

The Concept of the “Field” in Early Soviet Ethnography A Northern Perspective

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Abstract: The conceptualization of the “field” in early Soviet ethnography had its own dynamics and elaborations within the discursive arenas of the Leningrad ethnographic school. Beginning with the prehistory of the idea of the field among the Enlightenment naturalists and travelers, we turn toward a description of long-term expeditions of the first generation of Soviet ethnographers of the North. Comparing field diaries, photographs, questionnaires, lectures, and textbooks, we consider the patterns and flexibility in the concept of the field in the first half of the twentieth century. We conclude with a discussion of how post–World War II Soviet anthropologists departed from the ideas of participant observation and long-term fieldworking prominent in earlier conceptualizations of fieldwork in Soviet ethnography.

Keywords: field, history of anthropology, participant observation, Soviet ethnography

One of the fundamental principles of anthropology is that it is based on fieldwork.¹ It is the field that “helps define anthropology as a discipline in both senses of the word, constructing a space of possibilities while at the same time drawing the lines that confine that space.” It is the field that constitutes the very notion of “real anthropology” (Gupta and Ferguson 1997: 5).

In both Europe and Russia, the use of the term “field” comes from the discipline of natural sciences. In Europe, the term was borrowed by former zoologist Alfred Haddon from a discourse of fieldworker-naturalists (Stocking 1992: 20–24), whereas in Russia, naturalists set the tone for the formation of knowledge about peoples (Slezkine 1994; Vermeulen 2015). Besides, “the missionary ethnography” often manifests



itself in texts of Soviet ethnographers, the first generation of which considered missionaries not as much as rivals but rather as people more knowledgeable about the cultures they studied than anyone else (for the history of social/cultural anthropology, see Clifford 1988: 26).

We argue that the field was not a static concept in the history of Soviet ethnography, but that it, in fact, involved a certain set of statements, interweaving in various ways during different periods of time. As a result, we reason that the history of the field has multiple disruptions. When talking about his colleagues in one of his interviews, Alexei A. Nikishenkov, the former head of the Department of Ethnology at Moscow State University, noted that the traditions of expeditions of Lev Shternberg and Vladimir Bogoraz's school died after the war and today's "excursions" could hardly be called "fieldwork" (Elfimov 1997: 780). Despite this pessimism, evidence shows that today's anthropology in Russia is not in such dire straits; field practice is not only coming back, it is gradually gaining momentum.

Contemplating the concept of the field in early Soviet ethnography, we move away from its usual critique and instead focus on understanding the formation of this concept from two vantage points, namely from the view of a Russian anthropologist working in Great Britain and Russia (Arzyutov), and from the view of an American anthropologist (Kan). We focus on three concepts of the field that were defined before the epoch of Bromley's "ethnos," which could not be a part of field practice due to the difficulty of its "instrumentalization" (Sokolovsky 2009: 58–59). The three concepts of the field will be considered as follows: 1) the field project of Franz Boas, partially carried out by his Russian students and colleagues Bogoraz and Shternberg; 2) conceptualization and Sovietization of the field in the lectures and programs of Bogoraz and Shternberg; and 3) the teachers' appraisal of their students' field practice. Of course, it would also be interesting to consider the Soviet ethnography of the "developed socialism" produced by students of the students, as well as to explore fieldwork methods in relation to the history of expeditions, but such a study is beyond the scope of this article. Therefore, we take a chronological approach to examine the period of articulation of the object, and the methods of anthropology as a discipline. Another noteworthy consideration at this point is that because of our professional interests in Arctic anthropology and the history of anthropological thought, we refer for the most part to the "Leningrad school of anthropology" and draw on experiences of ethnographers specializing in Siberian studies.

The concept of the field can be split into two parts: (1) an object of fieldwork, which further transforms in a text as the field's specific reflection, and (2) a notion, imaginary, of what the field is. While dealing with the details of written field legacies, we followed the thoughts articulated by Nigel Rapport: "There is *inscription*—the writing of notes, keywords and mental impressions; there is *transcription*—the writing of dictated local texts; and there is *description*—the final writing of coherent reflections and analyses, facilitating a later retrieval of overall sense and order. Here, in short, is a prefigured and pre-encoded way of anthropologists discovering and describing things in the field" (Rapport 1991: 10).

Raising the question about the archeology of the field in early Soviet ethnography, we discern *what* stands behind the inscription, and *how* it is actualized in understanding the concept of the field as such. Analyzing the concept of the field, historians of anthropology question the existence of a peculiar "archetype," which, from the point of view of a history of continental thought, assumes a white European examining an aboriginal community outside of the European world (Stocking 1990;² elaborated in Gupta and Ferguson 1997). This view is directly related to the colonial background of European anthropology itself. The Russian history, by contrast, presents a case of "self-colonization," as observed by Alexander Etkind (2001: 180; 2011). Consequently, the Soviet/Russian ethnography of the peoples of the USSR/Russia appears to be a reflection on the "exoticized home." To a certain extent, we can consider the Soviet ethnography to be a case of "native anthropology," unlike the situation with European anthropologists who, not so long ago, used to separate the "field" and "home" in a geographical, political, or symbolic sense.

Organizationally and methodologically, the history of the field has two aspects. First, it can be viewed as a genealogy, in which the transmission of fieldwork knowledge can be observed in terms of periods in the relationships of various generations of teachers and students. Second, it can be situated in a wider context, in which we encounter not so much a genealogy, but rather an interweave of given schemata produced by the discipline or under the pressure of the political discourse, as well as of scenarios determined by their own heuristics.

The seemingly unified concept of the field, with all those archetypal formulas of Bogoraz and Shternberg (as well as, for example, British and American anthropologies) about the "stationary method" or a year-long residence in the field, included the modifications that were partially

related to political events or decisions of authorities, and partially to the hidden history of reflexivity within the discipline. In fact, the puzzle of the field method, and of the approach to the field, was assembled in a different way each time, but kept the common nomenclature of rubrics for ethnographic research such as food, housing, clothing, beliefs. Dealing with the same cultural group, different authors could complete these rubrics with different content. However, the difference in approaches is still compliant with the specific grid of the discipline, as Foucault figuratively coined it, which limits both the questioner and the answerer (Foucault 2002).

During the 1920 and 1930s, this grid was defined by the views of Shternberg and Bogoraz. It is noteworthy that Shternberg (1910, 1914) was one of the first Russian ethnographers who, after formulating the concept of the *inorodtsy*³ for Russian ethnography, applied it almost immediately to his field project in Siberia. In some ways, this movement was a form of discursive colonization of Siberia. The political demand was being conceptualized and instrumentalized by ethnographers, many of whom were actively involved in the political life of these regions (as census takers or as authors of reports to the Committee of the North, the Commission for the Study of the Tribal Composition of the Population of the Borderlands of Russia [KIPS] and so on), defining the place of local communities in the state classifications. Benedict Anderson described this process as “imagined communities” being constructed in colonies, not in Europe (Anderson 1991). David Anderson’s studies provide a Siberian illustration of such a project that, referring to the Circumpolar Census, also shows an interaction between a census taker/ethnographer and the implemented system of census taking (Anderson 2006). The practice of censuses became a primary factor in the formation of the discursive grid, and of the idea of nationality as the basis of classifications (Cadiot 2007; Hirsch 1997). At that time the order of differentiation was established according to ethnic characteristic, which happened to be related, especially during the first censuses, to the class divisions of the empire. For instance, Juliette Cadiot (2007: 63–90) writes that the respondents and census takers considered ethnic belonging as related to social status within the imperial hierarchies.

While preparing their texts, researchers were adjusting them to fit the mentioned discursive grid. Sokolovsky points out the phenomenological approach to the interpretation of the field where, since the times of the *Narodniks’* (populist) ethnography (Shternberg, Bogoraz, Jochelson, Klements, etc.), “an attempt to understand rather than to anatomize

was leading to such a strategy of textualization of the field experience that prioritized characterization, pure description, answer to a question "what," not "how" or "why." The ethnographer's objective was to provide his readers with a holistic image of phenomena, a Gestalt, aimed at the creation of effects of seeing and recognizing rather than pragmatic knowledge of how it works" (Sokolovsky 2011a: 212). Looking at specific texts written on the basis of field research, it is clear to see the way the author connects the field material with the grid of discourse. It is exactly these connections, or, rather, the failures in producing them that had become the subject of so many critical reviews.

A Brief Overview of the Background: The Eighteenth and Nineteenth Centuries

Talking about the field in the eighteenth and nineteenth centuries is possible only with a certain degree of accuracy. The epoch of naturalists of the eighteenth century did not identify a specific "ethnographic object" that would require going to the field to collect information about it. Despite those considerations, the terminology of the "ethno-" was already being actively developed in Russia by German naturalists (for more details, see Vermeulen 2015). The field was conceived in Russia not so much from the reasoning of armchair ethnographers, but rather as the result of a large-scale project of "great" expeditions that were expected to explore everything that was spreading beyond the metropole's borders. In 1734 and 1737, Vasilii Tatishchev designed and sent out his famous questionnaires consisting of queries about social and cultural differences in different regions of Russian Empire (Tatishchev 1950, Stepanov 1956). A similar structure of questions was later used by the "real fieldworkers"—Gerhard Friedrich Müller, Peter Simon Pallas, Johann Gottlieb Georgi among others (see questionnaires and questions in Müller 2009: 47–51; Russov 1900; Shirina 1994: 41–198). The questions in these surveys were producing a peculiar order of discourse which was shared by official authorities and used later in politics. It is surprising then that the actual contents of the profiles did not seem to be of much importance to the authorities because the data were published only in fragments. The order of the questions revealed a new understanding of reality, and the usual linearity of facts was transformed into a schema. It is also noteworthy that the academic biographies of the first Russian naturalists are related to the founders of the naturalist discourses (according to the interpretation of the authorship performed

by Michael Foucault [1984])—Lamarck and Linnaeus (Stocking 1968: 234–279). In addition, it has become possible since that time to observe a process of the immersion of the field as part of the naturalist discourse into the political life (Dragadze 1978: 61).

The Imperial Russian Geographical Society (IRGS; founded in 1845) later borrowed this style in the fieldwork of its correspondents, albeit significantly transforming it to comply with the new understanding of ethnography. The ideas of historical progress and of the integration of “backward peoples” into the European civilization also played a significant role in the conceptualization of the field (Knight 1995: 267–270).

The ordering of the concept of the field and the creation of field research programs was accompanied by the accumulation of knowledge that later became viewed as ethnographic, but actually exceeded the grid of the discipline (for example, reports and notes of missionaries, essays of travelers, and the like; Schweitzer 2001: 117–122). Moreover, Siberia was becoming a platform for testing the ideas of European nationalisms, which can be illustrated by Matthias Castrén’s searches of the Finnish ancestral home (Knight 1995: 271–277; Schweitzer 2001: 122–126).

In this regard, Bogoraz and Shternberg’s history of the field does not directly follow the field activities of the Imperial Russian Geographical Society and mostly followed Franz Boas and, respectively, to the German tradition, including the “anthropology” by Immanuel Kant (Stocking 1968: 195–233). Besides, it is complemented by the idea of nationality and the practice of censuses, in which both researchers participated as census takers: Shternberg at Sakhalin in 1891, 1892, and 1893 (Kan 2009b: 44, 64–71) and Bogoraz at Kolyma in 1897 (Mikhailova 2004: 102).

However, the field programs designed by Bogoraz and Shternberg, at the Faculty of Ethnography of the Geographic Institute and at the Department of Ethnography of Leningrad State University, accounted for the range of achievements in the area of field researches—those they had conducted themselves, as well as those conducted by naturalists of the 18th century and by correspondents of the IRGS (Bogoraz n.d.: 3, 1927; Makariev 1928). These programs combined the three discursive fields and resulted in the formation of a new “grid of specification,” which defined the discursive field of ethnography in the USSR and is still being applied today in Russia. This prehistory opens up an understanding of a nonlinear development of ethnography. It is noteworthy that the discursive field of Soviet ethnography also transcended internal borderlines and chronology. According to Valerian A. Kozmin, Professor Rudolf Its, while reestablishing the ethnographic education

at the Leningrad State University in 1968, was guided by the course of lectures prepared by Vera Kharuzina ("Ethnography," [1909, 1914]; "Introduction to Ethnography," [1941]) or, according to another report, by the well-known book by Irina Chebokсарov and Nikolai Chebokсарov, *Peoples, Races and Cultures* (1971). With that, he was breaking form with not only the habitual differentiation between the Leningrad School (of Shternberg and Bogoraz) and the Moscow School (of Dmitry Anuchin), but also with the chronological continuity.

Thus, it seems reasonable to start with a review of the concept of the field suggested by Franz Boas, as it significantly influenced Bogoraz and Shternberg's understanding of the field and fieldwork.

Franz Boas and the Concept of the Field

It is often considered that it is thanks to Franz Boas that field research has become the major method of American anthropology. Robert Lowie (1937), one of Boas's first students wrote that Boas should be viewed primarily as a fieldwork ethnographer. In many respects, this view reveals the influence of the "father of American anthropology" on his students and on several further generations of anthropologists. Most of Boas's students conducted fieldwork, many of them among North American Indians.⁴

"Although Boas[s] fieldwork included a certain amount of *'participant observation'*, his primary research technique was the collection of 'texts'—that is to say, of traditional material from individual Indian informants recorded in their native tongues" (Stocking 1974: 85). Stocking argues that Boas's predecessors also collected transcribed texts, but that he was the only one who made it "the keystone of the ethnographic style." This happened for two reasons. First, Boas considered mythology as well as the language and physical characteristics of people as a means of identifying and studying the relationships between the tribes on the northwest coast of North America. Second, he approached myths as clearly revealing "the peculiar customs and character of a people" (Boas 1889, cited in Stocking 1974: 85–86). Thus, like Adolf Bastian, Boas also considered folk myths and tales to be a typical expression of a people's *Volkergedanken*. Besides, he believed that a transcribed text is less prone to distortion by the ethnographer than other types of ethnographic material. The idea that the ethnographer should use the language of a people he or she is studying as much as possible is also inextricably linked with the key tenet of Boasian anthropology: the

need to grasp and interpret a foreign culture from the viewpoint of its bearers (see Rohner 1966).

To a certain extent, Boas had to use methods such as the transcription of texts and working with one or only few informants due to a lack of sufficient resources and time to conduct longer and more comprehensive field researches. We know that except for his initial fieldwork experience in the Canadian Arctic in 1883–1884 when he considered himself a geographer and was just beginning to specialize in ethnography, Boas did not do more than a few months in the field. He rarely lived in Indian families, and despite observing certain everyday practices and rituals of Indians, he hardly ever participated in the native communities' living in general. Some researchers of Boas's fieldwork in British Columbia and the states of Oregon and Washington point out that, apart from the intensive work among the Kwakiutl in 1889 and 1894, Boas mainly conducted surveys see, e.g., Rohner 1966).

It is worth mentioning that when he returned to methodological issues of ethnology in the last decades of his academic activity, as, for example, in the well-known article "The Methods of Ethnology," Boas did not articulate his own approach to the "field" and "field ethnography." Instead of fieldwork methods, Boas (1920) described the object and the nature of anthropological data. Thus, as Antonius Robben observes, "The lasting contribution of Franz Boas to ethnographic fieldwork is his emphasis on the careful ethnographic study of societies undergoing rapid change without reducing cultures to evolutionary laws, psychological processes, or collection of traits acquired through migration and diffusion. His critique is epistemological rather than methodological because he is more concerned with the type of knowledge acceptable to the scientific study of culture than with how to acquire that knowledge" (2012: 53). Nevertheless, before he started working for Columbia University, sponsors of Boas's fieldwork—museums or scientific associations—required him to collect not only texts but also concrete things and materials. Arguably, this fact positively impacted the development of Boasian anthropology in general, as well as his specific ideas about the objectives of a large-scale ethnographic expedition. In combination with Boas's personal long-standing interest in physical anthropology, this requirement resulted in the formation of American anthropology in the twentieth century as a holistic science encompassing, unlike its European counterpart, four subdisciplines—ethnography, linguistic anthropology, archeology, and physical anthropology. Such an integrated approach to the study of history and culture of certain indigenous peoples of North America and Siberia, as well as the rela-

tionships between them, was at the center of Boas's interest when he was developing plans for the Jesup North Pacific Expedition, including the tasks for its Russian participants.

The life of Shternberg and Bogoraz before the participation in the Jesup expedition was closely related to the Russian Populism (*narodnichestvo*), which ideology echoed in their fieldwork in the Far East.

The *Narodniks* as Ethnographer-Fieldworkers: Bogoraz and Shternberg

The exiled *Narodniks* started studying Siberian peoples for several reasons.⁵ First, they needed to somehow occupy themselves.⁶ Second, and more important, the very ideology of the *Narodniks* encouraged them to study the ways of living of a local "oppressed people," whom the progressive intellectuals were obliged to help (Kan 2009b). In this respect, Bogoraz's attention was initially attracted to the Russian population of Srednekolymsk, before focusing on Chukchi and the other indigenous peoples of the region. Shternberg became occupied with studying Nivkhs and the other aboriginal peoples of Sakhalin from the very beginning. Third, both exiles studied at Law School where they became familiar with ancient law and developed an interest in the issues of the social organization of primitive peoples (Shternberg to a larger extent than Bogoraz). Fourth, they both had an interest in languages and could speak several of them. Finally, both future ethnographers, like many other *Narodniks*, were inclined to take notes on people's ways of living. Vladimir Bogoraz known also as Bogoraz-Tan or Tan-Bogoraz was especially eager in this regard and it eventually resulted in his becoming a kind of a "two-faced Janus"—ethnographer Bogoraz and belletrist Tan (Mikhailova 2004).⁷ Shternberg also wrote a few stories from the life of the Nivkhs (Kan 2004).

Arguably, his interest in languages and literature explains why Bogoraz, already in 1890, one year into his exile, had started transcribing the folklore of the Russian population of Kolyma. According to Vdovin, "those activities were not only a linguistic, but also an ethnographic school for him, significantly contributing . . . to his subsequent success" (1991: 82). Along with the folklore and linguistic materials, the exiled *Narodnik* Bogoraz started collecting some specific ethnographic information revealed in the local Russian terminology. As soon as the Chukchi entered his "ethnographic field," Bogoraz faced the problem of communication and translation since he did not have an interpreter. As a result,

he was forced to learn the Chukchi language. Gradually the language became not only a means for Bogoraz to establish communication with his informants, but also the end in itself—that is, the comprehension of an unfamiliar culture through its language. As he wrote a few years later, “language is not simply a tool of communication with the natives without interpreters . . . , but it constitutes the best medium for acquiring knowledge about this people, the medium that is faultless and accurate” (Bogoraz 1930b: 85).⁸ Shternberg came to the same conclusion when, soon after the Nivkhs had become the center of his “ethnographic field,” he realized that “without a solid knowledge of the language, a genuine life of the tribe which interests me and especially its psychic aspects” would stay hidden from him (1908: viii).

In addition to the linguistic and folklore material, ethnographer-*Narodniks* collected information about various aspects of the aboriginal life and culture. Bogoraz apparently paid more attention to the material culture and religion, especially shamanism, than to the social organization. Shternberg, on the contrary, was especially interested in the system of kinship, forms of marriage, symbolism, and functioning of the clan, and other particulars of the social life of local peoples. It is well known that from the very beginning of his studies Lev Shternberg was committed to evolutionary theory and firmly believed that he had discovered the vestiges of group marriage in the life of the Nivkhs. However, the works of one of the authors of this article makes it clear that Lev Shternberg’s special interest to the composition and functioning of the Nivkh clan (and his fascination with it as an institution of “primitive socialism”) was determined primarily by his *Narodnik* views (Kan 2008b, 2009a, b).

It is hard to discuss some of the specific fieldwork methods of the exiled *Narodnik*-ethnographers Bogoraz and Shternberg due to the absence of developed programs of their research, and their unfamiliarity with ethnography. In a paper about Shternberg, Anna Sirina and Tatiana Roon (2004: 71) wrote that his method was intuitive. According to them, “at the early stages of his independent work, Shternberg did not have any professional preparation for carrying out fieldwork. In many respects, he acted on intuition and, in some cases, followed the instructions of the administration (during the first census of Nivkh). As follows from his notes and the early fieldwork reports, Shternberg himself defined the research methods: the observation, everyday records of field situations, complete interviews (rarely), and retelling of conversations (more often). This approach somewhat limited the range of themes and the depth of studies. The contents of the early Shternberg’s diaries are

Figure 1. Vladimir Bogoraz in the field (MAE RAS No. И-1376-187).



Courtesy of Peter the Great Museum of Anthropology and Ethnography (Kunstkamera).

fragmentary; dates, names of settlements where the studies were carried out, important details about informants and the interviewed, as well as some other significant particulars necessary for the further analysis of fieldwork materials are often missing" (Sirina and Roon 2004: 55).

The fact that most of Shternberg's ethnographic materials were collected in the process of census taking among the indigenous population of Sakhalin indeed influenced the formation of his methodology. As follows from the ethnographer's diary, interviews in the settlements he visited as well as his conversations with his Nivkh companion travelers prevailed over the method of "participant observation."⁹ Only rarely did he manage to observe the religious rituals or celebrations of Nivkhs firsthand. The census, requiring intensive but short-term work in each settlement with each and every family, was not a bad method of studying the social structure and the composition of kinship among Nivkhs, but it limited the opportunities to observe the manifestations of those structures in real life, not to mention the comprehension of nuances of the Nivkh worldview. In this respect, Bogoraz was a more serendipitous ethnographer who lived physically closer to Chukchi and, apparently, participated to a larger extent in their everyday and religious life.¹⁰ Both ethnographers quite often relied only on several knowledgeable "key informants," especially when it had to do with the collecting of linguistic materials, legends and myths, as well as discussing complex social institutions and religious beliefs (Kan 2009b: 41–51).

Although during their exile, Bogoraz's and Shternberg's scientific research was focused on ethnography and linguistics, the older tradition of complex naturalist study of indigenous peoples and their environments was influencing both researchers, and this is especially evident in the case of Shternberg. Already during his first expeditions to Sakhalin, Shternberg carried out small-scale archeological excavations, collecting Nivkh artifacts, as well as samples of minerals and plants.¹¹

Upon becoming a research fellow at the Museum of Archeology and Ethnography (MAE) in Saint Petersburg, Shternberg gradually came to an even broader understanding of the fieldwork objectives, in which he included a systematic collecting of anthropometrical data and of objects of the material culture, as well as archeological excavations whenever possible. A comprehensive expedition, such as the Jesup one, became his ideal. In 1910, he tried to conduct a similar expedition, albeit of a lesser scale, when he, in a company of his two student assistants, traveled to and from Sakhalin and the Far East for the first and the last time. Having spent a few months "in the field," the participants of the expedition gathered four large ethnographic collections for the MAE, recorded folklore texts on phonographic cylinders, and collected ethnographic, linguistic, anthropometric, and archeological materials (Kan 2009b: 194–199; Sirina and Roon 2004: 60).

The first Jewish ethnographic expedition in Russia (and in the whole world)—organized by Solomon An-sky (Rappaport) in 1912, funded by Baron Ginzburg and patronized by the Jewish Historical and Ethnographic Society, in which Shternberg was one of the leaders—should have become equally comprehensive and complex. While An-sky was primarily interested in collecting the folklore of Jewish locations, Shternberg insisted on carrying out a true anthropological expedition that would collect data about all aspects of the Jewish ways of living (spiritual and material life) and would also measure anthropometric particulars (Deutsch 2011: 58–62).¹²

Shternberg provided the most detailed accounts of his ideas about conducting ethnographic researches "in the field" in his letters to collectors,¹³ in conversations with students at the MAE and especially in his "Brief Program for Ethnography (applied to the ways of living of Northern peoples)" (Shternberg 1914) issued just before World War I. Following the detailed instructions on gathering objects of material culture for museums, thorough guidelines were provided on how to collect terminologies of kinship and other data on social organization, forms of marriage, religious beliefs, burial rites and other rituals, and, of course, shamanism.

For Bogoraz, the Sibiriyakov (Yakutskaya) Expedition, in which he was invited to participate as an ethnographer in 1895, became a school of comprehensive gathering. During three years, the exiled *Narodnik* was gathering not only ethnographic data on social and spiritual culture of the Chukchi and the Evens of Kolyma, but also objects of the material culture; he also took pictures and some anthropometric measurements (Bogoraz 1899). In 1899, after returning from exile, Bogoraz was assigned to systematize and describe the collection of the Chukchi items that arrived to the MAE from Nicholas L. Gondatti. It was a rewarding experience in working with objects of material culture that proved to be very useful during the Jesup Expedition and in his further work in the MAE.

Even more influential was the Jesup North Pacific Expedition organized by Boas and involving two Russian participants—Vladimir Bogoraz and Vladimir Jochelson (Bogoraz's friend and ally in the People's Freedom [*Narodnaia Volia*] Movement, an exiled ethnographer, and a participant of the Sibiriyakov Expedition). As Igor Krupnik (1996) points out, Boas had set ambitious and complex goals for the expedition, namely to collect comprehensive ethnographic and anthropometric material about the peoples of Eastern Siberia, the Far East and the Pacific coast of North America (from the state of Oregon to Alaska) in order to determine the origin of the American Indians (see also Boas 1898; Vakhtin 2005). At the same time, the Russian participants had to gather a large amount of artifacts related to the peoples of Siberia for the American Museum of Natural History—the sponsor of the project. The project fitted Boasian anthropology very well; it combined a detailed study of each specific culture with the examination of the issue of historical interconnections (migrations and the like) among neighboring peoples.

Although the Russian ethnographers had a whole year to carry out their research, in most cases they could not stay in each settlement for longer than three or four weeks due to the vastness of the territory they needed to cover. As a result, the ethnographic data collected by Bogoraz, as Krupnik (1996) demonstrates, were more accurate with regard to the ethnic groups Bogoraz had studied before the expedition and less accurate, or simply incorrect, with regards to groups he had been previously unfamiliar with. Besides, in addition to gathering the ethnographic material, the ethnographers had many other things to care about as Boas demanded that all the participants of the expedition strictly followed his instructions as if they were his "eyes, ears, and hands" (Vakhtin 2005: 265). Therefore, the fieldwork style of the Russian

ethnographers was reminiscent of the Boas's style of work at the coast of British Columbia. Like Boas, both Russian researchers paid a lot of attention to transcribing myths and tales. It seems that this happened as a result of their supervisor's instructions, although it also corresponded to their own interests.

Before the beginning of the expedition, Boas gave Bogoraz and Jochelson instructions, first by sending them his publications and some other new works on North American Indians and then by conducting a series of long conversations during their stay in New York before their trip to Alaska, and from there to Chukotka. After the completion of the expedition, Bogoraz spent a year and a half in New York systematizing his collections and preparing his field notes for publication. It is during this period that a life-long intellectual relationship and friendship between Bogoraz and Boas started (Kan 2008b).

Shternberg did not directly participate in the Jesup Expedition, but having met Franz Boas at the Congress of Americanists in 1904, he also established strong scientific and personal relationships with him. Considering that Shternberg was a major specialist in the culture of Nivkhs, Boas asked him to write a monograph on the subject for the series of the Jesup Expedition publications (Kan 2001). Both Bogoraz and Shternberg always treated Boas with great respect and, according to some sources, called themselves his students (Shternberg 1999: 245–256).

So, what was the influence of Boasian anthropology and Boas's understanding of the field on the anthropology of Shternberg and Bogoraz, who founded the Leningrad School of Soviet ethnography? First, Bogoraz and Shternberg valued Boas's broad erudition, his rigorous approach to any scientific problem, and his interest in historic interconnections among various peoples. As to the direct influence of Boas, it appears that the Jesup Expedition confirmed the already existing ideas of both Russian researchers that there was a need for a comprehensive study of cultures and peoples. Second, Boas provided professional and scientific grounds for the linguistic and folklore studies of Bogoraz and Shternberg, and for their experience-based idea about the necessity of learning the language of a people under study. They also learned from Boas how important it was to study every manifestation of a culture as part of its common Gestalt, and to try to comprehend the beliefs and actions of bearers of unfamiliar cultures from the point of view of those bearers. Even the idea of "cultural relativism," so tightly associated with Boas's name and especially his students, was not alien to Shternberg (and probably to Bogoraz either) who did not consider that the European culture was universally superior to non-European

ones. In the 1920s, Shternberg observed with regret the disappearance of certain elements of the Nivkh way of living and culture which he highly praised in his earlier studies (see Kan 2009b). Finally, although Boas himself did not practice the "stationary method," he undoubtedly supported his Russian colleagues who promoted it.

However, neither Bogoraz nor Shternberg became committed devotees of Boas's "historic particularism." Even Bogoraz, who was less concerned with theoretical issues than Shternberg and who changed his theories several times, managed to push his scheme of the evolutionary development of the Chukchi religion through and to include it in the monograph written for the Jesup Expedition. As to Shternberg, he was a committed evolutionist until the end, and considered Boas and his followers to be excessively dedicated to empiricism and skepticism whenever he spoke about theoretical issues and the interpretation of facts.

All these ideas Shternberg and Bogoraz tried to realize in the lecture courses at the Leningrad State University in everyday communication with their students.

Conceptualization of the Field: Lectures and Field Programs

The consistent evolutionism of Shternberg and, to a lesser extent, of Bogoraz was implemented in their work at the MAE and in their educational programs. The evolutionist paradigm encouraged the creation of classifications (on evolutionism in discussions and exhibitions in the MAE, see Kan 2008a). Having spent a long time in the field (read exile), the founding fathers of Soviet ethnography, when looking back at their field experience, preferred the style of the eighteenth-century naturalist for the description of their concept of the field. We offer two citations. First, Shternberg wrote in 1914: "Observe ethnographic phenomena with the same rigor, comprehensiveness, and objectivity as naturalists observe the phenomena and objects of nature" (Shternberg 1914: 212). He continued repeating those recommendations to his student ethnographers some years later (Shternberg n.d.: 17). And after Shternberg's death in 1927, Bogoraz echoed those instructions in his lectures on field ethnography: "Method of [field]work. Accuracy. Rigor and particularity of observations. It [fieldwork] tends to get closer to the methods of natural sciences and therein lies its basic affinity to the common methods of the Faculty of Geography" (1927: 1). It is noteworthy that at that time

the ethnographers' colleagues from the humanities sympathized with such an approach as well. Let us recall the title of Vladimir Propp's book, *Morphology of the Tale* (1968) and his argument that it is possible to apply the same approach to the tale as that used in botany, namely the "morphology of organic formations." Linguists also distinguish morphology as a specific subdivision of grammar. It should be noted that the ethnographers, especially Shternberg, considered the language as a given natural phenomenon serving as a clue to comprehending such dimensions as spiritual composition, religious views, and ornamentation. At the same time, this interest in language allowed Shternberg to extend his evolutionist views, providing an opportunity to sidetrack towards psychology and "cultural relations." However, the "vestiges of the past" (survivals, *perezhitki*), traceable in the local names of objects or phenomena, but unnoticed by the culture bearers, steered him back to evolutionism (Shternberg n.d.: 17).

The researcher was supposed to textually copy the surrounding world, attending to each detail of domestic life, ritual, material objects, and so on. "In gathering of the materials, an ethnographer is absolutely similar to a natural scientist. He collects material objects, records and classifies groups . . . of facts, and attempts to combine them into a more or less coherent whole. The ethnographer collects samples of human work. But the living human material is impossible to pack, to keep in alcohol, or to prepare as a stuffed dummy. Thus, the main element of gathering and observation is a human fact, recorded accurately and with abundance of details" (Bogoraz 1925: 7; see also Makar'ev 1928: 6). This so-called human fact (see a later idea of an "ethnographic fact" [Pimenov 1990]) could appear in any form; from a thing to a social institution. This view was analogous to some ideas of Boas's followers in Berkley in the 1920s and 1930s, for example Alfred Kroeber and Robert Lowie. In the case of American anthropology, the list of culture element distribution has transformed, thanks to George Murdock, into the Human Relations Area Files. These undertakings are comparable with the development of a number of historic and ethnographic atlases in the USSR that also attempted to classify and map objects of material culture. The latter case is, of course, reminiscent of the encyclopedic approach to ethnographic text (implying certain continuity) that logically relates it to both the German school of ethnology (*Völkerkunde*) as well as to the Boasian program of description realized in case of the Russian Arctic by Bogoraz and Vladimir Jochelson. In understanding Shternberg, the division of each "encyclopedia" was supposed to be consistent with the overall scheme: material culture, social organiza-

tion, and spiritual culture, with more multiple subdivisions, which Shternberg, following the spirit of natural sciences, called "species" (n.d.: 18). For their students, and especially for the students of the students, the theme of gathering became one of the main ones (Bogoraz n.d.: 5); this was the objective of the MAE as well as of the science in general as they were transforming, in Foucauldian terms, monuments into documents (Foucault 2002: 7).

The easily observable "geological" visibility of the field pre-established the frameworks of approaches and pre-concepts. Along with the naturalist view of the field, it was the construction of the revolution as a "rupture" when all the previous local achievements were revised and the new ones—defining a new way of living—were established. This idea can be traced in Shternberg's writings (Kan 2009a) and especially in Bogoraz's lectures: "to carry out ethnographic observations at the epoch of the revolution according to the old habitual static program is absolutely impossible. The revolution is an earthquake, a volcanic eruption. On the one hand, it reveals the primordial formations of social and spiritual culture, takes out deep scoriae and remnants of rare minerals. On the other hand, from these extruded formations, in the heat of the revolution, new soft soils impregnated with fertility and suitable for the germination of green and abundant shoots are being gradually created through grinding, pressing, and weathering" (1925: 3). The rupture as the theme "old and new Russia" was the main research program for students in 1923–1924 (Stanyukovich 1971: 32).¹⁴

Peoples were classified in layers: "the most desolate, desert and wild tundras still abound with such names, and geographical names give us the clue to the ancient differentiation of peoples of those territories" (Bogoraz 1925: 10). This context set some limitations to the field, not only enforcing the split of the culture into "old" and "new," but also constructing an archeology of the place, and dividing local communities into "ancient" and "non-ancient." Both of those approaches combined a "history of the people" with a "history of the territory"¹⁵ and that was later reflected in the elaboration of the theory of ethnos. It should be noted that these binary oppositions (old/new, ancient/non-ancient) deeply penetrated the field practice of researchers, changing with time certain contexts, as for example, in "old and new ways of living" (*staryi i novyi byt*) that had become "traditions and innovations," with the borderline between the two being, for a long time, identified with 1917. The stability of those binary oppositions is apparently also related to a "historicization" of the discipline that could well accommodate Bogoraz's vision of the field, although it did not fully accord

with the spirit of Marxism (for the early Soviet critique of Bogoraz and Shternberg, see Aptekar 1928; Bogoraz 1930a).

The naturalist context used in the lectures and fieldwork programs revealed yet another important aspect—the contact between the field and the body of the ethnographer.

Bogoraz played up the idea of “bodily experience” ironically: “Keep in mind,” he warned students at one of the first lectures, “only that can become an ethnographer who is not afraid of feeding a pound of one’s own blood to lice. Why to feed, one may ask? Because to understand and to study a people is possible only when you’re living the same life as they are. And the louse is a very widespread animal there” (Gagen-Torn 1994: 51; see also Arzyutov et al. 2014: 263). In a certain sense, the articulation of corporeality reflected not only the experience of the exile, but also the very spirit of *Narodniks’* views. Furthermore, we will see that the corporeality discussed in the programs and lectures constituted a serious challenge for the students in the field. In the bodily experience, so far defined only discursively, some mimetic contexts were also traceable. As Bogoraz instructed, “Do not embarrass your acquaintances and storytellers with skeptical notes and mockery. Be serious with them. *Always play their own game with them*” (n.d.: 6; emphasis added). Such a “game” quite often assumed that ethnographers had to dress in local clothes—that would facilitate their blending in

Figure 2. Glafira Vasilevich in Tungus dress. (MAE RAS: uncatalogued picture).



Courtesy of Peter the Great Museum of Anthropology and Ethnography (Kunstkamera).

Figure 3. Andrei Danilin in Teleut fur-cap (AMAE RAN 15/1/54: 15).



Courtesy of Peter the Great Museum of Anthropology and Ethnography (Kunstkamera).

within the surrounding cultural environments and the inclusion of a researcher in a culture (it is especially noticeable at a few existing field portraits of Andrei G. Danilin in the Altai region and Grigorii D. Verbov among the Nenets).

Here one can see the emergence of the participant observation method as the main method in all schools of ethnography and anthropology; the corporeal experience was becoming one of the axes in the elaboration of the concept of the field. Probably, those were the echoes of the *Narodniks'* approach, analyzed by Nikolai Ssorin-Chaikov (2008), which combined two viewpoints—"as is" and "as should be"—which, it can be argued, opened the field in the nineteenth century.

Ethnographer-fieldworkers were setting the tone in the perception of the field. Akhil Gupta and James Ferguson drew attention to this fact and argued that anthropologists were creating the imaginary of the field themselves. In some respects, Shternberg and Bogoraz, being fieldworkers who formulated rather broad concepts of the evolution of beliefs, ethno-geography, and so on, were not only teaching the methods of fieldwork but also creating the very "grid of specification," according

to which the ethnographic material should have been collected for its further use, analysis, comparison, and so on. On the one hand, Bogoraz viewed ethnography as consisting of several specific blocks: “Studying of a culture is carried out according to three divisions: 1) material culture, 2) spiritual culture, 3) social culture” but, on the other hand, he immediately claimed: “but in essence, there is just one culture” (n.d.: 6).

Thus, the programs and lectures of the 1920s and 1930s grounded the specification of the field, which was reflected in the ethnographer’s fetish—his or her field diary; Bogoraz recommended keeping a diary as well as thematically sorting fieldwork notes. The significance of the textual reflection of the field was only slightly affected by photography, whose technical aspects had not yet been discussed in special educational programs, but had been touched upon in Bogoraz’s programs. In general, the vision of the field was based not on a strict description of the reality, but rather on the necessity to fit one and the same grid: “an ethnographic picture, unlike a geological or botanical one, [dealing] with the extreme complexity and mobility of its object, should be organized in advance” (Makariev 1928: 31).

Bogoraz’s lectures on field ethnography bring us closer to the texts of his fieldwork diaries where observations are mostly focused on nature. Transcriptions of conversations are quite scarce in the diaries, and are mostly concerned with folklore. Researchers would later pay a great deal of attention to the description of material culture—to a large extent under the influence of students’ work in the MAE, and Bogoraz’s ideas about ethnography.

In addition from the Bogoraz’s lectures, the discussions on the field in conferences of the 1920 and 1930s changed the idea of the field, bought to fore the historicity of the discipline and its “marxisation.”

The “Fundamental Break”: 1925–1932

In his article on the history of the early Soviet ethnography, Solovey (2001) called the Conference of Ethnographers of Moscow and Leningrad in 1929—using the common metaphor of that time—the “fundamental break” (*korennoi perelom*) in Soviet ethnography. Rather briefly reviewing the history of the meetings during between 1925 and 1932, critics (e.g., Alymov 2006: 72–87; Bertrand 2002, 2003; Cadiot 2007: 174) have overlooked the debates about the field that were held at the meetings. However, the series of those meetings proves to be significant for understanding a specific theoretical aspect of the field. The performa-

tive character of the majority of researchers' statements discursively constructed the field as the main kind of ethnographic activity and made it the primary factor in the separation of ethnology as theory from ethnography as practice. This juxtaposition has become codified for a long time and is still influential today in the frameworks of contemporary—already Russian and already anthropological—knowledge. Despite the critique of ethnography, it is the field that is usually considered to be the heart of ethnography that not only creates the identity of a professional group, but also brings the problem of the ethnographic method to the forefront. Discussions about the methodology of ethnology and social-cultural anthropology were also held at that time in both Europe and the United States. However, speaking about the method, although the field was being gradually introduced in the conceptualization, it was never considered in terms of becoming an independent "discipline." In this respect, the experience of Alfred Radcliffe-Brown is indicative. Following the thoughts of W.H.R. Rivers, articulated in 1911, on the intersection of the idea of evolution with the diffusionist thesis, Radcliffe-Brown, in his message to the South African Association for the Advancement of Science in October 1923, juxtaposed ethnology and social anthropology, referring to the historic method of the former and the inductive method of the latter (Radcliffe-Brown 1923). Starting in the 1920s and 1930s, the trajectories of Soviet ethnography and British-American anthropology had been increasingly diverging from one another. While the former was becoming ever more "historical," the latter was tending toward a more sociological approach. For this reason, the field was becoming increasingly less important for the former and increasingly more important for the latter.

Also in the 1920s, the identification of ethnography as the field practice (a "non-science" in Aptekar's understanding) was related to the process of a historicization of the discipline or, frankly speaking, to an establishing of the total control over "total social facts" (in Marcel Mauss's terms) and their immersion into a history for the sake of proving their inviolability. In many respects, the field was becoming redundant. At the meetings of the 1920s and 1930s, primary attention was paid to commentaries on texts—above all, Joseph Stalin's—on the national question. However, despite all of that, the field that survived was the one that was related mostly to political projects of the state (censuses, activities of the KIPS, the League of Militant Atheists, and so on) when an ethnographer, as an expert in a local culture, was supposed to, when going on an expedition, provide accurate answers to concrete questions from above (as, for example, in censuses), to make a list of shamans,

or to take part in confiscations of ritual objects for museums or, even worse, for their destruction. Due to such pressure, ethnographers had to live by double standards—complying with the official claims, but also practicing ethnography as understood by their teachers (Shternberg and Bogoraz); the latter being sometimes quite a dangerous enterprise.

Let us return to the historicization. According to Juliette Cadiot, the concept of the origin in the formation of the idea of nationality manifests itself in the censuses of 1920 and 1927, that is, at the time generally corresponding to the period of the meetings (Cadiot 2007). Debates about the field at the Meeting of Ethnographers of Moscow and Leningrad took place on 8 April 1929 and the main addresses included the talks by Bogoraz, “The Stationary Method in Field Ethnography,” and by Boris Kuftin, “The Goals and Methods of Field Ethnography.” At the end of the meeting, Boris Sokolov, on behalf of Sergei Oldenburg, delivered a speech titled “Integration of Ethnographic Expeditions of Separate Institutions.”

Kuftin articulated the following at the 1929 meeting: “Ethnology is an historic science. Studying tribal, cultural-material, and economic compositions, it aspires to comprehend them in the process of their formation, as products of the social development of the entire human-kind at the stages of a more primitive, pre-industrial sociality; albeit more advanced [compositions, than that of the primal societies], but still keeping remnants of the latter in these or those parts” (Kuftin: 1–1v). However, the historicity and the already emerging influence of Nicholas Marr’s linguistics, which we discuss below. At approximately the same time, but in a more confident manner (including the use of terminology), Sergei Tolstov stated: “Ethnology is an historic science and it, as social sciences, to which it belongs, in general, should be built on the methodological foundation of the theory of Historical Materialism. This should be articulated clearly. Its distinction from, let’s say, history as such lies in [the fact] that it works primarily retrospectively [using] the historic method, i.e. studying facts of contemporary cultures, [it] reconstructs a history of social groups connected with them [cultures], i.e. an analysis comes from a culture to a society, because a culture existing at our time, today, reveals the stages of an historic development of the society completed long ago” (cited in Aptekar 1928: 12). In addition to Tolstov, sociologist Lassi delivered a vivid but quite incoherent speech (which was characteristic of the majority of speeches at those meetings): “Ethnography relies on field researches. Its field method is a simple gathering. Its laboratory is museums. A special area of ethnography is a scientific description of people’s ways of living; first of all, of the most

specific elements of a material folk culture, especially of a dying one. Ethnography collects, describes, classifies, compares (mainly external features), and popularizes all these materials as well as studies their diffusion, antiquity, and meaning. Ethnography deals mainly with a folk ideology in its linguistic, artistic forms. Folklore provides valuable material for a history of literature and art as well as for ethnology" (cited in "Disput 'Marksizm i etnografiia'" 1930: 15). At another point, Lassi added that ethnographic methods also include "observation and interrogation" ("Disput . . ." 1930: 20). After the war, the historicism in ethnography, which had been so fiercely promoted at those meetings, made even ethnogenesis an object of field research (Chinchaladze 1962: 40; Gromov 1966: 7).

Perhaps for the first time (and probably for the last in the whole Soviet period), those who were to be studied—representatives of indigenous peoples who, being students in Leningrad, participated in the meeting—were invited to an academic meeting, creating a discussion space where the opposition between the object and the subject of research was not discernible. One of the participants, Yukagir Theki Odulok, appealed to ethnographers carrying out expeditions: "One should treat an aboriginal as a human being. Aboriginals still do not believe. In every newcomer, they view an exploiter or at least a person who is going to take away from them not only all of their territory, but also all wealth, furs, crafts, and so on. Everyone who comes there always has a paper and a pencil. And this is the scariest thing for an aboriginal. The aboriginal is scared of it to death of it. If a Russian imagines the death as a skeleton with a scythe, for an aboriginal, the worst view is a kind of a large-nosed man with a paper and a pencil. So, this implies the following: any scientific fellow, researcher and especially an ethnographer should become an active builder of life" (Arzyutov et al. 2014: 327–328).

The discussion about the field in 1929 demonstrated the difference between the approaches of ethnographers from Moscow and Leningrad. The former insisted on short-term trips, which could provide enough materials for the organization of exhibitions (as in case of the Museum of Ethnology) and for the preparation of reports sufficient for the authorities. The latter, led by Bogoraz, referred to the experience of the predecessors and advocated what we today call "participant observation" as part of long-term fieldwork (Arzyutov et al. 2014: 254–335).

Sergei Oldenburg, an influential figure at the Imperial and early Soviet Academy of Sciences, considered the field to be an element of bureaucracy. He devoted an entire speech (delivered by Boris Sokolov)

to the idea of creating a unique state field project. He aspired to build a pyramid of expedition bureaucracy, namely to organize a special body that would coordinate all expeditions. It was as if he was seeking to establish some sort of Panopticon, keeping account of the knowledge gathered about the population within the empire, and monitoring the movement of ethnographers within the imperial spaces. He, like many others, believed that a greater coordination and a stricter administration would improve the overall quality of work. Oldenburg's project on establishing a certain type of a coordination center of field researches ("Ethnographic Expeditions Bureau of the RSFSR," which was envisioned to become the "All-Union Ethnographic Expeditions Bureau" in the future) was to pursue three main goals: 1) to create directories of ethnographers and maps of their expeditions with the indication of "white spots"; 2) to coordinate plans of expeditions; 3) to organize unified field programs.

Oldenburg's never-realized project was most probably seen to be competing with another project, which already existed in the USSR since 1926—the Special Committee on Studies of Union and Autonomous Republics, (chaired by Alexandr E. Fersman), also known as the Expeditions Committee. As its mission, the Committee considered the "studies by the Academy of Sciences of union-level and autonomous republics and regions. Conducted within the last two years, the reorganization of old boundless Russia according to a national-domestic criterion, first of all, has prompted a healthy desire for a self-cognition and for raising the welfare of the masses in various locales" (*Osvedomitel'nyi biulleten'* 1926). The Committee's main function was supervision of all expeditions. Specialists in various areas were supposed to write reports and publish them on the pages of the *Osvedomitel'nyi biulleten'* (issued two to four times a month), which was not for sale but was distributed through scientific institutions (it is a bibliographic rarity today).

Large-scale meetings that took place in 1932 were even more politically driven than the discussions of 1929. They debated the ideas of the struggle against religion (through cinema, among the other means) and nationality-building projects ("Stenogramma soveshchaniia arkheologov . . ." 1932; "Stenogramma soveshchaniia po voprosam . . ." 1932).

It should be noted that starting with these meetings, the researchers' discursive practices were becoming more complex. On the one hand, this growing complexity was due to the need to "ritually" refer to the works of Marx, Engels, Lenin, and Stalin (later replaced with the decisions of the most recent Party Congress, yet further transformed

into references to works of the heads of the Institute of Ethnography/Ethnology and Anthropology). On the other hand, certain shifts were happening in the discursive grid of ethnography. Certain research themes were almost completely shifted from central focus to the periphery, for example systems of kinship (especially in context of the issue of the existence of kinship). Tamara Dragadze's (2011: 27) observations are particularly relevant to this point as she faced the indifference of researchers with regard to the problems of kinship already in the 1970s, by experiencing their predilection for the themes of socialist transformation of ways of living where the idea of the class structure and struggle as well as that of socialist advancement could be realized. Arguably, the best example is the words of Leonid P. Potapov from his interview to Valerii A. Tishkov: "I have proved, exactly proved, basing on the concrete material, that the peoples of Altai used to have the class stratification and wealth inequality. In this, Lenin happened to be truly helpful, [namely,] his 'Development of Capitalism in Russia.' As you remember, Lenin criticizes adherents of average numbers there, himself providing completely specific data from A to Z. I used this approach for an analysis of the materials of the Census 1897. The outcomes were truly wonderful, a convincing picture of class stratification" (Potapov 1993: 110–111). In this regard, a struggle between ethnographer-fieldworkers in the Department of Ethnography of Siberia of the MAE is also noteworthy. The idea of studying the composition of kinship, and the issue of kinship in general, confronted the idea of studying the class structure as a necessary condition of "historic materialism" (see Nadezhda P. Dyrenkova's account on Potapov's studies: Dyrenkova 1938: 1–1v.). Specifically, those debates highlighted the problem of connecting field materials to the discursive grid that the discipline had constructed during the period of the meetings of 1925 through 1932.

Since the history of the meetings was, according to the astute commentary of Sokolovsky (2011b: 72), not so much academic discussions but rather the medium for the implantation of political decisions, the field had been assigned two main functions: (i) the reconstruction of a history, and (ii) the definition of ethnicity (Dragadze 1978: 65). Each of those functions bore fruits for the postwar ethnography. The former one facilitated an obsession with the idea of a dying culture and a compulsion to capture and to fix "the last something," whereas the latter gave birth to the primordialist "ethnophilia." The reconstruction of a history had moved forward considerably, creating the foundation for historic (in this case, ethnographic) source studies, based on very limited field researches and combining museum artifacts with archival documents.

The Disciples

Having witnessed the meetings and the emotional intensity permeating the discipline, many students took great care when selecting the themes of their research in the field. Arguably, only the themes related to artifacts and folklore were considered safe and convenient for interpretation. However, not all folklore was regarded as suitable and so it became necessary to omit or to remove contemporary “politically incorrect” folklore and narratives when in the field, or to pass them to the special archives (of, for example, Pushkin House [Pushkinsky Dom]; Komelina 2013). “Politically correct” materials, on the other hand, were published, as for example, the book by Dolgikh “Domestic tales of the Nenets” (“Bytovye rasskazy nentsev” [1962]) or the article by Vasilevich “Autobiographies of the Evenks” (“Avtobiografii evenkov,” [1938]).

The teachers envisioned the big themes, such as ancient migrations studied by Shternberg and Bogoraz in collaboration with Franz Boas and the Jesup Expedition. At the moment, however, they had just articulated the idea of “the origin of peoples,” which was later elaborated with an almost religious adherence. Infamous Marr must have also played a serious role in the development of the big themes. Specifically, his idea of the Japhetites, which produced a significant authoritarian impact on the humanitarian thought of that time (see details in Shnirelman 1993; Slezkine 1996), was refracted in Bogoraz’s ideas of the Proto-Asians. Bogoraz attempted to find a place for each people in his large-scale panorama of migrations. For instance, Shors were assigned to the “Paleoasiatic [Paleosiberian] root” (Bogoraz 1927: 42). These ideas were presented at international forums as the most progressive: Bogoraz (1926) delivered a speech “Paleoasiatic Tribes of South Siberia” at the Congress of Americanists in Rome. He also developed a course of lectures for the students of the Leningrad State University where he discussed his concept in detail (Stanyukovich 1971: 126).

It should be acknowledged that the desire to link the field with the migrationist concepts was not always successful. There was an expedition of Dyrenkova and Starynkevich (Khlopina) to Shors—“the most primitive Turks”—in 1927. Bogoraz and Shternberg viewed its goal to be the study of the Shorians’s “emergence in Western Siberia” either from the North (Bogoraz) or from the South (Shternberg) (Khlopina 1992: 108). Nevertheless, no publications by Dyrenkova and Starynkevich (Khlopina) dealing with the “south” or “north” in the history of Shorians appeared, and those particular ideas turned out to have little influence on their scholarship. Arguably, Bogoraz’s idea was realized

only in several works by Georgii Prokofiev—an outstanding Soviet researcher of Samoyedic languages who attempted, relying on his field material, to instrumentalize both the idea of ethnogenia (ethnogenesis) borrowed from Marr's linguistics as well as the concept of the "Paleo-asians" by Bogoraz (Prokofjew 1933; Prokofiev 1940).

The actual implementation of Marr's ideas was to a large extent reduced to ritualistic (or quite far-fetched) references, for example, in works based on the field materials of Dyrenkova (2012: 202) and Danilin (1932: 66–67). Marr's influence, or even Marrism, was noticeable first of all in the constructed discursive grid of the discipline with a necessary historicity and the class nature of sociality.

We would now like to move from the field as seen from the capital—to the actual field. Reading researchers' field diaries, a certain fact attracts attention, namely that the texts of those diaries were a realization of the naturalist discourse of the programs and lectures of the teachers. Descriptions of nature and descriptions of social relations and material culture alternate so often that it is quite difficult to differentiate between them. Ethnographers were indeed describing the surrounding world with the precision of a naturalist, integrating nature and society in one common field. At the same time, the diaries are written as a whole text—not a collection of fragmentary notes, but a continuous narration—combining both the tradition of keeping everyday personal diaries with the influence of travelers' diaries and notes, which had become an important source for the first ethnographers ("Aus Sibirien" by Wilhelm von Radloff, "Ocherki Severo-Zapadnoi Mongolii" [Sketches of the northwestern Mongolia] by Grigorii N. Potanin, for specialists in South Siberia; "Dnevnye zapiski . . ." [Everyday notes . . .] by Ivan I. Lepekhin; "Puteshestvie na Sever i Vostok Sibiri" [Traveling to the north and east of Siberia] by Alexander T. von Middendorff—for specialists in the culture of the peoples of Northern Siberia). As Ilia S. Gurvich points out with regard to the fieldwork of the Jochelsons family, the peculiar feature of diaries during those years was their beautiful prose. A diary was imagined to be a kind of a journalistic text, which can be characterized as essay-style writing (Gurvich 1963: 255). This practice was transferred to disciples.

Referring to the preserved diaries of the disciples, one can assume that the majority of these researchers used to take books of their predecessors to their fields. Such a field library was not, however, very diverse. The majority of Siberian anthropologists (for example, Georgii Prokofiev and Grigorii Verbov) relied on the works by Castrén who, in the 1920s, seemed to be the most important theorist in Siberian

ethnography and who produced large-scale descriptions of migrations, substantiating his theory with language materials. In addition to Castrén, the “field library” included Bogoraz (Varvara Kuznetsova) and Shternberg (Erukhim Kreinovich) (see Mikhailova 2015: 18).

Reading the works of predecessors in the field (or following their ways) resulted in the first experiences of “reflexive anthropology” (Burawoy 2003). The field as such was becoming the space of meeting not only with aboriginals, but also with the experience of the predecessors and their concepts.¹⁶ This interweaving has resulted in a considerable complication of the temporal perspective in anthropology. As we mentioned in the beginning, following Gupta and Ferguson, the Other was positioned not only within another space, but also within another time (“them” as primal, primitive and the like) (Fabian 1983). It was further complicated with yet another temporal dimension of already quite complicated constructions of time in ethnographic concepts. Such reflexivity of the field made it possible to integrate ethnogenesis as the origin of peoples into the ultimate human experience of fieldwork.

Let us return to the technique of fieldwork. Preparing young ethnographers for fieldwork, Bogoraz instructed them in his lectures: “ethnographic teams should not be big . . . the natural composition for an ethnographic expedition is two persons, usually a M[an] and a W[oman], for a more convenient approach to men and women” (1925: 8). Indeed, referring to the history of the Siberian field, we can see that ethnographic expeditions were carried out by small groups of researchers (for example, Nadezhda P. Dyrenkova, Lidia E. Karunovskaya, Lidia B. Panek, and Anna E. Efimova in the expedition to Altai in 1924), or by ethnographer couple, Ekaterina D. and Georgii N. Prokofiev in Yanov Stan.

Alone or as part of a small group, the students faced another, previously unknown, culture. At this point, the second stage of the “corporeal experience” of (already) Soviet ethnography started. From the diary of Dyrenkova, Mountain Shoria, 21 September 1925:

The host, sitting, was already sleeping, when he woke up [from time to time], he started making noise and singing. . . . [We] started dining. Despite the syphilis in the family, my hosts were all eating [together]. The host could not already stand up. Only the legs of the hostess were visible—she had fallen onto the bench. Trofim was sleeping, with his head [resting] on the folded net. His drunken mother was shouting at him and cursing. Uzuga kept repeating “Kazak” and “Tatarin” and, being drunk, was hanging on my shoulder. [We] went outside, to sleep in the balagan [a traditional wooden dwelling of Shors and other Turkic groups in Siberia], and were cold all night. (Dyrenkova 1925: 18)

The other examples to recall are tough field experiences of Ekaterina and Georgii Prokofievs (Gagen-Torn 1992) and a terrible tragedy of a student Natalia Kotovshchikova who died on the Yamal during Valerii N. Chernetsov's expedition (for a recollection of the story, see Golovnev 1995).

Despite their general preparedness for hardships in the field, young ethnographers did not expect this kind of an encounter with the Other. In this respect, the diaries preserved some sort of sincerity that was only present in a very vague form in official reports. Probably, such astonishment (but neither humiliation nor negation!) was influenced by the living conditions of the researchers, which were, to say the least, quite uncomfortable—researchers often lived in communal housing and experienced extreme hunger. It is worth mentioning that such a view of the field was also connected with the imaginaries about the field expressed in the following fragment of the archived text of Kreinovich, dated June 1928 and cited by Bruce Grant in his *In the Soviet House of Culture*: "We want to *erase* this line between subject and object, between us and them . . . The object of study must become subjects" (Grant 1995: 77).

It should be acknowledged that this contradicted contemporary famous diaries of Bronislaw Malinowski who demonstrated double standards of field practice as an obvious difference between a diary and a scientific text (Malinowski 1989; see critique in Geertz 1967, 1988: 73–101; Hsu 1979; Wax 1972). Researchers encountered a new side of life of their own country, and to struggle against it could mean, first of all, a negation of the advancements of the Soviet power, which could lead (and indeed led in some cases) to sad consequences (one can recall the fate of an outstanding ethnographer Glafira M. Vasilevich). To be an ethnographer-fieldworker meant, above all, to be an expert on local communities or even a culture bearer who maximally participated in the life of those people—actually changing it. Let us recall here Bogoraz's slogan that claimed that ethnographers are the "missionaries of a new way of living." This development project, however, was not free from the restrictive power that limited both the real participation of researchers and their interpretations. During this time, researchers transformed into "state ethnographers," as David Anderson coined it, and field research itself turned out to be a political project focused on endless ethnic (re)categorizations (see an example of Evenks and Dolgans in Anderson 2000a: 74–96).¹⁷ Having barely gained any strength, the discourse of the discipline remained stifled by those limitations for years—in many respects this explains its awkwardness. For some it was fear and for the others it was an obedient existence.

The triumph of field ethnography at the beginning of the twentieth century, which required long-term living in the field, was superseded, after World War II by short-term summer trips. Despite the practice, the term “expedition” was only adopted in ethnography after World War II, as well as the term “field diary”—probably due to the bureaucratization of the discipline, and due to such facts, as for example, the publication of small-format typographic diaries with lined pages and a template for a researcher’s profile at the front page. Before the war, it had been possible to find diaries that were labeled as “field journal” and “traveler’s notes,” however Bogoraz and Shternberg often referred to the expeditions of students as “excursions” or “trips” (*poezdki*).¹⁸

In the 1920s and 1930s, diary notes belonged primarily to ethnographers who kept them in their own private archives. After the death of a researcher, those archives were usually sorted out and transferred to the institution where he or she used to work. Before 1946, documents of the MAE, including, as a rule, the personal archives of remarkable scholars and those with the title of an academician or a corresponding member, were handed over to the Archive of the Soviet Academy of Sciences. Many personal collections of scientific fellows of the Museum were entrusted to its scientific departments, its library or to the scientific secretary. The MAE Archive (at that time the Institute of Ethnography of the Academy of Sciences of the USSR) was established in 1946. “Live” and “dead” diaries belonged to absolutely different spheres of circulation and utilization than field materials. The former continued to provide grounds for scientific researches of ethnographer-gatherers, while the latter became the objects of the state’s second colonization of scientific knowledge—after expeditions. However, this bureaucratic colonization enabled saving the majority of the documents. At the same time, the field that had become the archive was anew transforming “documents into monuments,” where contexts were becoming less and less important whereas the text was gaining significance because of its supreme authenticity.

Summarizing our discussion of the field, it is worth mentioning that, by the beginning of World War II, a specific concept of the field could already be identified. This concept was an interweaving of a significant number of discursive fields, reaching back to the epoch of Enlightenment and deriving elements from the experience of field activities of Vladimir Bogoraz and Lev Shternberg under the influence of Franz Boas.

Consequences and Paradoxes

Looking back, one can argue that the early Soviet period in the history of ethnography that we have outlined in this article is rightly considered to be the epoch of great advancements of Soviet researchers, despite the facts that the tradition itself was born in exile and many of its "stars" were at some point repressed by the state, killed in the war or passed away in besieged Leningrad. Assessing those achievements from our position today, with a distance of almost a century between the field of then and the field of now, certain comparisons are worthy of attention.

Working with the field programs, texts, and transcripts of lectures devoted to fieldwork between the 1920s and the 1980s, a certain difference between teaching ethnography in the 1920s and, for example, in the 1980s kept capturing our attention. Initially, the teaching of field methods was based on training ethnographers to prepare them for being in the field; it assumed mastering of various techniques, such as horse riding, drawing, and photography. These were not optional, but

Figure 4. The official map of the expeditions of the Institute of Ethnography, 1918–1947. It does not contain many field trips the prewar ethnographers carried out (MAE RAS No. И-1376-215).



Courtesy of Peter the Great Museum of Anthropology and Ethnography (Kunstkamera).

rather full-fledged courses. Over time, these courses gradually shifted to the periphery of the professional education and were replaced with training in the techniques of writing, that is learning how to fit any material within the grid of the discipline. Arguably, this can explain why all of Soviet ethnographic texts of the post-war period are so similar to each other, regardless of the location of field research.

It is remarkable that Soviet ethnography, throughout most of its history, could manage without teaching aids on field ethnography; there were hundreds, if not thousands, of expeditions and only two textbooks—one by Makar'ev (1929) and another one by Gromov (1966). Probably, this fact reveals yet another shift in the post-war Soviet ethnography, namely, that teaching of methods and techniques was transposed from a written into an oral practice. Referring to conversations with our colleagues, we can infer that the preparation for the field was carried out as an intra-school oral communication or simply through a participation in big collective expeditions (what a difference from Bogoraz's ideas).

The metamorphoses in understanding of the "ethnographic time" are also very interesting. After the discussions about the field, specifically those of the 1920s and 1930s, it took more than forty years for Soviet ethnography to recognize anew the existence of the "ethnography of modernity" (Shmeliova 1985; Iukhneva 1980), returning to the differentiation between the "old" and "new" ways of living, as at the time of Shternberg and Bogoraz. The problematization of "modernity" was issued from above: "The object of ethnography of modernity is ethnoses that have reached the industrialized-urbanized level [of development]" (1980: 14). In the field, such definitions apparently played only a secondary role: researchers tended to rely on quantitative indicators. On the one hand, it corresponded to the "fashion" of those years. On the other hand, it was caused by ethnographers' looking at sociologists who were fascinated with statistics and the development of the first programs for computers and with whom ethnographers sometimes collaborated, as members of the same institution.

During the same period, a peculiar process was taking place that eventually substituted the colonial asymmetry between residents of the imperial metropolis and indigenous groups at the periphery with the substantialized wholeness of the ethnos transformed into the "social body." Respectively, the field had become viewed as a kind of a hospital ward where the ageing and dying of certain functions and attributes and the emergence of new functions and attributes of ethnoses-patients were observed and recorded (the same metaphor appeared also in a recent article by Andrei V. Golovnev [2012]). Besides, it is significant that

the field as a detailed description and the field as a corporeal experience were slowly leaving the ethnographic scene, being replaced with a new wave of European anthropologists who included Siberian materials in anthropology of the North, as a strand of world social anthropology, relying primarily on the longitudinal field experience. This can be viewed as a new stage in the internationalization of the field (see Gray et al. 2004; Vakhtin 2006).

Acknowledgments

We thank David G. Anderson (University of Aberdeen, Scotland, UK), Natalia G. Komelina (IRL RAN, Pushkin House, Saint-Petersburg) for the comments to the text. We thank Olga Pak for the translation of the original Russian article into English. The translation and the preparation of the English version have been supported by the projects of the Economic and Social Research Council, the standard research grant ES/K006428/1 "Etnos and the Peoples of the North."

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Notes

1. This article is an expanded version of the paper published in Russian in 2013 (Arzyutov and Kan 2013). Some ideas on the conceptual views of Lev Shternberg have been previously published in the English-language papers by Sergei A. Kan (2001, 2008, 2009). The history of relations between Franz Boas and Vladimir Bogoraz after 1917 was investigated in detail by Kan in 2008.

2. In relation to the mentioned paper (Stocking 1990), let us note that, after the deaths of Shternberg (in 1927) and of Bogoraz (in 1936), the archetypal image of the ethnographer was personified by Nicholas Miklouho-Maclay (1846–1888). Initially, in 1938, an exhibition was organized at the MAE, dedicated to the 50th anniversary of Miklouho-Maclay's death (the practice of such "dedications" had been common in the Soviet tradition for a long time). Then, in 1941, two volumes of his *Travelling* were issued; in 1947, the major ethnographic institute in the USSR was named after him; and the first feature film about him appeared at the same time (directed by Alexander G. Razumnyi).

3. The term *inorodtsy* was mostly applied to the indigenous peoples of Siberia, Central Asia, and Russian Far East.

4. It is interesting that Boas never taught methods of fieldwork.

5. We do not consider the life, field researches, and theoretical aspects of the scholarship of Sergei M. Shirokogoroff (1887–1939) who, before the epoch of Bromley and Gumilev (the 1970s through the end of the 1980s) did not have a real influence on the destiny of field researches and theoretical discussions of that time. Shirokogoroff's field materials, along with his manuscripts, photos and letters, that have never been published, are now being prepared for publication by Dmitry Arzyutov and David Anderson.

6. Such a motivation is especially evident in several letters from Bogoraz to Shternberg.

7. Cf. what Bogoraz wrote about his initial learning of the Chukchi language: "Already in the very beginning of my work, I considered that any somehow serious ethnographic studies are unthinkable without the knowledge of the language. Later on, the conditions of the Chukchi-Russian relations revealed that the knowledge of the language is the only clue to the communication with the Chukchi and made me devote more than half of my time to studying their lexicology and grammar" (1900: 1).

8. Since 1894, Bogoraz published ethnographic stories and novels based on the Chukchi materials.

9. It is interesting that, having a kind and compassionate personality, Shternberg conducted his interviews in a firm and sometimes even tough manner. This is how he described his interview with some Orok Fiodor, a "great connoisseur of the Russian language," in his diary in 1893: "He had to sweat a lot, poor Fiodor, besides, somewhat stuttering, while I was carrying out long interrogations and pre-interrogations about the terminology of kin-

ship and the other customs of the Oroks, but he, as long as he could endure, kept answering my questions. I should admit that, like in all such cases, I was merciless. The tortures and suffering that accompanied Fiodor and his helping relatives' delivery of thoughts gnawed my consciousness very little" (Roon and Prokofiev 2004: 261).

10. Both Bogoraz and Shternberg tried, in all field situations, to establish friendly and sometimes close relationships with indigenous people, treated them with respect and, as much as they could, helped them with medications, food, and so on.

11. Subsequently, a share of objects collected by Shternberg was entrusted to a local museum of regional studies of which he was one of the initiators and founders.

12. Although eventually the expedition happened to be more modest than Shternberg had aspired, the program questionnaire, titled "The Human" and compiled under Shternberg's supervision by An-sky and his assistants, astonishes one with its rigorousness and scrupulosity (see Deutsch 2011).

13. See, for example, the letter from Shternberg to Kutomanov in Srednekolym'sk, dated 11 February 1912, in which, along with the detailed instructions as to what things of Chukchi and other local peoples should be gathered, there is a whole range of questions about social organization that the gatherer was supposed to ask (Shternberg 1912: 86–87).

14. It is noteworthy that Bogoraz supervised student ethnographers' studies of new ways of living with more eagerness than Shternberg. The latter edited only one collection of students' works that was, above all, focused on the theme of new and old wedding ceremonies (Shternberg 1926). Bogoraz's students were occupied with more politicized thematics, as, for example, *Komsomol v derevne* [Komsomol in a Village] (Bogoraz 1926b), *Evreiskoe mestechko v revoliutsii* [A Jewish locale in the revolution] (1926a), and so on.

15. Tim Ingold (2000: 132–150) interprets the concept of indigeneity as a dialogue of these two oppositions of those years in contemporary discussions in the milieu of indigenous intellectuals (see the example of Altai in Broz 2009).

16. See the article by Siragusa and Arukask in this issue.

17. David Anderson (2000b) presented his reflexivity on his own field experience in the article.

18. The Institute of the Peoples of the North (INS; founded in 1925, under the auspices of Leningrad State University) constitutes a special case of the field that is worthy of a thorough consideration. On the one hand, it carried out the Soviet project of the development of "small peoples of the North." On the other hand, it provided an example of the "domestication" of the field. Many specialists in Siberian studies from Leningrad worked for the INS, which itself could function as a peculiar field for them (see Liublinskaia 2006). For example, it is known that the already mentioned Turkologist Dyrenkova (1963) prepared her grammatical review of the Tofalar language based on the materials she had collected from her students at the INS.

List of Acronyms

- AMAE RAN Arkhiv Muzeia antropologii i etnografii imeni Petra Velikogo (Kunstkamera) Rossiiskoi Akademii Nauk [The Archive of Peter the Great Museum of Anthropology and Ethnography (Kunstkamera) of the Russian Academy of Sciences], Saint Petersburg
- ARAN Arkhiv Rossiiskoi Akademii Nauk [The Archive of the Russian Academy of Sciences], Moscow
- INS Institut narodov Severa [The Institute of the Peoples of the North], Leningrad/Saint Petersburg
- IRGO Imperatorskoe Russkoe geograficheskoe obshchestvo [The Imperial Russian Geographical Society]
- IRLI RAN Institut russkoi literatury Rossiiskoi Akademii Nauk (Pushkinskii Dom) [the Institute for Russian Literature of the Russian Academy of Sciences (Pushkin House)], Saint Petersburg
- LGU Leninskii gosudarstvennyi universitet [Leningrad State University]
- MGU Moskovskii gosudarstvennyi universitet [Moscow State University]
- SPF ARAN Sankt-Peterburgskii filial arkhiva Rossiiskoi Akademii Nauk [The Saint Petersburg Branch of the Archive of the Russian Academy of Sciences]

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