr who once asserted that all of the world's languages evolved *in situ* from four primordial elements, and whose theories were officially denounced by no less an authority than Stalin himself in a 1950 'discussion' of Soviet linguistics in the newspaper *Pravda*? In subsequent years, as I learned more about Marr and his career, the conference came to seem yet more baffling in retrospect. First of all, once branded a 'citadel of anti-Marrism' (Alpatov 1991: 58), Georgia seemed an improbable venue. Here and there in his writings and especially in the biography published just after his death, Marr cultivated an image of himself as a progressive internationalist who was repeatedly attacked by the narrowly nationalist

[1] Also participating was the politically astute academician Tamaz Gamq'relidze, who was only five years old when Marr died.

elite of his erstwhile homeland (Marr 1924; Mikhankova 1935: 25, 45-47, 69, 172-78, 224-5). And if Georgia was a citadel of anti-Marrism, then Tbilisi State University would have been its headquarters. Founded over Marr's objections in the newly-independent republic of Georgia in 1918, Tbilisi State University came to be staffed by many of his former pupils, who, 'possessed by nationalistic romanticism, renounced in their home setting the few scientific ideas they professed in Petersburg-Petrograd' (Marr 1925). By 'scientific ideas', of course, Marr was referring to his ever-evolving Japhetic theory (about which more will be said below). And perhaps the most surprising of all was the name of Arnold Chikobava on the program of Marr's 120th anniversary colloquium: the same person who had been an outspoken critic of Marr's theory in 1930s and 40s and who advised Stalin behind the scenes about its deleterious impact on Soviet linguistics (Alpatov 1991: 181-8; Chikobava 1985).

Those who seek to understand the relation between Marr and his Georgian colleagues during the Stalin years are confronted with two contrastive representations. On the one hand, there is Stalin's notorious characterization of Soviet linguistics as an 'Arakcheev regime' dominated by Marr and his disciples, invoking the name of a repressive minister from Tsarist times. Whatever prominence he might have enjoyed elsewhere in the USSR, however, Marr insisted that he had been a prophet without honour in his own homeland since the 1890s, when the Georgian intellectual elite placed him on 'the list of deniers of Georgian culture, and indeed enemies of Georgian national identity (*vragov gruzinskoi natsional'nosti*)' (Marr 1924; Mikhankova 1935: 25). A further consideration, which to my knowledge has not received sufficient attention from scholars, is the schizoglossic nature of the exchanges between Marr and his Georgian readers: Whereas Marr published almost exclusively in Russian, the reception of his work amongst Georgian readers emerged for the most part in Georgian-language publications. As a consequence, most Russian and Western commentators on Marrism have been party to only one voice in this conversation — Marr's — supplemented by the rather small proportion of Georgian responses available in Russian.[2]

As represented in their writings from the 1910s up until the 1950 denunciation of Marrism, Georgian academics did not form a unified bloc vis-à-vis their compatriot in Leningrad, either as followers or detractors. Quite the contrary: leading Georgian scholars at the time appear to have accorded Marr more or less the same treatment as they gave each other, disagreeing with him sharply on some issues, citing him with approval on others, and

[2] Cherchi and Manning 2002 is a noteworthy exception to this linguistic one-sidedness.

passing over certain of his writings and theoretical stances in silence.[3] It should also be noted that Marr's handling of his Georgian critics — when he mentioned them by name and not as a collectivity — was on the whole not significantly different.[4] In order to give a brief but representative overview of Marr's reception in Soviet Georgian academia, I will begin by laying out the dominant stances and some of the key figures in each camp, followed by a case study: an exchange of articles concerning a rather esoteric problem in Kartvelian linguistics, in which representatives of each group debated what were in fact fundamental issues concerning Georgian ethnogenesis.

The story of Niko Marr

First, a few words about the remarkable career of Nikolai Iakovlevich Marr (1864/5-1934): Born in western Georgia to an elderly Scottish father and a local Georgian woman, who (according to Marr's recollection) shared no common tongue, young Niko exhibited a precocious interest in languages and while still a student began exploring the hypothesis that the Kartvelian languages were related to the Semitic family. He was named *privat-dozent* at St. Petersburg University in 1891 and soon achieved recognition as one of the world's foremost specialists in Georgian and Armenian philology. In the years preceding the first World War, there emerged in Marr's writings a focus on hybridity, as reflected in elite vs. popular language varieties in ancient Georgia and Armenia, issues of linguistic and cultural contact and mixing, and the shifting nature of ethnic identity, especially in border regions. In works from the 1910s, Marr discussed what he believed were Kartvelian layers in the Armenian language, the mixed Armenian and Georgian heritage of the populations of medieval southwest Georgia, and the possibility that Shota Rustaveli, the author of Georgia's most-loved literary classic, the *Knight in the Leopard's Skin*, was not a twelfth-century Christian, as commonly supposed, but rather a fourteenth-century Muslim (Cherchi and Manning 2002; Dzidziguri 1985: 63; Mikhankova 1935: 172-3; Tuite 2008). During the same period, Marr revisited his earlier fascination with the deeper origins of the Kartvelian languages, and in subsequent years he progressively set aside philological work in favour of a single-minded focus on paleolinguistics, etymology, and ethnogenesis.

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[3] E.g. K'ek'elidze 1924, L. K'ik'nadze 1947 (*TbSU Šromebi* 30b) and Shanidze 1953, who all dished out the same sort of criticism to others as they did to Marr.

[4] See, for example, Marr's references to Dzhavakhov/Javaxishvili (Marr 1912: 50-1, 1920: 86-7, 228, etc.), Shanidze (Marr 1927: 324), Melikset-Beg (Marr 1925, 1926), Ingoroq'va (Marr 1928: 15).

In the interest of brevity, I summarize the key phases of Marr's linguistics theories (known under the name of 'Japhetidology', based on the name he used at first to designate the Georgians and their nearest ethnolinguistic kin, and later expanded to an all-encompassing theory of linguistic and ethnic origins) in the following diagram.[5] The progress of Marr's professional career is shown in a parallel column, demonstrating the extent to which the growing distance between Japhetidology and the linguistic and ethnological theories then dominant in the West coincided with Marr's rise to academic prominence in the USSR. In his earliest work, Marr employed etymological methods not radically beyond the bounds of the approaches of his West European colleagues. In the second and third periods, the search for evidence of hybrid origins comes to dominate Marr's linguistics. 'Japhetic' mutates from its initial manifestation as a language grouping modelled after the better known Indo-European and Finno-Ugric families to an ethnic and linguistic 'layer' (*sloi*) in the complexly structured soil from which languages emerged. Marr's attention was drawn in particular to Eurasian languages of unknown origin (such as Basque or Etruscan) and hypothetical speech varieties believed to have left traces in attested languages (such as the 'Pelasgian' lexical component in Greek). Belonging to neither the Indo-European nor Hamito-Semitic families, these languages were evidence of a 'third ethnic element' — Japhetic, of course — in the ancient history of the Mediterranean area. In its final phase, Japhetic was redefined as a 'system', or evolutionary stage, through which all languages pass as they evolve from primordial sound clusters (Marr proposed four proto-syllables as ancestral to all human speech varieties) to their present-day forms. Marr and his followers also emphasized parallels between their stadialist theory of language and the concepts of Soviet Marxism. One key assertion of late Japhetidology was that language structures depend on the cognitive predispositions of speech communities at different levels of socio-economic development, or, translated into Marxian terminology, that language was a component of the ideological superstructure that emerges from the economic base. It was this claim in particular that Stalin declared heretical in his 1950 article.

[5] The labels and chronology are based on Javaxishvili 1937 (49-77), Chikobava 1965 (327-8), Mikhankova 1935, and Cherchi and Manning 2002. Marr's Japhetidology is also discussed by Thomas 1957, Slezkine 1996, Tuite 2008, and — in relation to the linguistic theories of earlier thinkers — by Sériot 2005.

Marr's Japhetidology	Marr's academic career
Kartvelological period (1908-1916): Japhetic (= Kartvelian and 'pre-Aryan' language of Armenia) as branch of 'Noetic family' with Hamitic and Semitic	Head of Oriental Studies Dept., St. Petersburg University (1908), dean (1911); member of Academy of Sciences (1912)
Caucasological period (1916-1920): Japhetic 'layers' and the mixed heritages of the North and South Caucasian languages	First dean of newly organized Faculty of Social Sciences at Petrograd University (1918); Academy of the History of Material Culture founded (1919)
Mediterraneanist period (1920-1923): Japhetic as 'third ethnic element' in the creation of Mediterranean cultures: Etruscan, Basque, Pelasgian, etc.	Japhetic Institute founded (1921)
The New Theory of Language (1923-1934, continued by Marr's pupils to 1950): Japhetic as universal evolutionary stage or 'system'; radical autochthonism: *in situ* evolution of languages and cultures	Director of Leningrad Public Library (1924); joined KPSS, named Vice-President of USSR Academy of Sciences (1930); received Order of Lenin (1933)

The reception of Marr and Marrism in Georgia

In the years preceding the official denunciation of Marr's 'New Theory of Language' (*Novoe uchenie o[b] iazyke*), Georgian scholars in the fields intersecting with Marr's interests — linguistics, archaeology, ethnography, philology, history, and folklore — could be grouped into two camps, although some individuals were less easy to classify or shifted their positions. I will label them the 'Ibero-Caucasian' and 'Japhetic' cohorts, employing two competing terms for the larger linguistic grouping to which Georgian and the other Kartvelian languages were to be assigned. The Japhetic label is applied to those who represented themselves as adherents to Marr's later teachings, at least up to the 1950 'discussion'. This group comprised the relatively small group of Georgians who went to Leningrad to study with Marr in the 1920s and 1930s.[6] While several members of the Japhetic cohort published explicitly Marrist works in the years preceding 1950, all went on to have productive careers in linguistics and folklore studies in the post-

[6] Aleksandre Ghlont'i, who stubbornly professed allegiance to Marr to the very end of his days, delivered a revealing portrait of the Georgian Japheticists in Ghlont'i (1998).

Stalin period (most notably Mixeil Chikovani, who became Georgia's most influential folklorist).

The other, considerably more numerous group is called 'Ibero-Caucasian', this being the term for the theory of Caucasian ethnic and linguistic unity proposed by the historian Ivane Javaxishvili (1937), and subsequently revised and expanded by Chikobava and his colleagues (Chikobava 1965, 1979; Tuite 2008). This cohort is comprised of those scholars who shared many of the proposals about Georgian ethnogenesis first formulated by Marr up through the 1920s: the common origin of the three Caucasian language families and the complex history of contact and borrowing out of which Georgian and other Caucasian cultures emerged. The 'Ibero-Caucasian' group can be further divided according to the manner of engagement with Marr's ideas. Led by disciples of Marr from the years preceding Georgian independence and the founding of Tbilisi State University, the majority regularly cited Marr's pre-1920 writings, rejecting or criticizing some of Marr's proposals while acknowledging others. The later teachings, however, were mentioned sparingly and often simply passed over in silence. A minority, led by Chikobava, responded explicitly to later versions of Japhetidology, including the New Theory, which they submitted to critical, even harsh assessment (e.g. Chikobava 1945). It was this latter cohort that most actively participated in the 1950 *Pravda* discussion and the attacks on Marrism in the following two to three years.

<table>
<tr><th colspan="2">I. 'Ibero-Caucasian' camp</th><th>II. 'Japhetic' camp</th></tr>
<tr><td colspan="2">*Accepted much of Marr's pre-1920 work
*Believed in the genetic unity of Caucasian languages, also some ancient Near-Eastern languages (Sumerian, Urartean)
*Adopted historical approach, but with greater methodological rigour; acknowledged role of contact, borrowing, chronologically-distinct cultural-linguistic layers
*Accepted some Marrist etymologies, especially those linking the Caucasus to Near-Eastern and Classical civilizations</td><td rowspan="3">*Students of Marr in Leningrad from the 1920s-1930s
*Accepted New Theory of Language
*Principally in fields of linguistics, folklore, literature</td></tr>
<tr><th>Moderate</th><th>Critical</th></tr>
<tr><td>*Includes many pre-1918 students of Marr
*Collegial criticism of Marr (especially while he was alive), selective amnesia of later theories</td><td>*Explicitly critical engagement with Marr's later theories
*Active involvement in officially condoned criticism of Marr(ism) from 1950-53</td></tr>
</table>

HISTORIANS Ivane Javaxishvili (1876-1940) Simon Janashia (1900-1947) LINGUISTS Ak'ak'i Shanidze (1887-1987) Varlam Topuria (1901-1966) Giorgi Axvlediani (1887-1973) ETHNOGRAPHERS Giorgi Chit'aia (1890-1986) Vera Bardavelidze (1899-1970)	LINGUISTS Arnold Chikobava (1898-1985) Ketevan Lomtatidze (1911-2007) Tinatin Sharadzenidze (1919-1983)	LINGUISTS K'arp'ez Dondua (1891-1951) Aleksandre Ghlont'i (1912-1999) Shota Dzidziguri (1911-1995) PHILOLOGIST Ioseb Megrelidze (1909-1996) FOLKLORIST Mixeil Chikovani (1909-1983)

During the Stalinist era, therefore, Georgia hardly seems to have been the 'citadel of anti-Marrism' that Alpatov described. According to Ghlont'i (1998: 36-37), Marr lectured in Tbilisi on occasion and corresponded regularly with a student-organized 'Japhetological Circle' at Tbilisi State University.[7] On the eve of the 1950 linguistics discussion, the moderate Ibero-Caucasianists held the positions of greatest prominence in Georgian academia. Among them were several of Marr's earlier students (Shanidze and Axvlediani in linguistics, Chit'aia in ethnography) as well as one of his very last students, Mixeil Chikovani, who explicitly endorsed Marr's New Theory in his folklore studies textbook (1946: 143-46). Chikobava, who was soon to launch the opening salvo in the *Pravda* discussion, headed the Ibero-Caucasian linguistics departments at Tbilisi State University and the N. Marr Institute of Language.

Georgian ethnography under Marrism

Among the disciples of Marr who played leading roles in the Georgian academy during the brief period of independence (1918-1921) and the early

[7] It should, however, be mentioned that Ghlont'i was witness to a puzzling incident that may indicate mounting opposition to Marr in the Georgian establishment as early as 1933, or at least that Marr strongly suspected something of the kind. In the summer of 1933, Marr was invited to attend an official meeting at Georgian party headquarters, convened by K. Oragvelidze (later to become rector of Tbilisi State University, and shot in 1937) and including Chikobava, Shanidze, Janashia, Axvlediani, Topuria, and D. K'arbelashvili. Evidently expecting to be confronted with accusations, Marr refused to attend the meeting and left Georgia that very evening, never to return (Ghlont'i 1998: 40-42).

years of Soviet rule, Giorgi Chit'aia, like Abaev and Shanidze, had the exceptional destiny of a career that spanned the near totality of the Soviet period. Named to head the newly organized ethnography section at the Georgian National Museum in 1922, Chit'aia remained in this position until his death in 1986. Along with his wife and colleague Vera Bardavelidze, Chit'aia established the following theoretical and practical guidelines for Georgian ethnography during the Soviet period and indeed up to the present day:

(1) Scientific and empirical approach to intellectual and material culture, based on close observation in the field as well as the study of museum collections;
(2) Ethnography as a fundamentally historical social science, concerned with issues of ethnogenesis and socio-economic evolution (which during the Soviet period was expressed in conformity with Marx-Engelsian stadialism)[8];
(3) Professionalization of fieldwork (typically conducted by teams of researchers with distinct tasks and specializations) and museology;
(4) Development of a multidisciplinary approach (intellectual and material culture, language, physical anthropology, and archaeology) for the study of the specific features of Georgian culture throughout its long history and its links with other cultures of the Caucasus and the civilizations of the ancient Near East.

Attending Marr's lectures in 1911, Chit'aia was impressed by the philologist's erudition, polyglottism, and etymological 'wizardry' (*charodei etimologicheskikh razyskanii*; Chit'aia 1969, V: 419-21). In the following years, Marr came to befriend and serve as both a benefactor and teacher to Chit'aia (Chit'aia 1986). For all that, the influence of Marr's theoretical writings on Chit'aia's research was comparatively limited. In a manuscript entitled 'Niko Marr and the ethnography of Georgia' (Chit'aia 1958: 43-54), Chit'aia summed up what he took to be Marr's contributions to the discipline. He praised, first of all, Marr's ethnographic fieldwork in the western Georgian provinces of Guria, Shavsheti, Klarjeti, and Svaneti. The expedition to Shavsheti and Klarjeti was of particular significance since those historically Georgian provinces, now on the Turkish side of the border, were closed to Soviet researchers after the Revolution. From time to time Chit'aia and Bardavelidze cited with evident approval Marr's etymologically-based

[8] In one early article, Chit'aia (1926) contrasted the historical science of ethnography to the allegedly ahistorical bourgeois discipline of ethnology (based on the study of 'primitive' peoples and judged inappropriate to the study of the peoples of the Caucasus).

attempts to link various cultural phenomena in the Caucasus to the ancient civilizations of the Near East and the Classical world, although they often rejected etymologies they considered ill-founded (e.g. Bardavelidze 1941: 55, 64, 76-77, 108-118). Chit'aia also pointed to Marr's insistence that nations and ethnicities were to be understood as the products of complex historical processes, including convergence among heterogeneous social groups, and not as biologically unified 'racial' lineages.[9] On several occasions, Chit'aia referred approvingly to Marr's renunciation of his earlier hypothesis that the Georgian pre-Christian pantheon was mostly borrowed from neighbouring Iran (Chit'aia 1946: 28)[10], but only rarely did he mention Marr's post-1920 theories.[11]

Although Chit'aia was Marr's former student, the figure he held up as having the greatest impact on Georgian ethnography was another one-time disciple of Marr: Ivane Javaxishvili.[12] Javaxishvili's voluminous writings on Georgian history, the origins of Caucasian paganism, Georgian traditional law, agriculture, music, material culture, and so forth could be said to have laid out the principle research problems that kept Georgian scholars busy for decades. Furthermore, it was Javaxishvili's institutional stature, in Chit'aia's view, that sheltered Georgian ethnography from the ideological turbulence stirred up in Russia by Marxist zealots — many of them students of Marr's — in the late 1920s and early 1930s (Chit'aia 1975: 68-9; 1938b: 47; cp. Bertrand 2002: 122-135).[13]

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[9] This aspect of Marr's theory of ethnogenesis, before it mutated into the radical autochthonism and economically driven stadialism of his later writings, could be considered his most significant contribution to the social sciences.

[10] The denial of Iranian contributions to ancient Georgian culture should also be evaluated in light of the ideological context in Soviet academia during the later years of Stalinism, which was informed by a sort of anti-Aryanism (or anti-Indo-Europeanism). Examples include the shift in Chit'aia's treatment of the hypotheses that Georgians were at least partially of Aryan origin, proposed by J. Karst and T. Margwelaschwili (Chit'aia 1938a: 48ff; 1950: 89-90). In the latter paper, Chit'aia repudiated all attempts to link the Georgians to the Aryans, without mentioning the name of Margwelaschvili, who had been lured into Soviet East Berlin and shot in 1946.

[11] One exception is Chit'aia's criticism of Marr's Semitic-Japhetic and 'third ethnic element' theories (1975: 73-4).

[12] Marr appears to have had a somewhat greater impact on Caucasian archaeology during the 1930s and 1940s, judging from the monographs of L. Melikset-Beg (1938) and B. Kuftin (1949), both of which are generously laden with such Marrist etymological gems as the linking of Hebrew 'cherub' to the Svan *ɣerbet* 'God', or the attribution of the Rabelaisian name *Gargantua* to a Caucasian source (cp., Shnirelman 1995).

[13] In particular, Chit'aia denounced the 'nihilistic' attitude toward ethnography that inspired some participants at academic congresses held in Russia between 1929 and 1932 to call for the 'liquidation of ethnography and archaeology as scientific disciplines' (Chit'aia 1938b).

The remarkable case of Vaso Abaev

As I mentioned at the beginning of this chapter, Vasilii Abaev took the place of the mortally ill Chikobava at the 1985 Marr memorial conference in Tbilisi. I do not presume to know whether some sort of irony was intended by the replacement of the nemesis of Marrism by one of its foremost adherents. Perhaps a more fitting figure to whom Abaev could be compared (and contrasted) would be Ivane Javaxishvili, since Abaev is every bit as towering an authority in Ossetian scholarship as Javaxishvili is for the Georgians. Furthermore, as a researcher, Abaev was no less meticulous than Javaxishvili, even though he stuck with Marr to the bitter end. In his work from the 1930s and 1940s, Abaev managed to produce high quality, methodologically solid scholarship on Ossetian linguistics, ethnogenesis, and folklore, while continuing to display his affiliation to with the Marrist school. To take but one example, Abaev's 1938 Marr memorial volume paper on the Roman and Ossetic myths of twins (Romulus/Remus, Xsar/Xsærtæg) contains well-founded etymologies, formulated with reference to the Indo-Iranian and Indo-European language families, that would pass muster with any Western historical linguist.[14] Yet in the same paper Abaev declares his faith in Marr's New Theory, which he represents as a framework for revealing the 'strict regularity' (*strogaia zakonomernost'*) of prehistoric cultures and their stadial evolution, that is, their progression through a fixed sequence of social and economic stages (1938: 327). In practice, Abaev applied Marrist principles to his speculations about primitive thought, totemism, etc., while retaining a perfectly respectable historical-linguistic and etymological methodology. Abaev's 1948 paper on 'ideosemantics' (Abaev 1948), for example, is a tour-de-force of creating a silk purse out of the sow's ear of Marr's stadialist determinism. Abaev's approach to etymology showed an awareness of the flaws in Marr's linguistic methods, but, unlike Chikobava, it could be said that Abaev chose the path of quiet reform from within.

Linguistic Hybridity and Svan Ethnogenesis

In the early years of the twentieth century, Marr took a special interest in Svan, the most peripheral of the Kartvelian languages. In keeping with his

[14] In a 1949 chapter on Ossetian ethnogenesis, Abaev referred to the Indo-European language family as a 'system', in keeping with Marr's doctrine of the time, and acknowledged Marr's hypothesis that Indo-European might be the result of a 'long process of gradual assimilation and consolidation of numerous small, splintered prehistoric tribal and linguistic formations' (1949: 13); but on the whole, Abaev did not stray from accepted methods in his etymological and cultural-historical analyses. On Abaev's ethnological methodology, see also the recent master's thesis by Nadia Proulx (2008: 79-97).

theoretical stance of the time, Marr ascribed many of the distinctive features of Svan to borrowings from neighbouring speech communities as well as the mixing of language varieties from different sources (some of which were 'Kartlian-Mesxian' [= Georgian] and 'Tubal-Cain' [= Mingrelian-Laz] dialects, others from the Northwest Caucasian family [Abxaz and Adyghe]). Marr's hypothesis that the Svans and the Svan language were of mixed or hybrid origin was revisited by Georgian scholars until the very end of the Soviet period. Although at first glance the Svan language debate would appear to pertain to the recondite domain of Kartvelian historical morphology, the participants were in fact touching on fundamental issues of Georgian national origins: ethnic hybridity, prehistoric relations with neighbouring peoples, autochthonism, and chronology of settlement.

From Topuria in 1931 to Sharadzenidze in 1955, all participants in the Svan morphology debate shared the basic presuppositions Marr enunciated in 1911: (1) that all of the indigenous Caucasian languages were genetically related; and (2) that a diversity of speech varieties and ethnic communities, from both sides of the Caucasus, contributed to the emergence of the Svan language. The major point of contention was the socio-historical mechanism invoked to explain Svan ethno-linguistic hybridity: intensive contact between neighbouring communities, the spread of the Kartvelian language to an erstwhile Northwest Caucasian-speaking community, or the radically autochthonist explanation favoured by Marr in his later years. Throughout the debate, all participants — including Chikobava and Sharadzenidze, the two most active critics of Japhetidology in Georgia — acknowledged the importance of Marr's earlier work on Svan ethnogenesis. There is no indication throughout this exchange of articles that Marr was regarded as anything other than a respected colleague, even in the paper by Sharadzenidze published five years after the *Pravda* linguistics discussion.

The Debate Over the origins of Svan declensional morphology and Svan ethnogenesis

Marr, N. Ja. (1911)	Gde sokhranilos' svanskoe sklonenie? (Where is the Svan declension preserved?) *Izvestiia Imper. AN*	Svan as a mixed language; nearly all declensional morphology borrowed from Kartvelian and Abxaz-Adyghe dialects.
Topuria, V. (1931, 1944)	*Svanuri ena, I. zmna* (The Svan verb, 1931); brunebis sist'emisa-tvis svanurši (The case system in Svan) *Moambe* V#3 (1944)	Svan as language of mixed type (*narevi t'ipis ena*), with four declensional systems: 'Kartvelian', 'Adyghe', 'Svan', 'mixed'.

Chikobava, Arnold (1941)	Svanuri motxrobitis erti variant'i (One variant of the Svan ergative case), *TbSU Šromebi* XVIII	Adyghe-derived ergative morpheme in Svan older than 'Kartvelian' allomorph.
Janashia, Simon (1942)	Svanur-adiɣeuri enobrivi šexvedrebi. (Svan-Adyghe linguistic contacts). *ENIMK'-is moambe* XII. 249-278 [*Šromebi* III (1959): 81-116]	Cites Marr on Svan as mixed language ('when Japhetidology had not yet made language mixing into a universal principle') but ascribes Adyghe layer (*pena*) to intensive linguistic-cultural contact; Svan remains a fundamentally Kartvelian language.
Dondua, K'arp'az (1946)	Adiɣeuri t'ip'is motxrobiti brunva svanurši (Adyghe type of ergative case in Svan), *Iberiul-k'avk'asiuri enatmecniereba* I: 169-194	Response to Janašia: Adyghe-type morphemes were not borrowed from an external source, but rather evolved within Svan itself (*šekmnilia tviton svanuris c'iaɣši*), perhaps at the same time as they arose in Adyghe. Dondua's analysis characteristic of late-Marrist radical autochthonism, which favoured *in situ* evolution over borrowing or migration as an explanation for language and ethnic origins.
Chikobava, Arnold (1948)	Kartvel´skie iazyki, ikh istoricheskii sostav i drevnii lingvisticheskii oblik (The Kartvelian languages, their historical structure and ancient linguistic profile). *Iberiul-k'avk'asiuri enatmecniereba* 2.255-275	Marr's account of mixed character of Svan is undisputed (*besporno*): Svan emerged from a 'complex historical process of crossing [*skreshcheniia*] of Kartvelian and Abxaz-Adyghe dialects'. In Chikobava's view, this is to be ascribed to the earlier presence in Svaneti of an Abxaz-Adyghean speech community.
Sharadzenidze, Tinatin (1955)	Brunvata k'lasipik'aciisatvis svanurši. (On the classification of Svan declension), *Iberiul-k'avk'asiuri enatmecniereba* 7.125-135	Acknowledges the position of Marr, Janashia, and Topuria that one of the Svan declension types is of Adyghe origin but does not speculate on the historical circumstances of its emergence.

Mach'avariani, Givi (1960)	Brunebis erti t'ip'is genezisatvis svanurši (On the genesis of one type of declension in Svan), *TbSU Šromebi* 93: 93-104	Argues, on the basis of Old Georgian parallels, that the Svan declension in /m/ is of Kartvelian, not Adyghe origin; but does accept Janashia's argument that other Svan morphemes were indeed borrowed from Adyghe.
Oniani, Aleksandre (1989)	*Kartvelur enata šedarebiti gramat'ik'is sak'itxebi: saxelta morpologia.* [Issues in the comparative grammar of the Kartvelian languages: Nominal morphology]. Tbilisi: Ganatleba.	Rejection of Marr's, Chikobava's and Sharadzenidze's analyses of Svan as a mixed language with Circassian substratal features. All of the features in question derive from the Proto-Kartvelian ancestral language.

The 1950 Pravda discussion and its aftermath

On May 9, 1950, a three-page article by Arnold Chikobava appeared in *Pravda,* preceded by an editorial note that the newspaper intended to host a debate over 'the unsatisfactory state of Soviet linguistics'. In his opening salvo, Chikobava praised Marr's early philological work while subjecting his later theories to sharp and detailed criticism. I. Meshchaninov, Marr's most prominent disciple, replied a week later. Further articles, criticizing or defending Marr's ideas, were published over the next few weeks until Stalin's ex-cathedra denunciation of Marr's New Theory of Language appeared on June 20. In a paper published after his death, Chikobava (1985) described his prior meeting with Stalin; the encounter appears to have been arranged by the Georgian Central Committee first secretary K. Charkviani, possibly with Beria's assistance (Alpatov 1991: 168-190, Pollock 2006: 104-34). In Ghlont'i's view, it was only after Stalin's intervention that a genuine 'Arakcheev regime' came into being in which Marrists were the persecuted rather than the persecutors, but in fact few suffered serious consequences (certainly nothing comparable to the 1937 purges). Numerous linguists throughout the USSR added their voices to the attack upon the principles of Marrism, now characterized as 'anti-historical' as well as 'antiscientific'.[15] From 1951-53, during the peak of officially sanctioned — and, to a degree, officially required — anti-Marrism, criticism of the deceased linguist came from all quarters, often for relatively minor reasons (e.g. Shanidze 1953: 672).[16]

[15] See, for example, the introduction to Vinogradov and Serebrennikov 1952.

[16] Not everyone jumped on the bandwagon: Bardavelidze cited (with approval) ethnographic data collected by Marr on the very first page of a book published in 1953. In a 1954 issue of the *Journal of the Language, Literature and History Institute of Abxazia* largely given over to

Arnold Chikobava and certain of his colleagues (Tinatin Sharadzenidze and Ketevan Lomtatidze, among others) presented their competing methodological principles for the historical study of Caucasian languages, and by extension, Caucasian peoples. As exemplified in the Svan morphology debate, however, Chikobava retained many of Marr's assumptions concerning the role of convergence and hybridity in language evolution, even as he denounced the excesses of late Marrist stadial determinism, four-element glottogenesis, and unbridled etymologizing.

Fifteen years later, the public 'rehabilitation' of Marr was underway. Chit'aia's 1958 essay on Marr's contributions to ethnography was never published, but an article on the same topic did appear on the 100th anniversary of his birth in 1965 (Chit'aia 1958: 43 [footnote]). The Marr centenary was also the occasion for conferences in Leningrad and Tbilisi; the latter was attended by Chikobava, among others (Alpatov 1991: 212). An homage to Marr by K. Dondua — not included in the 1967 Georgian edition of his collected works — appeared in the Russian edition of 1975 (247-253). The 120th anniversary was marked by the publication of a biography on Marr as a 'scholar of Georgian culture' by Shota Dzidziguri (1985), as well as the symposium I attended on a late October afternoon in 1985.

In present-day Georgia, Marr's reputation remains largely favourable in academic circles. In other contexts, however, far less positive references to him can occasionally be found, as in a comment posted on a website in October 2006, in which Marr, followed directly by Chikobava, heads up a list of 'traitor-historians' (*predatelei-istorikov*) who were willing to 'hand out pages of Georgian history to Armenians, Abkhazians, and Ossetians'. If one were to trace the sources of these dismaying accusations against Marr and other Georgian scholars, the path would almost certainly lead back to a thousand-page biography of a tenth-century Georgian ecclesiastical writer by literary historian P'avle Ingoroq'va (1893-1990), delivered to the printers in 1951, during the height of the anti-Marr campaign, and published in 1954 (Ingoroq'va 1954). In his attempt to reconstruct the ethnic landscape of south-western Georgia and Abkhazia in the early Middle Ages, Ingoroq'va laid the groundwork for a sweeping rejection not only of Marr's work but also those assumptions regarding the origins of ethnic groups in general and of the Georgians in particular that Marr shared with Chikobava, Janashia, and Javaxishvili (Tuite 2008). Ingoroq'va's model of Georgian ethnogenesis rejected hybridity in favour of an image of national origins that emphasized continuity, homogeneity, and purity. Ingoroq'va's anti-hybridism was to

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Marrism-bashing, the Abkhazian ethnographer Shalva Inal-Ipa referred to Marr twice without criticism, even though he also cited Stalin's essay on linguistics.

have consequences that are still very much felt today, but that discussion is best left for another time and another venue.

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Chapter 12
Anthropology at its Margins: Essentialism and Nationalism in Northwest Caucasian Studies

Igor Kuznetsov

Northwest Caucasian anthropology remains marginal in three important ways: 1) In relation to the broader Caucasus region, the Northwest is often portrayed as the most Russified and therefore least authentically Caucasian part of the region (the Krasnodar region more so than Adygea); 2) Regionally-oriented anthropology as an academic discipline was institutionalized much later in the Northwest than in the rest of the Caucasus and is therefore less developed; 3) The boundaries between both anthropology and human sciences like history and sociology, on the one hand, and between professional anthropology and amateur regional studies or the speculative reasoning of nationalist politicians on ethnic issues, on the other, are uncertain. Though the total number of anthropologists and other scholars specializing in ethnic studies and folklore of the Northwest Caucasus now hovers at around one hundred; only five of the scholars from the Krasnodar region have academic degrees in anthropology (ethnology), with perhaps two or three additional anthropology PhD students. However, if we treat anthropology as a four-field discipline and include archaeology, then the total number is double. There are even more professionally trained specialists in Adygea: there are approximately 10 ethnologists and archaeologists and 25 folklorists and philologists, who could be viewed as linguistic anthropologists, with a total result of about 50 specialists out of a population of six million inhabitants residing in the Northwest Caucasus.

The relevant academic institutions in the Northwest Caucasus include:

Kuban State University: Chair of Archaeology, Ethnology, and Ancient and Medieval History (head: Nikolai Kirei; faculty: Nikolai Bondar and Igor Kuznetsov), with its Research Centre for Pontic and Caucasian Studies (Igor Kuznetsov, Vladimir Kolesov, Aleksei Kochergin, Vladimir Boitsov, Rita Kuznetsova, and Nona Shahnazarian), which publishes the journal *Bulletin: Anthropology, Minorities, Multiculturalism*.

The Museum of History and Archaeology: Division of Ethnography (head: Vladimir Kolesov); Kuban Cossack Chorus Research Centre for Traditional Culture (Nikolai Bondar, Svetlana Zhiganova, Mikhail Sementsov, Vassilii Voronin, Natal'ia Bogatyr', Irina Kuznetsova, and Svetlana Khachatrian).

Freelance Researchers: There are a small number of freelance researchers in Krasnodar (Vera Baglai and Erazik Harutiunian), Armavir (Vitalii Vinogradov), Sochi (Tamara Polovinkina), and Novorossiisk (Aleksandr Siver).

In Maykop, Adygea, there are the following institutions:

The Institute of Humanities of the Republic of Adygea: Department of Ethnography and Folk Art (head: Marat Gubzhokov; faculty: Samir Khotko, Galina Tkhagapsova, Marziet Anzarokova, Galimet Tlevtsezheva, and F.B. Koshubaeva); Department of Folklore (head: Nafiset Chuiakova; faculty: Shamsudin Khut, Sergei Zukhba, B.G. Tlekhas, S.M. Nekhai, and S.M. Bzhemukhova); Department of Nart Epos Studies (head: Asker Gadagatl'; faculty: T. Nekhai, M. Khabokhova, and S. Tkhatl').

The State University of Adygea: Department of Adygeyan Philology and Culture, Chair of Adygeyan History and Culture (head: Raisa Unarokova; faculty: Mira Unarokova, Aidemirkan Zafesov, M. Gubzhokov, Zaira Tseeva, and Ruslan Emizh); the Centre for Adygeyan Studies (head: R. Unarokova): the Centre for Caucasian Studies (head: Asker Shaov).

National Museum of the Republic of Adygea: Ethnography Department (head: Al'mir Abregov; employs several anthropologists).

The Rise and Fall of Regional Ethnography

Initial attempts were made to organize ethnographic research the Northwest Caucasus by members of the Caucasian Branch of the Imperial Russian Geographic Society such as Leontii Liul'e (author of the first Russian-Adyghe Dictionary (1846)) and by correspondents from the Office of the Caucasian Educational Network such as the son of Aleksandr D'iachkov-Tarasov (head of the Akhalkalak district), who travelled throughout the region (D'iachkov-Tarasov 1900) and Leo Rozenberg, a teacher who wrote accounts of the German colonists (1900) and Cossacks (1905). Together, the Imperial Russian Geographic Society and the Caucasian Educational Network groups published a series entitled *Collections of Materials for the Description of the Lands and Tribes of the Caucasus* (SMOMPK — *Sborniki materialov dlia opisaniia mestnostei i plemen Kavkaza*). At the same time, a number of local Cossack elites began to engage in regional studies, including ethnography. In 1879, Evgenii Dmitrievich Felitsyn (1848-1903) began

undertaking the first steps to found the Kuban oblast Museum of Ethnography and Natural History. Between 1910 and 1921, the museum grew to have seven departments, including departments of ethnography, archaeology, and anthropology (Korsakova 1997: 282; Esipenko 1997: 39-43).

In 1897, the Kuban oblast Society of Amateur Researchers (OLIKO — *Obshch'estvo liubitelei izucheniia Kubanskoi oblasti*) was organized in Ekaterinodar; OLIKO continued its activity well into the Soviet period. OLIKO amateur ethnographers collected data on Russian and Ukranian folktales and music (Bigdai 1896-8; Dikarev 1896; Kontsevich 1913, 1914) and conducted ethnographic research among Circassians (Seferbei Siiukhov), Greeks (Korolenko 1904; Sysoev 1901), Germans (Gorodetskii 1915), and Armenians (Korolenko 1904; Shcherbina 1916; Sysoev 1901). In the 1920s and 1930s, OLIKO ethnographers maintained regular professional contact with St. Petersburg- and Moscow-based ethnographers and linguists Aleksandr Miller, Anatolii Genko, Ivan Meshchaninov, and Nikolai Iakovlev.

Initially, in striving to establish their rule, the Bolsheviks were eager to meet the cultural demands of ethnic minorities. As a result of the indigenization policy (*korenizatsiia*) of the 1920s and early 1930s, several ethnic groups were granted semi-autonomous national territories: in 1922, the Circassian (Adygeyan) Autonomous Region was established (now, the Republic of Adygea); Nationality Districts were established for Shapsugs[1] (1924-45), Armenians (1926-53), and Greeks (1930-38) (Tveritinov 1992). At the same time, most of the Kuban Cossacks that supported the Whites during the Civil war were forced to emigrate. The Cossacks were subsequently repressed as part of the Bolshevik decossackification (*raskazachivanie*) campaign of the 1920s. During the collapse of the rural economy and the famine of 1932-33, about one million people from the North Caucasus died; entire Ukrainian-speaking Kuban *stanitsas* (semi-rural Cossack settlements) — for instance, Umanskaia and Poltavskaia — were exiled to northern and eastern regions of the USSR. Other minority groups like the Greeks were subjected to deportation, and minority intelligentsias ('bourgeois-nationalistic elements', in the parlance of the time) were repressed for allegedly harbouring nationalistic, separatist sentiments.

During the Civil war in Ekaterinodar (now Krasnodar), which ran from 1918 to 1921 new intellectuals from the USSR's academic centres were brought to Ekaterinodar. Local authorities hired these intellectuals to staff the Department of Philology and History at the Kuban Pedagogical Institute (which was also called the Institute for People's Education and, in the 1920-21 academic year, University). Among its ethnographers was Aleksei Ni-

[1] This term refers to Adyghe living outside Adygea.

kolaevich Gren, who headed the regional studies department. Gren graduated from the Department of Eastern Languages at St. Petersburg University in 1886 and took a job in Tiflis (Tbilisi) as a school supervisor. In Tiflis, Gren became an active contributor to the SMOMPK series, collecting over 4,000 texts from the Caucasus and publishing Svan and Chechen-Ingush folklore (Gren 1890, 1897). From 1923-25, Gren taught courses on the history of religion, Adyghe history, Caucasian studies, and general linguistics at the pedagogical institute in Krasnodar. He also planned to open a section for the study of Adyghe (Circassian) culture, but circumstances of the turbulent times prevented him from doing so (Buzarov et al. 1999). In 1926, Gren abruptly moved to Perm.

A Knockdown

Beginning in the mid-1920s, local intellectuals began to feel a sense of impending threat. Anthropology's calm existence had lasted less than ten years. This may explain why, at age 64, Gren left Krasnodar, shifting from his life-long focus on Caucasian studies to a new Finno-Ugric research agenda.

Between the 1920s and 1930s, a conflict between Bolsheviks and purported separatists and 'bourgeois specialists' emerged. Non-Marxist anthropology was to make way for new Soviet *etnografiia* (Slezkin 1993: 40-62). This development delivered a series of severe blows to regional ethnographic studies. Anton Leshchenko, a member of OLIKO and the director of the Regional Studies Museum since 1924, was dismissed from his post in 1930 and was later repressed (Formozov 1998: 196). Another OLIKO member, Seferbei Siiukhov, was also arrested (Buzarov et al. 1999). The same year, Mikhail Sadilenko, head of the Ukrainian department at the Krasnodar pedagogical institute was exiled, and the department was closed down. In 1933, a number of Ukrainian linguists and historians were arrested and executed, and the Leningrad All-Russian Special Commission for Combating Counter-Revolution and Sabotage (VChK) launched the so-called Slavist or Academic affair, a criminal prosecution brought against several famous academicians, which resulted in 1934 in the arrest of several Slavic language specialists in Krasnodar (Chumachenko 2001). In 1937, a number of leading Adyghe ethnographers were arrested (Buzarov et al. 1999). The effects of this purge were felt for a generation. It would take 10 years to restore some anthropological institutions in Maykop, and over 30 years in Krasnodar. Nevertheless, some ethnographers from Leningrad and Moscow managed to continue conducting ethnographic research in the region: in 1939, Evgenii Shilling along with a group of researchers from Moscow conducted research on Shapsug religious beliefs (Tokarev and Shilling 1940).

Another distinguished researcher, Leonid Lavrov, who later became the head of Caucasus Department at the Leningrad Institute of Ethnography (1957-61), completed his dissertation on the Ubykh language in 1939. Because of his personal background (his father was a Cossack priest and his family was from the Ukrainian-speaking stanitsa Pashkovskaia), Lavrov was refused admission to Kuban Pedagogical Institute. He finally enrolled in the Department of Geography at Leningrad University in 1928 (Bgazhnokov and Abazov 2009: 3-5). On several occasions throughout his career, Lavrov had to change his research agenda in order to save himself. First, the Cossacks and Ukrainians were erased from his list research specializations due to political repressions in his motherland; later the Ubykh, Abaza, and Shapsug were erased, as studying these groups associated one with Lavrov's Leningrad mentor, Anatolii Genko, who was tortured to death in a Leningrad prison; finally, Lavrov would abandon his research on the Balkars and Karachais after they were deported to Central Asia by Stalin's regime. Lavrov eventually left behind anthropology altogether, focusing on Arabic epigraphs in the Caucasus for his doctoral research (Lavrov 1966; cf. Kolesov 2009). Lavrov is buried near his mother's grave in the Pashkovskaia cemetery. Paradoxically, for being such a renowned expert on the Northwest Caucasus, Lavrov did surprisingly little for the development of anthropology in and of his own region.

The Postwar Aftermath and Revival from 1960-1980

Post-war Adyghean ethnographers established strong links with ethnographic institutions in Moscow. In February 1945, the Adyghe Research Institute for Language, Literature, and History was re-established, along with its former Department of History, Archaeology, and Ethnography. Evgenii Zevakin, a Moscow-trained historian and ethnographer, was appointed department head (*Stranitsy otechestvennogo kavkazovedeniia* 1992: 28). From 1943-45, Zevakin worked on his habilitation at the Moscow Branch of the Institute of Ethnography, and in 1946 he returned to Adyghea to organize Adyghe folklore research in the Shovgen district. His students Elmaskhan Kodzhesau and Min Kutas Azamatova later published their respective research on the Shapsug family (Kodzhesau 1954) and daily life in an Adyghe village (Azamatova 1961). Both went on to head important ethnographic institutions in the republic: Azamatova directed the History and Ethnography Museum from 1949 to 1964, and Kodzhesau superseded Zevakin as director of the department in 1954. From 1959-61, Adyghea became the site of a large-scale fieldwork project called the 'Complex expedition for the study of social and cultural change by the peoples of the USSR', which was initiated by the Caucasus Department of the Institute of Ethnography (Moscow

branch) under the guidance of Batraz Gardanov (in 1960-61 by Lavrov). The members of the expedition included a number of ethnographers from the region and from Moscow who later became well-known specialists on Caucasus anthropology: Iaroslava Smirnova, Natal'ia Volkova, Boris Kaloev, Veniamin Kobychev, Galina Sergeeva, Mukhtar Meretukov, and Elmaskhan Kodzhesau (*Stranitsy otechestvennogo kavkazovedeniia* 1992: 200).[2] Their expedition was part of a larger project dedicated to the ethnographic study of Soviet modernity that was initiated by Moscow ethnographers in the early 1960s.

Unlike Adyghea, the Krasnodar region continued to be studied by 'outside' scholars: Lavrov made research trips to Krasnodar from Leningrad in 1949, 1959, and 1963 (Lavrov 1982), and Lidia Zasedateleva, from the Ethnography Department at Moscow University to Krasnodar to study Cossack family customs. In 1965, N. Volkova conducted extensive archival research on Circassian Armenians (Volkova 1966). From 1966-67, anthropologist Zhenya Khachatrian worked in the cities of Sochi and Tuapse among Armenian refugees from Turkish Pontus and their descendants (2007: 203-222). From 1976-78, Hranush Arakelian, one of Lavrov's students, did her fieldwork among the few remaining Circassian Armenians in Krasnodar, Armavir, Maykop, and in the Adyghean villages of Bzhedughabl and Koshehabl (Kharatian 1981, 1984).

In the 1950s and early 1960s, there was a renewed interest in ethnographic studies of Cossacks: a group from the Russian Department of the Institute of Ethnography (Moscow), including Liudmila Chizhikova, Margarita Shmeleva, and Vladimir Gorlenko (Kiev), collected materials for the Russian Ethnographic Atlas in 28 settlements of the Kuban region. Local folklorists and their students took part in this research as well (Chistov 1967: 12-14).

Contemporary anthropology in Krasnodar stemmed from a different source than it did in Adyghea. In Krasnodar, anthropological studies were being conducted primarily by the university, which was separated from reseach centres by administrative barriers. In 1969, the local teacher's training institute was reorganized as Kuban State University. As a result, students at Kuban State's Department of History got a new course in ethnography (anthropology). Nikolai Ivanovich Kirei (b. 1940), a specialist in the history of Algeria, led this course. A group of ethnologists and archaeologists split from the Chair of General History to form a new department of Ancient and Medieval History (which was renamed the Department of Archaeology, Ethnology, and Ancient and Medieval History in 1995). Kirei graduated

[2] In 1990, M. Meretukov became the head of the newly formed Ethnography division at the Adyghean Research Institute for Humanitarian Studies.

from Rostov University, where he studied ethnology with Iurii Knyshenko). Kirei established close links with the Department of Ethnography at Leningrad University and its head Rudolf Its. In 1988, another ethnology lecturer at Krasnodar University, Nikolai Bondar (b. 1949), graduated from Leningrad University. Bondar organized a Kuban folklore and ethnographic expedition, and several generations of students received fieldwork training by participating in this program. After their first expedition, the students formed the Research Centre for the Study of Traditional Culture, with Bondar serving as the Centre's director. He successfully restored the Kuban oblast Society of Amateur Researchers (*Obshchestvo liubitelei izucheniia Kubanskoi oblasti*, OLIKO), which conducted research on the region's minority groups (Bondar 1990), but his special research focus remained Cossack and Russian ethnography.

Simultaneously, a new program for ethnographic collections was initiated at the Krasnodar Regional Museum. From 1968-73, a new water storage reservoir was under construction in Krasnodar. Several Adyghe settlements were to be flooded by the planned reservoir, and Klavdia Volkova, a curator from the museum, made several trips to these villages to collect ethnographic artefacts (Marina Strugova, personal communications). In 1977-78, plans were made to organize an open-air ethnographic museum modelled off a Cossack and an Adyghean village. A special ethnographic team headed by Natal'ia Korsakova (b. 1949), a former student of Kirei, was created for this purpose, and in July 1978 about 700 artefacts were collected for an ethnographic exhibition in Mostovskoi district (*Korotko ob ekspeditsiiakh* 1979: 166-67). Several other collection trips were undertaken between 1978 and 1985 (N. Korsakova, personal communications), but the Krasnodar open-air museum was never actually implemented.

The Search for a Theoretical Framework

An abundance of field research on the Northwest Caucasus did not result in the development of empirically-based theory: no theories were generated from locally collected data, with the rare exception of the phoneme theory of an outstanding Russian anthropological linguist, Nikolai Iakovlev, which he formulated on the basis of his study of Adyghean languages (Iakovlev 1923). His mathematical formula for constructing alphabets served as a methodological basis for deriving alphabets for more than 70 languages of the peoples of USSR, although at the time of their original publication, most of his theories were regarded as heretical (Alpatov 1993: 271-288; Serdiuchenko 1932: 147). Most of the theoretical frameworks that were discussed and used by local researchers were put forward and developed by Moscow and Leningrad 'armchair anthropologists' such as Iulian Bromlei and Viktor Kozlov or

borrowed from other disciplines and research traditions. In this section, I shall briefly discuss several of the 'theories' that were most influential in terms of regional theoretical fashions. They include theories of ethnos, cultural semiotics, and critiques of locally supported pseudo-academic constructions like 'ethno-demographic balance' and 'cultural incompatibility'.

When anthropology of the Northwest Caucasus was revived in the 1970s and 1980s, essentialist conceptualizations of ethnicity were predominant in Moscow. Their foundations were laid by Bromlei's theory of etnos. 'Peoples' (with attached cultures, religions, and languages) had served as privileged objects of ethnographic research in Russia since the inception and institutionalization of ethnography (ethnology) as a distinct discipline. Bromlei suggested another term, *etnos*, as a more formal and less colloquial designation for 'people'. But Bromlei's terminological innovation was not the only one. Bromlei understood the etnos as a 'social organism', the existence of which was dependent on a community, territory of residence, language, culture and heredity and on the presence of a single ethnic self-consciousness.[3] The cultural unity of an etnos was 'indivisibly linked to the unity of their psychic setup, which appears specifically in the nuances of their characters, particularities of their value orientations, tastes, etc.' (Bromlei 1981: 23). Thus, culture was conceived of as a unified and unitary entity stemming from the primordial psychological bond linking all the members of a particular ethnos to one another.

At Kuban State University, such ideas were advanced by both Kirei and Bondar. In the late 1980s, these ideas began to clash with more constructivist conceptualizations of ethnic identification. For local scholars, it was difficult to look at ethnic groups and identities as something 'constructed', which is perhaps unsurprising, given that by the 1980s, ethnicity had been deeply institutionalized in the Soviet Union. Rogers Brubaker asserts that the Soviet institutions of ethno-territorial federalism and personal ethnic identification comprised a principle by which the representation and classification of social reality were organized as a standardized schema of social accountability and as an interpretive network for public discourse; Soviet institutions provided a collection of boundaries and legitimate forms of collective and

[3] An earlier definition was given by Bromlei's colleague, Viktor Kozlov: 'An ethnic community is a social organism that has formed, over a certain territory and out of a group of people with similarities of language, common traits of culture and everyday life, some common social values and traditions and a considerable blending of different social components achieved in the course of the development of economic and social cultural relations. The basic features of an ethnic community are ethnic self-identity and self-name, language, territory, specific features of psychic make-up, culture and every-day life as well as some specific forms of socio-territorial organization or the drive to create one' (Kozlov 1967: 111).

individual identity (Brubaker 1996: 24). Public reaction to the collapse of the Soviet regime led to 'a reordering of people's entire meaningful worlds' (Verdery 1999: 34-35); people thus had to find new forms of action or else give new meaning to their existing practices.

My own experience provides anecdotal evidence that, in the eyes of the senior generation of local ethnographers at Kuban State University, parting with the essentialist notions of ethnicity was tantamount to ethnic nihilism and was viewed as unpatriotic and even subversive. At the same time, many of the arguments, postulates, platforms, and programs of post-Soviet ethno-political movements were taken from Soviet ethnological academic publications (Tishkov 1997: 7). The debates between politicians, experts, and, academics in Krasnodar is just another case in which academic and pseudo-academic theoretical constructions have been used as resources in ethnic politics and in various local nationalisms. A prominent example of this is the role assigned by the regional administration to Cossacks. By the 1990s, after administrative reform and the partitioning of the Republic of Adygea from the Krasnodar region to form a separate subject within the Russian Federation, out of a total population of five million, the majority of people living in Krasnodar were Russians (app. 85 % or 4,314,783 persons), including Cossacks (from 20 to 39% or 500,000 to 1,000,000 persons, with the upper range of this estimate being given by Tutsenko 2001: 39), followed by Armenians (app. 4.76% or 241,964 persons) and Ukrainians (app. 2% or 98,570 persons). Nevertheless, Cossacks were allocated a status similar to that of titular nationalities in the Soviet republics. According to article 2, item 1 of the 1997 Krasnodar Krai Statutes, 'Krasnodar Krai is the historical territory in which the Kuban Cossacks came into existence and is the Russian people's age-old place of residence'. The image of Krasnodar as a Cossack territory is reflected in the Kuban Rada's banner and coat of arms.[4]

Conceptions of Cossack identity and culture were developed by local historians and anthropologists closely associated with the Cossack movement (e.g. Bondar). Academic attempts to mediate between nationalist and regionalist ambitions, on the one hand, and the common late-Soviet 'ethnic' theoretical framework, on the other, produced an approach that treated the Cossack renaissance as 'ethnic' in nature, and romantic appeals for Cossacks to return to their cultural roots were taken as evidence of a Russian 'sub-ethnos' (Bondar 1995). Bondar, who formulated this position by the late 1980s in his dissertation (1988), insisted that Kuban Cossackdom had emerged as a result of the integration of Russian- and Ukrainian-speaking groups. This view neglects the early 1930s anti-Ukrainian campaign of

[4] 'Kuban Rada' is the name for the regional parliament. 'Rada' reproduces the name of a Cossack state institution from the time of the Civil War.

forced assimilation as a cause of the declining of Ukrainian population in the region. Bondar did not deny that some Kuban Cossacks continued to speak Ukrainian, but he emphasized the influence of Russian on these Ukrainian speakers (Bondar 2000: 149). Similarly Bondar has argued that cultural differences between Russians and Ukrainians, particularly in folk songs, were gradually levelled out, and elements of Ukrainian and Russian traditions formed a single system. After conducting intensive fieldwork in the same Kuban stanitsas, ethno-musicologist Nadezhda Suprun-Iaremko strongly disagreed with Bondar's conclusions:

> In arguing his position that 'Kuban Cossackdom is a sub-ethnos of the Russian ethnos', the Kuban ethnographer has interpreted the traditional culture of the Kuban Cossacks so tendentiously and artificially that many of his conclusions have lost any sense (Suprun-Iaremko 2003: 55).

The key term in this debate is 'sub-etnos'. On the one hand, Bromlei's etnos/sub-etnos hierarchy helped Bondar to answer those who denied the existence of Kuban Cossackdom as an *ethnic* category by claiming it to be an *estate* ('soslovie') in the social system of the Russian Empire. On the other hand, Bondar's concept of Cossackdom helps Cossacks to be viewed as both Cossack and Russian patriots simultaneously. Bondar is well aware that the disputed issue — whether Cossacks are a 'sub-etnos' or a separate, autonomous 'etnos' — has political implications: 'Ethnological research containing conclusions on the specific ethnic status of the Kuban Cossacks causes anxiety for some opponents who fear that it might be used by separatists' (2000: 156). For this reason, Bondar disagrees with radicals in the Cossack movements who follow their emigrant leaders and proclaim Cossacks to be a separate Slavic people or, in Bromlei's terms, an etnos (cf. Kuz'menko 1996).

In the 1980s, the Tartu-Moscow school of cultural semiotics exerted a strong influence on anthropologists in both the capitals and in provincial regions. Due to the limited availability of this group's publications in libraries, the Tartu-Moscow school took on something like an elite, dissident, or even revolutionary aura. Albert Baiburin, a student of Iurii Lotman who utilized a semiotic approach to the study of Russian folklore and rituals, played a central role in practically demonstrating how the semiotic approach, with its metaphor of 'culture as text', might be applied to the analysis of material culture in ethnographic research (Baiburin 1981). In Krasnodar, Baiburin's example was followed by a younger generation of anthropologists that applied his methods to the analysis of the customs of Armenians living on the coast of the Black Sea (Kuznetsov 1995) and Cossack folklore (Manuilov 1995).

The Politicization of Research and Pseudo-Academic Constructions

Soviet social sciences did not equip administrators to deal with the large-scale migrations and social conflicts that resulted from the collapse of the Soviet system and the ethnic cleansings that occurred in South Caucasus and Central Asia, and so the dramatic challenges of the early 1990s forced authorities to seek input from local experts. As there was a high demand for works conceptualizing the new realities and the time to develop them rather short, true academic efforts to understand the situation were soon replaced by the pseudo-scientific concepts of ethno-demographic balance, ethnic status, and cultural incompatibility. Ethnographers in the centre (mostly in Moscow) were the first to speak out on the dangers of internal migration (e.g. Kozlov 1987: 163; Perepelkin 2000: 161). This position was soon replicated throughout Russia, as a number of conflicts induced internal migration. Krasnodar social researchers were among the many who fell prey to these pseudo-scientific concepts.

Migration and the challenges of accommodating migrants formed a much disputed ground for testing alternative theoretical models and hypotheses and provoked debate among local politicians, social scientists, historians, and anthropologists, often couched in nationalistic rhetoric and scholastic argumentation. Discussion of the purported negative influence of migration started in academic and political circles in Krasnodar in the late 1980s, at the same time that legal reforms limiting the civil rights of migrants (i.e. persons who had resettled from the then Soviet republics of Central Asia and the Caucasus) began to take shape. Although in regional political and academic debates, migration and ethnicity had previously been closely interlinked, real internal migration of ethnic minorities to the region remained insignificant. In the early 2000s, more than 80% of migrants to Krasnodar were ethnic Russians, a figure that reflected the pre-migration proportion of Russians in the total population of the region. As it turns out, the region's ethnic composition did not undergo any dramatic changes in the 1990s, despite the fact that, at the time, regional demographic changes were considered to be an acute problem. A case in point is provided by the situation of the Meskhetian Turks, who provide a textbook example of 'the Russian experience of ethnic discrimination' (Osipov 1999).[5] In Krasnodar krai, Meskhetian Turks have been marginalized and are viewed as 'illegal migrants'. In post-Soviet Rus-

[5] Meskhetian Turks were exiled from Meskheti (Southern Georgia) to Soviet Central Asia in 1944 by the Stalinist regime. In late-Soviet times (1989-90), most Meskhetians moved to Northwest Caucasus after a wave of pogroms swept through areas in the Ferghana Valley with high densities of Meskhetian Turks (Tomlinson 2004: 18-21).

sia, the most effective way of controlling the influx of 'illegal migrants' has been the system of registering people in their place of residence, which replaced the old Soviet system of residence permits (*propiska*), reintroducing a situation in which citizen rights depend on residential status. In districts with Meskhetian populations (Krymsk, Abinsk, Apsheronsk, and Belorechensk), local authorities refuse to register Meskhetians, are unwilling to exchange old 'Soviet' passports for new Russian ones, and refuse to issue passports to young people upon reaching legal adulthood, while registry offices refuse to register their marriages. Such violations of Meskhetian civil rights provoked an international response, ultimately leading to the decision by the US State Department to grant P-2 refugee status to over 10,000 Meskhetians, mainly from Krasnodar krai, in 2004.

Social scientists played a role in providing the ideological foundation for this policy in the region. Migration and inter-communal relations (*mezhnatsional'nye otnosheniia*) studies became very popular in Krasnodar and were readily supported by local authorities. One of the prominent experts on such issues was Mikhail Savva, who received his training in anthropology under Dr. Bondar's tutorship and conducted fieldwork for his graduate thesis among local Kurds. Both of his dissertations, however, were written in political science, and he defended them in the 1990s at the Institute of Philosophy in Moscow. Simultaneously, he headed the Department of Ethnic Issues in the *krai* legislature, worked at the Krasnodar Mayor's Office, and held the post of Vice-Minister in the Russian Federation on Nationalities Relations. His theoretical preferences were typical for someone involved in applied anthropology, which was practiced by social scientists who maintained close links with local administrations, and which may be of interest to those seeking to understand the politics of knowledge in late Soviet and early post-Soviet Russia. In accordance with Soviet etnos theory, Savva treated ethnicity as 'a fundamental category, a way of organizing human existence, most closely linked to the category of culture' (Savva and Savva 2002: 25). Due to this postulated fundamental nature of ethnicity, Savva viewed most social processes (including migration) first and foremost as ethnic, and he thought of the population of any territory as being essentially composed of ethnic groups. Ethnic groups have collective identities; hence, they are viewed as social actors, driven by the 'national' (meaning ethnic-based) interests of the etnos. As a result of this immanent clash of interests, inter-ethnic relations are thought to sooner or later lead to ethnic conflict, which means that ethnic diversity is always a threat to an ideally homogeneous 'ethno-cultural space' and peace (a view also advanced in Bondar 1998).

The idea of the inevitability of ethnic conflicts was supported by the conjectures of local sociologists, who believed that ethnic culture was a combination of 'values' and 'normative settings', equally shared by all representatives of a particular 'ethnos'. These same local sociologists believed that ethnic conflict was a 'collision of incompatible basic values of peoples forced to come into contact with one another' (Petrov 2003; Rakachev and Rakacheva 2003: 76).

The concept of 'ethno-demographic balance' presumes that there is a statistical correlation between the share of a 'native population', xenophobia, and violence. According to this theory, if the number of migrants exceeds 10-15%, 'ethnic balance' is destroyed, and the host society becomes incapable of 'digesting' any more migrants, with xenophobia and ethnic intolerance therefore increasing. Mikhail Savva was the first from the local research community to alert authorities that Krasnodar krai's ethnic balance was on the brink, with the migrant share of the population reaching 13% (Savva 1994). Local authorities used such 'academic findings' as a ground for refusing to grant citizenship to Meskhetian Turks. Aleksandr Osipov, a Moscow-based expert in the history and anthropology of Meskhetian Turks and a human rights activist, has labelled the concept 'speculative' and has referenced a critique of the notion by Stolcke in *Current Anthropology* (Osipov and Cherepova 1996: 77-78; Stolcke 1995: 3).

Another dubious concept that emerged in political discourse about minorities was the concept of 'ethnic status', which was used to explain disparities between ethnic groups (Savva and Chuprov 1992: 20-30). By making ethnic group identity ubiquitous and an essential component of social orientation and action (Savva 1997: 7), Savva aimed to infer ethnic conflicts from individuals' primordial attachment to their respective ethnic groups. He interpreted the differential social position of groups as the basis and a necessary condition of ethnic conflict (Savva 1997: 46-53, 1999a, 1999b). A similar concept of 'cultural incompatibility' had been employed by some local researchers and journalists in analyzing the causes of ethnic conflicts (especially in the case of conflicts between the 'native population', on the one hand, and, on the other, 'migrants' and so-called 'non-Slavic etnoses', especially Meskhetian Turks; Savva 2002a, Tkachuk 2002, Vandysheva and Tur'ialai 2002).

The essentialization of ethnicity in analyses of regional conflict provoked critique from the younger generation of social researchers (Kolesov, Kochergin, and Leibovskii 2002), resulting in a split of the regional research community into 'constructivists' and 'essentialists', with 'constructivists' accusing their opponents of culturalism and racialism and 'essentialists'

reproaching their adversaries for their unscholarly behaviour and subjectivism (E. Savva 2005: 115, 116; Savva 2002b: 81, 82).

Soviet Etnografiia and Contemporary Northwest Caucasus Anthropology

During the Soviet period, anthropology of the Northwest Caucasus possessed a number of peculiar features. Because, after its rapid ascent in the 1920s, the discipline was marginalized for several dark decades, it was isolated from mainstream developments like the rise and fall of Marrism. After a slow recovery of the discipline in Adygea after World War II and in Krasnodar in the late 1960s, local anthropologists were able to return to their traditional preoccupation with collecting artefacts and observing and describing traditional culture. This was in contrast to their colleagues in Moscow and St. Petersburg, who studied social rank, class differentiation, and the culture of contemporary rural communities.

At the same time, the local academic community shared many assumptions and prejudices with Soviet *etnografiia*: local scholars were busy correlating cultural differences and presumed evolutionary stages and exoticizing their research objects by focusing on so-called primitive survivals of matriarchy, the clan system, and totemism, amongst others. Local scholars believed that the most authentic data could be found in the most isolated and distant places, and over time, they shifted from undertaking long-term participant observation to relying on brief visits with informants in Cossack stanitsas, Adyghe auls, and Armenian villages. However, this kind of fieldwork was suitable for the Northwest Caucasus since local scholars were in fact able to maintain close contact with native anthropologists as well as their informants all year round in stark contrast to anthropologists from Moscow and St. Petersburg. Studies of the Northwest Caucasus undertaken by social researchers were first of all focused on formal aspects of culture, not upon exchange or social networks. In both Maykop and Krasnodar, very few of the works comprising this body of scholarship could be labelled as 'social anthropology'. Until recently, cultural studies (semiotics) remained preferable. From this point of view, anthropology of the Northwest Caucasus is more 'cultural' than 'social'.

Chronologically speaking, one can hardly speak of differences between the Soviet and post-Soviet period in local anthropological research. In Krasnodar, there was no 'normal' (sufficiently prolonged) tradition of Soviet ethnography to allow such a contrast to be drawn; in Maykop, Soviet-era and post-Soviet ethnography do not sufficiently differ either paradigmatically or institutionally to warrant such a broad contrast. The only notable develop-

ment since the early 1990s is the increasingly applied character of research projects and the decline of fieldwork-based research due to insufficient funding. Furthermore, most anthropologists in Maykop and Krasnodar have been deaf to the concerns of mainstream postsocialist studies, excluding their interest in the rapid ethnicization of social relations in the region, which they understand as resulting from the challenges created by ethnic conflicts and migrations. As is the case in post-Soviet academia more broadly, in the Northwest Caucasus, there have been hardly any studies of poverty and criminality, social security, the educational system, corruption, or gender inequality (Sokolovskiy 2009: 62). Another serious shortcoming of recent research has been the neglect of anti-colonial debates. In the 1920s, Tsarist colonialism was subjected to criticism, but this was ultimately curtailed by Stalinist repressions; subsequent Soviet ideologues denied the colonial past of conquered peoples and territories, including the Caucasus. It remains difficult to study post-colonial criticism in Putin's Russia, where imperial rhetoric is prevalent.

So what should we make of the ideas of local anthropologists? Are they empirically grounded conclusions or merely illustrations of the construction of nationalist (Russian, Adyghe, etc.) myths? Even well trained scholars researching the senior generation with long field experience find it difficult to discard nationalist biases, and the confusion within the local academic community over Meskhetian Turks is a case in point. Historians of science have recently started to pay attention not only to how paradigms change due to long-term discussions between various schools but also to dramatic 'one-act' quarrels between professionals. Indeed, 'intellectual conflicts are not something outside knowledge' (Spoerhase 2006). It is important to understand the norms that such conflicts regulate, the institutions and mechanisms that exist to solve these conflicts, and the forms of public discourse and rhetoric that are recognized as legitimate by disputees. All of the debates mentioned in this chapter, which were discussed by researchers of the Northwest Caucasus, are characterized by a number of common features: opposing sides typically avoid any discussion of specific conceptualizations and theoretical models, relying instead upon mutual accusations of insufficient scholarly knowledge and insufficient professionalism and looking to local and regional authorities to mediate. In my opinion, this peculiar manner of conducting debates is evidence of the fact that anthropology is still in its early stages of institutionalization in the Northwest Caucasus. Despite the fact that research and educational centres with professionally trained faculty have existed in the region for half a century, the achievements of these institutions have remained modest. The border between dominant nationalist ideology and academic knowledge has been

permanently violated. To my regret, the ideological and political partisanship that has emerged amongst academic researchers, who are compelled to apply anthropological knowledge to the tasks set by nationalistic local administrators, competes with and often takes the upper hand over professional commitments and academic freedom.

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INDIVIDUAL CONTRIBUTIONS AND THEIR POLITICAL CONSTRAINTS

Chapter 13
Iakubovskii and Others: Canonizing Uzbek National History

Alisher Ilkhamov

Following Michel Foucault's genealogies of the mutual links between knowledge and governmentality (Foucault 1970, 1972, 1980) and Eric Hobsbawm's claim that the historical profession is essential to nationalism and politics (Hobsbawm 1983, 1992), the idea that there are critical links between academic, especially historical, knowledge, power, and nation-building is nothing new. But the Soviet and post-Soviet experiences probably represent the most evident case of this link because, under the Soviet regime, historical knowledge was itself, at least in some instances, a form of power. This truth becomes apparent when we look at select Soviet academics, both Russian and of other, smaller nationalities. One such academic was Aleksandr Iur'evich Iakubovskii (1886-1953), a Soviet historian, Russian by national origin, and expert on Mongol and Central Asian history, whose personal role in canonizing the official doctrine of Uzbek national history was remarkable. In spite of his continuing legacy in contemporary Uzbekistan, Iakubovskii has still not attracted adequate attention from historiographers. This can be partly explained by the fact that Iakubovskii indeed did not belong to the pantheon of grand figures of Russian and Soviet historical, anthropological, and oriental studies such as Vasilii Bartol'd, Ignatii Krachkovskii, Nikolai Marr, Iulian Bromley, and others; rather, Iakubovskii belonged to what can be viewed as the second tier of Soviet historians. Another explanation is that, thus far, post-Soviet historiographers have not paid sufficient attention to the evolution of conceptions of national and native history during the Soviet period, the interaction between academia and political power, and the struggle between various schools of thought within academia itself. By tracing the biography and professional development of Iakubovskii, we can reconstruct the evolution of Soviet historical schools and thereby better comprehend the intellectual legacy of the Soviet Union and the extent to which it has been preserved, overcome, or rejected by post-Soviet nation-states and associated academic elites.

Let us set out by depicting the historical circumstances under which Iakubovskii and scholars of his generation emerged and took over key positions in the Soviet academy. First of all, it is important to note that this was a critical historical moment marked by two historically exceptional events: the demise of the Tsarist Empire and the dawn of the new Soviet regime. Claiming to be formed in accordance with advanced social theory, the Soviet state envisioned a new role for social sciences to play in society. After the upheaval of 1917, followed by a decade of consolidation of the Bolshevik regime, on the one hand, social sciences had to be adjusted to the leading role of Marxist grand theory, but, on the other hand, social sciences had to address the daily needs of governance and socialist construction. The disciplines of ethnography, history, archaeology, and so-called oriental studies that made Asian and Islamic societies specifically subject to academic inquiry were particularly in demand in light of the national self-determination agenda, which was an essential part of the Bolshevik political program. The Soviet regime implemented this agenda by creating the quasi-nation-states that would constitute the Soviet federalist formation, thus fulfilling the program of national self-determination and, at the same time, preserving the former Tsarist Empire's territorial integrity.

Given the fact that, from the viewpoint of classical Marxist theory, the native population in Central Asia lacked the historical prerequisites for socialism and modern nation-state formation, the Bolsheviks adopted a strategy of transitioning from feudalism directly to socialism, bypassing (or omitting) the stage of capitalism. This was a challenge that could be met only by balancing direct political control from Moscow with the satisfaction, to a certain extent, of the ethno-cultural sentiments of indigenous populations and their intellectual elites. Scholars, orientologists, ethnographers, and historians were seen as essential instruments in fulfilling this two-fold agenda and were called on to take part in pursuing three following tasks.

The first task was to elaborate the ethnographic and statistical basis for defining the borders of new Soviet republics. This stage lasted from the very beginning of the Soviet era until 1929, and during this time the Soviets, in collaboration with experts (orientologists, ethnographers, linguists, and historians), commissioned statistical studies followed by the delimitation and creation of new nation-states from scratch. Some ethno-statistical studies were already implemented between 1917 and 1923 by Igor‘ Magidovich (1924), Mikhail S. Andreev (1924), and Ivan Zarubin (1925) and were initially driven by the rather pragmatic need for a rational classificatory schema of ethnic and sub-ethnic groups residing in the region, initially just for the sake of inventorying and registering populations. The political rationale for these studies had not been yet formulated. Debates over the question

of which constituting principles should be chosen in order to fulfil the Bolshevik promise of national self-determination were sparked somewhat later. Two camps emerged, one advocating that states be built around selected titular ethnicities[1], and the other arguing that an autonomous Turkestan could be formed not on the basis of the ethno-national principle but rather on the basis of the historical co-existence of various indigenous peoples and ethnic and linguistic groups.[2] The Bolsheviks sided with the former, ethnocentric approach, and thus a policy of national delimitation in Central Asia ensued that ran against the Marxist view that nations are the product and integral element of capitalist development. In fact, the Bolsheviks and most of all Lenin himself revised the classic Marxist theory of nationalism and adopted a new view according to which nations could emerge and thrive under the socialist state with the guidance of a vanguard communist party and the assistance of the Russian proletariat. To fulfil in practice this revisionist position, social sciences were conscripted and given carte blanche to realize their own aspirations and views, though only within the framework of an already made political decision concerning the principles and tasks of nation-building. The results of this new political demarcation of quasi-nations were consolidated by the first census surveys in 1926 and 1937 (Abashin 2004; Hirsch 2005; Ilkhamov 2006). Iakubovskii himself was not involved in these developments, but he did participate in the follow-up agenda that required historical science to substantiate and validate official claims that the ethnocentrically constructed nation-states — the so-called Union republics — were not artificial formations but rather had deep historical roots.

The second task that was set for scholars involved the canonization of national languages as indispensable attributes of the titular nationalities of the various new republics. In the case of Uzbekistan, this process began in

[1] This project was advocated by the Turkestan Commission (*Turkkomissiia*) of the Soviet Central Executive Committee and the Russian Soviet Federal Socialist Republic (RSFSR) Council of People's Commissars. The *Turkkomissiia* commissioned the demarcation of borders for the newly created national formations in Turkestan. In turn, the position of the *Turkkomissiia* was influenced by G. Broido, a Bolshevik expert and official, who was preoccupied with existing ethnic conflicts in the region, especially between Uzbeks and Turkmen in Khorezm, where he was sent as an expert on ethnic issues and witnessed these conflicts (Nurpeisov and Grigor'ev 1997).

[2] This position was advocated by T. Ryskulov and M. Kh. Sultan-Galiev (both national-communists) who also promoted the idea of a unified, autonomous nation-state uniting all Turkic peoples of the Russian Federation (Ilkhamov 2006: 22-23). Indirectly, this position was also supported Vasilii Bartol'd, who in a letter from 1924 criticized ethnocentric nation-state building for blindly copying the European experience and not taking into account local social peculiarities and historical circumstances. This letter was published in *Kur'ier petrovskoi kunstkamery* in 1995.

the 1920s and was completed by World War II. During that period, particular dialects were chosen to provide the foundation for the titular nationality's national standard literary language, which, it was hoped, would give rise to a national literary culture. As existing scholarship suggests, advocates of the Kypchak dialects and adherents of Chagatay literati heritage vied for the national standard. Kypchak advocates asserted the Steppe origins of the Uzbek nation, whereas Chagatay advocates viewed contemporary Uzbeks as descendants of sedentary peoples who had more profound local roots and history — specifically, the Uighur (later known as Chagatay) cultural tradition.[3] Once again, Iakubovskii himself was not personally involved in this debate about the 'correct' language identity of the titular nationality. His further academic activity was dedicated to the consolidation of the victorious view according to which the Uzbeks were not latecomers to the region of Central Asia but had, in fact, assimilated those Uzbek tribes, the nomadic invaders who were led by Sheibani at the beginning of sixteenth century.

From the assertion of the Karluk dialect (associated with the Chagatay cultural tradition) as the constituent element of the modern Uzbek language to the canonization of Uzbek national history, only one interceding step was necessary, and a school of historians largely influenced by Iakubovskii played a critical role in bringing this about. This step had to be made for the sake of legitimating the young quasi-nation-state formation. The nation-state could be legitimated by assigning to the titular nationalities proof of their ancient historical origins and by arguing that they did not lack for coherence in their cultural traits, language, national symbols, and historical background. The conceptualization of national histories was an essential part of this legitimation agenda.

Conceptualizing Russian National History

Alexander Iakubovskii was born and spent most of his life in St. Petersburg, which was well known as the historic centre of Russian academic sciences, especially for the field of humanities, anthropology, and oriental studies. In 1913, he graduated from the department of history and philology at Petersburg University. In 1924, he completed his studies in the oriental department of the same university. As a student and young scholar, Iakubovskii benefited from personal communication with various patriarchs of Russian academia like V. Bartol'd, S. Ol'denburg, N. Marr, I. Orbeli, and I. Krachkovskii, who happened to live, work, and teach in the same city at the same time. From his teachers he apparently inherited a sense of the intellectual culture and high standards of diligence characteristic of old academic

[3] On the canonization of the Uzbek language, see Baldauf 1991, 1992; Fierman 1991.

schools. It is no accident that he was later recognized as one of the most gifted disciples of Bartol'd.[4]

In 1924, after graduating from Leningrad University, he entered a distinctly different world marked by Bolshevik political domination, with a complex relationship arising between the new authorities and the academy. Although, for a time, academia enjoyed some freedom thanks to an agreement between Lenin and academic Ol'denburg, under Stalin's rule, mass and systematic repressions began, targeting so-called socially alien elements. First amongst these was the bourgeois intelligentsia, who, according to the Bolshevik master plan, were to be progressively eliminated from Soviet institutions, neutralised, and isolated from the society, eventually being supplanted by a new generation of intelligentsia. The repressions were an integral part of the regime's use of fear as a means of political control and were at times as random as the lottery, such that anyone — even the innocent — might find themselves among the regime's victims. Luckily, along with a number of other orientologists, historians, and ethnographers, Iakubovskii was unharmed and survived the purges initiated by the ruling regime. He survived both of the repressions in 1928 that specifically targeted the Academy of Sciences, as well as the mass cleansing of Leningrad, in which thousands of people (and, notably, party leaders and intellectuals) were deported from the city or imprisoned following the assassination of the city's Obkom (Oblast Committee of the Communist Party) First Secretary Sergei Kirov, in 1934. Until the relevant archival materials are happened upon, it will remain difficult to say whether Iakubovskii was able to hold out thanks to good fortune alone or if he had political connections. In any case, he not only survived but even improved his position within the academic hierarchy in those years. In 1935, at the age of 51, he secured a prestigious position as a professor at Leningrad University.

To be fair, a number of other distinguished orientologists also managed to survive the purges and terror campaigns. For instance, Vasilii Bartol'd and Sergei Ol'denburg lived until 1930 and 1934 respectively, and went unharmed by the Bolshevik regime. In Bartol'd's case, this occurred despite the fact that he dared to oppose the Bolshevik plan to create new national republics in Central Asia along ethno-territorial lines. Others like I. I. Krachkovskii, N. I. Konrad, and N. I. Marr continued their work, even as they were forced to compromise by embracing, at least formally, approved Marxist methodologies. Others were repressed: for instance, in 1937, Aleksandr Samoilovich was arrested, accused of spying for Japan, and ultimately

[4] As Yuri Bregel attests, Iakubovskii was considered one of the Bartol'd's three most distinguished disciples, along with L. A. Zimin and I. I. Umniakov (Bregel 1980: 391).

executed. Ethnographers like Nikolai Nevskii, Andrei Vostrikov, Iurii Shutskii, and many others were also repressed.

As for Iakubovskii, his allegiance to Stalinist national policy was not formal, although the question remains whether he was committed to Marxist theory overall. What is unquestionable is that, along with some other historians, Iakubovksii indeed proved his ability to address and even anticipate Stalin's growing interest in Russian history. Already trying on the mantle of absolutist ruler, Stalin sought out sources of inspiration in Russian history in order to draw historical lessons for governing the contemporary Soviet state. It was in this period that Stalin began to deliberate how to best complement his communist creeds with a national ideology in order to foster patriotic feelings among Soviet citizens and thus to secure the loyalty of the population to the regime.

At the beginning of his academic career, Iakubovskii specialized in Mongolian history, namely, the thirteenth century epoch of Mongol expansion into ancient Rus. First published in 1937, his monograph *Zolotaia orda i ee padenie* (The Golden Horde and its Fall), which he co-authored with B. D. Grekov, was so well received by the political leadership that the book was republished on two occasions (in 1941 and 1950).[5] In 1952, Iakubovskii and Grekov were awarded the Stalin Premium, the highest award for Soviet intellectuals at the time. The book was indeed academically appealing and, what is most important, remarkably responsive to the political agenda set by Stalin's regime. On the one hand, one can consider the book as a continuation of long-standing Russian academic traditions. The text was well written and even somewhat entertaining, while remaining academically rigorous and grounded by extensively referencing original medieval sources. What most likely pleased the political leadership was that the book utilized a Marxist methodology while simultaneously advancing the new, nationhood agenda. The book amply cited the works of Lenin and Stalin. Following class theory, Iakubovskii and Grekov 'correctly' interpreted historical facts by revealing the class antagonisms between the *noions* (Mongol aristocracy) and ordinary nomads. Nevertheless, the two authors were careful not to over-rely on Marxist theory in their descriptions of the Russian domain. The example of Mikhail Pokrovskii, another Soviet historian, who was dethroned as the chief Marxist historian, demonstrated that Stalin would tolerate neither the bashing of past Russian autocrats nor the blanket rejection of tsarist imperial rule. Drawing lessons from Pokrovskii's failure, Iakubovskii and Grekov did not

[5] The Golden Horde was one of the *uluses* (i.e. states or autonomous parts of the Mongol empire) established as a result of Genghis Khan's conquest of vast territories from China to Eastern Europe and the Middle East. It was established in the territory of contemporary Astrakhan.

allow themselves to go too far in interpreting Russian history through a Marxist analytic. Their application of Marxism was rather selective, only being employed to a full extent with regards to the Mongol empire. In assessing the historical role of Muscovite kings, Marxist analysis was deemphasized. Figures like the Muscovite kings were to be judged chiefly with reference to their contributions to Russian statehood.

Iakubovskii rebuked those authors, mainly of the older generation, whose studies of the Mongol invasion acknowledged its somewhat positive impact upon international trade, political stability, and the development of infrastructure and even culture within the domains of conquered territories (see, for instance, Bartold 1956). He also refuted the position that positively portrayed Genghis Khan's role in the consolidation of Mongol and Turk tribes. He insisted that Genghis Khan should first and foremost be understood as a representative of the aristocracy, which was exploiting ordinary Mongols. Furthermore, the historian claimed that 'the Golden Horde was an artificial state formation, established by violent conquest of alien lands', and that such a state 'could not exist in any way other than by violence, by grabbing from conquered peoples' (Grekov and Iakubovskii 1950).

By reducing the complexity of the Mongol empire and its relationships with Russian kingdoms and by demonizing the figure of Genghis Khan, Iakubovskii, willingly or not, greatly contributed to the concept of the Tatar-Mongol yoke, which was henceforth presented as a critical landmark of Russian history, the chief intrigue of its drama and political development. According to Iakubovskii and Grekov and their associates, the Tatar-Mongol conquest had a disastrous effect on the development of Russian statehood, and the Muscovite Rus were only able to expel the Mongols thanks to economic development and the consolidation of the Russian nation under a strong centralized state.[6] The subtext of this interpretation of history could be read as follows: (1) National sovereignty had been and remained the highest priority; (2) National sovereignty was constantly threatened by foreign enemies, and the remedy to this could be found only in the consolidation of the nation under a wise and strong leadership. By drawing these lessons from Russian history, one was prompted to recognize the legitimacy of Stalin's dictatorial regime and its historical mission of consolidating the nation in order to defend it from both internal class enemies and alien imperialists.

[6] Historians have noted the fact that the Golden Horde was at war with certain Russian kingdoms while simultaneously aligning itself with others.

Turning to Uzbek National History

The experience of reviewing and canonizing the Russian national history demonstrated by *Zolotaia orda* provided a model for subsequent writing and canonizing histories of smaller Soviet nations and nationalities, and this was the agenda with which Iakubovskii found himself personally involved. In selecting Uzbekistan as a locale in which to apply his methodological principles, Iakubovskii was driven by the unexpected Nazi invasion of the USSR. When the war began, a number of academic institutions were evacuated from the centre to the east of the country. Other scholars were conscripted into the Army or *narodnoe opolchenie* (the people's volunteer corps). From the Leningrad Institute of Oriental Study alone, 37 scholars fell on the battlefields of Leningrad. At the age of 55, Iakubovskii was amongst the group of academics evacuated to Tashkent. Soon after resettling in Uzbekistan, he resumed his academic activities, this time in close cooperation with the Uzbek Academy of Sciences. By 1941, under the auspices of the Navoi Anniversary Committee, Iakubovskii published *K voprosu ob etnogeneze uzbekskogo naroda* (On the Question of the Uzbek People's Ethnogenesis), a seminal article that was released in a separate brochure.[7]

It was no coincidence that the article appeared precisely at this moment. In the first phase of the war, the Soviet army suffered a series of heavy defeats and massive territories were ceded to the occupying Nazis. In part, the defeat was brought on by the low morale of the Soviet army on the eve of the war. The class consciousness nurtured by the Bolsheviks had been nearly obliterated by years of repression, the devastation of millions of households (especially in rural areas), attacks against churches, the persecution of believers, and other atrocities committed by the Stalinist regime against intellectual and spiritual elites across the country. After years of repression and hardship, the population of the Soviet Union, especially in national republics, could be inspired to fight for the regime only by the lash of the whip or else by some set of ideas other than class ideology. At this critical moment, nationalist-minded historians like Iakubovskii were likely to come in handy. Once again, these historians had a chance to assert that, as the cornerstone of the party's political philosophy, the concept of class struggle had to be complemented with patriotic ideology. Given the country's multi-ethnic composition, the ideology of patriotism was to be comprised of patriotically coloured historical narratives that were attributed to each of the smaller nations comprising the Soviet Union. At the same time, the ideology of patriotism was to remain consistent with the general line of pan-Russian national history, which was presupposed to culminate in wide-

[7] The Committee was created by the People's Commissary Council of the Uzbek SSR.

spread pan-Soviet patriotism. Likewise, Stalin's regime and its historian supporters had to offer to the peoples of Central Asia an inspirational concept of national history that would raise national consciousness and self-dignity without conflicting with the imperatives and trajectories of Russia's glorious past. This task was to be realized by discovering and articulating the 'greatness' and 'great pasts' that could be ascribed to the component nations. 'Greatness' was associated with the nation's ancient origin and its ancestors' historical achievements. The only problem that could impede the realization of the task of canonizing the Uzbek national history was the continuing mismatch between the language, self-identity, national symbols, territory, and culture of the given nation. This predicament could only be faced by inventing and shaping a glorious national history.

In regard to Uzbekistan, scholars had to address the Mongol-Turkic associations of the ethnonym 'Uzbek' and specifically its connections to the confederation of Dasht-i-Kypchak tribes whose leaders, Abulkhair Khan and Sheibani Khan, claimed to be descendant from Genghis Khan. Associations with the Dasht-i-Kypchak tribes were inconsistent with the task of deposing the Mongolian Empire from the pedestal of historical glory and would imply that modern Uzbeks were of relatively recent and less prestigious historical origin, as they would be seen as the offspring of the 'uncivilized' nomadic invaders of the Maverannahr. Such an understanding was particularly fraught, given that many members of the Tajik population viewed themselves as victims of the Uzbek invasion.[8] In addition, assertions of the modern Uzbeks' Mongol-Turkic origin could foster a sense of historic rivalry between their ancestors and the Ancient Rus', thereby undermining the ideology of peoples' friendship and the concept of the beneficial impacts of Russian conquest on Turkestani society. Therefore, what had to be emphasized was that the Uzbek nation's ancestors had, like the Russians, struggled against the Mongol invasion, thereby making them natural allies of the Russian kingdoms.

In order to address the misalignment between the various attributes of the imagined nation, Iakubovskii proposed decisively separating the etymological and historical links of the term 'Uzbek' (in its modern meaning) from its Dasht-i-Kypchak roots. Asserting the Uzbek nation's ancient ancestry and association with the more ancient and rich Chagatay cultural heritage, Iakubovskii stated: 'For a long time a view dominated (and still persists nowa-

[8] Regardless of any academic or institutional resolution of the question of Uzbeki national historical origins, disputes over this matter already had erupted in the Soviet period at the time of Tajikistan's separation from Uzbekistan, when, in 1929, Tajikistan, which initially was an autonomous part of the Uzbek Soviet Socialist Republic, acquired the status of union republic.

days) that the Uzbek nation descends from nomadic Uzbeks who began to appear in Central Asia in the fifteenth century and conquered it all under Sheibani-Khan's guidance only in early sixteenth century' (Iakubovskii 1941: 3).

Thus it was possible to play down the fact that it was the Dasht-i-Kypchak nomadic tribes who first acquired the name 'Uzbek' and who were descendants of the tribes existing under the Golden Horde. By denying the identification of a people's history with the history of its name, Iakubovskii contended that the Uzbek nation was formed principally before Muhammed Sheibanid's invasion of the territory of contemporary Uzbekistan. According to Iakubovskii, the migration of Dasht-i-Kypchak Uzbeks merely completed the formation of the Uzbek nation, a process that was originally initiated in the eleventh century at the time of the Karakhanids by the early Central Asian Turks.

The logic of Iakubovskii's approach lay in 'distinguishing the environment of the formation of certain peoples from the history of their names' (Iakubovskii 1941: 19). To designate the pre-Sheibani period, he introduced the term 'old Uzbek': 'Does the above mentioned not entitle us', he writes, 'to overcome sheer formalistic considerations regarding the name "Uzbek", to refer to the entire Turkic past in the territory of Uzbekistan to the sixteenth century as "old-Uzbek"? We believe it does' (ibid.: 19).[9] However, sorting out this terminological predicament was only the first step in shaping Uzbek national history. Next, it was necessary to discover the 'greatness' of the Uzbek historical past, first of all, by forming a pantheon of great Uzbek historical figures. And here it became clear that the nation's golden heritage would be constituted mainly by figures from the pre-Sheibani epoch and culture, namely, Alisher Navoi, Ulugbek, and even Tamerlane (albeit with some reservations put forward concerning the ravages committed by Tamerlane in the territories he invaded). By foregrounding Chagatay rather than Golden Horde historical heritage as Uzbek national symbols, Iakubovskii ensured a conflict-free historical background for the relationships between the peoples of Turkestan and Russia: Tamerlane, who defeated Tokhtamysh, the Golden Horde's khan, could be viewed now as a natural ally of the Muscovite Rus' and even as a contributor to the salvation of the Rus' from the Tatar-Mongol yoke.

American Turkologist and historian Edward Allworth held a similar view of why Iakubovskii was so ignorant or dismissive of Sheibanid heritage. In *Modern Uzbeks,* Allworth writes that Iakubovskii's revisionist

[9] Sheibani Khan was the leader of the Uzbek nomadic tribes that invaded the Maverannahr (the region between the Amu Darya and Syr Darya rivers, also known as Transaxonia) at the beginning of sixteenth century.

history of Central Asia was informed by Marxist considerations and a privileging of the position of Russian national patriots against the Golden Horde. This prejudice was exemplified in the notion of the 'Tartar-Mongol yoke', which was held to imperil the national consciousness of the Russian patriot (Allworth 1990: 241). The virtues of Iakubovskii's formula were evident for Russian and Uzbek patriots, and this is why his account of national history was so well received both in Moscow and Tashkent and served as the theoretical framework through which Uzbek national history was canonized. Iakubovskii's formulation could be extended beyond the particular case of Uzbek national history and cleared the way for a flexible treatment of historical data suitable to the search for the ethnogenesis of any nation. Iakubovksii's meta-ethnogenetic approach was implicitly based on the following presuppositions:

(1) The ethnonym of a nation should be considered as a label of no significance from the standpoint of understanding the nation's ethnogenesis and national identity formation;

(2) The importance of ethnonyms, their origins, and initial meanings should be downplayed, while the 'objective' features of the people's *modus vivendi* (e.g. a sedentary versus a nomadic way of life) and dialectal similarities (e.g. the *Turki* language) should be stressed;

(3) Any people who used to live in a given territory should not only be considered as direct ancestors of the nation in question but should also be referred to by the nation in question's ethnonym, especially when both the ancestor and present-day groups spoke Turkic languages. In the case of Uzbekistan, such an ancestor was found in the Karakhanids, the Turkic-speaking dynasty that came to power in Central Asia in the eleventh century, which was far enough in the past for this group to be considered the ancient ancestor of the contemporary Uzbek nation. Some authors discovered Uzbek ancestors in even earlier times with the Turkic Kaganate (Göktürks).

The problem, of course, was not that nations were viewed as descendant from people who had, in the past, happened to live in the same territory, whether or not they shared a common language or genealogy; rather, the problem lay in politicizing the question of national origin in order to legitimate contemporary rule of the national territory and an exclusive appropriation of certain historic symbols. Foregrounding the discourse of ethnogenesis inevitably leads one to retrospectively project present-day political realities onto the past. By treating history this way, it becomes unpredictable, as it is open for constant revision in light of contemporary needs and the demands of political ideologies, in particular. This subsuming of the historical past to the present was not unique to Uzbekistan. Indeed, national histories are characteristically invented for the sake of modern-day political agendas. One

can argue that most of the titular nations comprising the Soviet Union, including the Uzbeks, Tajiks, Turkmens, Kazakhs, and others did not avoid this temptation to mythologize their respective national histories.

Intellectual and Ideological Roots

However, it would be oversimplifying matters to reduce Iakubovskii's concept of national history to only bare political calculations and his desire to please the political authorities. Iakubovksii's ideas can also be seen as stemming from a pre-existing school of thought that predated both his entry into the academy and even the Soviet regime itself. Two figures can be mentioned here: Nikolai Marr and Vasilii Bartol'd, both of whom served as Iakubovskii's academic mentors. Comparing Iakubovskii's and Marr's views, one finds notable similarities. Iakubovskii's approach was not so different from Marr's project of reconstructing a national lineage for the Georgians and Armenians by searching for a single proto-historical ancestor and proto-language, while ignoring constant processes of inter-cultural exchange and borrowing and interpreting borrowings as only corrupting the authenticity of a nation's identity.[10] Iakubovskii and his associates could at least to some degree subscribe to such a view. For example, Iakubovskii downplayed the significance of the advent of Sheibani-led Dasht-i-Kypchak tribes at the beginning of the sixteenth century. Later, Iakubovskii's followers among local academicians in Uzbekistan fiercely denied the influence of Iranian, Arabic, and Turkish cultures on indigenous peoples in Central Asia in the medieval past.

What is more, Iakubovskii and his generation of Russian orientologists were swayed by Marr and Bartol'd's liberal ideas about encouraging patriotic sentiments among national minorities in the Tsarist Empire, a current of thought known as the 'small homeland' ideology that emerged in the 1870s and 1880s. Adherents of this ideology believed that the non-Russian peoples of the Russian Empire — referred to as *inorodtsy* — could be best integrated into and made to be genuinely loyal to the pan-Russian nation-state if they were given an opportunity to acquire a greater sense of their homeland and made proud of their own historical origin and history.[11] After the October revolution, proponents of the 'small homeland' ideology were given carte blanche for the realization of their views, as it was coincident in the Bolshevik program of national self-determination. However, a decade and a half later, the aspirations of scholars and local elites dedicated to the preservation of local peculiarities and self-consciousness began to

[10] On Marr's views, see Slezkine 1996.
[11] For an account of this movement, see Tolz 2009.

diverge from the Bolshevik general line in national policy. The Bolsheviks had opted to launch a radical and comprehensive onslaught on the local religions and traditional practices they considered to be backwards and as threats to Bolshevik political domination and social transformation. By the end of the 1930s, many local elites and intellectuals had suffered purges and repressions that the Stalinist regime justified in the name of the struggle against bourgeois nationalism and its local representatives. It is interesting that the purges coincided with a shift in the political leadership toward embracing pan-Russian national patriotism and a growing rejection of overly critical readings of the tsarist colonial past and the role of Russian tsars. The camp of new pan-Russian nationalists consisting of B. Grekov (Iakubovskii's co-author), V. Shestakov, E. Tarle, S. Bushuev, and others held that Russian colonial rule was a lesser evil for the conquered peoples than Turkish, Iranian, or British rule would have been. In 1934, in a jointly signed statement, Joseph Stalin, Andrei Zhdanov, Secretary of the VKP(b) (All-Russian Communist Party of Bolsheviks) on Ideology, and Sergei Kirov, First Secretary of Leningrad Obkom, criticized the summary of the secondary school textbook (never published) *Istoriia SSSR* (The History of the USSR) presented by the team of authors headed by N. Vanag, who was known as the disciple and follower of Marxist historian M. Pokrovskii, and as being at odds with nationalist historian B. Grekov (Stalin, Zhadanov, and Kirov 1936). These historians contended that Russian history should be written in conjunction with the history of the other peoples populating the Russian empire, a hint which prompted the historians to elaborate a scheme assigning to Russia the role of the older brother of the smaller nationalities. As for Stalin himself, he was totally supportive of neither camp, instead preferring to serve as an arbiter. After orchestrating the campaign against Marxist historian Pokrovskii and his associates in 'vulgar sociologism', which was followed by Pokrovskii's removal from the Directorship at the Institute of Red Professors, Stalin stopped short of permitting nationalists to completely abandon Marxist analysis in the historical sciences. Instead, he established parity between the Marxist and nationalist schools, although only over time, and especially during and after the World War II, he increasingly favoured the latter. Yet, the Marxist historians and critics of the tsarist colonial past like A. M. Pankratova, a convinced critic of Russian colonialism, regained their positions after the death of Zhdanov in 1948, and particularly during the period of Nikita Khrushchev's thaw (Weinerman 1993: 437-448).

It is interesting to juxtapose Iakubovskii and Pankratova, who were to some extent similar while differing in key ways. Both were evacuated to national republics at the beginning of the World War II, she to Almaty and he to Tashkent. Both favoured fostering local national consciousness, though

they differed in how they pursued this goal. Whereas Pankratova primarily drew upon Marxist positions, Iakubovskii mostly primarily drew upon the Russian national patriotic school. Being a pan-Russian nationalist, Iakubovskii nevertheless adhered to the tradition of Russian imperial Orientology of encouraging local nationalism as the most effective means of securing the loyalty of national minorities to the pan-Russian state.

Should Iakubovskii's views be understood as continuations of this liberal tradition of Russian Imperial Orientology? To a certain extent, the answer to this question is yes. Iakubovskii's views opposed those of his teacher, Vasilii Bartol'd, who believed that national and global history evolved toward mutual rapprochement and unification of cultures, under the influence of global and regional powers that bore responsibility for the modernization and development of the smaller nations they ruled. Although Bartol'd was not a Marxist, these views may have resonated with the Bolshevik aspiration to build a society in which national differences would eventually be overcome under the patronage of the Russian proletariat, and with Russians serving as the Soviet smaller nations' 'bigger brother'. Bartol'd's views would have impressed Soviet ideologues, who favoured the national development of the former colonies only insofar as these ideologues embraced Russian domination and the politics of so-called *sblizhenie* (rapprochement with Russian culture). Iakubovskii belonged to this second generation of ideologues that further contributed to the idea of *sblizhenie*. At the same time, he criticized some of Bartol'd's views, especially Bartol'd's identification of various positive consequences of Mongol occupation.

Iakubovskii's views can also be understood as expressing the evolutionism characteristic of nineteenth century social sciences. By and large, Russian, and subsequently Soviet, orientologists, ethnographers, ethnolinguists, and historians failed to leave behind evolutionism, in contrast with their counterparts in Western Europe, who embraced structuralism in the twentieth century. One can say that Iakubovskii's linear vision of Uzbek national history marked a step backward from Bartol'd's more global approach to history as well as his tendency to consider local peoples in Turkestan as being at the crossroads of inter-regional processes and developments.

Iakubovskii's concept of national history was marked by an old fashioned organicist evolutionism that failed to see how the human world evolved through the constant interaction between elements within synchronic local and global structures, the process that, by definition, was transformational and open-ended, with no predestined outcomes. There was some opposition to Iakubovskii's school, but it came from a scholarly camp whose views were distinct only in respect to a specific factual question, namely, who should be considered as the direct ancestor of the modern

Uzbeks: the Karakhanid Turks or the Dasht-i-Kypchak Uzbeks. Interestingly, the debate between these two camps took place in two volumes of *History of the Peoples of Uzbekistan* (Bakhrushin, Nepomnin and Shishkin 1947; Tolstov and Zakhidov 1950) published in Tashkent. The history of this publication resembles a detective story. It is interesting to note that the second volume was published first, in 1947. It contained an account of Uzbek national history starting out from the sixteenth century, with the invasion of Maveranahr by Uzbek tribes led by Sheibani Khan. It would seem likely that the volume's editors (S. V. Bakhrushin and others) wanted to hasten the release of the second volume, expecting objections from Iakubovskii and his followers. There were indeed reasons for this: the volume's introductory article on the historiography of Uzbekistan's peoples gave as its primary references the works of V. Bartol'd, A. Semenov, and P. Ivanov, all of whom considered the era of the Sheibanids as the cradle of the Uzbek people. In order to adjust to this state of affairs, Iakubovskii himself wrote the preface to the first volume issued in 1950. He again reiterated the view that in Soviet historical science 'it became a universally recognized truth that the history of a nation is often older than the history of its name' (Tolstov 1950: 8). This argument once again underpinned the same concept of Uzbek ethnogenesis that he had articulated in 1941.

Iakubovskii's Divided Legacy

The example of Iakubovskii and his concept of national history can be taken as evidence that in the Soviet era, the historical sciences were put in the service of political agendas, Soviet national policy, and, more broadly speaking, the wider program of social transformation and social engineering. Most striking is that Iakubovskii's conceptual project has proven to be quite sustainable and, to a certain extent, continues to be employed by the academic establishment in present-day Uzbekistan. Already by the end of the 1940s, Iakubovskii could witness the offspring of his school. In 1949, the Academy of Sciences of Uzbekistan held a session exclusively dedicated to the state of affairs of the historical sciences, specifically on the subject of Uzbek national history. Although Iakubovskii did not attend the session in person, his presence was nevertheless felt. A number of his followers, local academics like V. Iu. Zakhidov, R. N. Nabiev and others, launched an onslaught against historians whose views directly or indirectly contradicted Iakubovskii's disposition (Zakhidov 1951; Nabiev 1951). The meeting was preceded by another discussion of the second volume of the aforementioned *History of the Peoples of Uzbekistan* in which the volume's authors were subjected to harsh criticism. At the meeting organized by the Academy of Sciences, some of these authors, notably Aleksandr Semenov, were again

subjected to criticism. As a result, Semenov was understandably compelled to admit his 'mistakes', especially regarding his position on the origin of Uzbeks. Nevertheless, his confession did not help him to retain his position in the Uzbek academy in which only a few years prior, in 1942, he had gotten his Doctorate in Sciences, and, in 1943, being elected as a correspondent member of the Academy of Sciences of Uzbekistan. In 1951, he decided to leave Uzbekistan to seek refuge in neighbouring Tajikistan. There, protected and promoted by Bobodzhon Gafurov, the then First Secretary of the Communist Party of Tajikistan, who had a personal interest in the historical sciences and his own aspirations to shape Tajik national history, Semenov was even appointed as Director of the Institute of History, Archaeology, and Ethnography in Dushanbe (Germanov 2009).

However, the major reason for the 1949 academic convention in Tashkent was not the publication of the volume in which Semenov happened to be one of the authors. The convention was organized as a part of the all-Union campaign against cosmopolitanism declared by Andrei Zhdanov, Stalin's right-hand man in questions of ideology. The curious fact was that whereas Moscow's campaign mainly targeted Western influences on Soviet culture and society, it was exploited in Uzbekistan by local scholars like Zakhidov and others who targeted historical accounts of Iranian, Arabic, and Turkish influence on the societies and cultures of Central Asian peoples in the past. This attack against historians who did not share Iakubovskii's view of Uzbek national history had clear nationalistic undertones, and thus for the first time since the crackdown on nationalistic Jadids in the 1930s, local intellectual elites openly expressed national-patriotic sentiments, masking their mood in Communist phraseology and publicly declaring their opposition to 'bourgeois nationalism', and an 'imperialist conspiracy plot'. Against these alien influences, they put forward the notion of *samobytnost'* (originality), through the prism of which, according to the new guidelines, the local peoples of Central Asia were to be characterized in tracing their history. The message of Zakhidov and associates was as follows: We, local peoples (read Uzbeks), have always had our own original culture that only rarely borrows from foreign, especially Iranian, cultures; and those who claim the opposite just deny the Uzbek people their *samobytnost'*. In terms of cultural borrowings, an exception was made only for Russia, whose influence was presented as objectively positive and beneficial for indigenous peoples of Central Asia. Later, with the demise of the Soviet Union, this exception was totally omitted so that the account of Russian influence shared the same fate as the discourse of Iranian influence: both were downplayed by the state-endorsed doctrine of Uzbek national history. From the viewpoint of this post-Soviet development, which marked the rejection by the nascent regime of all bene-

ficial influences that Russian imperial rule might have had on the Uzbek nation, Iakubovskii's master plan can be considered a failure. Zakhidov's encouragement of Uzbek national-historical consciousness was achieved at the cost of repudiating Uzbeks' historical ties with other surrounding peoples and cultures. If 'cosmopolitanism' for Zhdanov implied Western influence on Soviet intellectuals, then Uzbekistani academics like Zakhidov tried to use this campaign against 'cosmopolitans' to condemn those Russian historians who viewed Uzbek culture as having been influenced by the Muslim World, for instance, Iranian and Arabic societies and cultures. In condemnation of this kind of 'cosmopolitanism' Zakhidov and associates were supported by such high-ranking Soviet academics as Iakubovskii. For Iakubovskii, who also tended to downplay the importance of Iranian, Mongol, and Arabic cultural influence in the past upon the formation of the Uzbek national identity, this denial had only one rationale: ensuring that the Uzbeks henceforth and forever more would be seen as the smaller brothers of the Russian nation.

Already in 1949, the claim of *samobytnost'* pronounced by local scholars was so strong that not only past 'bourgeois' orientologists like Bartol'd but even modern-day Soviet historians and orientologists, including A. Semenov, E. Bertels, and others, were blamed for succumbing to cosmopolitanism — that is, presenting past local societies and cultures as being infused by Iranian and other cultures. At the same time, historians like A. Iakubovskii, S. Tolstov, and V. Shishkin were praised for their endorsement of the original, ancient, and autochthonous roots of the Uzbek nation (Nabiev 1951: 49). This nationalist trend in Uzbek history grew stronger over time, especially after the death in 1948 of Zhdanov, who had been a key figure in the establishment of Great Russian patriotism with its respective revision of Russian history, and the death of Stalin in 1953. From this point forward, local national elites began to play their own games, not necessarily always accepting the conflict-free trajectories of national and Russian histories. Local nationalist historians and pan-Russian patriots began to disagree over the question of whether or not the Russian conquest had positively impacted the region's economic and political development and whether or not local states, notably Bukhara khanate, were in decline prior to the Russian conquest. In 1954, the Uzbek scholar K. M. Mirzaev published a book in which he insisted that, prior to the Russian conquest, the Bukhara khanate was in a process of centralized state formation (Mirzaev 1954: 443). Another Uzbek scholar, M. G. Vakhabov, argued that Tsarist Russia did not in fact succeed in fostering domestic capitalism (Vakhabov 1954: 446-447, cited in Weinerman 1993: 446-447). Curiously, before endeavouring to write an Uzbek history, Vakhabov was secretary in charge of ideology for the Com-

munist Party of Uzbekistan. This fact highlights how nationalism had come to pervade both intellectual and political elites in Uzbekistan by the mid-1950s, albeit in the form of revisionist national history.

Five years after the publication of the ill-fated *History of the Peoples of Uzbekistan,* a new four-volume edition, *History of the Uzbek SSR* (1955-1958) (Tolstov 1955; 1956-1958), was published in Tashkent under Iakubovskii's methodological guidance. His doctrine was extended by S. P. Tolstov, who was the chief editor of the new, four-volume edition. It was Tolstov who, as a representative of the Centre, oversaw the editing of the first volume of *History of the Peoples of Uzbekistan* published not before, but after the second volume. After Iakubovskii's death, Tolstov reiterated his senior colleague's contention that 'one should distinguish the history of the ethnonym "Uzbek" from the history of the Uzbek nation' (Tolstov 1955: 167). The same line was adopted by Uzbek historians like M. G. Vakhabov in his monograph *The Formation of the Uzbek Socialist Nation* (1961), which completed the canonization of Uzbek ethnogenesis in the spirit of Iakubovskii's concept. By denying the significance of the ethnonym in the formation of Uzbek national identity, M. Vakhabov concluded that the Uzbeks formed a single nation back in the early second millennium A.D. (Vakhabov 1961: 24).

In the post-Soviet period, the canons of national history postulated by Iakubovskii remained for the most part untouched by Uzbek official historians, while other aspects of national history, mainly the periods of Russian colonization and of the Soviet Union, were radically reconsidered; in fact, as has been previously indicated, this revision began much earlier in the mid-1950s. What post-Soviet scholarship has preserved, however, is Iakubovskii's aforementioned three-fold paradigm of identifying the age of the nation in question, its ancestors, and the core of its national cultural heritage. Present-day Uzbek official historians like, for instance, the current Director of the Institute of History, D. Alimova (Alimova and Zununova 2005), and its former Director, A. Askarov (Askarov 2006), as well several others have been engaged in a long-lasting dispute with Tajik nationalistic historians, notably R. Masov. The subject of this dispute is the question of which of the two nations they represent, Uzbekistan or Tajikistan, should be considered native to the region. In grounding their arguments, Uzbek historians continue to redundantly cite the works of Aleksandr Iakubovskii, invoking him as a proponent of their position. Ironically, Iakubovskii has been equally venerated by Tajik historians for contributing to the discovery of the Tajik glorious past. In both republics, Iakubovskii was awarded the title *zasluzhennyi deiatel' nauk*, or Distinguished Contributor to the Sciences.

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Chapter 14
Ol‘ga Sukhareva and the Academy of Sciences of Uzbekistan

Olga Naumova

Ol‘ga Aleksandrovna Sukhareva (1903-1983) was the leading Russian anthropological expert on Central Asia. As a disciple of the outstanding orientalists Aleksandr A. Semenov and Mikhail S. Andreev, who both followed in the best traditions of Russian oriental studies, Sukhareva had an encyclopaedic knowledge of the humanities, from anthropology to oriental history, Iranian philology, art criticism, and the study of religion. All of her research was based on her own original and sometimes unique anthropological material. A native of Samarkand, Sukhareva began to study the life of the local population when she was 15 years old. She intimately knew these people: their destinies, and all sides of their everyday life. Owing to her faultless Tajik and her command of the Uzbek language, she could detect subtle points of folk life that were inaccessible to most researchers in interviews. Sukhareva’s field material was the result of 30 years of stationary research in one location; in other words, the product of the entire life of a single anthropologist being spent amongst the people she studied. Later, when she left for Tashkent and then for Moscow, she continued her field research by tirelessly supplementing and specifying her knowledge of folk life. Besides her scientific research proper, she was extensively involved in organizing anthropology as a discipline in Uzbekistan: she trained numerous Uzbek anthropologists, oversaw the Department of Ethnography at the Institute of History of the Uzbek Academy of Sciences, organized anthropological expeditions, and conducted large collective research projects with the Department.

In a certain sense, Ol‘ga Sukhareva has been and remains a symbolic figure of the evolution of anthropological knowledge in Uzbekistan. She served as the link between the pre-revolutionary Russian orientalists studying peoples of the Central Asian fringes of the Russian empire and the Uzbek anthropologists of the Soviet period, who studied their own people.

In the first years of Soviet rule, as a discipline in Central Asia, anthropology was mainly the domain of Russian scholars who had begun their academic careers before the revolution. Since 1918, the Turkestani Oriental Institute in Tashkent had been the centre of anthropological training in Uzbekistan; in 1924 it became part of the Central Asian State University as the Oriental Studies Department (20 let *Sredne-Aziatskogo gosudarstvennogo universiteta* (1920-1940) 1940: 12). The Oriental Institute hosted 'a constellation of brilliant orientalists of the highest class' (Litvinskii 1973: 6), including its head A. E. Shmidt (an Arabist, specialist in Muslim culture, professor, and corresponding member of the Academy of Sciences of the USSR, 1871-1939), A. A. Semenov (Iranianist and leading expert on the history of Central Asia, 1873-1958), A. A. Divaev (a remarkable collector and expert on Central Asian folklore, 1856-1932), M. S. Andreev (anthropologist, renowned specialist on Turkestan history, corresponding member of the Academy of Sciences of the USSR, 1873-1948), N. G. Mallitskii (a well-known geographer, historian, and anthropologist of Central Asia), and I. I. Umniakov (well-known orientalist, a disciple of V. V. Bartol'd and S. F. Ol'denburg, 1890-1976) (Lunin 1963: 333-334). Up until 1922, Bartol'd taught a course called 'The History of Turkestan' at the University. Turkologist S. E. Malov, linguist and orientalist E. D. Polivanov, and linguist and anthropologist I. I. Zarubin cooperated with the Institute.

In the 1920s and 1930s, these orientalists prepared a new cohort of anthropological experts on Central Asia, and Ol'ga Sukhareva was one of them. Undoubtedly, M. S. Andreev influenced the formation of this young generation. B. V. Lunin, a specialist in Central Asian historiography, thinks it is possible to claim that, under the guidance of Andreev, a new anthropological school was formed. Besides Sukhareva, the list of Andreev's students included such well-known anthropologists as E. M. Peshchereva, A. L. Troitskaia, V. G. Moshkova, N. N. Ershov, and A. S. Morozova (Lunin 1963: 335, 344). M. S. Andreev was the disciple of V. P. Nalivkin, from whom he learnt that 'the work of the ethnographer demands irreproachable knowledge of languages and, in turn, it is impossible to learn language thoroughly, as well as the soul of a people, without studying that people's life, customs, and folklore' (Litvinskii 1973: 5). Andreev had an extensive and profound knowledge of the field of anthropology and linguistics of Central Asia, Iran, Afghanistan, East Turkestan, and India and was 'an unsurpassed collector of field material' (ibid.: 6). Knowledge of local languages and love of fieldwork were characteristic of his disciples' studies as well.

One of the stated goals of Soviet nationalities policy was to train academically educated staff from the ranks of indigenous nationalities. The generation of anthropologists of Uzbekistan that followed Sukhareva and her

fellow students was represented mainly by Uzbeks. There were practically no Russians among them because at that time no proper attention was given to language training for anthropologists, and without knowledge of the local language, the quality of field research sharply declined. The growth of national consciousness that started in the Soviet period ('Why are our people studied by outsiders? We know ourselves better!') created a favourable background for the 'Uzbekization' of Uzbekistan anthropology and the coming to the fore of Uzbek scholars in regional centres. Whether this shift to local researchers constitutes a new stage in the anthropological study of Central Asia remains to be seen. Only by analyzing the careers of individual scholars and by studying of history of Central Asian anthropology can we lay the groundwork for well-founded conclusions.

Sukhareva made a valuable contribution to Uzbek anthropology through both her research and in training Uzbek anthropologists, but eventually she had to leave Uzbekistan. However, in this paper, I do not attempt to address the relation between the rise of nationalism and the development of anthropology in Central Asia. Instead, I deal with Sukhareva's work in Uzbekistan: the basic direction of her research at the Academy of Sciences of the republic, the atmosphere in the 1950s and 1960s in the academic institute where Sukhareva worked, and the reasons for her departure.[1] It is my hope that the information that I provide in this chapter will be useful for researchers interested in more general problems related to the history of Central Asian anthropology.

Ol'ga Sukhareva began her work at the Academy of Sciences of Uzbekistan in 1946. At that time, she was already an accomplished scholar. After having graduated from the Oriental Studies Department of Central Asian State University in 1925 in Tashkent, she returned to her hometown of Samarkand and worked until 1927 as the head of the teaching department at the Tajik pedagogical technical school for women, as a teacher of Tajik pedagogical courses (from 1930 to 1933), and as a translator for the Tajik state publishing house (in 1933). In 1933-34, she taught the Tajik language at the Philology Department of the Uzbek State University.[2] As an expert on Tajik, she was hired to work on the Latinization of the Tajik alphabet, which took place between 1924 and 1930. At this time, Sukhareva also wrote a textbook on the Tajik language for Russians (*Manual for the Study of the Tajik Language,* which appeared in two volumes), which, until the time of the transition to the Cyrillic alphabet, remained the only manual of its kind (Rastorgueva 1962: 42). Even now, many consider the *Manual* to be best textbook for the Tajik language (Sinitsyna 1986: 44). According to Suk-

[1] For a detailed account of Sukhareva's biography, see Naumova 2004.

[2] From 1960 onward, the institution was called Samarkand State University.

hareva's autobiography, which is in the possession of her daughter, her brochure in Tajik 'Imloi nav' ('New spelling') on questions of Latinization was also published around this time.[3]

The museum period of Sukhareva's life began in 1934 when she took a job at the Samarkand Museum as a senior researcher, subsequently taking a position in the 'Labour and Life of a Woman of Uzbekistan' Department.[4] There, she organized collections on embroidery, weaving, and clothes as well as a collection of new artefacts. With her arrival at the museum, anthropological fieldwork and collection work were enhanced by anthropological research. By the end of 1940s, with Ol'ga Sukhareva's active participation the museum had gathered 5,500 ethnographic exhibits constituting 116 collections. Some, like the collection of Suzanis (famous Uzbek embroideries), were of great importance (Sukhareva 1949: 198-199).

During her stint at the museum, Sukhareva's research interests focused on the study of embroidery. Due to her efforts, the museum's collection of embroideries grew many times over: by 1940 it included 800 objects. In 1940 Sukhareva prepared a manuscript entitled *The History of Samarkand Decorative Embroidery from the Middle of the 19th century to the Revolution* (Sadykova 1975: 169).[5] While working at the museum, Sukhareva was simultaneously preparing her master's thesis entitled *Survivals of Animism among the Plain Tajik* (Sukhareva 1940). Her thesis defence was held on February 15, 1940 at the ethnographic branch of the Philological Department of Leningrad State University. Later, Sukhareva published her thesis as an article in the book *Domusul'manskie verovaniia i obriady v Srednei Azii* (Pre-Moslem Beliefs and Rituals in Central Asia) (Sukhareva 1975). According to Sukhareva's autobiography and her personal records, which are kept at the Institute of Ethnology and Anthropology's RAS Archives, she continued to work at the museum until 1946, and during her last years there she held more than one office: she was also a senior history lecturer at Samarkand Teacher's College (1943-44) and at the Historical Faculty of Uzbekistan State University (1944-1946), where she led courses on world anthropology and the anthropology of the Central Asian peoples.

In 1946 along with her family, Sukhareva moved to Tashkent, where she began to work at the Institute of History and Archaeology at the Uzbekistan Academy of Sciences, first as a senior scientific worker, and from 1952

[3] I am very much obliged to Elena Akbal'ian, Sukhareva's daughter, who gave me a chance to look through her mother's personal archive.

[4] The museum would later be known as the Ikramov State Museum of History, Culture, and Art of Uzbekistan.

[5] This manuscript was approved for printing only after Sukhareva's death (Sukhareva 2006).

onward, as the head of the Ethnography Department.[6] In the 1950s, the department was staffed by, amongst others, S. Inogamov, M. Bikzhanova, K. Shaniiazov, F. Aripov, and I. Dzhabbarov, in addition to laboratory assistants and post-graduate students who also worked there. Judging by the memoirs of T. H. Tashbaeva, who worked in the department in the 1960s, Sukhareva was a rigorous and demanding leader. I think that this can be explained by the titanic number of research projects that were carried out by the department at this time. In the mid-1950s, members of the department worked on the chapters for *The History of the Uzbek SSR* (*Istoriia Uzbekskoi SSR* 1956) and its ongoing fieldwork expeditions for research on collective farm peasantry demanded close attention (Sukhareva 1955a). One result of this research was Sukhareva and Bikzhanova's monograph on the settlement of Aikyran (Sukhareva and Bikzhanova 1955). At the same time, Sukhareva began arduously working on 'The Uzbeks' chapter for the voluminous *Peoples of Central Asia and Kazakhstan* (1962: 165-407). In addition to these projects, each scholar had his or her own research topic and agenda. All of this required meticulous organization of collective efforts.

T. A. Zhdanko kept Sukhareva's letters from 1955, 1956, 1958, 1960 and 1966, which were for the most part all about this collaborative work.[7] The letters are doubtlessly interesting for the discipline's historiographers and demonstrate how the work on this well-known edition was carried out, as well as the enormous organizational and academic work undertaken by Sukhareva. From one of Sukhareva's letters, we learn that in 1955 the department staff discussed a draft version of a chapter sent from Moscow and ultimately rejected it: '[S]eparate mistakes and defects are less of a problem. The structure […] is much less than perfect too. And the language is completely awful. It is uninteresting and hard to read. I recollect the first variant […] [I]t had the same defects; it was intolerably boring and banal, without any living thought' (Sukhareva 1955b). She went on to propose a general plan for her future work:

> It is clear that to write *The Uzbeks* is especially difficult — there is a lot of material, and at the same time nothing profound; all the pieces are isolated. It is necessary to have your own data concerning not just one area but all the major regions or to have an opportunity to fill in the missing data with inquiries. It is probably necessary to or-

[6] At the same time, she led a course on 'Art and crafts of Uzbekistan' at the Historical Faculty of Central Asian State University (personal records).

[7] Tat'iana Zhdanko (1909-2007) was an expert in the history and ethnography of Central Asian peoples. From 1953 to 1988, she headed the Central Asia and Kazakhstan Department at the Institute of Ethnography, Academy of Sciences of the USSR, Moscow. She was on friendly terms with Ol'ga Sukhareva from 1930 onward.

> ganize a group of authors and to fulfil this task together; one person cannot do it alone. How to organize it when we have so much work, I cannot imagine... (Sukhareva 1955b).

Besides managerial problems with *The Uzbeks*, the book also required decisions to be made on methodological issues. 'You are right', Sukhareva writes in her next letter, 'that the most difficult thing is how to make the text ethnographic. The fact is that none of us know how to do that' (Sukhareva 1955c). In the established tradition of ethnographic research in which this line of inquiry was subsumed to historical studies, the primary difficulty was writing about modern life from an anthropological point of view. Few people had any experience studying what is now called the 'anthropology of modernity'. In 1960, recollecting the work on the text of *The Uzbeks*, which had been edited in Moscow, Sukhareva remarked that it was 'an insufficient portrayal of the modern Uzbeks, with a predominant focus on old things. That deficiency was characteristic of our version as well, and during the editing it was amplified, since in many sections it was modernity that was reduced […] It goes without saying that we've inadequately written about modernity because it is much less investigated and is difficult to pin down' (Sukhareva 1960c).

Another deficiency that had to be overcome during the preparation of the edited volume was the archaicization of traditional life. Criticizing the section on social organization submitted by K. L. Zadykhina, Sukhareva remarked: '[D]egrees of kinship are provided with reference to a classification system, i.e. are terribly archaic. In general, everything is made ancient and archaic in such a way that we shall resolutely and, I think, rightly object. [In particular], circumcision (which has no ancient roots in Central Asia, by the way) has been depicted as a straight line 'descendant' from initiation of the most primitive type' (Sukhareva 1958d).

Work on *The Uzbeks* began in 1956: the project plan was articulated, and Sukhareva invited authors from Moscow, Leningrad, and Tashkent to contribute chapters and sections. Each section of the book was to contain a predetermined number of pages (Sukhareva 1956, 1958d). In 1957, research expeditions were organized, and a portion of the chapters were written. Sukhareva discussed various aspects of the work with the authors, corresponded with colleagues from other cities, and submitted contributions to the sections on 'Clothes', 'Public and family life', 'Basic occupations', and 'Cities and settlements'. In 1958, she started to collect the authors' contributions.

In one of her letters, Sukhareva described working with the authors: '[B]efore the discussion of all the sections we all reread several times everything written [by the authors], and the text is revised many times, and only

then it somehow fits in the general plan and becomes exciting. But I am afraid even to tell you the number of pages that we probably shall present. As everyone had to rethink, prove, and reformulate everything pertaining to the issue, entire research projects on each issue result' (Sukhareva 1958d).

One such ambitious research agenda was produced by Balkiz Karmysheva who, as Sukhareva writes, 'has sent us a very good and detailed article on tribes, but similar to me in the case of the "Crafts" section, four times larger than the word limit. She has done so in order to get a verified (first of all, for herself and then for the reader) and consolidated account, as such accounts are non-existent thus far. Something really integral and new resulted' (Sukhareva 1958c).

By the end of May 1958, almost all of the chapters had been collected. Sukhareva wrote to Tat'iana Zhdanko:

> The first stage of the work — collecting all of the six sections — has come to an end […] I feel that we burden you with the cumbersome task of reading all of our hundreds of pages, but there seems to be no other way out. Still I am sure that due to the fact that each issue has been thoroughly explored and all the relevant facts have been accounted for, the final versions will be much better. Certainly, the path we have chosen is more difficult and longer, but it provides not just an overview of the published materials but an entirely new account of what has not yet been published (Sukhareva 1958e).

Ol'ga Aleksandrovna subsequently began 'a serious editing' of the collection of articles:

> As the head of all this work, I am working with authors and with the editors of separate sections. For example, I have talked many times with Vakhabov[8], who finished editing the entire first section ('Ethnic History'), when he was working as the author on separate parts of this section and afterwards, when he merged into single whole all the parts written by S. Inogamov, Karmysheva, and Zadykhina. Just now he is correcting this text according to my remarks (Sukhareva 1958e).

After being revised, the section was supposed to be sent to Moscow to the Central Asia and Kazakhstan Department of the Institute of Ethnography, and then back to Tashkent to be further discussed and edited.

Despite all of the difficulties ('it has entirely exhausted me'), Sukhareva was very pleased with the result: 'Though everything is certainly still raw, I like it; it seems very interesting. It will be uneasy to cut it — we have

[8] M. G. Vakhabov was a doctor of historical sciences and an expert in Central Asian modern history. From 1951 to 1955, he served as the chief of the Department of Modern History at the Institute of History and Archaeology of the Uzbekistan Academy of Sciences.

exceeded the limit too much, but each issue has been thoroughly discussed and explained according to the current criteria. […] I am still not sure how it will turn out eventually, but now I am quite satisfied' (Sukhareva 1958e).

Sukhareva planned to prepare the collected texts over the next two years for publication in Tashkent as two collective monographs: *The Uzbeks* (appearing in Uzbek) and *Essays on the Ethnography of Uzbeks* (appearing in Russian).

The year 1958 was very intense for Sukhareva. She spent two months editing V. G. Moshkova's book on Central Asian carpets and preparing it for publication. Valentina Moshkova was Sukhareva's classmate. From 1929 to 1945, Moshkova collected valuable data on carpets and carpet-making, but she had no time to publish it. At the end of her life she was very ill. Sukhareva took Moshkova home and looked after her, and Moshkova died in her hands in 1952 (Sinitsyna 1986: 105). After her death, Moshkova's records were archived by A. S. Morozova, who was a close friend of both Moshkova and Sukhareva. Sukhareva was the editor-in-chief of the resulting monograph; however, she took an active part in completing the text by, for instance, selecting illustrations. Work on this book most likely began in the mid-1950s. In 1956, Sukhareva wrote to Zhdanko: '[Morozova] is working at it [Moshkova's book] quite successfully. We have prepared excellent tables on [carpet design] elements, and are preparing the photographs. This year [one chapter of the book, named] *Carpet-making in Uzbekistan* will be completed, and next year, *[Carpet-making] in Turkmenia*' (Sukhareva 1956).

Sukhareva planned to finish working on the book in 1959. However, the president of the Uzbek Academy of Sciences included Moshkova's monography in the list of the works published for the congress, and Sukhareva and Morozova had to work 'very much and intensively' in order to finish the book in two months: 'Anna Sergeevna [Morozova] has worked quite a lot, and I quarrelled with her over this matter many times […] almost to the point of bringing us both to tears. Also we need to make a lot of corrections, and in some places, together with Anna Sergeevna, to rewrite some portions anew' (Sukhareva 1958c).

Moshkova's book was sent to the publishing house by autumn 1958. Exhausted from completing the book, Sukhareva was hospitalized for two months with severe headaches: 'I have "finished off" my head in editing *The Carpets* (Sukhareva 1958f).[9] However, Moshkova's *The Carpets of the Peoples of Central Asia at the End of the Nineteenth and Beginning of the Twentieth Centuries* (Moshkova 1970) would not be published until 1970.

[9] Besides preparing the collected volume, in 1958 she also wrote 250 pages of her own book *Bukhara*.

Besides undertaking collaborative projects and administrative work, in Tashkent Sukhareva continued to work on the research she had begun in Samarkand. She published some papers on national decorative arts and crafts, on Central Asian headdresses, and on the cult of Muslim saints (Sukhareva 1948, 1950, 1953, 1954; Sukhareva et al. 1954). But her most important and favourite work from the Tashkent period became her anthropological research on the city of Bukhara.

Sukhareva mentioned several times that it was M. S. Andreev who first gave her the idea of doing a monographic study of a Central Asian city, and of Buhkara in particular. In 1940 he headed an expedition of the Central Asian State University, which included M. S. Iusupov, N. V. Rusinova, O. D. Chekhovich, and L. I. Rempel', to Bukhara. However, the group's work was interrupted by World War II. When Sukhareva moved to Tashkent, Andreev brought her into the Bukhara project, and in 1947 they went to Bukhara together. Afterwards, Sukhareva made trips to Bukhara each year until 1950, subsequently returning in 1956 and 1960. In 1948, she also worked in Qarshi and Shahrisabz.

Work on the Bukhara book was pursued in parallel with a number of other activities. In her letters to Zhdanko of 1955, 1956, and 1958, Sukhareva repeatedly mentions the delays and obstacles preventing her from working on her chosen research subject. The Institute was pressing her to finish the Bukhara project, but they would not grant her a sabbatical or any free time to work on it: '[T]hey just persecute me here with deadline violations even though everyone knows that the delays are due to the same administration instructing me to do other work' (Sukhareva 1955c). Sukhareva was upset over the slow work on the chapter 'The Uzbeks' for *Peoples of the World*: 'Having handed over "The Uzbeks", I can take time off to finish *Bukhara*, for which I need no less than six months, completely freed from other tasks' (Sukhareva 1958b). She would subsequently write, 'I am already on my last breath, tired, eager for *Bukhara*' (Sukhareva 1958d).

Petitions from Moscow did not help. During an inspection of the Institute by the Commission of the Academy of Sciences of the USSR, Sukhareva discussed the issue with academician M. N. Tikhomirov, who was the head of the commission: '[H]e has supported me tremendously, approves of all my ideas, and has congratulated me on my choice of research subject. And yet, it is more important perhaps that he has told the [Institute's] administration of the necessity of creating conditions for me to complete this work' (Sukhareva 1956).

In the same letter, discussing her choice of research subject, she recollects a comical episode:

> Another member of the commission, I. I. Smirnov, declared at a session of the Academic Council of the Institute that the deficiency in our plans is the absence of research on topical and controversial issues, and that the only topic of such kind is the 'Late Feudal City' [i.e. *Bukhara*]. Our colleagues timidly reminded us that there were topics of great significance such as collective farm building, but Smirnov stuck to his guns. Despite the comic effect of the statement, I was very grateful to him. As a result, I think I will get a whole year (Sukhareva 1956).

However, Sukhareva would not be granted her sabbatical.

Despite her extensive commitments, Sukhareva had time to write articles in which she attempted to solve the problems posed in her primary research projects. Thus, in early 1958 she asked Zhdanko whether it was possible to publish her long paper (over 100 pages) on Central Asian crafts, entitled 'On the Question of the Genesis of Professional Cults' (Sukhareva 1960b), in which she expressed her 'long-cherished ideas on crafts, embroidery art, shamanism, and sorcery related by a common idea, a common conceptual scheme' (Sukhareva 1958c). The same year she proposed that Zhdanko publish an issue of *The Central Asian Ethnographic Article Collection* devoted to crafts, including articles from P. A. Goncharova on gold seamstresses, N. V. Rusinova on printed fabrics, M. A. Bikzhanova on the Tashkent jewellers, and A. K. Pisarchik on builders and on Nurata weaving. Sukhareva contributed her own papers on weaving, moulding, and jewellery-making. Evidently *The Central Asian Ethnographic Article Collection* had not been published, as some months later Sukhareva suggested that Zhdanko publish her article on metal moulding in a collection of anthropological articles (Sukhareva 1971). At the end of 1958, she submitted one more paper on fibre processing (Sukhareva 1958g).

Her work on Bukhara went on for 15 years, ultimately resulting in her habilitation thesis *The Late-Feudal City: Bukhara at the End of the 19th and the beginning of the 20th Centuries* (Sukhareva 1962a), which she successfully defended in Moscow in 1962. The collected materials were published in four books (Sukhareva 1958a, 1962b, 1966a, 1976).

Among the many manuscripts Sukhareva submitted for publication in Moscow, there was her well-known booklet *Islam in Uzbekistan* (Sukhareva 1960a). Initially, this brochure was written as a chapter for the monograph *The Tajik*, which was to be published by the Tajikistan Branch of the Academy of Sciences of the USSR in collaboration with the Moscow Institute of Ethnography. The war interrupted this plan, and the monograph remained unfinished (Pisarchik 2002: 21). Sukhareva attempted to publish this chapter as a separate edition in Uzbekistan. It should have been reduced and adapted

for the general public before publication, but Sukhareva wanted to retain its academic character. In 1955, she wrote to Zhdanko:

> Is it possible to publish my *Islam*, which Gleb Pavlovich [Snesarev] reviewed? It went over very well here and will be published in an abridged version, as it appears in a series intended for a wide reading public, whereas my text is essentially academic and is too large — over 100 pages. The chapter that I wrote for the monograph *The Tajik* and that was used by me in the preparation of this booklet […] was favourably reviewed by Krachkovsky […] who noted that the material is quite new and portrays not Islam in general, but its specific forms in Central Asia. This is repeated in Gleb Pavlovich's review too […] If my work can be published by you as an academic work, I will provide all the references and add some interesting material on pre-Islamic beliefs […] I could finish the manuscript in such an expanded form by the end of the year (Sukhareva 1955b).

Apparently Zhdanko responded positively to Sukhareva's proposal because a month later she wrote that she was very much pleased with the favourable reception her offer had met, as she did not want to publish the simplified version of *Islam*. She planned to revise and amend the text, to expand it to 150 pages, and to provide illustrations. 'After all, this subject is very close to my interests and is fascinating to me', she concluded (Sukhareva 1955c). However, for some reason the publication of *Islam* in Moscow was not realized, and Sukhareva renewed her attempts to publish it in Tashkent. In 1957, the publishing house of the Academy of Sciences of Uzbekistan planned to publish the text: 'My *Islam* has brought unexpected popularity to me […] You see in all the Soviet Union now nobody is engaged with this subject, neither in the study of Islam generally, nor in Islam of various peoples' (Sukhareva 1957).

However, in late 1957, the publishing house drastically cut back on humanities books, and *Islam* was dropped. Nevertheless, Sukhareva had reason to hope the text would be published in 1958: '*Islam* will rise [sic] on a crest of the wave coming from Moscow: the letter has been received from some central authorities, in which the absence of anti-religious work (and lack of the relevant research) on Islam is underlined. A draft decision of the Presidium [of the Uzbekistan Academy of Sciences] is prepared, where one of the points deals with the publication of my *Islam*' (Sukhareva 1958b).

As a result, *Islam in Uzbekistan* was published in Tashkent in 1960 (Sukhareva 1960a). The brochure, as was indicated in the preface, 'was designed for [anti-religion] propagandists and for the broad reading public'. In fact, the book retained its academic character, but according to Soviet-era standards, it had been supplemented with an 'antireligious' introduction.

Despite this ideological influence, the text remains a classical anthropological work on everyday Islam.

Sukhareva's work at the Academy of Sciences of Uzbekistan was intensive and productive. However, conditions for her research activities were not favourable there. She had uneasy relations with the director of the Institute, Ia.G. Guliamov.[10] The reasons for this are difficult to determine. Most likely, Sukhareva's independent character had something to do with it. She would defend her opinions without looking to her bosses for direction. In her correspondence, she complained about the institute's 'stuffy atmosphere'. Guliamov routinely interfered in the work of the various departments of the institute: he hired researchers without consulting department heads and ordered laboratory assistants to work on particular research projects of his choice. Not a single one of Sukhareva's suggestions for staff additions was accepted. She was brave enough to fight the director during the institute's communist party bureau meetings, although she was not a member of the party. She did the same during Academic Council sessions. She stated her claims openly, for example, when Bikzhanova's dissertation thesis was not admitted for defence, and when the administration sent a post-graduate student — who the department wanted to send to Moscow for training — to gather cotton. In 1958 during a meeting of the Academic Council, Guliamov charged Sukhareva with failure to comply with the state plan. Specifically, he accused her of taking too long to complete the Bukhara project and of engaging in 'personal enrichment'. By 'personal enrichment', Guliamov was referring to the publication of Sukhareva's translation of Aini's story *Death of the Usurer* (Aini 1957) in 1956 and her public lectures on Islam. As she wrote to one of her correspondents, she actually gave four lectures (which had been, she recollected, 'a great burden') but entirely free of charge. The bias of the director's statement was so obvious that the Academic Council did not support him. Later, Sukhareva confessed that it was possible for her to maintain her equilibrium and calmness because this 'slander', as she called it, was so ugly and improbable (Sukhareva 1958g). However, she would write, 'Of course [this atmosphere] is an obstacle for my work, and it will remain. It has disturbed me very much; you see, all work at the Academy is fraught with obstacles that are completely irrelevant to the research issues proper' (Sukhareva 1958g).

On several occasions, Sukhareva requested that the administration relieve her of her post as department head. In 1967, she retired and moved from Tashkent to Moscow to live with her younger daughter. Before her

[10] Ia. G. Guliamov was an archaeologist and an expert in Uzbek ancient history. From 1956-1959, he was the acting director of the Institute of History and Archaeology of the Uzbekistan Academy of Sciences.

departure from the Institute of History and Archaeology of the Uzbek Academy of Sciences, she wanted to be assured that her successor would not disrupt the work of the Department: 'Taking into account the situation at the institute, I wanted to refrain from naming the candidate (I am afraid they might on Ia.'s [Guliamov's] orders opt for the contrary), but they persistently asked me to recommend someone, and I have recommended Shaniiazov. He is a decent person and the only one that is heartily engaged with the young generation [of researchers]' (Sukhareva 1966b).

Sukhareva left her position only when K. Shaniiazov was commissioned as the department head. She also left her post-graduate students behind in Tashkent. In the same letter she wrote, 'I urgently have to complete my tutorship with my post-graduate students and junior researchers who are working on their dissertation theses' (Sukhareva 1966b).

However the director and the administration of the institute by that time so opposed Sukhareva that after her departure they required almost all the post-graduate students to change their dissertation topics and to find new supervisors for their studies.

When Sukhareva left Uzbekistan she was full of plans for research, and she was able to continue her fruitful work. As she was a well-known and widely acclaimed scholar, she received a position at the Central Asia and Kazakhstan Department (Institute of Ethnography of the USSR Academy of Sciences), where she worked until the end of her life. She prepared her last book on Bukhara for publication and wrote a score of articles for thematic collections on Central Asian settlement and dwellings, beliefs, family, and other topics. During her final years, she researched Central Asian clothes — the general problems of their origin, geographic spread, and history in particular regions. Living in Moscow, she continued to help Central Asian scholars: she supervised post-graduate students and edited manuscripts from Tajik and Uzbek anthropologists. Overall, Sukhareva made substantial contributions to Central Asian anthropology and was one of the leading anthropologists of Central Asia in the Soviet era. The research of Sukhareva and others from her generation forms a unique and distinct stage in the history of Central Asian anthropology.

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Chapter 15
Tragedy of a Soviet Faust: Chaianov in Central Asia

Alexander Nikulin

We do not know if socialism with a human face is possible, but due to the work of Aleksandr Chaianov, we can be sure of economics with an anthropological face. Chaianov not only had a unique ability to create elaborate analytical models of the economy, based on abstract, mostly marginal diagrams; he also always refined his analysis by thoughtfully considering sociological and anthropological empirical data. This is why Chaianov, a professional economist, is essentially perceived as an anthropologist and sociologist in modern social sciences (Durrenberger 1984; Sahlins 1972; Shanin 2009).

Whereas Chaianov's extensive and fruitful studies of the agrarian economy of Western European countries and European Russia are quite well known (Bourgholtzer 1999; Chayanov 1986), his research on Central Asia has received much less attention and scrutiny (Nazarenko 1988: 35-38). Chaianov visited Turkestan (as the territories of present-day Uzbekistan, Turkmenistan, Kyrgyzstan, and Tajikistan were collectively referred to and administratively governed in Russia at the beginning of the twentieth century) in 1915-1916 and 1926-1927 and Kazakhstan from 1934-1937. Chaianov's travels in the Central Asian territories resulted in a number of publications that will be examined herein.

The research that Chaianov undertook in Central Asia is interesting primarily from two points of view. First, aside from throwing more light on the peculiarities of Chaianov's research methodology, his work in and on Central Asia demonstrates the scope and limits of empirical fieldwork in Central Asia in early Soviet times. Despite the popularity of collective farm studies by that time, some of this genre's vital aspects were not open to scrutiny. Second, Chaianov's Central Asian texts provide significant descriptions of the policy and ideology of the Russian centre towards its Asian possessions and Chaianov's voluntary and involuntary participation in all of this. Whereas in the 1920s, Chaianov participated in studies of Turkestan as a free scientific consultant, in his studies of Kazakhstan in the 1930s, Cha-

ianov worked as an exiled convict, and he was eventually executed in the heart of Asia at the height of Stalin's purges in 1937.

This chapter consists of three parts. The first part is devoted to an analysis of Chaianov's Turkestani scientific legacy of the 1910s and 1920s; the second part provides a detailed analysis of Chaianov's last published work on cattle breeding in Kazakhstan; the third part is a biographical study of Chaianov's life as a political exile in Kazakhstan from 1934 to 1937.

Turkestan, 1917-1927: Water Resources and Cotton Pricing

Chaianov first visited Central Asia in 1915, but this visit is hardly documented. At the time, World War I was raging, and the Russian government was pursuing a policy of tough regulation and economic control, especially in regards to agricultural production. Because of the dissolution of global agricultural trade, which, among other things, led to a blockade of Russian grain exports and cotton imports, the Russian government sought to elaborate a program of autarkic agricultural production.

As a producer of cotton and products for the Russian market, Turkestan became an object of targeted studies for scientific-administrative commissions of the Russian empire. Chaianov, at that time a 28-year-old professor of agrarian economics, was studying water resources in Turkestan and found himself on one of these commissions. His research activities resulted in his article 'Studies of the theory of water resources' (Chaianov 1917), which Chaianov began in 1915 in Samarkand and completed in 1917 in Moscow. In this work, Chaianov focused on the problem of irrigation. Before writing the article, Chaianov had already accumulated some experience in this field, for as early as 1908, during his post-graduate period in Italy, he studied the economy of the oldest European irrigation systems in Lombardy. Attempting to first abstractly generalize European and Asian irrigation systems, Chaianov relied on the model of the oasis in order to study irrigation, as the German agriculturalist Von Thünen had suggested. This oasis model was complemented with descriptions of the soil properties and water resource replenishment. Chaianov's plentiful diagrams demonstrated how crop yields depended on irrigation levels; gross revenue and net profit (both when water is free and when there is a charge for it); how competition induces expansion of the irrigated area; and the yield, crop capacity, and sowed area achieved with different sizes of irrigated areas.

In regards to institutional problems, Chaianov called attention to the importance of water ownership, traditional and scientific regulation of water resources, popularizing water use regulations, the problems of compromise in the distribution of water, and, finally, the problem of water tariffs as a means of striking a balance between traditional order and scientific-rational

estimation. In the final stage of his studies, Chaianov stressed the significance of empirical anthropological research, stating that without empirical studies 'the theory of water use tariffs, just like the whole theory of the total water economy, remains in an absolutely rudimentary state' (Chaianov 1917: 8).

Here we can see that, as was often the case in his work, Chaianov begins the first stage of his analysis with refined economic outlines of the problem, devoting the subsequent stages to a logical, institutional, and, above all, empirical and anthropological elaboration of the object of his research.

In the late 1920s, as an influential professor, author of several fundamental monographs, and director of the Research Institute of Agricultural Economy, Chaianov returned to Central Asia as a field of study. His renewed interest resulted in the report 'Theoretical foundations of estimating the expediency of irrigation systems' (Chaianov 1926) and the article 'On the prime cost of cotton-wool on the farms of Central Asia' (Chaianov 1927). Chaianov's report was delivered on September 28, 1926 at a session of the Cotton Department in the presence of a number of leading economists, engineers, and specialists on Central Asian irrigation systems. The by now 10-year old 'Studies on the theory of water resources' (Chaianov 1917) provided the basis for Chaianov's report. To the information contained in the latter, Chaianov added a new significant component; he proposed a theoretical formula for estimating the profitability of irrigation.

Chaianov's report roused mixed feelings among the session's participants. On the one hand, they were impressed by Chaianov's variety of marginal models; on the other hand, they criticized the report for applying unlikely theoretical models to the reality of irrigation work. They declared Chaianov's formula for estimating the profitability of irrigation to be too schematic. In his concluding remarks, Chaianov agreed with this criticism, expressing his personal interest in undertaking empirical studies of the economics of irrigation systems.

Generally speaking, Chaianov and his institute were intruding into a research field that had been quite fruitfully and thoroughly studied in the 1920s by a group of Soviet scientists specializing in Central Asia. As early as 1920, the Soviet authorities began to establish special research organizations in Turkestan led by well-known economists such as G. N. Cherdantsev, N.N. Kazhanov, N. K. Iaroshevich, Iu. I. Poslavskii, V. V. Rusinov, and S. F. Arkhangelskii. These research organizations conducted a census of Turkestan and prepared a collection of articles entitled *The Central Asian Economic Region* (Cherdantsev and Poslavskii 1922). By 1927, 10 monographs

had been published in the 'Modern Kishlak in Central Asia' series (see also Bobrovnikov, this volume).

However, these experts on the economy of Central Asia sometimes lacked the wider horizon and deeper theoretical background necessary to explore Central Asia through the prism of different branches of science and, overall, to tie it in with the Russian and international socio-economic situation. This is why it was Chaianov's Moscow-based Research Institute that was chosen by the Cotton Committee to undertake a special examination of the problems of cotton-wool on Central Asian farms, as the Institute by this time had become the leading research centre for pricing agricultural products in the USSR and abroad.

In order to set the 'Soviet price' of optimal cotton profitability both for the state and family farmsteads in Soviet Central Asia, Chaianov analyzed statistical data on family budgets in cotton-producing households (83 budgets in total). One of the main problems of the study was that the prime cost of cotton production widely fluctuated, and, it seemed, within these boundaries it was impossible to find any regularity.

Nevertheless, Chaianov managed to solve this problem in a rather unexpected way. He used an American research paper that discussed a five-year study of the prime cost of wheat in North Dakota, where the prime cost of one bushel of wheat also widely fluctuated, from 0.5 to 8 dollars. The American methods for analyzing the prime cost of Dakota wheat revealed a clear pattern. By adapting the American method of calculating prime cost, Chaianov was able to generate stable distribution curves for Soviet cotton farms. As a result of his analysis, he concluded that the mean prime cost of cotton-wool on rented farms was 4 rubles and 83 kopecks, and on well-off farms it was 4 rubles and 51 kopecks. Chaianov pointed out that in the conditions of expanding cotton production in Central Asia brought on by the first five-year plan, any stable forecasting of the cotton prime-cost would depend on a variety of factors, and it would only be possible to analyze these factors through extensive empirical studies of cotton-producing family farms.

The toughening of Soviet policy in the late 1920s had fatal effects on the research being carried out by Chaianov and the institute. Beginning in 1928, Chaianov and his colleagues were sharply criticized as ideologists of the kulaks and supporters of the petty bourgeois policies of Stalin's opponent, Nikolaj Bucharin. In 1929, Chaianov was removed from his post as Director of the Institute of Agricultural Economics (Solomon 1977). In these conditions, it was out of the question for Chaianov to continue his empirical studies in Central Asia, all the more because agriculture in Central Asia was being forcibly collectivized at the time.

In 1930, during a purge of the intelligentsia, Chaianov was arrested in connection with the trumped-up case of the so-called Labour Peasant Party and was sent to prison for five years (Krugov 2009). As paradoxical as it may seem, this arrest served as a prologue to Chaianov's peculiar return to studying Central Asia. After four years in a Russian prison in Yaroslavl, Chaianov was exiled to Asia to serve his last year. In the Asian prison, he managed to return to his research activities. However, his field of study was no longer Turkestan but Kazakhstan, as Chaianov was exiled to the capital of Kazakhstan, Alma-Ata.

Kazakhstan 1934-1935: *Kolkhoz* cattle-breeding

For a long time, it was believed that Chaianov wrote his last research papers in 1929-1930, before he was arrested. Chaianov's rehabilitation during perestroika made it possible for scholars to gain access to data related to the last years of Chaianov's life and his work in prison and exile. It was discovered that from 1934 to 1936 Chaianov managed to carry out research on the agricultural institutions of Alma-Ata, and 1935 witnessed the publication of Chaianov's brochure 'The main issues in organizing *kolkhoz* cattle-breeding in Kazakhstan' (Nusinov 1935).

Chaianov's authorship was not mentioned in the brochure; instead it was indicated that the brochure had been written by a team of researchers from the Kazakh research institute of agricultural economy and had been edited for publication by the assistant director of the institute, I. S. Nusinov. The exiled Chaianov's name could not be mentioned, even though both the institute's archives and a special investigation conducted by the authorities of the interior in 1937 and 1956 conclusively proved that it was Chaianov who wrote the brochure.

The story of the brochure is absurd and tragic. In 1935, the Soviet initiated an All-Union agricultural exhibition that was intended to demonstrate the achievements of the newly established *kolkhoz* system. Each region and republic of the Soviet Union was to prepare a series of materials about its achievements in the sphere of agriculture. For its exhibition, Kazakhstan decided to present a special analysis of Kazakh cattle-breeding techniques, relying on a vast collection of empirical data collected by Kazakh researchers in Northern Kazakhstan in 1934-1935.

Chaianov was asked to write the analytical text for the exhibition, as he was the most qualified expert in Alma-Ata. Chaianov worked with the materials and wrote the text in the spring of 1935. In the summer of the same year, the brochure was released. The following autumn, a scandal erupted, and the brochure was declared harmful and an act of sabotage. The 3,000

copies in circulation were withdrawn from shops and libraries and almost completely destroyed.

At that time, it was dangerous to objectively address the problems of cattle-breeding in the USSR. In response to forced collectivization, peasants preferred to annihilate their livestock rather than hand it over to the *kolkhoz*. When peasants actually complied and turned over their cattle to *kolkhoz* farms, the cattle typically received worse care. As a result, the first five-year plan for cattle-breeding failed disastrously. Comparing 1929 and 1934, meat production dropped by almost 50%. At the time, in a report, Stalin angrily commented that, in general, cattle-breeding, in comparison with field crop cultivation, was a backward branch of economy (Stalin 1934: 44), and moreover that, during the first five-year plan period, kulak public enemies had particularly damaged cattle-breeding (ibid.: 45). In 1932-1933, the whole country was hit by a devastating famine due to the total appropriation of agricultural products for the needs of industrialization. Famine was especially rampant in traditionally fertile regions of the USSR: in Ukraine, the South of Russia, the Volga region, Southern Siberia, and Northern Kazakhstan (which is where, in 1934-35, the research analyzed in Chaianov's brochure was carried out).

As Stalin noted in his report to the 17th congress of the Communist Party, during the second five-year plan period, at which point the territory of the USSR was blanketed by a network of collective farms, the problem of cattle-breeding 'was at the moment as urgent as was yesterday the successfully solved grain problem' (Stalin 1934: 47). Stalin's government expected *kolkhoz* cattle-breeding output to soar. To this end, Chaianov's brochure, too, opened with a standard ideological statement: 'Socialist cattle-breeding in Kazakhstan shows a dramatic upsurge' (Nusinov 1935: 3); however, several lines below this the reader was warned that 'the subject matter of this paper was chiefly the faults and failures (alongside the achievements of the leading *kolkhoz*es)' (ibid.: 4). Further, throughout the book, Chaianov's otherwise objective and sound analysis of Kazakhstan's cattle-breeding was occasionally punctuated by required ideological references to the directives of Stalin and the Party.

This brochure was remarkable first of all for its objective social analysis, which was atypical for works from the 1930s USSR. The brochure was written neither by a Soviet apologist nor an anti-Soviet dissident. It was a research paper by an agricultural professional who sought to be as impartial and clear as possible in his analysis of the state of cattle-breeding in Northern Kazakhstan. However, in their size, economic indicators, organizational forms, and natural conditions, the Kazakh *kolkhoz*es examined in the study were typical for the vast forest-steppe territory of the USSR, spreading from

the steppes near Saratov and Samara to the foothills of the Altai Mountains and Siberia. In his brochure, Chaianov repeatedly remarked that his analysis provided a kind of snapshot of Soviet *kolkhoz* cattle-breeding. Let us take a closer look at this snapshot.

Chaianov's brochure consists of ten short chapters. The first chapter gives a general description of the 27 examined *kolkhoz*es of the Poludensk Machine and Tractor Station (MTS), located in the Bulaev Region of the Karaganda oblast, a territory in Northern Kazakhstan along the border of the Russian Federal republic. In these *kolkhoz*es, which were established between 1929 and 1934, the rural population primarily consisted of Kazakhs and Russians, with two *kolkhoz*es being predominantly composed of Germans. *Kolkhoz* activities centred on spring wheat and meat-and-milk livestock-breeding.

From its very first pages and throughout Chaianov's brochure, we discover that the new *kolkhoz* system, created in the name of total equality, inherently contained diverse and considerable socio-economic differentiation. Thus, eight *kolkhoz*es cultivated areas of less than 500 hectares; twelve *kolkhoz*es cultivated plots ranging from 500 to 1,000 hectares; and four *kolkhoz*es farmed a plot exceeding 1,000 hectares.

Differences in the scope of cattle breeding were even more pronounced. *Kolkhoz*es in which there were over 200 heads of publicly-owned cattle were considered to be large; those with less than 20 heads of cattle were considered to be small. Medium-sized *kolkhoz*es had about 50 heads of cattle. To organize a profitable farm, it was necessary to have a certain minimum number of cattle, and three *kolkhoz*es had such small herds — from 6 to 12 heads — that they were not selected as sites for special dairy farms.

Differences in the scope of livestock loss by collective farms were also impressive. As of November 1, 1934, losses were as follows: on the best farms the loss rate was 0.3%; on medium-sized farms, 11.6%; and on the worst farms, 51.4%. On average, cattle loss was 27.6%. At the same time, Chaianov notes that in family households the average was 8.4% — in other words, more than three times lower than on *kolkhoz* farms. Addressing this matter, Chaianov commented:

> This state of affairs is caused by bad technical facilities and the absolute organizational slackness of said dairy farms. We know that *kolkhozniks* carry the newborn calf into the warm hut, while many dairy farms up to now have not even started building warm maternity quarters […] On these farms the newborn calves lie in the cold for hours and many of them die of pneumonia. The dampness of the litter, absence of clean air, untidiness, negligence […] all these fac-

> tors ruin the calves in the later months of their growth (Nusinov 1935: 20).

A separate part of the brochure was devoted to the analysis of building and equipment provision on dairy farms. Chaianov notes that in Northern Kazakhstan winters were severely frosty; the temperature in the region sometimes dropped below -30 ˚C, while the barns in most cases were poorly heated. Typically, the barns were made of birch poles on bare ground, with no ceiling.

From 1930 onward, the *kolkhoz*es in Poludensk built several standard cow-houses, based on recommendations from Moscow and Kolkhozstroi's designs. This design proved to be extremely unsuccessful. It consisted of a single wall made of thin poles coated with clay on one side and a cement floor, without a ceiling. In winter in Northern Kazakhstan, the temperature in this structure was as low as in the open air. In winter and even in summer, the cold cement floors of such quarters caused extensive catarrhal diseases among the cattle and especially the calves, which brought about numerous animal deaths through pneumonia.

With regards to the state of equipment on these farms, matters were even worse. Practically none of the farms had enough milk cans, and the milk gauge was a luxury, with only some farms in possession of the tool. There were no drinking bowls for calves and no thermometers. The farms needed barrels, tubs, and cauldrons. Not a single farm had balances. In Chaianov's opinion, this was due not to excessive material costs but to a lack of initiative and responsibility among cattle-breeders (Nusinov 1935: 31).

However, the main organizational problem on the majority of the *kolkhoz*es was the fluctuation of cattle-raising workers. In many *kolkhoz*es, the masters of cattle breeding teams were replaced several times each year. As they lacked knowledge of zoology, they were sent to short term courses, but this was typically not sufficient. Often, organization of labour in the teams was not a unified process, being torn into separate functional fragments; as a result, no single individual was responsible for the well-being of the livestock.

Kolkhoz farms were tasked with providing a rational amount of employment per worker and on this basis, analyzing the productivity of individual workers. However, in the *kolkhoz*es of Poludensk, these problems were solved 'by eye' (i.e. just roughly). Surely, there were official standards set out in the guidelines of Narkomzem (Soviet Ministry of Agriculture), but actual conditions on Poludensk farms differed from these quotas.[1] In Poludensk, a single woman working as a milkmaid was overworked, on aver-

[1] According to this standard, one milkmaid was to attend to 10-12 cows, and one cattle farm worker was responsible for 40-50 cows.

age milking 15 cows and working 9-10 hours per day. On the other hand, Poludensk men working as cattle farm workers, whose task it was to feed cattle and clean up manure, were responsible for fewer cows than the national average — on average, 38.

Annual milk production varied considerably from *kolkhoz* to *kolkhoz*. The maximum yield was reached in the *kolkhoz* named after Maks Gel'ts (Max Hölz), which produced 1,410 kg per cow; the minimum yield was in the Rote Fahne *kolkhoz*, which averaged only 450 kg per cow. On the whole, in 1934, the average annual yield of milk in the Poludensk *kolkhoz*es was 877 kg, a little lower than the average annual yield of milk in private households. These low indicators were caused by, first, frequently poor care of the animals, second, considerable seasonal fluctuations in the milk yields of milk due to fodder shortages in the winter and spring, and mainly, because the widespread use of cows to plough land due to a lack of tractors and horses. For instance, an inspection of the Kalinin *kolkhoz* revealed that from February 1 to July 1, the cows used in the sowing campaign produced an average daily yield of 3.8 kg of milk; the average for cows not being used to work fields was 7.5 kg. For all of the Poludensk *kolkhoz*es overall, 50-85% of the cows were exploited in the field, thereby causing milk-yield figures to plummet for this period.

Basing his estimates on observations of cattle-keeping norms and feeding patterns in the area, Chaianov estimated that average milk yield for Poludensk could, in principle, reach as high as 1500-1700 kg, but in actual practice yields were half this amount. Nevertheless, as Chaianov pointed out, all the farms fulfilled — and some over-fulfilled — official quotas for milk production. According to the plan, the *kolkhoz*es were to deliver to the state 60% or more of their milk yields. Plans for the other branches of cattle-breeding in Poludensk were also fulfilled. Chaianov did not comment on why the plan was fulfilled despite the poor level of labour organization.

Chaianov's analysis revealed numerous contradictions specific not only to cattle-raising on Poludensk *kolkhoz*es but also to the *kolkhoz* system as a whole. The concealment of the socio-economic differentiation behind mass averaging and wage-levelling, low levels of motivation, and incoherent labour organization — all of these problems would haunt the Soviet *kolkhoz* system up through its last days. Chaianov's so-called 'photograph' of the Poludensk *kolkhoz*es captured the squalor of Stalin's *kolkhoz* regime. It was no wonder that the authorities were enraged by the proposal to show this 'photograph' at an exhibition on Soviet agrarian achievements. Thus the authorities withdrew and destroyed the brochure, and a couple of years later they killed its author.

Exile and death in Alma-Ata, 1934-1937

In 1930, at the height of collectivization efforts, Chaianov was arrested and charged with establishing and leading the so-called Labour Peasant Party (LPP), whose goal was allegedly to overthrow the Soviet regime and restore capitalism in Russia. In large-scale arrests of alleged LPP agents in Moscow and the provinces (altogether, more than one thousand people were arrested), both university professors and local village agronomists were rounded up. Even now, when the actual existence of the LPP has been decisively substantiated, it is clear that LPP-connected arrests subdued the elite of the Russian agrarian intelligentsia, whose resistance was feared by both Stalin, who aimed to liquidate the kulaks as a class, and Chaianov and his colleagues, as the kulaks' ideologists (Ikonnikov 1988).

Chaianov was sentenced to five years in prison. He served his prison sentence in Yaroslavl. During his last year of imprisonment, he was exiled to Alma-Ata. Both the tsarist and Soviet regimes widely practiced exile as form of punishment for political adversaries. For instance, both Lenin and Stalin were at one point exiled by the tsar. In the 1920s and 1930s, Stalin often resorted to exiling particularly troublesome political and scientific figures to Central Asia. Thus, in the late 1920s, Trotskii was exiled to Central Asia. In the 1930s, the conditions of the exile were getting tougher. Authorities expected convicts to behave as exemplary Soviet citizens, and convicts were surrounded by secret police and public reminders of their political guilt.

In relation to this well-known exile, local authorities often found themselves in a difficult position. On the one hand, the local administration was flattered to host such a famous person; besides, some authorities could even feel personal sympathy toward Chaianov, either as a result of humane considerations or due to a sentiment of opposition to Moscow. Local authorities attempted to find ways to involve the exiled person in local life, often expecting to receive the exiled individual's professional assistance in exchange. On the other hand, the provincial authority had to placate Moscow. This is why local officials took great pains to determine the attitude of Moscow to its political adversary. Local officials had to be prepared for Moscow to suddenly order clampdowns on particular exiles, even to the point of liquidating them (which often happened); more rarely, Moscow would order an exiled individual to be sent back to the centre.

Ultimately, there was no absolute central power either in Moscow or in Alma-Ata; the authorities consisted of concrete, living people expressing their personal stance towards the exiled, thereby either personally improving or toughening the convict's life. Besides, in exile public opinion effected the exiled. The Party and the secret police tried their best to control this public opinion, but it was never fully possible. After all, the convict himself had his

own private world, personal will, intentions, and desires. The convict's personality came into daily contact with the reality of the exile, and the effect of this contact was varied and unpredictable.

Chaianov arrived in Alma-Ata in July 1934, escorted by a secret police agent. At the local secret police office, they explained to Chaianov the rules and restrictions for exiled individuals: once a week, he would have to register himself at the commandant's office; he was not allowed to go anywhere beyond the city limits in order to find a job or accommodation.

Chaianov described his first impressions of local life in his very first letter to his mother from Alma-Ata, dated July 6, 1934:

> Before the revolution, Alma-Ata, which in Russian means 'father of apples', used to be a remote provincial town called 'Vernii' (*Loyal*). Four years ago it became the capital of Kazakhstan, acquired the necessary facilities, and grew considerably. Now it has 150,000 inhabitants; however, on account of frequent earthquakes, all the houses are small and one-storied, so it occupies a vast territory. Its long and wide streets are all planted with Lombardy poplars and irrigated with aryks [ditches]; in a word, it is not a town, but a real park. The main attraction is the adjacent mountain ridge with its snowy peaks [...] There are many shops, many newsstands in which, alas, there are practically no Moscow newspapers, only local ones; and there are even more kiosks with ice cream and kvass [...] I dropped in at the town canteen, and for one big piece of bread and a plate of semolina they charged me one rouble. I think I will continue having my meals at this canteen. Tomorrow I am planning to visit the local research institutions to look for a job [...] I am very nervous — because of many new, long-ago-forgotten impressions of the free life (Zhalmagambetov 1991: 243).

Chaianov started his employment search by visiting the Kazakh Narkomat (Ministry) of Farming. To his surprise, he was received by the People's Commissar (minister) Syrgabekov himself, who was extremely benevolent. The Commissar asked Chaianov to be his personal consultant, to do scientific work at the Socialist Farming Research Institute, and to teach at the Institute of Agriculture.

In the two months before the academic year began at the beginning of September, Chaianov acclimated to the rhythm of his new way of life in Alma-Ata. In a letter to his mother dated July 21, he wrote:

> At the students' hostel of the agro-institute I was given a huge room with three windows (looking west), it is cool, light and furnished. I was also given dinner tickets to the canteen of scientific workers, signed an agreement with the Narkomat of farming for my first

> job… Yesterday, on our day off, we went to the mountains by bus… — very high spurs of the Tien Shan. The Alma-Ata peak is more than 4,200 meters high, which is higher than the Jungfrau in Germany [sic, the mountain is in Switzerland]… we had breakfast on the bank of a mountain brook and lay in the Sun, on the stones (Chaianov cited in Zhalmagambetov 1991: 244).

On August 20, 1934, Chaianov submitted his first report to the Institute of Agriculture 'On patterned sampling of materials related to the organizational-technical indices in sovkhozes and kolkhozes'. The report was a great success. As one Kazakh colleague wrote in his diary about Chaianov's presentation: 'He set the tasks of studying not just abundant statistical data but the concrete living economic bodies of the sovkhozes and kolkhozes, drawing a long-term plan of work at the crucial points […] The report produced a wonderful impression with its brilliant structure, excellent oratorical techniques, and the purely professorial system and perfection of speech' (Zhalmagambetov 1991: 244).

In the course of only one month, Chaianov became thoroughly engrossed in the tense routine business of 1930s Soviet scientific-administrative life. Indeed, when Chaianov started teaching on September 1, his workload increased considerably, including his participation in public work. Students liked Chaianov's lessons. Though statistics is not the most fascinating subject, and in the mid-1930s, the students in Alma-Ata were not very proficient in mathematics, and, as Chaianov himself noted with some annoyance, most of them did not even understand logarithms. Nevertheless, Chaianov taught statistics and mathematical calculations to his students in a clear and captivating manner.

He also enthusiastically participated in the students' theatrical activities. With Chaianov's participation as a consultant director, the students successfully staged several theatrical performances. Some students started to visit Chaianov more or less regularly, discussing painting, literature, and art with the professor. Before long, Chaianov's relatives sent him a collection of West-European engravings, as collecting was one of Chaianov's favourite hobbies. Over tea, Chaianov would explain to his Alma-Ata students the distinctive features of the art of antique west European engraving (Snegin 1989: 167-169). And yet, Chaianov's main intellectual interest was neither teaching nor the fine arts but rather his research activities. The heads of the institute where he worked treated him with respect, entrusting him with ever more numerous and important scientific tasks.

Chaianov's popularity with the scientists and administrators as well as with the students and professors was growing steadily, which the Alma-Ata Ministry of Home Affairs vigilantly noted in its reports to Moscow. The

Ministry was advised to limit and strictly control any independent activity on the part of Chaianov: this included his research, teaching activities, and public work. In a letter dated January 16, 1935, M. Shefler, the head of the administration for Higher Education of the Soviet Ministry of Agriculture, recommended that the director of the Kazakh Agricultural Institute, K. D. Churin, take the following course of action: 'In any case, as far as Chayanov is concerned, after speaking to the local party bodies and the police, you can, in a preliminary private conversation with A. V. Chayanov, indicate that separate leadership for the research work at the institute is not viable and that you thus relieve him of the duties of heading the research department' (Schulze 2003: 151).

In the spring of 1935, the Kazakh Agricultural Institute held a meeting dedicated to the 50th anniversary of Moscow professor Vasilii Vil'iams, a top Soviet soil scientist. Chaianov, who was personally acquainted with Vil'iams, addressed the large audience in attendance, providing vivid recollections of Vil'iams. In response, the gathered students, professors, and party workers expressed their gratitude with sincere applause. Right after this, on April 19, 1935, the central Kazakh newspaper, *Kazakhstan Pravda*, published an article entitled 'Enemy given the floor', which stated:

> The leaders of the Agricultural Institute helpfully gave the floor to notorious professor Chaianov, an implacable enemy of the kolkhozes, who quite recently in his works propagated 'kulak socialism', an ideologist of the counter-revolutionary 'labour peasant party', connected with the interventionists […] Certain party members present at the meeting rewarded Chaianov with applause […] This meeting has demonstrated the vital urgency of restructuring the party work at the Institute […] and intensifying the preparation for the purge (Zhalmagambetov 1991: 248).

The newspaper subsequently published threatening texts against Chaianov. Thus, the article 'Chaianov's Guests' (*Kazakhstan Pravda* 1935a) named those students and party workers who were particularly close to Chaianov. The article pointed out the danger of on-going conversations with the counter-revolutionary Chaianov, and communists and Young Communist Leaguers were called upon to be vigilant.

The reason for this fierce control of Chaianov was that his term of exile was coming to an end. In Moscow and Alma-Ata, in a disconcertingly Kafka-esque manner, anonymous high-ranking officials were determining what to do with Chaianov next. The Kazakh NKVD sent inquiries to the NKVD of the USSR; after receiving an official reply from Moscow, on June 16, 1935, the Ministry of Interior of Kazakh Soviet Socialist Republic officially concluded: 'The available materials give grounds to believe that,

ideologically speaking, Chaianov has not been disarmed, but rather is adapting himself to the new situation in the country […] Chaianov Aleksandr Vasilevich, serving his time in Alma-Ata, shall have his term of exile extended for another three years' (Zhalmagambetov 1991: 250).

Chaianov was not too surprised by this decision, as he knew his adversaries quite well; he was more alarmed by their growing aggressiveness towards him. He was prohibited from teaching students. Many of his former associates now strove to dissociate themselves from him. On the whole, the extension of Chaianov's exile signalled to his associates that it was dangerous to have anything to do with the disgraced professor. Even so, the leaders of the Alma-Ata academic agrarian elite continued to support him. After all, they wanted to recruit his talent and professionalism to the project of developing agrarian science in Kazakhstan. Hence, they got Chaianov to take part in preparing for the Kazakhstan's first All-Union agricultural exhibition. The exhibition was to be held in Moscow in the autumn of 1935 and was intended to showcase the outstanding successes of the new *kolkhoz* system.

In an effort to conceal their provincialism, all the regions and republics of the USSR tried to present their economic and scientific work in the best possible light. Chaianov's initiative to write an analytical brochure on *kolkhoz* cattle-breeding in Kazakhstan received widespread support amongst Kazakh economists. In the summer of 1935, Chaianov wrote the brochure. Naturally, the name of the actual author was never mentioned in the brochure for ideological and censorial reasons.

Chaianov also played a key role in preparing the layout for the Kazakhstan pavilion for the All-Union agricultural exhibition, which was originally opened in Alma-Ata and later moved to Moscow. In a letter to his wife dated October 28, 1935, Chaianov provides the following description, filled with bitter humour, of his work on the Kazakh exhibition pavilion in the cold autumn of 1935:

> I caught a cold in the cool pavilion, but it was impossible to give up the job. I was working at a temperature of 39.6 degrees. All the time, in the draught, drove in the lorry to the exhibition and back in the rain […] By the time of the exhibition opening we were 100% ready. On returning home, I collapsed on the couch, with my clothes on, and slept for 17 hours. You can picture the result — I look haggard and old. The worst part is that my stove at home has not been repaired; this is why it is cold and damp, like in the street. I blow my nose into the towel and cough, realizing that all this is unthinkable exploitation; but at the same time it is clear to me that without me it would be almost impossible to get the effect we have now […] Our huge pavilion is not a bit provincial, and we can fearlessly show it in

> Moscow. Although, for me, it seems, this is Sisyphean toil (Zhalmagambetov 1991: 250).

It turned out to be Sisyphean toil indeed. For his contribution to the preparation of the exhibition, the organizers showed no gratitude to Chaianov; on the contrary, there soon followed a severe punishment, with fatal consequences. A malicious scandal emerged over the brochure on Kazakh cattle-breeding. Chaianov's critics contended that the brochure concentrated mainly on the drawbacks of Kazakh cattle-raising, saying too little about its merits.

On November 29, 1935, *Kazakhstan Pravda* published a review of the brochure, 'A harmful book about the kolkhoz farms', that the work to appalling abuse (*Kazakhstan Pravda* 1935b). In the same month, with no explanation, Chaianov had been fired from the Kazakh Institute of Agricultural Economics. Due to mounting ideological pressure, the leadership of the institute could no longer protect Chaianov. Under these conditions, Chaianov wrote a special application to the Kazakh Ministry of Interior, in which he objected to his dismissal from the institute and submitted a list of the work he had completed during his stint there.

The Kazakh police simply ignored Chaianov's letter and did not answer him. However, Chaianov received personal support from the commissar in charge of farming in Kazakhstan, who, despite the threatening criticism, retained Chaianov as his personal consultant. In early 1936, Chaianov worked in the commissar's office, working on agricultural planning and studying the economics of tractors and transport. By persevering, Chaianov was trying to make the most of his exile; he was even beginning to feel that the situation around him was changing for the better. On January 25, 1936, in a letter to his wife, he wrote:

> Now I am working like a true Stakhanovite. Two-hundred fifty printed pages in two months — this is really a lot. Today I even had to beat all the records, and by an urgent request of NKZ [Ministry of Kazakh Agriculture], in one day, though with two assistants, I had to write 35 pages of text about the work of the tractor fleet […] The penance imposed on my house is slowly lifting, and people are beginning to drop by at my place, though only for business. The professors I know from the Agricultural Institute are no longer crossing to the other side of the street when they meet me. These are small things of everyday life, but you cannot imagine how oppressed I was by this absolutely groundless ostracism from human society (Zhalmagambetov 1991: 253).

Despite the upbeat attitude of this letter, this is the last more or less optimistic missive remaining from Chaianov. Chaianov's later letters from 1936

disappeared; in 1937 he was arrested. We know practically nothing about the last 18 months of his life in Alma-Ata.

The year 1936 witnessed the first rumblings of Stalin's subsequent sweeping purges of 1937. Throughout the USSR, the day-to-day situation was becoming increasingly menacing and alarming. By the end of 1936 and in early 1937, several of the colleagues and chiefs who had supported Chaianov in Alma-Ata were arrested. Not long after, they were all shot, amongst them the farming commissar, Syrgabekov, and the assistant director of the farming research institute, Nusinov. Amongst numerous other charges, these individuals were denounced for their ongoing relations with the counter-revolutionary Chaianov.

Towards the end of 1936, Chaianov finally lost his employment and accommodation. At the end of 1936, E. D. Eiginson, a student from the Kazakh Agricultural Institute who had once participated in Chaianov's drama club, ran into Chaianov not far from the Alma-Ata prison, where the latter was held for some time for interrogation. According to Eiginson, Chaianov 'had grown a beard; he was wearing a ragged sweater. The soles of his shoes were tied with wire' (Baliazin 1990: 202).

On March 17, 1937, Chaianov was arrested for the last time. After three weeks of intense interrogations in which he stubbornly denied the charges brought against him, he finally was broken and 'admitted' to acts of sabotage against the Soviet economy and agriculture. His most damning admission was his 'acknowledgement' of spying for Great Britain, an offence that was automatically punishable by death. Chaianov's indictment stated that he had been recruited as early as the second half of the 1920s by John Maynard Keynes (Schulze 2003: 156). Indeed, Chaianov personally met with Keynes, who visited Moscow in the mid-1920s. The investigation stated that Chaianov was passing strategic data on Soviet grain, cotton, and wool reserves to Keynes from Kazakhstan.

On October 3, 1937, Aleksandr Vasilevich Chaianov was executed by a firing squad in Alma-Ata.

Conclusion

Today it is natural to ask: what was the real reason for Chaianov's execution? In a recent article entitled 'Chayanov's treble death', T. Shanin soundly asserts: 'Chayanov was killed for his theory of differential optimums and vertical cooperation' (Shanin 2009: 85), which constituted powerful alternatives to Stalin's policy of forced collectivization. Stalin did not need any alternatives, instead extirpating them in order to ensure that only his own dictatorial path would develop in the USSR.

The essence of Chaianov's theory of differential optimums lies in his definition of the optimal borders between different agrarian sectors and rural territories, the development of which was to be regulated through vertical agricultural cooperation. Chaianov's approach amounted to a kind of anthropological-marginal institutionalism, whereby the existence and development of various institutions was to be studied and regulated by marking their inner and outer borders, as well as their optimal combinations. Interpreting socio-economic reality through this lens, Chaianov proved to be a virtuoso in anthropological-marginal modelling of economic institutions.

Chaianov's theory of optimums forms the foundation for his modelling of family economies, institutions of co-operation, agrarian policy, mixed (multi-structural) economy, and the macro- and micro-regional development of Russian/Soviet European and Asian territories. Even in his romantic prose, it is possible to discover his attempts to define the borders of people's destinies. Chaianov's stories tend to culminate tragically, with the protagonist either perishing in a struggle with a devilish adversary or else emerging from such a struggle with mental trauma.

Chaianov has been compared to Faust, first and foremost for his universal encyclopaedic ingenuity, harmoniously combining science and arts. But in Chaianov's fate, we also find the tragedy of Faust, who in his studies came close to the ultimate universal borders of cognition. Speaking metaphorically, Chaianov was confronted by the devil of Stalin's state, who, disguised as a high-ranking Soviet bureaucrat wearing a leather jacket, with a revolver and a portfolio, could fervently address the scientist with the following words: 'Professor Chaianov, comrade Faust, we are discovering and creating the horizons of a new world, surmounting the borders of the old one. Undoubtedly, your talent must serve the cause of revolutionarily transforming life. We intend to support you and your research with the help of our proletarian state. You will never find such a wonderful opportunity in bourgeois society. Now let us sign a contract'. Before long, the 'devil' of Stalin's bureaucracy reversed ideological course, becoming discontented with the independent behaviour of the Soviet Faust, condemning him to repression, death, and posthumous oblivion.

Chaianov's newly discovered intellectual legacy, both in European and Asian studies, is highly relevant to the contemporary search for the optimums and margins of the globalizing world.

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EPILOGUE

Chapter 16
Description and (Re)Construction in Soviet-Era Anthropology

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For many years, I taught a graduate course on the history of anthropological thought at Yerevan State University. Once, at an informal meeting after exams, I asked the students which of the trends we had discussed in the course they found most interesting. To my surprise, many of my students indicated their preference for the section on Soviet-era anthropology. Listening to their explanations, I realized that my students were interested in this period not because of its theoretical approaches; rather, they were compelled by the way in which Soviet-era anthropology intertwined with real life, sometimes with dramatic consequences for anthropologists, and, as some students added, other anthropological trends ('eras') became more apprehensible when compared to Soviet-era debates. This could be an additional (existential and didactical) answer to the general question 'Why should we / they care?' about the necessity of studying Soviet anthropology, which Florian Mühlfried and Sergey Sokolovskiy raise in the Introduction.

Another question for the readers concerns the area: Why focus on the Caucasus and Central Asia when dealing with Soviet-era anthropology? The title of the introductory chapter — 'Soviet Anthropology at the Empire's Edge' — already implies an answer to this question: Soviet-era anthropology might better reveal itself in the peripheral provinces. This focus on the peripheral provinces might give an additional meaning to the telling motto the editors proposed for this volume ('let us be provincial for a while in order to be cosmopolitan'), since it broadens the general problem of the centre and periphery to the one of the centre and provinces. Still, we should keep in mind that the centre in its turn could be peripheral (provincial) in relation to some other centre. Thus in 1991, enjoying the wind of freedom, the Moscow Institute of Ethnography hurried to change its name to the Institute of Ethnology and Anthropology, appealing, I think, to a kind of a intra-disciplinary nostalgia and local understanding. As this volume's editors point out in the introduction, throughout the Soviet era, by the term 'anthro-

pology', Soviet scholars understood only physical anthropology, in contrast to Western understandings whereby 'ethnology' and 'anthropology' are understood as more or less synonymous.

It is widely believed that there is a mystical relation between human beings and their names — from names that describe their bearers in myths and literature to the use of nicknames and pseudonyms. The name of the discipline that deals with human societies seems to reflect this same regularity: the Soviet-era term 'ethnography' in a sense demonstrates the discipline's descriptive mission — a mission that has been repeatedly criticized by Western anthropologists and that was defended by Soviet ethnographers. The term 'ethnology', on the other hand, denotes Logos, higher meaning, interpretation, and knowledge, and thus it was ill suited to the Soviet system of humanities. No wonder that *ethnology* was not 'canonized' as a discipline. Moreover, at the all-Russian archaeological and ethnographic meeting in 1932, the discipline of ethnography was deprived of its claims to higher knowledge when it was defined as an auxiliary, secondary historical discipline. This doomed historicity should be kept in mind when re-evaluating historically-oriented Soviet-era ethnography, which was deeply embedded in evolutionary theory. Higher knowledge of human society, one may guess, belonged to the master of history, historical materialism. Hence *materia* — or matter — the key concept of Marxist philosophy, was present in the traditional binary classification and separation of the subject of ethnography into 'material' and 'spiritual' cultures — a vague division that Western anthropologists hardly comprehended in full (see, for instance, Tuite's appeal to 'intellectual and material culture' in this volume) and that some late Soviet-era anthropologists sought to avoid (see Dabaghyan, this volume). The prevalence of the material over the spiritual ('being determines consciousness') was captured in the very name of the State Academy of Material Culture (forerunner to the Institutes of Archaeology and Ethnography), which was founded in 1921 by Nikolai Marr, a key figure in Soviet-era ethnography discussed by Kevin Tuite in this volume.

The Four Paradigms and the Legacy of Soviet Era Humanities

In this volume, along with Marr, three other prominent Soviet-era scholars are presented: historian Aleksandr Iakubovskii (by Ilkhamov), anthropologist Ol'ga Sukhareva (by Naumova), and economist Aleksandr Chaianov (by Nikulin). These four scholars can be seen to represent four distinct paradigms in Soviet-era humanities scholarship, however different their roles seemed to be in the history of the discipline and despite the differences in their academic careers and biographies. To compare, Marr reached the top of the academic staircase within his lifetime, while Chaianov was arrested,

exiled, and eventually executed. Before he was dethroned in 1950 sixteen years after his death, Marr was considered authority number one, and his fantastic theoretical contributions to linguistics strongly influenced the direction of humanities at that time. In comparison to Marr, who was regarded as authority number one, the other three figures were known only in much narrower circles. I find it significant that this volume includes some throwbacks to the development of Soviet linguistics and anthropology from the epoch of Marr and Marrism, together with the dramatic story of Chaianov, a man with no less brilliant (though arguably much more scientific) capacities and visions, whose case presents the lost perspectives and missed opportunities of Soviet-era anthropology and adjacent disciplines. The case of Iakubovskii demonstrates another important paradigm — the demiurgic power of anthropologists. While Sukhareva's lifelong devotion to Central Asian anthropology provides a good example of quite another aspect of Soviet-era anthropology — meticulous ethnographic fieldwork — the problem of this paradigm's legacy is also discussed in this volume. Anthropologists like Sukhareva — for example, Giorgi Chit'aia and Vera Bardavelidze in Georgia (discussed by Tuite), Stepan Lisitsian in Armenia (discussed by Dabaghyan), and Atiga Izmailova in Azerbaijan (discussed by Mammadli) — provided, to the extent of their abilities, the 'material base' of Soviet-era anthropology. Luckily, they did so without the groundless fantasy of Marr or the demiurgic voluntarism of Iakubovskii, and, what is equally important, without seriously endangering their lives.

This last circumstance — the threat to personal safety — should also be taken into account in understanding Soviet-era anthropology. This science was so intertwined with and controlled by ruling ideology that scholars shared a specific totalitarian experience of survival and downfall. For example, in the late 1930s, the prominent linguist and trenchant critic of Marrism, Evgenii Polivanov, as well as Marr's pseudo-scientific henchmen, Valerian Aptekar, who worked at a publishing house, and historian Sergei Bykovskii, were arrested and shot (Alpatov 1991: 55-56, 114). Many anthropologists suffered the same fate (Tumarkin 1999 [2002], 2003). In order to survive, they had to modify their research agendas, as Leonid Lavrov did several times (Kuznetsov, this volume), and/or relocate, as Aleksandr Semenov did when he left Uzbekistan to seek refuge in Tajikistan (Ilkhamov, this volume).

In the late-Soviet era, anthropologists still faced danger, though not the danger of death. As Tamara Dragadze points out in her chapter, to be a scholar in imperial Russia and subsequently the Soviet Union required a degree of bravery unparalleled in the West. Such bravery sometimes involved including obligatory citations of Marx or Lenin in scholarly texts, as

described by Mühlfried and Sokolovskiy in the introduction to this volume. Such obligatory citations by scholars (and especially by philosophers) were included to assuage censors while apparently simultaneously allowing their expert colleagues to understand that these were not mainstream Soviet texts. Besides such sophisticated and difficultly detectable means for evading Soviet censors, authors and, one would think, publishers used another form of mimicry by assigning 'authorized' titles for potentially controversial works. For example, Elena Revunenkova's key study of Malaysian shamanism was published under the quite neutral title *Peoples of Malaysia and Western Indonesia* (1980), only hinting at its real object in its sufficiently vague subtitle, 'Some Aspects of Spiritual Culture'.

Another important question raised in the introduction and differently answered by the contributors to this volume is how we should deal with the works of Soviet anthropologists from the perspective of the present. This question is not unrelated to the question of what is at stake in studying Soviet anthropology and whether or not there is something worthwhile in the legacy of Soviet-era anthropology, notwithstanding limitations imposed on these texts by the then-regnant ideology. While the contributors to this volume have provided fresh insight into these matters, it is important that readers not be left with an arrogant impression vis-à-vis Soviet-era humanities in general. The case of Mikhail Bakhtin is instructive. Bakhtin was in many ways the forerunner of post-structuralism (e.g. his discussion of Dostoevski's polyphonic texts (Bakhtin 1929)) and, paradoxically, certain aspects of structuralism (e.g. his study of carnival (Bakhtin 1965), which was published thirty years after its initial completion). In accordance with the Soviet model, Bakhtin was exiled for five years and for many years was prohibited from residing in the Soviet Union's major cities. Yet, his work continues to be a major point of reference for cultural scientists all over the world. Another example is provided by the comparatively less fortunate Aleksandr Zolotarev, who died in a prison camp in 1943. Completed in the late 1930s, Zolotarev's fundamental study (Zolotarev 1964) of dual organization and the cult of twins was also ahead of its time (Ivanov 1968), but following the Soviet 'tradition', it was not published until 1964 and with several chapters omitted (in particular, the chapter on the Caucasus). The historical-evolutionist introductory sections to Zolotarev's book and others like it provide exemplary material for scholars interested in the history of Soviet anthropological thought and need to be put in context.[1] However, such introductory sections could even be omitted without serious detriment

[1] Interestingly, by 1931 Zolotarev had already written an even more advanced work, but it was severely criticized by Bykovskii for misunderstanding the Marr's teachings (Artemova 2003: 203-207).

in future abridged editions. This observation bears on the question of how to deal with the works of Soviet anthropologists in the present. There are numerous examples of Soviet-era scholars whose works cannot simply be labelled 'right' or 'wrong' from the standpoint of the present — for instance, the scholarly approaches of the prominent linguist and folklorist Vasilii Abaev, who was one of Marr's students and whose opinions were partly influenced by Marr's earlier works (Alpatov 1991: 29, 33, 44, 139, 166; cf. Tuite, this volume).

It should be said that despite the obviously anti-scientific character of Marr's doctrinal ideas (especially those from his later works), some of his notions, even the bizarre ones, were based on highly intuitive thinking and in a sense foreran modern findings in nostratic linguistics, even if nostratic linguistics often takes the opposite position on similar matters (Alpatov 1991: 25). Marr's early works also inspired a productive interpretive approach in literary criticism, as represented in the works of such bright figures as Ol'ga Freidenberg and Izrail' Frank-Kamenetskii.

In any case, Marr's highly emblematic figure shows the ambivalence of Soviet-era humanities. It also reflects Mühlfried and Sokolovskiy's apt insistence on the importance of being 'very specific about locality when talking about anthropology in socialist and postsocialist societies'. Thus Marr was and still is considered to be a prestigious figure in Armenia — a result of his archaeological excavations and studies, mainly related to Armenia. From the 1890s onward, while in his native land of Georgia, as Tuite (this volume) notes, Marr was decried by the Georgian intellectual elite as an enemy of Georgian culture and the Georgian nation. This reaction may strike some readers as enigmatic, but it can be better understood if one considers, for example, an article from Marr (1898) written in reply to the famous Georgian poet Akakii Tsereteli's anti-Armenian publication.

In general, it seems to me that this volume tries to chart a 'peaceful' path through two of the most problematic areas of the former Soviet Union — the Caucasus and Central Asia — despite some hints like Ilkhamov's (this volume) passing reference to Uzbek-Tajik disputes over controversial visions of ethnogenesis, which were to some degree fostered by historians and anthropologists, and which indicate that, in a sense, we are dealing with a situation in which the foundations of ethnic/national conflicts and wars have been laid by intellectuals. No wonder the Caucasus and Central Asia continue to be beset by bloody conflicts.

Colonial Anthropology and Native Anthropologists

The USSR is often thought of as an empire: for instance, in Francine Hirsch's *Empire of Nations* (2005) and in the title to the introduction of this

volume. And anthropology in an empire, one may guess, would be part of the colonial enterprise. However, I don't think that Soviet and post-Soviet anthropology can be fully comprehended exclusively through colonial and post-colonial discourses. Nevertheless, as the direct successor to colonial Russian anthropology, Soviet anthropology could not avoid colonial trends, though here also we should be specific about locality: even if Central Asian anthropology did not develop directly out of the colonial model, it followed it typologically. Anthropologists from the academic centres of Moscow and Leningrad journeyed to Central Asia to discover the Soviet Union's ethnographic periphery and founded local schools and institutes. Georgia and Armenia, on the other hand, already had their own anthropological traditions, which, however, increasingly came to emulate Soviet metropolitan trends. However, here too, traces of the discipline's colonial past can be found. Thus Dabaghyan (this volume) supposes that 'Russia was present in Armenian ethnography from its very origin' in second half of the nineteenth century. For example, he sees this colonial background manifesting in the involvement of Khachatur Abovian, the founder of modern Armenian-language literature and author of a number of ethnographic accounts, in the imperial administrative system. But, interestingly, Abovian's ethnographic contributions did not play a significant role in the formation of Armenian ethnography. Abovian's ethnographic legacy seems to have come to occupy its specific position in the history of Armenian ethnography only rather recently and retrospectively. At the same time, while the originators of Armenian ethnography overlooked Abovian's contributions, his works influenced others like the novelist Proshian, who understood Abovian's ethnographic descriptions as a specific directive and hence included in his novels many detailed ethnographic descriptions based on his native village life (Lisitsian 2005 [1926]: 379).

Thinking in terms of colonialism, I am afraid, shifts Soviet-era anthropology into a post-colonially correct but not too productive track. Hopefully, the questions raised by Mühlfried and Sokolovskiy in their introduction and discussed in the fourth and fifth sections of the book regarding the 'colonial' can help us to understand these dimensions of this problem. Another popular characterization — 'totalitarian' — also seems too generalized and politicized. Perhaps then a more neutral reference to centre-periphery relations (keeping in mind the need to attend to specificities of time and place) is better suited to contemporarily understanding Soviet-era anthropology. Compare the following commentary on the centralized character of Soviet anthropology from the introduction: 'when there was a change of director in its heading institute, the Institute of Ethnography in Moscow [...] an associated change in paradigm and research focus was sure to follow'.

This observation can be confirmed even in the present post-Soviet situation judging from the terminology used by some scholars — cf. the relict Bromleian 'ethnoses' in Mammadli (this volume) — while Bromlei's successor as director of the now post-Soviet central institute, Valerii Tishkov, wrote a book entitled *Requiem for the Ethnos* (Tishkov 2003).

Academic (and ideological) control and censorship were realized by the centre: all theses had to be presented in Russian and had to be approved by the Higher Certifying Commission, which was located in Moscow. Unsurprisingly, such centralized control was not enthusiastically embraced in the non-Russian periphery. However, this situation was far from unambiguous. Thus, Dragadze (this volume) describes both the positive (avoidance of possible local intrigues) and negative (for instance, the conflict that arose between local Georgian and Soviet understandings of national history) sides of such outside control. I would add some other ambiguous circumstances here. Native scholars were offended by the rejection of national languages by the centre, even though according to non-official opinion such outside control raised academic standards and discouraged low quality dissertations in native languages. Having long worked as the academic secretary of the doctoral defence council in Yerevan, I can say that this colonialist-tinged opinion unfortunately often turned out to be true.[2] There was another positive side effect of the Soviet empire: particular historical and cultural conflict situations were often (but not always) solved by the 'Big Brother' from Moscow in accordance with existing academic standards. Finally, the obligatory Russian language of these theses (which were typically subsequently published as books) made possible their entry into countrywide academic discourse.

With the end of the centralized system, new centres have begun to appear, supplanting the former Soviet academic metropolis. In this volume, therefore, Dragadze asks whether or not the West is replacing Moscow. The failed project in a Dagestani village described in Bobrovnikov's chapter demonstrates one such attempt to found a new 'Western' academic centre.

Frames of Description

The descriptive mission of ethnography, already presupposed in the name of the discipline, was fundamentally in conflict with Soviet ideology. The first expression of this conflict was the problem with *kraevedenie* (provincial studies). This body of research, produced by both professionals and ama-

[2] This council at the Institute of Archaeology and Ethnography of the Armenian Academy of Sciences was the only one in the Caucasus to hold defenses of both *kandidat* and doctoral theses.

teurs, documented the local lore, customs, history, and geography of these writers' native land. The problem was not with the natural objects of investigation of *kraevedenie* but rather with anthropological realities: it turned out that memory of the past was prevailing over the new Soviet lifestyle and value system, although bringing about such a lifestyle and value system was also one of the aims of *kraevedenie*. The situation with the peoples of North and Far East was less ambiguous: these peoples, who, according to the evolutionist Marxist classificatory system, represented the primitive communal system, had to skip over the feudal and capitalist stages of history and proceed directly to socialism, and anthropologists were among those called upon to help realize this typical colonial project. These peoples were understood to be doomed or fated to step into this new Soviet life, and, it was thought, documenting their present primitive conditions would neither hasten nor delay their fate, especially if anthropologists followed the imperative Soviet-era principle of learning while teaching.

The case of what were understood as more highly evolved peoples was not as simple. In describing groups thought to be more highly evolved, anthropologists were in fact addressing the rejected past by describing the present, and not appealing to the socialist and communist future. As a result, *kraevedenie* was severely criticized and champions of the trend were persecuted. Of course, we should be specific about locality here as well. Armenian *kraevedenie* was not severely suppressed as was Russian *kraevedenie*, and the leader of Armenian provincial studies, Stepan Lisitsian, luckily avoided persecution, perhaps because he had ceased working in this field by 1928 (Vardanian 2005: 168-170), just prior to the persecutions in the centre. Alternatively, Lisitsian may have escaped the fate of his Russian colleagues due to the low number and quality of anthropologists in Armenia at the time (see Dabaghyan, this volume), which meant that no one paid much attention to his activities and thus that there was no one to potentially denounce his scholarship.

It is quite telling that Lisitsian's fundamental work, *The Armenians of Zangezur* (1969), which was completed in 1945 but only published in 1969, primarily dealt with the ethnography of this region before the revolution (Lisitsian 1969: 10; Vardanian 2005: 292-300). This salutary argumentation, I think, played a no less important role in creating the salvage paradigm in the Armenian ethnographic tradition (see Dabaghyan, this volume) than did the exhortations of pre-revolutionary ethnographers that vanishing cultures should be documented while they still existed in reality and memory. In this same era, Georgian ethnography also gravitated toward a similar salvage paradigm (see Dragadze, this volume). However, the alibi-like 'late 19th — early 20th century' that appeared in so many titles from this time period

(Dabaghyan, this volume; Appendix 1, this volume) seems not to have played such a crucial role for Georgian anthropologists as for their Armenian colleagues, especially as the former were adhering to the field methodology articulated by Chit'aia and Bardavelidze (Tuite, this volume). Interestingly, this time period (the late 19th — early 20th century) eventually came to be regarded as a sort of Golden Age by many late-Soviet and contemporary Armenian anthropologists, providing 'canonical' ethnographic texts to be glanced back at as national benchmarks. In many cases these benchmarks were simply concrete ethnographic profiles of a changing system, often not so accurately presented by amateur enthusiasts.

Schematizing, we can conclude that in Armenia and Georgia, countries with national ethnographic traditions, it was not considered politically incorrect to study ethnographic realities of the recent past, including the vague realm of 'spiritual culture'. The situation in Central Asia was markedly different. There, anthropology was introduced by specialists from the centre. While traditional material culture and some 'neutral' subjects like kinship could be studied without obvious risks to Soviet ideology (see Naumova's chapter on Sukhareva, this volume), religious beliefs and practices required a special approach. This approach involved not ideologically-laden explanatory theories accounting for religion (see Schoeberlein's discussion of such ideology-laden theories concerning Islam in this volume) but rather the notion of survivals discussed by DeWeese (this volume). DeWeese supposes that the notion of survivals in fact enshrines only a pretension toward a historical perspective and that the real impact of the theory of survivals was to 'drain meaning — and understanding — from the present'. I am not sure whether this was a conscious pretence, but for a Soviet ethnographer it was indeed easier to write about pre-Islamic survivals than about a modern functioning religion.

In the late 1940s, Sergei Tolstov, the director of the Moscow Institute of Ethnography, who 20 years earlier had been one of the active persecutors of *kraevedenie*, raised a new banner for anthropology, calling upon anthropologists to investigate the *kolkhoz* lifestyle. In so doing, Tolstov was calling for a focus on the present, instead of past-oriented survivals. He had hardly solved the riddle of survivals, but perhaps his intuition of the way in which the concept was manipulated made him feel uneasy about using it.

It should be remembered that Soviet anthropology at this time was mainly focused on villagers (*paganus* in Latin) who, following the other meaning of the word *paganus*, were related to the *pagan*, which was understood as ancient and original and thus attractive for anthropologists who, like the discipline they served, began with the strange and exotic to come later to the familiar and ordinary. But, I think, Tolstov's call did not follow the

exoticizing trend in anthropology; rather, it was inspired by a desire to ideologically demonstrate the privileges of the new *kolkhoz* (Soviet) lifestyle over the old, pre-Soviet one. Abashin's and Bobrovnikov's contributions to this volume deal with different aspects of this anthropological/ideological mission.

Given this intertwining of anthropology and ideology, it is perhaps then not surprising that collective farm monographs depicted only those sides of daily life that were officially permitted. Abashin writes about the multilevel character of censorship. No less important than explicit external censorship was self-censorship, which became a standard mode of working for Soviet anthropologists. There were no pure dissidents in anthropology: contexts of anti-Soviet resistance and refusal were so variable that almost anyone could find him or herself imperilled. As one might guess, the most dangerous anthropological endeavour would have been objective description. Hence Chaianov's so-called photograph of the Poludensk *kolkhoz*es brought him to his dramatic end (Nikulin, this volume). Sergei Alymov's recent study (2010) provides an interesting point of comparison with Kushner's classic *kolkhoz* study of the 1950s (Kushner 1958).

Orientation toward the present continued to be prerogative in Soviet anthropology also in later years — compare the 'Soviet modern living standards' research agenda with its telling name, which was initiated from the centre in the 1960s (Kuznetsov and Mammadli, this volume). In the 1970s and 1980s, ethno-sociologists began to focus on Soviet modernity in a number of regions including the Caucasus and Central Asia. Like any quantitative sociological research project covering an expansive territory, this regionally oriented research had its problems, and amongst these was the not infrequent inattention to local specificity. Thus, Tamara Dragadze (this volume) vividly describes how her rural Georgian interlocutors led researchers from Moscow astray when the researchers tried to ask them questions that had worked well enough in Tatarstan. However, I would like to note another, more instructive result of this regional body of research. First of all, anthropologists began to focus on the city. In the 1980s in Azerbaijan, small towns were chosen as objects of investigation. In 1987, results of ethno-sociological research undertaken in Sumgait were published (Abbasov and Salimzade 1987) with the telling title *Sumgait — gorod bratstva* ('Sumgait — a town of brotherhood'). In late February 1988, the infamous anti-Armenian pogroms took place in Sumgait. The fact that such an optimistic work could be published so soon before this outbreak of violence may indicate that the seemingly more advanced investigations of the late Soviet period were either inaccurate or else continued to be politically committed. Mammadli (this volume) supposes that for Azerbaijani researchers who

studied urban populations, it was apparently impossible to highlight evident problems related to inter-ethnic relations, given the dominant ideology of proletarian internationalism. Viktor Karlov, a Moscow-based ethno-sociologist and co-author of the research questionnaire for Azerbaijan, contended that the contradiction between the study's account of Sumgait and the dramatic unfolding of events soon after its publication could be explained by several factors. In particular, he contended that the researcher who had conducted the survey in Sumgait had outlined social discontent and not inter-ethnic tension (Karlov 2010: 228-229). Putting aside the obvious propagandistic touch of the book's name, on the one hand, and essentialist ideas about never vanishing inter-ethnic animosity, on the other hand, I would agree with Karlov's conclusion: although violence in Sumgait may have been motivated by social discontent, it was easily reshaped into inter-ethnic violence. Generally speaking, I think that violence should be discussed from a constructivist perspective. As the main instrument of a culture of violence, violence itself could be constructed and manipulated, as in the case of Sumgait. Another question surrounds the concrete patterns of the culture of violence, including the ones displayed in Sumgait (Abrahamian 1999, 2006: 247-270).

Inventing and (Re)Constructing

In the previous paragraph, I alluded to the discrepancy between anthropological description and described realities. I did not discuss the specificities of observing and writing or the interesting theme of errors in anthropology. Soviet anthropology made its own substantial contribution to this general problem. However, in the context of Soviet-era anthropology, it is more instructive to focus on both what was permitted and what anthropologists were expected to describe. There was also another sphere of anthropological writing, which dealt with the historical realities invented or (re)constructed by Soviet anthropologists.

The search for ancestors and the re-evaluation and construction of national history are canonical components of nation-building processes, and different nations comprising the Soviet empire were involved in distinct nation-building processes at different points in time (Abrahamian 2006: 7-24). These creative processes were naturally triggered by Soviet revolutionary perturbations and modern crises and turmoil (Mammadli, this volume). Sometimes nation-building involves rather fantastic visions, but it almost always positions itself against similar national visions being produced in neighbouring nations, hence the resultant 'wars of memory' (Shnirel'man 2003), which are often followed by real wars.

Among nation-making schemes, the most sophisticated and instructive seems to be the Uzbek construction realized by Aleksandr Iakubovskii and thoroughly analyzed by Ilkhamov (this volume). The Uzbek case is interesting for a number of reasons. First, the Uzbek model was introduced not from within, but from above, by a historian from the centre. Here, we are actually dealing with a rare case of colonial nation-making that expresses native nationalism, but at the same time has to fit in with 'Big Brother' Russia's national vision of history, which was earlier constructed by the same Iakubovskii. 'Inner' national constructions usually don't dare to fit other constructions; they usually, as we know, contradict neighbouring constructions, in this case the Tajik one. To construct a general picture, Iakubovskii had to manipulate other important nation-making constituents — name and language. And all of this became possible because this scholar happened to be evacuated during World War II from Leningrad to Tashkent.

Fantastic visions of language and history were present in Marr's works, however these visions were crucial (and sometimes even fatal) not for the nations involved in making these constructions but for those that rejected them. With his zeal for describing Soviet modernity (though within the limits set by the censors), Tolstov posited a bizarre reconstruction of prehistoric life in which men and women lived separately (in order to work more productively) and came together only occasionally for festivals at which they relaxed and copulated (Tolstov 1935). Such reconstructions did not affect modern nations, as Tolstov's reconstruction was plotted at the earliest stages of human history.

Beginning in the mid-1970s, a new construct was introduced in Soviet anthropology: *sovetskii narod* or the 'Soviet people or nation'. This new community was thought to have a specific Soviet identity and ideology and shared the Russian language. Anthropologists involved in this project (which was supposedly initiated by high ranking Soviet ideologists) were eager to show that this new Soviet community was already an ethnographic reality (Abrahamian 2006: 75-76).[3] Future analyses may reveal that while such a society may have existed at that point in embryonic form[4], the overly-eager executors (including anthropologists) of this model of and for society brought an opposite result, ultimately provoking nationalist sentiment at the edge of the empire (Mammadli, this volume). We see that this last, future-oriented project of Soviet-era anthropology was also doomed to failure.

[3] We also find this tendency in the post-Soviet era in the works of researchers who confirm local nationalist theories or dubious concepts like 'ethno-demographic balance' (see Kuznetsov, this volume).

[4] I don't mean the stereotyped, caricatured image of *Homo Sovieticus.*

Of course, I am simplifying and schematizing; there were additional factors that brought the Soviet empire to its end. However, one could argue that the artificial simplification of the structure of the empire by forced cultural equalization resulted in an increase of entropy that led the Soviet empire's living organism to its 'thermodynamic death'. Hence the system's counteraction: to avoid this levelling, less amorphous even if primitive structures were created. In the case of the endangered, unstructured 'levelled' empire, these were the primitive, though structured seceded former republics (Abrahamian 1990: 67-68). Later (and in some instances simultaneously), national autonomies emerged, with a number of republics being constructed that emulated the Soviet model but as smaller empires. In order to avoid increased entropy, a more complex structure must be created, even if this structure is artificial. Using the concept of entropy to understand such historical developments may at first seem too abstract and 'structuralist'; however, this approach can explain other similar systems in critical situations, for example, the European Union, which has undertaken its own levelling policies.

Like in the beginning of the Soviet era when anthropologists were eager to realize new anthropological visions (though many of them later suffered for this), perestroika inspired those anthropologists who were ready to face the time of changes. In accordance with the aforementioned entropic approach, some Soviet anthropologists (for example, Galina Starovoitova and Igor' Krupnik) proposed changing the Soviet federal structure into a confederation or an even more complicated, decentred form that could avoid the ineffectiveness of Stalin's *matryoshka*-like hierarchic national structure. Starovoitova compared the possible future post-Soviet space, which was already establishing economic and cultural networks, with the European Union, which was trying to establish such a network.[5] However, Gorbachev was late (as usual) with his decision to conclude a new Union treaty, and the Soviet empire lost its only chance for a more civilized transition 'back' to capitalism, instead experiencing another dramatic jump, like in the beginning of the Soviet era.

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[5] For another interpretation of these perestroika-era visions, consult Dragadze, this volume.

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Kushner, P. I. (ed.). 1958. *Selo Viriatino v proshlom i nastoiashchem: Opyt etnograficheskogo izucheniia russkoi derevni*. Moscow: Izd. AN SSSR.

Lisitsian, S. 1969. *Zangezuri hayere*. Yerevan: HSSH GA hratarakchutyun.

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Revunenkova, E. V. 1980. *Narody Malaizii i zapadnoi Indonezii (nekotorye aspekty narodnoi kul'tury)*. Moscow: Nauka.

Shnirel'man, V. 2003. *Voiny pamiati: Mify, identichnost' i politika v Zakavkaz'e*. Moscow: Akademkniga.

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Appendix 1

List of Dissertations in Armenian Ethnography Defended in and after the Soviet Era

Dissertation titles were translated by A. Dabaghyan only if not referenced by their authors in English. Titles are followed by the place and year of defence, and the institution conferring the degree. As a portion of the dissertations were published under modified titles, the text following the date of defence is provided to lead interested persons to editions of these works available in libraries. The citation with the author's name and year, following the data on the dissertation (e.g. Published as: Vardumian 1956) prompts the reader to view the full title in the bibliography of this volume's chapter by Dabaghyan. Studies without such extensions after the defence date are available as unpublished manuscripts in the repositories or libraries of the institutions that awarded the degree and are indicated in the list of abbreviations at the end of this appendix. Asterisks appearing before an author name indicate that the degree awarded was a Doctorate and not a Candidate of Sciences.

1950s

Karapetian, Emma T. *Vykup v svadebnykh obriadakh armian i ego sotsial'no-ekonomicheskie korni* [Ransom in Armenian Wedding Ceremonies and its Socio-Economic Roots]. Moscow: MIE 1949. Published as: Karapetian 1950.

Vardumian, Deren S. *Kolkhoznyi byt Loriiskogo krest'ianstva* [The Kolkhoz Life of the Peasants in the Lori Region]. Tbilisi: IHAE GAS 1953. Published as: Vardumian 1956.

*Lisitsian, Srbui S. *Armianskie narodnye pliaski i teatralizovannye predstavlenia* [Armenian Folk Dance and Theatre Performance]. IHAE GAS 1959. Published in 3 volumes: Yerevan 1958/1973/1983.

1960s

*Karapetian Emma T. *Semeinye otnosheniia i patronimiia u armian* [Family relations and Patronymic Groups Among the Armenians]. Tbilisi: IHAE GAS 1967. Published as: Karapetian 1966.

Petrosian, Emma Kh. *Teatral'nye i igrovye elementy v srednevekovykh armianskikh miniatiurakh* [Elements of Theatre and Play in Medieval Armenian Miniatures]. Yerevan: IA AAS 1967. Published in HAB 7 (1975).

Mkrtumian, Iurii I. *Formy skotovodstva v Vostochnoi Armenii (po etnograficheskim materialam)* [Forms of Stock-Breeding in Eastern

Armenia (based on ethnographical materials)]. Moscow: FH MGU 1968. Published as Mkrtumian 1974.

Panian (Ter-Sarkisiants), Alla E. *Sovremennaia sem'ia u armian (po materialam sel'skikh raionov Armianskoi SSR)* [The Modern Armenian Family (based on materials from the rural districts of the Armenian SSR)]. Moscow: MIE 1968. Published in Moscow, 1972.

Seghbosian, Karl V. *Remeslennye traditsii i ikh proiavleniia v bytu leninakantsev* [Artisanal Traditions and their Appearance in the Lifestyle of Citizens of Leninakan]. Tbilisi: IHAE GAS 1969. Published as Seghbosian 1974.

Davtian, Serik S. *Ocherki po istorii armianskogo vyshival'nogo iskusstva* [Essays on the History of Armenian Embroidery]. Yerevan: IH AAS 1969.

1970s

*Bdoian, Vard A. *Zemledel'cheskaia kul'tura v Armenii* [Agrarian Culture in Armenia]. Tbilisi: IHAE GAS 1968. Published in Armenian as: Haiastani yerkragortzagakan mshakuite, Yerevan 1970.

Vardanian, Rafael' O. *Sistema mer dliny i poverkhnosti zemli v Armenii* [System of Measurements of Length and Land Surface in Armenia]. Yerevan: IH AAS 1970.

Khachatrian Zhenya K. *Javakhki hayeri zhoghovrdakan parere* [Folk Dances of the Armenians of Javakhk]. Yerevan IH AAS 1971. Published in HAB 7 (1975).

Petrosian, Levon N. *Peshie i v'iuchno-verkhovye sredstva peredvizheniia na Armianskom nagor'e (istoriko-etnograficheskoe issledovanie)* [Means of Pedestrian and Equestrian Transportation in the Armenian Highlands (A Historical-Ethnographic Study)]. Yerevan: IH AAS 1972. Published in Armenian: Pokhadramijotsnere Haikakan lernashkharhum (patma-azgagrakan usumnasirutyun). HAB 6 (1974): 93-157.

*Abramian, Valentina A. *Remiesla v Armenii v IV-XVIII vv. i tsekhovye organizatsii armian-remeslennikov v gorodakh Zakavkaz'ia s XVIII po nachalo XX vekov* [Crafts in Armenia in the IV-XVIII Centuries and the Guilds of Armenian Craftsmen in Transcaucasian Cities from the XVIII to the Early XX Centuries]. Yerevan; IAE AAS 1973. Published earlier in Armenian as: 1) Arhestnere Haiastanum IV-XVIII darerum. Yerevan 1956; 2) Hayots hamkarutyunnnere Andrkovkasi qaghaqnerum XVIII-XX darerum. Yerevan 1971.

Vardanian, Liliia M. *Traditsionnye muzhskie vozrastnye gruppy u armian (konets XIX – nachalo XX vv.)* [Traditional Male Age Groups of the

Armenians (Late XIX – Early XX Century)]. Moscow: HF MSU 1976. Published in HAB 12 (1981).

Akopian, Mania V. *Sotsial'no-ekonomicheskii stroi i byt armianskoi sel'skoi obshchiny (konets XIX – nachalo XX vv.)* [Socio-Economic Structure and the Life Mode of an Armenian Village Community (Late XIX – Early XX Century). Moscow: FH MGU 1976. Published as: Akopian 1988.

Dolzhenko, Irina. *Khoziaistvo i obshchina russkikh krest'ian Armenii v poreformennyi period* [Economy and Community of the Russian Peasants of Armenia in the Pre-Reform Period. M. FH MGU 1976. Published as: Dolzhenko 1984.

Arakelian, G. R. *Kustarnye promysly v Vostochnoi Armenii vo vtoroi polovine XIX veka* [Household Industries in Eastern Armenia in the Second Half of XIX Century]. Yerevan: IH AAS 1976.

Petrosian, Anushavan S. *Shelkovodstvo v Armenii* [Sericulture in Armenia]. Yerevan: IH AAS 1978. Published in Armenian as: Sheramapahutyune Hayastanum [Silk-worm Breeding in Armenia]. Yerevan 1987.

Karapetian, Ruben S. *Formirovanie naseleniia Erevana (po materialam etnosotsiologicheskogo obsledovaniia naseleniia stolitsy Armianskoi SSR)* [Formation of the Population of Yerevan (Based on Ethno-Sociological Survey Materials Collected in the Capital City of the Armenian SSR). Moscow: MIE 1979. Published partially in: Naselenie Erevana 1986:9-80.

1980s

Abdullaev, A. I. *Material'naia kul'tura azerbaidzhantsev, prozhivavshikh v Armenii v XIX – nachale XX vekov (istoriko-etnograficheskoe issledovanie po materialam Vaiots-Dzorskoi zony)* [Material Culture of Azerbaijanis Residing in Armenia in the XIX to early XX Century (Historical-ethnographic study, based on materials of the Vayots Dzor Area). Tbilisi: IHAE GAS 1981.

Kharatian, Zaven V. *Struktura i dinamika religioznykh perezhitkov v semeinom bytu armian (po materialam XIX – nachala XX vv.)* [The Structure and Changes of Religious Survivals in Armenian Family Life (based on materials from the XIX – early XX century)]. Moscow: MIE 1981. Published as: Kul'tovye motivy semeinykh obychaev i obriadov u armian. HAB 17 (1989): 7-61.

Arakelian [Kharatian], Granush [Hranush] S. *Etnicheskaia istoria cherkesogaiev* [Ethnic History of the Cherkesso-Armenians]. Leningrad: LOIE 1982. Pubished as: Arakelian 1984.

Vardumian, Goar [Gohar] D. *Kul'ty dokhristianskikh bozhestv u armian (istoriko-etnograficheskoe issledovanie)* [Cults of Pre-Christian Deities among the Armenians (A Historical-Ethnographic Study)]. Moscow: FH MSU 1982. Published as: Dokhristianskie kul'ty armian. HAB 20 (1991): 61-151.

Galstian, Ambardzum [Hambardzum] P. *Razvitie dvuiazychiia u armianskogo naseleniia Erevana (etnolingvisticheskoe issledovanie)* [Development of Bilingualism among the Armenians of Yerevan (An Ethno-Linguistic survey)]. Moscow: MIE 1983.

Marutian, Arutiun [Harutyun] T. *Inter'er armianskogo narodnogo zhilishcha (po materialam vtoroi poloviny XIX – nachala XX veka)* [The Interior of Armenian Folk Dwellings (based on materials from the second half of the XIX – early XX century)]. Moscow: MIE 1984. Published in HAB 17 (1989): 65-143.

Darveshian, Mamo Kh. *Skotovodcheskoe khoziaistvo kurdov v Armenii (po materialam vtoroi poloviny XIX – nachala XX veka)* [The Stock-Breeding Economy of the Kurds in Armenia (Based on Materials from the Second Half of the XIX – early XX century)]. Yerevan: IH AAS 1984. Published in Yerevan, 1986.

Ogandzhanian (Ohanjanyan), Ruben, S. *Mezhlichnostnye natsional'nye otnosheniia v gorodakh Armianskoi SSR (na materialakh etnosotsiologicheskykh issledovanii)* [Inter-personal National Relations among the City-Dwellers of the Armenian SSR (Based on Materials from Ethno-sociological Surveys)]. Moscow: MIE 1985.

Nagapetian [Nahapetyan], Rafik A. *Sem'ia i semeinaia obriadnost' u armian Akhdznika (vtoraia polovina XIX – nachalo XX v.)* [Family and Ritual Life among the Armenians of Aghdznik (Second Half of the XIX – Early XX century)]. Yerevan IH AAS 1986.

Saakian [Sahakyan], Artsruni S. *Kul'tovo-memorial'nye pamiatniki v sisteme armianskoi srednevekovoi narodnoi kul'tury* [Monuments of Worship and Memory in the System of Medieval Armenian Folk Culture]. Yerevan IH AAS 1986.

Sarkisian [Sargsyan], Gamlet [Hamlet] G. *Etnicheskii sostav naseleniia Vostochnoi Armenii i ego dinamika v XIX – nachale XX vv. (v predelakh Armianskoi SSR) (etnodemograficheskoe issledovanie)* [The Ethnic Composition of the Population of Eastern Armenia and its Dynamics in the XIX – early XX century (Inside the Borders of the Armenian SSR) (An Ethno-Demographic Study)]. Yerevan IH ASS 1987. Published as: Sarkisian 2002.

Arzumanian, Alla A. *Udiny. Voprosy etnicheskoi istorii i kul'tury (istoriko-etnograficheskoe issledovanie)* [The Udis: Issues of Ethnic

History and Culture (Historical-Ethnographic Study)]. Yerevan: IAE AAS 1987.

Mkrtchian, Artur A. *Obshchestvennyi byt armian Nagornogo Karabakha (vtoraia polovina XIX – nachalo XX veka)* [The Social Life of the Armenians of Nagorno-Karabakh (Second Half of the XIX – Early XX century)]. Yerevan: IAE AAS 1988. Published as Mkrtchian 2010.

1990s

Kharatsidis, Elefterii K. *Khoziaistvennyi byt i material'naia kul'tura grecheskogo naseleniia Vostochnoi Armenii vo vtoroi polovine XIX – nachale XX veka. (istoriko-etnograficheskoe issledovanie)* [The Economic Life and Material Culture of the Greek Population of East Armenia in the Second Half of the XIX – Early XX Century (A Historical-Ethnographic Study)]. Yerevan: IAE AAS 1990.

Gasparian, Aramais O. *Pchelovodstvo v Armenii (istoriko-etnograficheskoe issledovanie)* [Apiculture in Armenia (A Historical-Ethnographical Study)]. Yerevan: IAE AAS 1990. Published in Armenian as: "Meghvapahutyune Hahastanum". Yerevan 1989.

Petrosian, Emma Kh. *Ritual'no-mifologicheskaia i teatral'no-zrelishchnaia sistema armian* [Mytho-Ritual and the Theatre Complex of the Armenians]. Yerevan: IAE AAS 1990.

Zakarian, Bella E. *Sistema rodstva u armian* [The Kinship System of the Armenians]. Yerevan: IAE AAS 1990.

Galstian, Migran V. *Sovremennoe otkhodnichestvo v Armianskoi SSR (etnosotsiologicheskoe issledovanie)* [Modern Seasonal Labour Migration in the Armenian SSR (An Ethno-Sociological Study)]. Yerevan: IAE AAS 1990. Published in Armenian as: Artagnatsutyune Hayastanum (19-rd d. yerkrord kes – 1980-akan tt.). HAB 26 (2009): 7-88.

*Atoian, Karlen P. *Narodnye traditsii fizicheskoi kul'tury u armian s drevneishikh vremen do kontsa XIX veka (istoriko-etnograficheskoe issledovanie)* [Popular Traditions in the Physical Culture of the Armenians from Antiquity to the End of the XIX Century (A Historical-Ethnographic Study)]. Yerevan: IAE AAS.

Poghosyan, Svetlana H. *Shiraki ev Javakhki hayots avandutayin taraze (XIXd. verj – XX d. skizb)* [Armenian Traditional Costume in Shirak and Javakhk (Late XIX – Early XX Century)]. Yerevan: IAE AAS 1994.

Tadevosyan, Aghasi Z. *Darbine hayots tzisakargum (patmazgagrakan usumnasirutyun)* [The Blacksmith in Armenian Ritual Life (A His-

torical-Ethnographic Study)]. Yerevan: IAE AAS 1994. Published in: HAB 23 (2007): 101-183.

*Vardanian, Rafik H. *Hayastani chapern u kshirnere (V-XV dd.)* [Armenian Measures and Weights (V-XV Century)]. Yerevan: IAE AAS 1994.

*Odabashian, Asia A. *Hayots zhoghovrdakan havataliqnere (kazme ev tzisakarge)* [Armenian Folk Beliefs (Structure and Ritual)]. Yerevan: IAE AAS 1995.

Minasyan, Anahit M. *Shiraki ev Javakhqi hayots zhoghovrdakan bzhshkutyune (patmazgagrakan usumnasirutyun)* [Armenian Folk Medicine in Shirak and Javakhk (A Historical-Ethnographic Study)]. Yerevan: IAE AAS 1995.

*Petrosyan, Anushavan S. *Dzknorsutyune Arevelyan Hayastanum* [Fishing in East Armenia]. Yerevan: IAE AAS 1996.

Stepanyan, Armenuhi A. *Hay zhoghovrdakan tarazi zardanakhsheri khorhrdimaste (tzisayin, gunayin, nshanayin hamakargere)* [Semantics of Ornamentation of the Armenian Folk Costume (Ritual, Colour, and Semiotic Systems). Yerevan: IAE AAS 1996. Published in: HAB 22 (2007): 7-128.

Mkrtchyan, Samvel Zh. *Hayastani Hanrapetutyan bnakchutian ardi tonatzisakan hamakarge (etnosotsiologiakan hetazotutyun)* [The Modern Festival-Ritual System of the Population of the Republic of Armenia (An Ethno-Sociological Study)]. Yerevan: IAE AAS 1997. Published as: Tonere hayots ardi kentsaghum (etnosotsiologiakan usumnasirutyun). HAB 22 (2007): 130-237.

*Ter-Sarkisian, Alla E. *Armiane: Istoriia i etnokul'turnye protsessy [The Armenians: History and Ethno-Cultural Processes]*. Moscow: MIE 1997. Published as: Ter-Sarkisiants 1998.

Avagyan, Nazik K. *Hayots Zhoghovrdakan taraze XIX d. – XX d. skzbin (patmazgagrakan usumnasirutyun)* [Armenian Folk Costume in the XIX – Early XX Centuries (A Historical-Ethnographic Study)]. Yerevan: IAE AAS 1998.

Hobosyan, Suren G. *Hayots dzithanutyan mshakuyte XIX d. yerkrord – XX d. arajin kesin (patmazgagrakan usumnasirutyun)* [Armenian Oil Extraction Culture from the Second Half of the XIX to the First Half of the XX Century (A Historical-Ethnographic Study)]. Yerevan: IAE AAS 1998. Published in: HAB 23 (2007): 7-99.

2000s

Margaryan, Nikol M. *Andznanvan entrutyune arevelahayots mej (XIX-XX dd.) (azgabanakan usumnasirutyun)* [Selection of Personal Names

among the East Armenians (XIX-XX Centuries) (An Ethnological Study)]. Yerevan: IAE AAS 2000.

Dabaghyan, Artak A. *Hayots zinanshanayin hamakargi hasarakakan ev tzisakan arumnere II (BC) – XIII (AD) dd. (patmamshakutayin hetazotutyun)* [Social and Ritual Patterns of Armenian Heraldic System from the II B.C. – XIII A.D. Century (A Historical-Cultural Study)]. Yerevan: IAE AAS 2001.

Gabrielyan, Mkhitar R. *Hasarakakan ev mshakutayin gortzentatsnere HH ardi giughum (patmazgagrakan usumnasirutyun)* [Social and Cultural Processes in a Modern Village of the Republic of Armenia (A Historical-Ethnographic Study)]. Yerevan: IAE AAS 2001. Published as: Haykakan giughe antsumayin shrjanum [An Armenian Village in Transition] Yerevan 2001.

Bazeyan, Karine G. *Hayots Aseghnagortzakan mshakuyte (patmazgagrakan usumnasirutyun)* [Armenian Embroidery Culture (A Historical-Ethnographic Study)]. Yerevan 2002.

Sargsyan, Armine Kh. *Kanants veraberogh avandakan argelanqnere hayots mej (patmazgagrakan usumnasirutyun)* [Traditional Taboos Related to Women among the Armenians (A Historical-Ethnographic Study)]. Yerevan: IAE AAS 2002.

Cholakian, Hakob M. *Antiochi merdzaka Routchi hovti hayere (patmazgagrakan usoumnasirutyun)* [Armenians of the Routch Valley near Antioch (A Historical-Ethnographic Study)]. Yerevan: IAE AAS 2002.

Arakelova, Viktoria A. *Yezidskii panteon* [The Yezidi Pantheon]. Yerevan: IAE AAS 2002.

Hakobyan, Arsen A. *Tatakhos hayere* [The Tat-Speaking Armenians]. Yerevan: IAE AAS 2002.

Zaryan, Armanda R. *Kine iranakan avandakan kentsaghum* [Women in Traditional Iranian Culture]. Yerevan: IAE AAS 2003.

Khachatryan, Armenak S. *Boshaner. Patmazgagrakan usoumnasirutyun* [The Boshas: A Historical-Ethnographic Survey]. Yerevan: IAE AAS 2003.

Poghosyan, Ashkhunj, A. *Hayots gorgagortzakan mshakuyte (patmazgagrakan usumnasirutyun)* [Armenian Carpet-Weaving Culture (A Historical-Ethnographic Study)]. Yerevan: IAE AAS 2003.

Markosyan, Artak M. *Hayastani Hanrapetutyan bnakchutyan teghasharzhere 1989-2001 tt. (etno-zhoghovrdagrakan hetazotutyun)* [Migrations of the Population in the Republic of Armenia from 1989-2001 (An Ethno-Demographical Study)]. Yerevan: IAE AAS 2004.

Zolyan, Mikael S. *Gharabaghyan himnahartse vorpes adrbejantsineri patmakan hishoghutyan ev azgayin inqnagitaktsutyan dzevavorman*

gortzon (Adrbejani petakan azgayn qaghaqakanutyan hayetsakargi verlutzutyun [The Karabakh Problem as a Factor in the Formation of the Historical Memory and National Consciousness of Azerbaijanis (An Analysis of the State Concept in National Policy of Azerbaijan)]. Yerevan: IAE AAS 2005.

*Vardanian, Liliia M. *Vklad Stepana Lisitsiani v razvitie armianskoi etnografii* [Stephan Lisitsian's Contribution to the Development of Armenian Ethnography]. Yerevan: IAE AAS 2005. Published as: Vardanian 2005.

Sargsyan, Amalia R. *Paytamshakutyan arheste hayots mej XIX-XX dd. (patmazgagrakan usumnasirutyun)* [The Craft of Woodworking among the Armenians in the XIX-XX Century (A Historical-Ethnographic Study)]. Yerevan: IAE AAS 2005.

Israelyan, Astghik R. *Pahpanaknere hayots zhoghovrdakan havataliqneri hamakargum* [Amulets in the System of Armenian Folk Beliefs]. Yerevan: IAE AAS 2006.

Pikichyan, Hripsime V. *Erazhshtutyune ev erazhishte hayots avandakan artnin ev tzisakan kyanqum* [Music and the Musician in Armenian Traditional Everyday and Ritual Life]. Yerevan: IAE AAS 2006.

Antonian, Iuliia Iu. *Magicheskoe tselitel'stvo i gadanie v Armenii (po materialam XX veka)* [Magical Healing and Divination in Armenia (Data from the XX Century)]. Yerevan: IAE AAS 2007.

Stepanyan, Gohar Yu. *Hayots tonakan varqayin hamakarge (est 19-rd dari verji – 20-rd dari skzbi azgagrakan niuteri)* [The System of Festive Behaviour of the Armenians (Based on Ethnographical Materials from the Late 19th to the Early 20th Century]. Yerevan: IAE AAS 2007.

*Marutyan, Harutyun T. *Kolektiv ev patmakan hishoghutyune hayots inqnutyan hamatekstum (est XX dari patma-azgagrakan nyuteri)* [Collective and Historical Memory in the Context of Armenian National Identity (Based on Historical-Ethnographic Materials from the XX century)]. Yerevan: IAE AAS. Published as: Marutyan 2009.

*Nahapetyan, Rafik A. *Aghdznikahayeri amusnaharsanekan sovoruytnern u tzesere (patmazgagrakan usumnasirutyun)* [Wedding Ceremonies and Rituals among the Armenians of Aghdznik (Ethno-Historical Research)]. Yerevan: IAE AAS 2008.

Boyajyan, Artashes G. *Leninakan-Gyumri. Bnakchutyan sharzhentatse 1950-2000 tt.* [Leninakan-Gyumri: Population Movements 1950-2000]. Yerevan: IAE AAS 2008.

Shagoian, Gaiane A. *Invarianty i transformatsiia v armianskoi svad'be* [Invariants and Transformation in the Armenian Wedding]. Yerevan: IAE AAS 2009.

Abbreviations

IH AAS — Institute of History of the Armenian Academy of Sciences (National Academy of Sciences of Armenia)
FH MSU — Faculty of History at Moscow State University
HAB — Hay Azgagrutyun ev Banahyusutyun (nyuter ev usumnasirutyunner) [Armenian Ethnography and Folklore (Materials and Studies)], Yerevan
IAE AAS — Institute of Archaeology and Ethnography of the Armenian Academy of Sciences
IHAE GAS — Institute of History, Archaeology, and Ethnography of the Georgian Academy of Sciences
LIOE — St. Petersburg (Leningrad) Department of the Institute of Ethnography and Anthropology of the Russian Academy of Sciences
IE — Institute of Ethnography and Anthropology of the Russian Academy of Sciences
MKAEN — International Congress of Anthropological and Ethnographical Sciences
TIE — Trudy Instituta Etnografii

Appendix 2

Table 1. Ethnology and Anthropology Chairs at Russian Universities

City	University	Department	Chair	Year founded	Research areas/ institutions	Professions obtained by the students	PhD programs
Abakan	Khakassian State U.	Institute of Law and History	Archaeology, ethnography, and regional history	1994	Archaeology and ethnography of Southern Siberia; studies of the cultural heritage of the peoples of Southern Siberia		
Barnaul	Altai State U.	History	Archaeology, ethnography, and historical sources study	1988	Regional research	Archaeologist, historian, ethnographer	
Chelyabinsk	Chelyabinsk State U.	History	Archaeology, ethnography, and natural and social history	1997	History and culture of the South Ural peoples; religious factors in politics	Ethnologist	
			Ethno- and socio-political processes in the regions of the Russian Federation	2000		Political anthropologist	
Chita	Chita State U.	n/a	Social and cultural anthropology	2000		Anthropologist, culturolo-gist	Studies of religion, philosophy of culture, and philosophical anthropology
	Trans-Baikal Humanitarian Pedagogical U.	Philosophy		2004		Social anthropologist	
Cheboksary	Chuvash State U.	History	Archaeology, ethnography, and regional history	1990	History and ethnography of the peoples of the Volga-Kama region; studies of traditional culture of the peoples of Chuvashia; history of religions of the Middle Volga region		Ethnography, ethnology, and anthropology
Ekaterinburg	Ural State U.	History	Archaeology, ethnology, and special historical disciplines	1999	Regional research (ethno-cultural history of the Ural region)		
	Ural State Technical U.	n/a	Social anthropology and psychology	2001	Social work and public relations		

City	University	Department	Chair	Year founded	Research areas/ institutions	Professions obtained by the students	PhD programs
Gorno-Altaisk	Gorno-Altaisk State U.	History	Archaeology, ethnology, and historical sources study	n/a	Regional research	Archaeologist, historian, ethnologist	
Irkutsk	Irkutsk State U.	History	Archaeology, ethnology, and ancient history	1988	Archaeology and ethnography research lab (established 1970)	Archaeologist, historian, ethnologist	
Izhevsk	Udmurt State U.	Sociology and philosophy	Philosophy and sociology of culture	2004		Social anthropologist	
Kazan	Kazan State U.	History	Ethnography and archaeology	n/a	Ethnography and ancient history of the peoples of the Volga-Ural region; ethnic archaeology		
Krasnodar	Kuban State U.	History, sociology, and international relations	Archaeology, ethnology, ancient and medieval history	n/a			
Makhachkala	Dagestan State U.	History	Archaeology and ethnology	2002	Regional research	Archaeologist, historian, ethnologist	
Moscow	Moscow State U.	Biology	[Physical] Anthropology	1919	General anthropology, anthropo-genesis, human morphology, ethnic anthropology, population genetics, evolutionary anthropology, medical and ecological anthropology, ethology	Anthropologist, primatologist	Physical anthropology, biology
		History	Ethnology	1939	Ethno-sociological lab. (established 1988); Centre for applied ethnology (established 1995)	Historian, ethnologist	Ethnology and ethnography
		Geography	Ethnography and geography	1884-1919		Psychologist	

City	University	Department	Chair	Year founded	Research areas/ institutions	Professions obtained by the students	PhD programs
(Moscow)	Russian State U. for Humanities (RGGU)		Centre for Social Anthropology	2000	Political and legal anthropology; evolutionary anthropology; visual anthropology; human ethnology	Social anthropologist	Theory and history of culture
			Institute for Cultural Anthropology	1997-2000			
	Moscow City Psychology and Pedagogical U.	Social psychology	Ethno-psychology and politico-cultural development	2003	Inter-ethnic relations; studies of aggression and extremism		
	Russian State Social U.	Social work, pedagogy, youth studies	Social anthropology	n/a			
	Moscow U. of Design and Technologies	Institute of Social Engineering	Social anthropology	n/a	Ethnic conflict studies; anthropology of organizations; urban anthropology	Social anthropologist	
	Russian State U. for Physical Culture, Sports, and Tourism		Culturology, socio-cultural anthropology, and social communication	1994	Integrative anthropology; body culture	Applied culturology	Culturology
Novosibirsk	Novosibirsk State U.	Humanities	Archaeology and ethnography	1992			
	Novosibirsk State Technical U.	Humanities Education	Social work	n/a			Social anthropology
Omsk	Omsk State U.	History	Ethnography and museology (formerly 'Ethnography, historiography, and historical sources study')	1985	Ethno-archaeology; visual anthropology; ethnology of peoples of Western and Southern Siberia. Lab. of ethnography and history of Siberian Germans; Research group for Ethno-Archaeology	Social anthropologist (since 2004), ethnologist; museologist	Ethnology, ethnography, and anthropology
Orel	Orel State U.	Philosophy			Philosophy and cultural studies; sociology and political studies		Social anthropology

City	University	Department	Chair	Year founded	Research areas/ institutions	Professions obtained by the students	PhD programs
St. Petersburg	St. Petersburg State U.	Physics Mathematics	Ethnography and geography	1887			
	Petrograd U.	Geography		1918			
	Leningrad State U.	Philology	Ethnography Dept.	1938–44			
		History	Archaeology and ethnography	1949-52			Ethnology
			Ethnography and anthropology	1968	Historical ethnography; ethnic history and culture of the North-West region		
	St. Petersburg State U.	Sociology	Cultural anthropology and ethnic sociology	1992	Political anthropology; intercultural communication studies; anthropology and sociology of migration; gender and kinship studies; urban and organizational anthropology; studies of globalization		Social anthropology
		Republican state institute of humanities	Social anthropology and psychology	n/a			
	European U.	Anthropology	[prior to 2008 ethnology]	1994	Cultural anthropology; folklore studies; sociolinguistics and semiotics; Ethnic Studies Program and Ethnic Tolerance Training Centre for Field Ethnological Research; Permanent Ethnographic and Folklore Expedition “The Russian North”		Ethnography, ethnology, anthropology; folklore studies; theory and history of culture; linguistics
Saratov	Saratov State U.	n/a	Archaeology and ethnography	2005	Ethno-cultural history and historical anthropology		

City	University	Department	Chair	Year founded	Research areas/ institutions	Professions obtained by the students	PhD programs
(Saratov)	Saratov State Technical U.	Social work	Social anthropology and social work	1998	Social anthropology of modernity; medical anthropology; economic anthropology; visual anthropology; studies of youth subcultures; social adaptation; organizational anthropology	Social anthropologist and social worker	Sociology
Syktyvkar	Syktyvkar State U.	History	Archaeology, historical sources study, and ethnography	n/a	Ethno-archaeology; regional studies		
Tomsk	Tomsk State U.	History	Archaeology and regional history	1991	Lab. of history, archaeology and ethnography of Siberia (since 1968); Lab. of Social Anthropology at the Chair of Russian History (since 2004)		Ethnography, ethnology, anthropology
	Tomsk Pedagogical State	History Geography	Archaeology and ethnology	n/a			Ethnography, ethnology, anthropology
	Tomsk Branch of the Russian Social U.		Social anthropology (extra-mural courses)			Social anthropologist	
Tyumen	Tyumen State Oil and Gas U.	Institute of Humanities	Social work	2006		Social anthropologist	
Vladivostok	Far-Eastern State U.	Institute for International Relations in the Asia-Pacific Region	Social and political anthropology Social anthropology	1998 2000	Comparative analysis of political systems		
	Kuibyshev Far East State Technical U.	n/a	Social anthropology	1999	Urban anthropology; ethno-cultural processes in the Far East		Ethnology, ethnography, anthropology, and history
	Vladivostok State U. of Economics and Service					Social anthropologist	

33 currently working chairs (10 chairs of ethnography and archaeology; 3 chairs of ethnology and archaeology; 18 chairs of social and/or cultural anthropology; 2 chairs of physical anthropology)

Table 2. Chairs according to the type and period of founding

Conservative old style ethnography		**New Ethnology/ Social Anthropology**	**New 'Eclectic' Anthropology**	**Physical Anthropology**
Old Chairs	*New Chairs*			
• MGU (History) • SpbU (History) • Kazan • Omsk • Tomsk	• Abakan • Barnaul • Chelyabinsk • Cheboksary • Ekaterinburg (History) • Gorno-Altaisk • Irkutsk • Makhachkala • Novosibirsk • Syktyvkar	• Spb European University • RGGU (Russian State for Humanities) • Kuban • Saratov	• Chita • Ural State Technical • Russian State Social (Moscow) • Novosibirsk State Technical • SpbU (Sociology) (2 chairs) • Vladivostok (3 chairs) • Tyumen • Orel • Izhevsk • Moscow City Psychology and Pedagogical U. • Tomsk Branch of the Russian Social U.	• MGU (Biology) • Russian State U. for Physical Culture, Sports, and Tourism (Moscow)

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