

Univerzita Karlova
Fakulta humanitních studií

Integrální studium člověka – Obecná antropologie



**The late socialist Czechoslovak ethnography and folklore studies
and its influence on the Czech tradition of sociocultural anthropology
after 1989**

**(Československá etnografie a folkloristika v období pozdního socialismu
a její vliv na formování tradice české sociokulturní antropologie
po roce 1989)**

Disertační práce

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2020

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V Praze, 2. září 2020

Nikola Balaš

Abstrakt

Předkládaná práce se snaží prozkoumat československou etnografii a folkloristiku v období pozdního socialismu a poskytnout tím základ pro pochopení postsocialistické transformace etnografie v antropologii a etnologii. Hlavní teoretický rámec, ze kterého práce vychází, představuje kritická sociologie vědy Pierra Bourdieu. Teze se převážně zaměřuje na dvě instituce, kde se etnografie pěstovala – na Katedru etnografie a folkloristiky Univerzity Karlovy v Praze a na pražskou pobočku Ústavu pro etnografii a folkloristiku Československé akademie věd v období pozdního socialismu, tedy období pokrývajícího 70. a 80. léta 20. století. Kromě intelektuálního rozměru etnografie se práce snaží rozkrýt rozmanité praktiky etnografů jako například výzkumné metody, jazykové znalosti, psací návyky, vědecké hierarchie nebo postoje ke vzájemné kritice. Hlavním argumentem této práce je, že zatímco nálepka etnografie v 90. letech 20. století vymizela, praktiky etnografů se i nadále podílely na utváření vznikající české antropologie a etnologie. Práce intenzivně čerpá z etnografických spisů, z rozhovorů s bývalými etnografy a ze sekundární literatury. V menší míře pak čerpá z dochovaných dokumentů.

Klíčová slova: dějiny etnografie a folkloristiky, dějiny české antropologie, dějiny antropologie, postsocialismus, kultura sváru, akademická historie, kritická sociologie, orální historie

Abstract

This thesis is an attempt to provide an account of the late socialist discipline of Czechoslovak ethnography and folklore studies and provide a basis for understanding of ethnography's post socialist transformation into anthropology and ethnology. The main theoretical framework of the thesis is the critical sociology of science of Pierre Bourdieu. The thesis focuses especially on two ethnography institutions – the Department of Ethnography and Folklore Studies at Charles University in Prague and the Prague branch of the Institute for Ethnography and Folklore Studies of the Czechoslovak Academy of Sciences in the late socialist period, which covers the 1970s and 1980s. Apart from providing some intellectual dimension of ethnography, the thesis aims to uncover other dimensions of ethnographers' practices such as research methods, language competences, writing habits, academic hierarchies or attitudes to mutual criticism. The thesis argues that whereas ethnography as a label disappeared in the 1990s, ethnographers' practices continued to shape the nascent Czech anthropology and ethnology. The thesis makes an intensive use of ethnographers' scholarly writings, interviews with former ethnographers and also uses some documentary evidence and secondary literature as its sources.

Keywords: history of ethnography and folklore studies, history of Czech anthropology, history of anthropology, postsocialism, culture of contention, scholarly history, critical sociology, oral history

Acknowledgements

One of the ideas behind this thesis is that science is a collective enterprise. For any discipline to thrive, it is important that its practitioners have platforms to share ideas and comment on each other's work. Where there is no sharing there can be no progress. The work on this thesis testifies to the fact that even ephemeral encounters had a considerable impact on the ultimate shape of this thesis. Some contributed with a few words, some with a recommendation of a book while others sacrificed their time and gifted me with an e-mail, comment, consultation or shared important materials with me. Yet there is another dimension to the collective nature of scholarly community and it is of no less importance. It is mutual criticism. Even if it seems that criticism produces discord rather than consonance, tension instead of comity, I believe that it serves as one of the driving forces of the development in science. Where there is no criticism there can be no progress. And rather often than not, this thesis also benefited from criticism. For these reasons, I would like to thank here to all those whom I met on the road and who contributed to my research of which this thesis is the latest and, I hope, not final, product. Apart from all of my informants who helped me to get a better grasp of Czechoslovak ethnography and folklore studies, ethnology and sociocultural anthropology and whom I do not mention here for reasons of anonymity, I would like here to thank to Michał Buchowski, Chris Hann, Robert Parkin, David Mills, Olga and Josef Kandertovi, Soňa Švecová, Karel Šima, Daniel Sosna, Radim Tobolka, Lucka Konarovská, Petr Vašát, Slávka Ferenčuhová, Kristýna Hejzlarová, Petr Skalník, Yasar Abu Ghosh, Dalibor Státník, Zdeněk Uherek, Zuzana Korecká, Michal Rybák, Viktor Stoilov, Milan Ducháček, Juraj Podoba, Antonín Kostlán and professor Vladimír Nahodil. I would also like to thank to my erstwhile colleagues from the Centre of Theoretical Study of Charles University where I spent two years as an administrative assistant. Although my chief preoccupation was paperwork I had a rare opportunity to witness a thriving community of scholars coming from different disciplines indulging in mutual and often critical discussions. I believe, that compared to some other research institutions of Czech higher education and research, the Centre is a small miracle. While I enjoyed the company of all, my gratitude goes especially to Jitka Pelikánová, Antonín Macháč, Alexander Matoušek, Eliška Fulínová, Zdeněk Konopásek, Pavel Kouba, Radan Haluzík, Arnošt L. Šizling, Petr Meduna and Darja Zoubková with whom I had many an opportunity to discuss various issues related to my thesis. Given their various backgrounds, our discussions gave me a fairly good comparative perspective on matters on which I was

working. Special thanks go to Ludmila Kopalová of the Library of the Institute of Ethnology of the Czech Academy of Sciences, Michaela Tydlitátová and Hana Popelová of the Library of Náprstek Museum, and Vladimír Hulík of the Ministry of Education, all of whom I bothered with my unremitting demands and who did their best to help me. For the help with translations of Czech titles to English, I am indebted to Robin Cassling who helped me with my earlier article for *Sociologický časopis / Czech Sociological Review*. I am also grateful to Eva Hrabáková for her help with several issues related to English. Very special thanks go to Karolína Pauknerová, Anna Kvíčalová, Jan Kapusta, Johana Musálková, Jiří Woitsch and Václav Hubinger who kindly invested their time to a critical scrutiny of this thesis and who offered me useful advice and commentary which had nothing but a beneficial impact on the thesis. The original idea to write on the history of Czech anthropology was not mine. It was an idea of Hedvika Novotná who suggested it when we were sitting in a pub. I hope that she will not regret her suggestion once she reads this thesis. My gratitude also goes to Bára Straková who substituted for me during the first-year enrolment which I could not attend personally. The most important thanks go to my supervisor, Dana Bittnerová, for her exceptional tolerance, support, critique and inspiration. Lastly, I would like to thank to my parents who have supported me during my university years as well as my decision to pursue a PhD.

* * *

I was fortunate enough to have the possibility to present the developing thesis during several occasions. The first was a conference on the history of Czechoslovak ethnography which took place in March 2016 in Prague and which was organized by several institutes of the Czech Academy of Sciences. The second occasion was the Young Scholars Working Group conference, which was a prelude to 2017 SIEF conference in Göttingen. Several ideas which I presented during these two occasions are better developed in Chapter 7. What currently constitutes Chapter 2 was originally a presentation delivered the Thursday seminar organized by the Centre of Theoretical Study in Prague in February 2019. Lastly, some ideas which the reader can find in Chapter 5 were included in a short presentation for the workshop concerned with Czech humanities during the Normalization era. The workshop was organized by several institutes of the Charles University and the Czech Academy of Sciences. The workshop took place in October 2019. Some ideas which can be found in this thesis previously appeared in writing. *Academia without Contention? The*

Legacy of Czechoslovak Ethnography and Folklore Studies in Czech Anthropology appeared in the 2018 volume of *Sociologický časopis/Czech Sociological Review* (Balaš 2018) and my review of the collective monograph titled *Etnologie v zúženém prostoru* appeared in *Studia Ethnologica Pragensia* in 2017 (Balaš 2017).

Table of Contents

1. INTRODUCTION	II
1.1 What Was Ethnography and Folklore Studies?	13
1.1.1 Exceptions	16
1.1.2 Translations	16
1.1.3 Retrospective Uses	17
1.1.4 Idiosyncrasies	18
1.2 The Limits of Intellectual Approach	19
1.3 Theories and Methods	25
1.3.1 The Concept of Field	26
1.3.2 The Field of Ethnography and Folklore Studies and the Forms of Capital	27
1.3.3 The Dynamics of the Field	31
1.3.4 Ethnographers' Habitus	35
1.3.5 Material	37
1.3.6 Oral History Method	39
1.3.7 Comparative Method	42
1.3.8 Notes on Translations	44
1.4 Chapters	45
2. ETHNOGRAPHY AND FOLKLORE STUDIES AS AN INTELLECTUAL TRADITION	47
2.1. The Czech Case: The Three Accusations	47
2.2 The First Accusation: Marxism-Leninism	49
2.2.1 Subject Matter	51
2.2.2 Historicism	51
2.2.3 Laws of History	52
2.2.4 Base and Superstructure	53
2.2.5 Against Idealism	53
2.2.6 Historical Teleology	56
2.2.7 Partisanship and Value Partiality	57
2.2.8 Troubles with Survivals	59
2.2.9 Marxism-Leninism in the 1970s and 1980s	63
2.3 The Second Accusation: Nationalism	67
2.3.1 Ethnography of the Czech National Revival	68
2.3.2 Ethnography of Ethnic Processes	77
2.3.3 Idealism Comes from the Soviet Union	79
2.3.4 Ethnic Processes in Action	82
2.3.5 Nations, Cultures and Traditions as Survivals	85
2.4 The Third Accusation: Positivism	86
2.4.1 Descriptivism	87
2.4.2 Atheoreticity	89
2.4.3 Was It Really the Case?	90
2.5 Partial Conclusions and Other Considerations	91
2.5.1 A Point of Departure	93

3. THE FIELD OF ETHNOGRAPHY AND FOLKLORE STUDIES IN THE LATE SOCIALIST PERIOD	96
3.1 A Problem	96
3.2 The Sovietization of Národopis in the 1950s	97
3.3 The Liberalization of the 1960s	102
3.3.1 A Note on Czechoslovak “Anthropology”	104
3.4 Ethnography in the Late Socialist Period	109
3.4.1 Barriers to Entry: Pursuing Careers in Ethnography	111
3.4.1.1 Personal (Dis)continuities in Ethnography	111
3.4.1.2 Newcomers to the Discipline	114
3.4.1.3 The Communist Party and Ethnographers’ Careers	116
3.4.2 Struggles for Domination	119
3.4.2.1 The Command of Institutions	119
3.4.2.2 State Planning	122
3.4.2.3 Robek as the “Chief”	125
3.4.2.4 Academic and Symbolic Mastery	127
3.4.2.5 Robek and the Limits of Intellectual Freedom	129
3.5 A Bourdieusian Lesson	135
4. BEING AN ETHNOGRAPHER: SOME ASPECTS OF ETHNOGRAPHY AS A SCHOLARLY PRACTICE	139
4.1 Another Meaning of “Ethnographic”	140
4.2 The Nature of Research	144
4.3 Hierarchies within Ethnography	147
4.3.1 The Division of Labour in Ethnography	148
4.3.2 The Cult of Ancestors and the Absence of the Reappraisal Discourse	151
4.3.3 The Discourse of Maturation	154
4.4 Erudition and Scholarly Fame	155
4.5 Language Proficiency	157
4.6 Ethnography as a Collective Enterprise	162
5. ATTITUDES TO WRITING	166
5.1 A Quantitative Analysis	166
5.1.1 Authors	167
5.1.2 The Publishing Landscape	169
5.1.2.1 Journals and Edited Volumes	170
5.1.2.2 Books	172
5.2 Publication Activities Measured: Numbers of Texts	173
5.2.1 Standard Articles	173
5.2.2 Material Articles	176
5.2.3 Conference Papers	178
5.2.4 Books	180
5.2.5 Repetitiveness	182

5.4 Explaining Attitudes to Writing	184
5.4.1 The Role of Scarcity	186
5.4.3 The Discourse of Maturation Reconsidered	193
5.5 The Question of Quality	194
6. THE CULTURE OF CONTENTION AND ITS ABSENCE IN ETHNOGRAPHY	196
6.1 Anthropology and the Culture of Contention	196
6.2 Criticism in Ethnography	202
6.2.1 Criticism in Articles	202
6.2.2 Discussion Sections	204
6.2.3 Criticism in Reviews	207
6.2.4 Face-to-Face Criticism	207
6.3 Consequences of the Absence	209
6.3.1 The Unavailability of Theories?	212
6.3.1 Theories in Their Inchoate State and Underdeveloped Description	216
6.4 Explaining the Absence of the Culture of Contention	219
6.4.1 An Explanation by the Diminished Degree of Autonomy	220
6.4.2 An Explanation by Academic Autonomy	221
7. CONCLUSION: CONTINUITIES AND DISCONTINUITIES	226
7.1 Intellectual Continuities and Discontinuities	228
7.2 Habitual Continuities and Discontinuities	230
7.3 A Clash of Two Worlds	236
7.3.1 Savages from the West	237
7.3.2 The First Czech Anthropologists	243
7.4 New Directions	245
APPENDIX I: GLOSSARY	247
APPENDIX II: TRANSLATIONS	249
SOURCES	251
Archival	251
On-line	251
Bibliography	252

I. Introduction

The current Czech sociocultural anthropology began to take roots in the groves of Czech academe¹ shortly after the Velvet Revolution in 1989. Anthropology was viewed as one of the disciplines promising a fresh new start for Czech social sciences and humanities afflicted by the forty-year rule of Socialism in Czechoslovakia dominated by the all-pervading ideology of Marxism-Leninism and by the unchallenged rule of the Communist Party. Now, it has been three decades since anthropology appeared on the scene and it is about time to look back and investigate what the Czech tradition of sociocultural anthropology has grown into. This thesis began as such an investigation. It originally aimed to provide an account of the establishment and the early history of Czech anthropology. However, as I was getting on with my research, it became increasingly harder to make sense of what had been going on without delving even deeper into the past. At one point it became apparent that it would be impossible to understand the history of Czech anthropology without going before 1989. Hence, my research ended up as a history of the discipline of ethnography and folklore studies in the late socialism. Its main goal is to map and describe the discipline of Czechoslovak ethnography and folklore studies in the late socialist period.

The adjective “late socialist”² covers two decades: the 1970s and 1980s and the thesis is mainly concerned with the situation at two institutions in Prague – the Department of Ethnography and Folklore Studies of the Faculty of Arts at Charles University (*Katedra etnografie a folkloristiky*; hereinafter referred to as the Ethnography Department) and the Institute for Ethnography and Folklore Studies of the Czechoslovak Academy of Sciences (*Ústav pro etnografii a folkloristiku*; hereinafter referred to as the Ethnography Institute). For the sake of argument, the thesis makes necessary forays beyond its main spatiotemporal locus. It moves between the late 1940s and 1990s and also pays attention to other institutions some of which were also located in Prague and some of which were not. For the thesis is mainly interested in the situation in Prague, it is appropriate to speak of *Czech* ethnography and folklore studies. I will speak of *Czechoslovak* ethnography and folklore

¹ This thesis uses the words “academe”, “academia” or “academic” to refer to both university institutes and academic institutes and does not follow the Czech distinction between the adjectives “akademický” (related to the Academy of Sciences) and “univerzitní” (related to universities).

² The “late-socialist” is borrowed from Alexei Yurchak, but in the context of this thesis the designation refers to a quite different temporal frame than Yurchak does in his book on the Soviet Union (Yurchak 2005: 4).

studies every time that I will speak of the general situation in the discipline in Czechoslovakia.

The other main goal is to point out to continuities between the pre-1989 Czech ethnography and folklore studies and the post-1989 Czech sociocultural anthropology. This connection might come as a surprise to many. The contemporary discourse, as we shall see, holds that ethnography and anthropology are two distinct and incommensurable disciplines. There used to exist some anthropological, so to say, strands within Czech ethnography during the times of socialism, but all the attempts to gain independence on ethnography at the time were sooner or later nipped in the bud mostly because anthropology was perceived as an ideologically suspect discipline, one of the instruments of the evil Western capitalism in its imperial and colonial pursuits. So why to speak of the pre-1989 history of Czech anthropology? Nevertheless, it can be as well pointed out that the Czech sociocultural anthropology which emerged in the 1990s was not a greenfield project. It involved many scholars with previous experience as professional ethnographers and folklorists and, moreover, anthropology began to grow within institutions formerly connected to ethnography and folklore studies.

In the pursuit of the two goals, this thesis makes use of the conceptual apparatus derived from the sociology of science of Pierre Bourdieu, which I will present later in this introduction. Before that, two issues need to be made clear. Firstly, it is necessary to briefly acquaint readers with the main subject matter of this thesis, that is, the discipline of Czechoslovak ethnography and folklore studies. Secondly, it is necessary to review the discourse on anthropology in the postsocialist Europe and previously written accounts related to the writings exploring the history and mutual relations of the Czech and Czechoslovak traditions of ethnography and folklore studies, ethnology and sociocultural anthropology. The second point is especially important because the thesis is designed as a polemical piece which aims to contribute to the discourse.

Before I hurl the reader straight in the midst of the problem I would like to say that I am not motivated purely by the joys of idle curiosity. I am sympathetic to recent studies which aim for a thorough analysis of the world of Czech higher education and call for a more reformist approach (Dvořáčková et al. 2014; Pabian 2014; Šima & Pabian 2013; Vlk et al. 2017). As the crucial concern of this thesis is the state of current Czech sociocultural anthropology, I try to identify some of the weak points which account to its specific development which might hinder the recent development of anthropology. I hope that

bringing these issues to the light of day might help us to improve our most cherished discipline.

1.1 What Was Ethnography and Folklore Studies?

Ethnography and folklore studies was the name of a discipline which was introduced in Czechoslovakia by the end of the 1940s by a group of young Marxist-Leninist scholars who looked up with devout admirations to Soviet ethnography and attempted to create an independent tradition of ethnography in Czechoslovakia. The introduction of the Soviet-inspired ethnography had some intellectual, or epistemological, as well as moral and political dimensions. Some authors even speak of a paradigm shift instigated by the introduction of ethnography in Czechoslovakia (Scheffel & Kandert 1994: 16). For various dimensions of ethnography will be minutely elucidated and expounded in later chapters, I will limit myself to a few necessary remarks here.

The origins and institutionalization of Czechoslovak ethnography and folklore studies (*etnografie a folkloristika*) can be traced to the late 1940s and early 1950s. The discipline of ethnography and folklore studies was supposed to replace an older discipline known as *národopis*, the name of which literally means to write about the nation or write about nations.³ Likely for this reason, some authors prefer to use the Anglicism *nationography* or *nationography* when speaking of *národopis* (Grill 2015: 17; Skalník in Hann et al. 2007: 36). The roots of *národopis* in Czechoslovakia can be traced back to the nineteenth century when *národopis* emerged as a result of the intellectual effervescence mostly among the Czech middle classes which partook on the so-called Czech National Revival. As an academic discipline, *národopis* focused on the study of populations, their traditions, material and spiritual cultures, diets, architecture, clothing, arts or customs and such an enumeration is by no means comprehensive.

According to Katherine Verdery, contrary to a more universalistic scope of anthropology, *národopis*, as a discipline from the family of similar disciplines, which were practiced in various countries of Central and Eastern Europe (CEE), was rather local in its interests. Verdery claims that these different scopes, the local scope of *národopis* and the

³ It is important to stress that the name of the discipline allows to be read in two ways: either as an inquiry concerning the nation, which means the Czech nation, or in a more general sense as a study of the nations. Even though that Czech *národopis* was mainly preoccupied with the Czech nation, the name implies national plurality and some scholars of *národopis* also researched on other nations (cf. Niederle 1953).

universal scope of anthropology, resulted from two different socio-political milieux. While the origins of anthropology were intertwined with empire-building, *národopis* played an important role in nation-building (Verdery in Hann et al. 2007: 49).⁴ Not surprisingly, *národopis* has been repeatedly accused of nationalism (cf. Grill 2015; Holubová et al. 2002: 231–270; Scheffel & Kandert 1994). Although many of the scholars of *národopis* would have probably softened this claim by invoking patriotic virtues or the value of Czech national and cultural heritage instead of nationalism, they would not have probably denied that the prime object of their interest was the Czech nation, its culture and society.

No matter how fiercely the young Czechoslovak Marxist-Leninists promoted ethnography at the turn of the 1940s and no matter how much they desired to part company with the tradition of *národopis* (cf. Macková 2016: 343), which they denounced as a bourgeois discipline, the term *národopis* never went out of use throughout the forty years of socialism in Czechoslovakia. The term survived the Marxist-Leninist turmoil and by 1964 we find two scholars Václav Frolec and Dušan Holý arguing that *národopis* is an overarching term for two of its more specific branches – *ethnography* on the one hand and *folklore studies* on the other (Frolec & Holý 1964: 7). Moreover, no later than by the 1980s, *národopis* also officially designated a study programme which students studied at universities and its use was also officially consecrated by the registers of the Ministry of Education.⁵ Though students attended lectures and seminars in ethnography and folklore studies at the Ethnography Department in Prague, and even though they considered themselves to be ethnographers or folklorists, they officially graduated from a study programme in *národopis*.

After the Velvet Revolution of 1989, the term *ethnography* fell into disuse as it was burdened with an unwelcome Marxist-Leninist heritage. What remained of ethnography and folklore studies split its allegiance between sociocultural anthropology and ethnology of which the latter very quickly predominated. For example, the departments in Prague and

⁴ In her contribution, Verdery credits works by Tamás Hofer, Eugene Hammel and Joel Halpern from the late 1960s for noticing anthropology's closeness to the building of great European empires. She opposes such empire-building projects to nation-building projects which can be linked to Central and East European ethnography traditions (Verdery in Hann et al. 2007: 49). Verdery reacted to the same distinction previously made by Chris Hann who credited the distinction to George W. Stocking (Hann in Hann et al. 2007: 9). The relation between colonial empires and anthropology and between nation-building and ethnography was also mentioned by Jaroslav Kramařík (Kramařík 1972: 197). As Kramařík did not refer to anyone in particular, it is a question whether he had come up with the idea independently or whether he had borrowed it from someone else.

⁵ See *Nariženi vlády ČSSR 89/1980 Sb.* and *Nariženi vlády ČSSR 33/1986 Sb.* *Národopis* is categorized under codes 71-05-8 and 71-10-8. I was unable to find earlier official documents that would prove the usage of the term.

Brno, which had once used to be the hubs of Czechoslovak ethnography, have ethnology in their name today: The Department of Ethnology at the Faculty of Arts of Charles University in Prague (*Ústav etnologie*) or the Department of European Ethnology at the Faculty of Arts of Masaryk University in Brno (*Ústav evropské etnologie*). The same is the case of the Institute for Ethnology of the Czech Academy of Sciences which has its two branches in the two previously mentioned cities. The main disciplinary journal *Český lid* (lit. transl. as the Czech People) acquired the English epithet *Ethnological Journal*. The only surviving term is folklore studies (*folkloristika*). Folklore studies covers the research on folk literature in its manifold manifestation such as poetry, fairy, tales, mythology or proverbs, folk music, songs and dances; and it originally developed in close connection to philology and literary theory (Janeček 2017; Slavkovský & Botík 1995). Although its prominent representatives also contributed to *národopis*, it presented rather an independent current until 1954 when, as we shall see, it was merged with ethnography. The discipline of folklore studies does not have any independent departments or study programmes today and it is taught and practiced within the confines of ethnology.

The term *národopis* fell into disuse in the 1990s and the word today carries a rather obsolete, antiquarian or perhaps nostalgic odour. Nonetheless, some journals (*Národopisný věstník*, *Národopisná revue*) or the national professional association of contemporary ethnologists (*Národopisná společnost*) continue to use it. No more does it designate study programmes and no more is it viewed as a kind of overarching term.

This thesis follows an institutional use of these terms. All what came between 1945 and 1948 is referred to as *národopis*. The short period between 1948 and 1954 speaks of both *národopis* and *ethnography* depending on scholars whom it refers to. The discipline in the period between 1954 and 1989 refers to ethnography (shorthand for ethnography and folklore studies). The period after 1989 refers to ethnology and sociocultural anthropology. This use should simplify some orientation according to an uncomplicated temporal benchmark.⁶ However, the readers should be aware of the fact that the usage of these labels

⁶ For example, the former Department of Ethnography and Folklore Studies at Charles University was renamed to Department of Ethnology at the beginning of the 1990s, the Institute for Ethnography and Folklore Studies of the Czechoslovak Academy of Sciences renamed to the Institute for Ethnology only by the end of that decade. Some simplification is also involved when I refer to concrete institutions and not only to disciplinary labels. For example, in the 1990s, the department in Prague was known under three consecutive names. The original Department of Ethnography and Folklore Studies (*Katedra etnografie a folkloristiky*) was renamed to the Department of Ethnology (*Katedra etnologie*) and shortly after to the Institute of Ethnology (*Ústav etnologie*). I prefer to speak about Ethnography Department (or alternatively the Ethnology Department) so as not to sow confusion by using the terms *Institute of Ethnology* at Charles University and the *Institute for Ethnology* at the Academy of Sciences.

has not been unanimous and there are at least four problems that could further complicate any orientation and problematize our terminology: exceptions, translations, retrospective uses and idiosyncrasies.

1.1.1 Exceptions

The origins of the terms ethnology, ethnography and *národopis* go back at least to the nineteenth century and it seems, from an unqualified point of view, that these terms coexisted side by side quite freely and, at least up to the early 1950s, could obtain various meanings (cf. Petrářová 2016: 76; Sklenář 2013: 43). What these terms used to stand for before the 1940s is not explored in this thesis. This is just to warn the reader that the situation concerning these terms probably reflected different issues and dilemmas which are not involved in the distinctions which contemporary scholars as well as this thesis perceive between these terms.

If we hold to the temporal frame of this thesis, there existed an exception which was the name of a seminar that existed at Masaryk University in Brno (from 1960 to 1990 known as the University of Jan Evangelista Purkyně). The seminar ran between 1945 and 1964 and its full name was the Seminar for Ethnography and Ethnology (Válka et al. 2016: 24). Moreover, during the 1960s, Czechoslovak ethnographers established intensive contacts with European ethnologists and actively participated on joint projects in ethnocartography whose intellectual father had been the Swedish ethnologist Sigurd Erixon (Woitsch 2016a: 190, 199–207).⁷ Hence it is possible that ethnographers in the 1960s might have downplayed differences between ethnography and ethnology.

1.1.2 Translations

Several troubles are related to the acceptable translation of the Czech words *národopis* and *etnografie*⁸ into English. The most obvious choice, which is also the choice of the author of this thesis, is to translate *etnografie* as ethnography for the apparent similarity of both words. However, some authors prefer to translate *národopis* as ethnography too (Scheffel

⁷ These examples contradict Peter Skalník's categorical statement that the term ethnology could not appear during the socialist period and the only acceptable terms during the communist era were *národopis* and ethnography (Skalník 2018: 6). The fact that *Český lid* published Skalník's short report titled *Ethnology and Social Anthropology in Japan* in 1970 (Skalník 1970) indicates that the situation was a bit more complicated.

⁸ Up to the 1960s, the proper word was *etnografie*, but the h was dropped later.

& Kandert 1994). The trouble is that the term *etnografie* has peculiar historical connotations as it denotes a discipline which was intimately related to the forty years of Communist rule in Czechoslovakia and Marxist-Leninist ideology. Hence translating both *národopis* (referring to the discipline which existed in Czechoslovakia before 1948) and *etnografie* as ethnography tends to equate *národopis* with *etnografie*. One might argue that both terms – *národopis* and *etnografie* – were in use in the times of Socialism and no issues ensue from equating them by the translation. However, this renders invisible some crucial differences between the pre-1948 *národopis* which had next to nothing in common with Marxism-Leninism and which the Marxist-Leninist ethnographers desired to repudiate. Very likely for these reasons, some authors insist on using the original term *národopis* to avoid the unwanted ideological contamination that is inadvertently carried by the term ethnography (Kandert 2005; Lozoviuk 2005; Skalník 2005a; Skalníková 2005).

On the other hand, translating the term *národopis* in English as *ethnography* is forthcoming to English readers, but there are limits as well. The term ethnography might sow confusion as it obtains two different meanings in anthropological discourse. First, anthropologists have referred to field monographs as ethnographies (sg. ethnography) and to facts presented therein as ethnographic descriptions (Ingold 2008), but such a use of the term was absent in Czechoslovak ethnography.⁹ More recently, ethnography and ethnographical as specific designations have acquired more specific meanings in relation to some developments in anthropology (Holbraad & Pedersen 2017: 110–156; Ingold 2014). In neither of these two senses does the term ethnography match with what is meant by ethnography in this thesis. *Ethnography* to which this thesis refers is an academic discipline and is on a par with other terms that also designate academic disciplines as such.

1.1.3 Retrospective Uses

Another issue concerns retrospective uses of the labels. When Antonín Robek wrote his book titled *The Outline of the History of Czech and Slovak Ethnography* (Robek 1964), he used the term ethnography in a sweeping manner to cover works of scholars from the late eighteenth century up to his times without worrying much about differences between various disciplinary designations and labels. Hence, we find names of past scholars of

⁹ The terms which would correspond to ethnography (in the sense of field monograph) in ethnography and folklore studies were “monografické zpracování” (monographic treatment), “monografické bádání” (monographic research) or “monografická studie” (monographic treatise) (cf. Chotek 1966: 287; Kadeřábková et al. 1981: 47; Kramařík 1951a: 131; Robek 1974b: 155; Skalníková & Fojtík 1969: 262).

variegated intellectual preoccupations and inclinations collectively referred to as ethnographers in Robek's book. The same strategy can be found among current scholars who tend to refer to past scholars as ethnologists and to their scholarly projects as past incarnations of ethnology. The recent collective monograph titled *Ethnology in a Confined Space* is about the discipline known under the names of *národopis* and ethnography and folklore studies shortly before and during the period of socialism in Czechoslovakia (Woitsch et al. 2016) and it refers to the same authors to which Robek referred some half a century earlier and whom he tagged as ethnographers. Similarly, the title of an earlier publication – *Czech Ethnology 2000* – tends to promote the retrospective use of the label ethnology (Holubová et al. 2002).¹⁰ In either way, the use of these overarching labels in effect tends to stress the continuities and similarities at the expense of discontinuities and differences.

1.1.4 Idiosyncrasies

Even more problems stem from the fact that various commentators tend to offer their own understanding or coin less frequent terms. These idiosyncrasies usually appear in two situations: either when academics engage in a free play of definitions related to what their discipline is all about or when they use it as a weapon of inclusion or exclusion in academic struggles. These definitions are usually accompanied by highlighting differences and/or similarities between disciplines which are being compared.

A fitting example of the idiosyncratic terminological uses are original classifications of Josef Jančář (Hlaváček & Bortlová-Vondráková 2018: 84–85), Daniel Dědovský (Dědovský 2018) or Josef Kandert (Kandert 2005). The last of the trio, when referring to Czechoslovak ethnography and folklore studies of the socialist period, even uses the terms *národopisci* (practitioners of *národopis*) and anthropologists interchangeably.

If we look for examples from across the borders, we find uses by Michał Buchowski who in one of his articles roofs the various intellectual traditions that can be found in the region of CEE under a common term *ethnoanthropology* and then speaks quite freely of ethnology, ethnography and anthropology (Buchowski 2012a: 68). In a manner similar to

¹⁰ It is fair to add that some of the contributions to these two publications strive to reflect terminological differences and implicitly contest the overarching label which the titles veil.

Buchowski, Jasna Čapo aims to abridge all the supposed differences and contribute to the creation of a “transnational European ethnology/anthropology of Europe” (Čapo 2014).

When speaking of idiosyncrasies, it is not always easy to tell whether some definition is just a mere result of scholastic ruminations that emerge in the space of intellectual free play and can in best serve as a way of acquiring fame *qua* scholar by coining brand new labels or by offering fresh views on differences that obtain between the terms, or whether these definitions serve as a means for some hard ends of academic politics that aim to redefine the legitimate discourse and its boundaries as to include or exclude some groups, institutions or individuals. Besides, it is not to say that translations and retrospective uses are free of academic politics. A specific view on the past as well as a sophisticated translation of a term might serve power struggles in the same way as a particular definition of a discipline.

1.2 The Limits of Intellectual Approach

So far, I have tried to provide some preliminary understanding of what ethnography and folklore studies was and I tried to caution readers against some possible misunderstandings. It has been noted that scholars are liable to offer their own understanding of what ethnography (as well as *národopis*, ethnology, anthropology, folkloristics etc.) is and in what respect it differs from kindred disciplines or even kindred national traditions of these disciplines. Since the 1990s, Czech scholars began to increasingly partake on discussions with their colleagues from abroad and the definitions of various disciplines became stakes not just in the local Czech discourse, but also in a larger European, if not global, scholarly scene. Without downplaying the dimension of academic politics which these debates have on national as well as on supranational level, I would like to point out to a common denominator of these debates. As I will try to demonstrate, the common denominator tends to reduce a very complex issue to a matter of a few simplistic and sometimes gravely misleading points. As a result, these debates produce tools, insights and knowledge which are largely inappropriate for understanding of what these debates purportedly aim to understand, that is, academic disciplines.

When Czech ethnologists, historians and anthropologists indulge in their favourite pastime they try to provide some ultimate definition of their science and demarcate it from kindred disciplines, usually from ethnology, sociology or history (Balaš 2016; Dědovský 2018; Hann et al. 2007; Nešpor & Jakoubek 2004; Wolf 1971: 13–33, 1999: 88–96). If we

take anthropology as an example, these authors usually ask questions such as: Is anthropology a specimen of social sciences or humanities? Should it also concern itself with the so-called biological facts of human nature in explanation and understanding of social phenomena? Is it a study of native societies or can anthropologists also study industrial and post-industrial societies? Is it a qualitative or quantitative enterprise? Is it explanatory or interpretive? Should its methods be synchronic or diachronic? Should it be concerned only with the native point of view or are we allowed to work with concepts alien to the natives? Is the long-term participant observation crucial for being admitted to the tribe of anthropologists? Are anthropologists allowed, apart from knowledge garnered by means of participant observation, to use written documents, statistic surveys or archaeological evidence as its sources too? And how then does it differ from history or archaeology? What does make it different from sociology? Or from European traditions of ethnology, ethnography and folklore studies, which are also known in local vernaculars as Hungarian *néprajz*, Czech *národopis*, German *Volkskunde*, Croatian *etnografija*, Estonian *etnograafia*...? And how do these differ from each other? These debates can be scholastically multiplied *ad infinitum* and every new attempt at a clear-cut demarcation or classification adds a pinch of chaos to the already tangled debates.

We can see that these issues are in no way new and limited to contemporary Czech anthropology and ethnology. These concerns preoccupied Czechoslovak scholars as early as in the inter-war period (cf. Válka et al. 2016: 19–23) and they also preoccupied the minds of Czechoslovak ethnographers (Holý & Stuchlík 1964; Skalníková & Fojtík 1969: 256–257; Tůmová 1964). The situation in anthropology does not seem to have been any different (Evans-Pritchard 1951: 48; Lévi-Strauss 2006: 290–320; Radcliffe-Brown 1952: 1–3) and my guess is that similar debates have existed also in the diverse community of European ethnologists. These debates multiplied as a consequence of the fall of the Eastern bloc when the situation posed new challenges for scholars from previously insulated scholarly turfs in their Socialist nation states.

When it comes to definitions themselves, scholars indulging in these debates almost unanimously adopt an idealist stance: What matters most are the ultimate intellectual peculiarities or differences which set various scholarly disciplines apart. This means that any discipline can be defined (and thence understood) on the basis of concepts and theories which it employs and on the basis of knowledge it creates. Different disciplines approach reality and its phenomena from distinct intellectual standpoints. Although these accounts rarely mention Thomas Kuhn and his fabled *The Structure of Scientific Revolutions* (Kuhn

2008 [1962]), they usually share Kuhnian assumptions because they tend to equal disciplines with paradigms and history of disciplines with the history of ideas or theories. This does not mean that the accounts are unanimous on other points. Some stress radical differences between the disciplines, others point to pervasive similarities. However, the general framing of the problem is that various disciplines represented by their respective labels present *intellectually distinct wholes*.

A perfect example that demonstrates the widespread intellectualist attitudes is represented by the contributions to the “multiple temporalities debate” sparked by Chris Hann’s article which appeared in *Czech Sociological Review* in 2007 (Hann 2007).¹¹ In the article, Hann commented among other things on mutual relations of the western tradition of sociocultural anthropology and on the local traditions of ethnography in the countries of the formerly socialist Europe. Although Hann observes that the two broadly conceived traditions have not been so different as they might seem, he nonetheless insists on the existence of some differences. Hann charitably waives any imperial project of implanting western anthropology in the region and whatever intentions he had when he was writing his article, his article speaks against trends both foreign and local which aim to replace local ethnography traditions with the western anthropology project. In such an arrangement of having two “distinct intellectual communities” (Hann in Hann et al. 2007: 10) Hann sees only benefits, because the two can maintain a fruitful and a mutually enriching dialogue.

Although subsequent reactions touched many different issues and ramified in unpredictable ways, contributors to the debate almost unanimously used intellectualist jargon to frame the whole issue. In her rejoinder to Hann, Milena Benovska speaks of “the paradigm of anthropology” and its alleged “intellectual imperialism” (Benovska in Hann et al. 2007: 14). Alexander Bošković writes that although the disciplines are closely related, it would be impossible to become an anthropologist overnight without being aware “of any theoretical developments since early 1960s” (Bošković in Hann et al. 2007: 16); and David Scheffel welcomes Hann’s intentions to respect “local intellectual traditions” (Scheffel in Hann et al. 2007: 33). Zdeněk Uherek, when commenting on differences between Western anthropology and CEE ethnographies, invokes Thomas Kuhn and his notion of paradigms (Uherek in Hann et al. 2007: 42). Juraj Podoba, again, speaks of “different intellectual traditions” and of the necessity of modernising ethnography “theoretically and

¹¹ Hann’s original article alongside subsequent reactions appeared in *Sociologický časopis /The Czech Sociological Review*. Here, I quote from its English version which appeared in *Max Planck Institute for Social Anthropology Working Papers* (Hann et al. 2007).

methodologically” (Podoba in Hann et al. 2007: 30). Buchowski writes in a similar vein to Bošković that it have not been difficult (or at least in Poland) to reconcile anthropologists and ethnologists, but maintains that whilst these two groups read the same books and address similar issues, they remain attached to “their distinct intellectual traditions” (Buchowski in Hann et al. 2007: 21).

Some of the authors mentioned specific features which account for the supposed intellectual differences. For Zdeněk Uherek, the first generation of Czech anthropologists to emerge after 1989 were originally ethnographers who switched to anthropology and their work was influenced by their positivist and historical orientation. As Uherek notes with a tongue in cheek, it seems that for their successful anthropological metamorphosis they just had to adopt an interpretive, ahistorical, culturally relativistic and anti-evolutionistic approach (Uherek in Hann et al. 2007: 47). Whereas Uherek sees interpretation and cultural relativism as central to anthropology, his account is at odds with that of Don Kalb who, describing folkloristics as too idealist, ascribes to anthropology a more materialist grounding (Kalb in Hann et al. 2007: 24). I will just add that Kalb by folkloristics probably had on mind the same discipline to which other participants of the debate referred to as ethnography.

The intellectualist cast of the debate by far transcends the multiple temporalities debate. Several years before the debate took place, Buchowski had pointed out to the existence of hierarchies which he had wittily described as “a pecking order of wisdom” (Buchowski 2004: 5). The “pecking order” comments on an unequal standing of knowledges produced by western anthropologists and their colleagues from the East. In Buchowski’s picture, CEE scholars devour and refer to the western anthropology production, but their western colleagues maintain rather a condescending attitude towards knowledge produced by them. For Buchowski, the omission of local scholarship is even more striking in cases when western anthropologists wrote on topics related to CEE countries and avoided any references to local scholarship dealing with the same issues (Buchowski 2012b: 24). Although Buchowski’s account does not chart any concrete differences between anthropology and ethnography as intellectual traditions in his article, Buchowski frames the problem in intellectual terms. He appeals to his colleagues to discard their prejudice and to judge these intellectual products not on the basis of their origins but on the basis of their intellectual content.

The intellectualist framework of Buchowski’s contribution was highlighted eight years later by Hana Červinková. Not only Červinková explicitly used the term “intellectual

traditions” (Červinková 2012: 161). Commenting on Buchowski’s paper (Buchowski 2004), she wrote:

*Buchowski also stresses that while his goal was to point out the existence of the **conceptual divide** between western and central-east European anthropological production and look for possibilities of convergence between these **theoretical and methodological traditions**... (Červinková 2012: 158; emphasis mine)*

While Buchowski or Bošković remain open to the possibility of some sort of synthesis and some like Jasna Čapo go even as far as to call for the project of a transnational ethnology/anthropology (Čapo 2014: 55), others like Chris Hann would prefer to keep the borders intact although they would probably welcome more intensive exchange across them. Other voices are less amiable and would appreciate the local ethnography traditions to vanish and be replaced by anthropology. One of those is Petr Skalník who dismisses Hann’s charitable position towards local traditions of ethnography and adds that anthropologists “did not miss anything substantial by knowing nearly nothing about ‘socialist era anthropology’” (Skalník in Hann et al. 2007: 36). Here the “socialist era anthropology” of course refers to ethnography, which Skalník also variously calls ‘nationgraphy’ or ‘peoplegraphy’.¹² For Skalník: “social anthropology with its **revolutionary theory** and method causes havoc in the ranges of the traditional nation or peoplegraphy” (Skalník in Hann et al. 2007: 38; emphasis mine).

According to another and more recent statement which cannot leave us in doubt, the disciplinary history emerges: “from the history of ideas, theories, conceptions of research methods and finally, from the results and consequences of scholarly efforts.” (Kandert 2018: 53). Again, and again, now and then, the debate is reduced to the postulate of at least two intellectually distinct disciplinary traditions. Of course, sometimes we read not only of theories, but also of research methods, sometimes we also learn about political matters as for example in Katherine Verdery’s account which mentions non-intellectual features such as political forces giving rise to specific intellectual content (the difference between the nation-building and empire building ethnographies). Nonetheless, all the accounts

¹² I believe that Skalník reserves the term ethnography for strands within Czechoslovak ethnography which he praises and the terms nation- and peoplegraphy to strands which he denounces (cf. Skalník 2005a: 69–72). The term peoplegraphy (*lidopis* in Czech) was also in use by the scholars of *národopis* (cf. Stránská 1936).

ultimately end up with a postulate of different intellectual traditions or intellectual disciplines.

This is what I call intellectualism in this thesis and the inspiration comes especially from *Pascalian Meditations* written by Pierre Bourdieu (Bourdieu 2000). Intellectualism is an invisible and dominant framework. It constitutes almost a natural way of speaking, writing and thinking about academic disciplines. Intellectualism is not only a feature of the discourse on the post-Socialist fate of European anthropologies and ethnographies and their relation to Western sociocultural anthropology. Intellectualism grows rampant in classical historical accounts of anthropological schools or in intellectual biographies (Barth et al. 2005; Bendix 1962; Fardon 1999; Hall 2010; Harris 1968; Kuper 1996, 1999; Leaf 1979; Lukes 1972; Parkin 1996; Stocking 1987, 1995; Tambiah 2002). This is not to say that authors of these works revel just in guiding their readership through delicate intellectual labyrinths of the brightest representatives of our disciplines or in showing the fruits begotten by the geniality of our predecessors. This is not to deny that historical accounts also provide vivid biographical details, ample historical context and tell us a great deal about institutional history. However, the non-intellectual matters usually serve as a less important backdrop to the more significant, that is intellectual, affairs. As the Bible says, by their fruit you shall know them.¹³

While this thesis shares the project of comparing various disciplines within the realm of social sciences and humanities, it largely departs from the intellectualist approach. This thesis wants to approach its subject matter from an angle which gives the supposed intellectual differences far smaller a place as regards understanding and explanation and pursues the goal of a systematic comparative study. Unfortunately, to tame the revolutionary excitement, this thesis cannot in principle fully depart from intellectualism as it must address some issues which concern intellectual specificities of ethnography and folklore studies. What this thesis offers is not a radical departure, but a reversal of importance. Whereas intellectualism tends to present us with distinct scientific disciplines, philosophical systems, schools of thoughts, theories, families of concepts and resulting knowledge as products of intellectual achievements of individual human minds or of intellectual collectives, this thesis shows what we can gain if we dare to approach

¹³ This is not to say that various authors share the same motives for focusing on intellectual issues. Following Merton, at least two broad modes of intellectual history can be distinguished – the history of science and the systematics of science, but these two kinds get usually mixed up (cf. Maršálek 2012: 60). Some good examples of histories of science which make do without intellectual considerations are works by David H. Price (2004, 2008) or William Clark (2006).

intellectual objects as products of non-intellectual features of scholarly work. Whereas intellectualism approaches intellectual objects as produced by intellectual capacities, this thesis approaches intellectual features as produced by other than intellectual capacities. While intellectualism tends to present intellectual objects as somewhat connected to their cultural and social milieu, I will attempt to present a more detailed connection. Hence this thesis explores often side-lined arboreal facts other than fruits – branches, trunks, leaves, blossoms, the soil and climate.

The problem of the widespread definitional attempts is that they cannot be in principle comprehensive, because they involve only a fraction of what science is usually all about. This thesis wants to show that apart from intellectual matters, the content of ethnography or anthropology as disciplines usually involves much more than scholars' intellectual ruminations. Regardless of the theories currently in vogue, scholars also have some approach to their work, have certain publication strategies and choose between possible publication opportunities that are available to them, they go to conferences and respect academic hierarchies, or have some deeply ingrained writing and research habits. In social sciences, this approach is by no means new. It only seems to me that it is not rigorously pursued. In this thesis I profess an approach that was developed by Pierre Bourdieu in his criticism of intellectualist approaches and in his works in sociology of science. The fundamentals of Bourdieu's sociology of science as well as other methodological tools which this thesis employs are explored in the remaining part of this chapter.

1.3 Theories and Methods

The critique of intellectualism is not the only inspiration taken from the sociology of Pierre Bourdieu. Bourdieu's sociology offers an approach which serves as a suitable alternative to intellectualist musings concerning proper disciplinary definitions, that is definitions which also play the role of explanations or understandings of what ethnographers, ethnologists, folklorists or anthropologists usually do. This thesis employs three pivotal concepts of Bourdieu's sociology – the Triade of *field*, *capital* and *habitus*. It is necessary to provide some outline of these three concepts here as they will be employed in the main body of the thesis.¹⁴

¹⁴ Bourdieu's theory presents a complex body and it is impossible to outline it in its entirety here. I only limit the discussion to the concepts which are essential to this thesis and which I develop here. Readers are also to

1.3.1 The Concept of Field

One of the recurrent themes in Bourdieu's sociology is its refusal of what Bourdieu calls the irenic view on science, whose iconic representative is Thomas Kuhn. Kuhn's landmark work presents the development in science as a successive sequence of paradigms, that is intellectual systems created by scientists to explain occurrences in the world by means of providing some internally coherent view on the world. Paradigms, however, usually fail to explain all occurrences in the world. Inexplicable occurrences which a paradigm is unable to explain are called anomalies. In the course of the ongoing research work based on a particular paradigm, anomalies begin to pile up and sooner or later the paradigm begins to crumble under the stress which these anomalies exert upon the paradigm's internal structure. The paradigm eventually falls apart and is superseded by a new and a superior paradigm which newly accounts for the erstwhile anomalies; and the process goes on and on. As paradigms rise and fall so the science develops and progresses towards its more perfect forms of understanding the world. Although Kuhn makes in his book several sociological observations the main thrust of his argument rests on intellectualist assumptions for intellectual progress is explicable solely in intellectual terms. In Bourdieu's words, such a view portrays the intellectual development of science, that is the succession of paradigms, as resulting from activities of the scientific community which knows "no law other than that of a pure and perfect competition of ideas, infallibly decided by the intrinsic force of the true idea." (Bourdieu 1991a: 8)

To tackle this view on the development of science, Bourdieu applied the concept of *field*, a concept which he also used to study the modern state, economics and bureaucracy (Bourdieu 2005; Bourdieu et al. 1994), the church and religion (Bourdieu 1991b), modern art (Bourdieu 2010), mass media (Bourdieu 1998a), science and intellectuals (Bourdieu 1975, 1991c, 1991a), philosophy (Bourdieu 1991d) or academics and higher education (Bourdieu 1988, 1996), and the concept can be also applied to study other phenomena such as the sport, the military, medicine or the culinary world. Bourdieu developed this concept to study mostly realities of French history and society, but he held that the concept alongside others was transposable to other societies (Bourdieu & Wacquant 1992: 75) as it was

be reminded that Bourdieu's theory is not all-encompassing and as a proper theory necessarily reduces the complex tangle of social life. Bourdieu's theory also works with abstract concepts and I will try as much as possible to bring them down to earth.

similarly transposable between various segments of society. The concept of *field* is a concept particularly suited to study modern societies which evince a great deal of compartmentalization, a phenomenon that has been noted since the advent of sociology in the nineteenth century and which found its expression in Émile Durkheim's work on the division of labour (Durkheim 2004) or in Max Weber's notion of value spheres (Brubaker 1984: 6). The concept also allows itself to be transposed to sub-segments, i.e. subfields, of particular fields.

Approaching scientific disciplines as *fields*, Bourdieu also goes against explanations which relate the development of a science to the development of the social structure of a corresponding society (Bourdieu 1991a: 11). Such a view treats science as a mirror of the society at large. Bourdieu's approach to science navigates between the internalist and externalist views. The concept of *field* allows Bourdieu to approach society as consisting of various interlocking fields each of which has a specific degree of autonomy on other fields as well as on the overarching field of power represented by the state politics (Bourdieu & Wacquant 1992: 76, 104–105). Bourdieu introduces a specific variable by saying that the autonomy of any given field “varies with the intensity of the constraints and controls exercised, directly or indirectly, by external powers,” (Bourdieu 1991a: 15). Each *field* can be seen as possessing a specific degree of autonomy which differs from other fields. Rather than emphasizing uniformity across a particular society, his model is better suited to explore internal divergence. In such a view, any society is approached as a multi-speed complex in which different fields possess various degrees of autonomy and to some extent develop at different rates and in different directions.

The autonomy of a field can be ascertained in several ways. First, there are “barriers to entry” (Bourdieu & Wacquant 1992: 100). If the barriers are harder to overcome from the outside and when it is harder to enter the field, then the field can be said to evince a greater degree of autonomy and *vice versa*. The second way of ascertaining the degree of autonomy is related to the concept of *capital*.

1.3.2 The Field of Ethnography and Folklore Studies and the Forms of Capital

Since any field possesses a greater or lesser degree of autonomy (it would make no sense to speak of a field if it possessed no autonomy) on other fields and the field of power, field is also “... a field of forces whose structure is defined by the continuous distribution of the

specific capital possessed, at the given moment, by various agents or institutions operative in the field.” (Bourdieu 1991a: 6).

Since any field is autonomous, forces which operate within the field are forces that are *internal* to the field (either purely internal or coming from the outside, but refracted by the boundaries of the field). The structure of forces operating within the field is based on an unequal distribution of various forms¹⁵ of *capital* between agents or institutions which appertain to the field. Bourdieu recognizes several basic forms of *capital*: *social*, *economic*, *cultural* and *symbolic*.

Social capital stems from the “possession of a durable network of more or less institutionalized relationships of mutual acquaintance and recognition.” (Bourdieu 1986: 51). It can consist of group memberships, acquaintances, personal connections, friendships etc.¹⁶ *Economic capital* refers to economic wealth, that is money or resources directly convertible into money such as property rights, shares, bonds, debts, cheques etc. (Bourdieu 1986: 47). *Cultural capital* is a form of capital that comes in “the form of long-lasting dispositions of the mind and body,” (Bourdieu 1986: 47) that is skills, proficiencies, abilities, proclivities, tastes, preferences etc. *Symbolic capital* is “any property... when it is perceived by social agents endowed with categories of perception which cause them to know it and to recognize it, to give it a value.” (Bourdieu 1998b: 47). To provide a more concrete idea, *symbolic capital* refers to fame, repute or honour wielded by agents, objects or institutions.

To these forms of capital, we can add several forms operative in the field of ethnography and folklore studies. It will be useful to distinguish three other forms. *Party capital* will refer to the capital related to the membership in the Communist Party of Czechoslovakia and to other organizations related to the rule of the Communist Party such as the Union of the Czechoslovak-Soviet Friendship (*Svaz československo-sovětského přátelství*) the Revolutionary Trade Union Movement Actors (*Revoluční odborové hnutí*) or the Socialist Union of Youth (*Socialistický svaz mládeže*). This kind of capital could be

¹⁵ It is useful to hold to Bourdieu’s English translators’ distinction between *forms* (or kinds) of capital (see below) and *states* of capital. Three states in which we can encounter capital are: *embodied*, *objectified* and *institutionalized* (Bourdieu 1986).

¹⁶ *Social capital* refers among other things to a complex web of ego-centred connections which involve friends, family members, enemies etc. Some of these phenomena are hard to unravel as they require rather very intimate questioning, involve working with gossip-like information and generally require an immense bulk of information. The analysis presented in this thesis does not aim to provide a systematic account of these relationships among ethnographers and mostly captures the most visible connections only. Moreover, the nature of ego-related information could compromise the anonymity of my informants.

enhanced by active participation in these organizations and, as we shall see, it opened some career opportunities unavailable for those who possessed no *party capital*. Those who did not possess any *party capital* were not members of any of these organizations at the time. We can also speak of negative *party capital*, possessed by those who were stripped of their Party membership. However, at the same time, *party capital* was not unanimously viewed as beneficial as it could afflict some personal relationships with people opposing the Communist Party rule. When the Communist Party lost its dominant position in Czechoslovakia in 1989, *party capital* did not only lose its potency, but it began considered as negative: the larger the previously accumulated volume, the larger the future burden (cf. Verdery 1996: 90–91).

Academic capital on the other hand applies to all ethnographers and folklorists and is derived from their research work and can be measured by the intensity of their research and lecturing activities, by the time they spent working on research issues, or by the volume of production, by the subtlety or novelty of their arguments, by conscientiousness of their approach toward the problems of science etc. In short, academic capital refers to how one is able to fulfil expectations related to his or her position *qua* academic, that is as a researcher or lecturer. This striving for this kind of capital roofs divergent strategies and their combinations – focus on research, focus on writing, focus on lecturing, focus on popularization etc.¹⁷

Regarding personnel policies there has been one untouched strain related to ethnography, which prevents us from arriving at a more comprehensive verdict. As it was demonstrated on the life histories of historians from the Faculty of Arts, scholars could enhance their position by cooperating with the State Security (*Státní bezpečnost*) even without the necessity of becoming a Party member (Jareš et al. 2012: 233–235, 287–291). So far, the debate concerning ethnography after 1968 only reckoned with the differences between trustworthy Communist Party members and the rest. It cannot be ruled out that Robek helped some people after 1968 not only because his institutions could profit from

¹⁷ For this reason, I was tempted to further differentiate *lecturing*, *research* and *intellectual capital*, but eventually decided not to as this move would not yield much fruits. Also, since ethnography was avowedly an engaged science, I was also tempted to include *engaged capital* (which would be similar to either engaged or advocacy anthropologies or popularization science) and uncover strategies that aimed to maximize this form of capital. The leading figure of the late-socialist ethnography, Antonín Robek, for example, helped to create a TV-series and co-wrote a book of bed-time stories for children (see Chapter 3). Besides, many other ethnographers worked at ethnography museums visited by the general public. Some of them contributed to *Lidé a země*, a popular educational journal monthly. However, the reasons why I did not further differentiate these kinds of capital is that this would require a detailed study of the relations between ethnographers' production consumed by the wider public.

these people's rich publication activities or high symbolic capital, but also because these people got a backing from the State Security, or in other words, possessed *clandestine capital*.¹⁸ What seems to be better mapped are some pressures that the State Security directly exerted on personnel policies. For example, its officials asked head of the Ethnography Department to restrict the contacts of one of the department employees with colleagues in the West and they also requested the head not to extend the employee's position after reaching the retirement age (Jareš et al. 2012: 249).

The above forms of capital will be further utilized for the purpose of the argument of this thesis. If we return to the considerations related to autonomy, Bourdieu proposes a specific variable related to *capital*: "The greater the autonomy of the field, the more struggles for power over capital... tend to confine themselves to strictly scientific grounds." (Bourdieu 1991a: 15). So, if a field evinces a great degree of autonomy then what matters most within the field are also forms of capital which are internal to the field. If the autonomy is greater, we then find agents investing in the improvement of their positions by creating stable networks with colleagues, in increasing their academic competence (such as learning a new language, reading more books, developing a new method or improving their writing) and in pursuing the image of an accomplished scientist. On the other hand, if a field evinces a lesser degree of autonomy, then one's position within the field can be improved more by relying on forms of capital which are external to the field. Under such conditions, we can expect that an investment into building good relationships with politicians or becoming a Party member can be seen as more effective strategies for the success in the field of science.

It is important to note that *capital* is a concept which does not have any direct referent. It is not that the concept does not refer to any real things: money is as real and effective as personal connections, fame or extraordinary intellectual abilities. It is that we do not observe these things in some guise of capital. Capital is more an abstraction, a heuristic tool whereby the researcher can highlight and further work with relevant features of the material in question. In this thesis I prefer to speak in terms more akin to the perspective of the actors, that is more in terms of money, connections or fame and less in terms of capital.

¹⁸ As concerns ethnographers in the late socialist period, according to materials available in the Security Services Archive, Antonín Robek and Vladimír Kristen cooperated with the State Security (cf. TS-809716 MV and TS-714387 MV). Needless to add that the pursuit of clandestine capital was not a hundred-percent secure way to achieve a guaranteed academic career (Jareš et al. 2012: 264).

For the distribution of forms of capital is unequal, any field can be also approached as a *space of positions* (Bourdieu 1991a: 9–10), also called *field of positions* (Bourdieu 1991a: 11). By construing field as a multidimensional object by introducing various forms of capital along different axes, we can show how the agents are related to each other in a particular field by tracing the overall volume of capital which these agents possess. Here the volume of capital serves as a heuristic device allowing us to simplify things a bit and to highlight important vertical and horizontal relations. However, not all forms of capital are easily quantifiable. Some institutional positions are too unique to be quantified as they allow their occupants' actions unimaginable to the rest. It is not a matter of possessing more or less capital, but a matter of possessing or not possessing something exclusive. Perhaps, for this reason, Bourdieu wrote that it is not only volume or quantity, but also a structure of particular forms of capital (Bourdieu & Wacquant 1992: 99).¹⁹ For these reasons, it is sometimes better to speak of the field of positions in terms of actual hierarchies, that is available institutionalized positions or non-institutionalized but otherwise acknowledged positions which endow agents with different possibilities to act. This look, apart from the fact that it circumvents the difficulties of the quantification of capital, brings us closer to agents' perspectives too. Agents aim to secure concrete positions or concrete means, not more or less capital that will lead them to the positions or ends that they yearn for.

1.3.3 The Dynamics of the Field

At any given moment, the field can be seen synchronically as a space in which various forms of capital are unevenly distributed among the actors and institutions. If we want introduce some dynamics into the system, we have to see the field as

a field of struggles or a space of competition where agents or institutions who work at valorizing their own capital – by means of strategies of accumulation imposed by the competition and appropriate for determining the preservation or transformation of the structure – confront one another. (Bourdieu 1991a: 6–7)

¹⁹ Unfortunately, the *structure* in this passage can be interpreted in the way that it is the structure that emerges from possessing various volumes of various forms of capital, not the structure that is immanent to just one of these forms.

The field is a “space of play” (Bourdieu 1991a: 8) in which its occupants try to valorize, that is, to accumulate or conserve (Bourdieu 1991a: 7) various forms of *capital* by pursuing manifold *strategies*. It would be a misunderstanding to see accumulation in terms of economic utilitarianism which views all human agents equally driven by their desire to maximize economic profit (Hage 2013: 79). By recognizing various forms of capital, Bourdieu is able to show that we can discern various strategies aiming at accumulation of different forms of capital. These strategies are usually divergent and contradictory as acquisition and accumulation of some forms of capital precludes acquisition of others. For example, an ethnographer who would decide to brighten up his or her possibilities of career advancements in ethnography by becoming a Communist Party member could afflict some of his or her personal relations within and outside of the field.

The valorization of capital means that an agent pursues strategies related to the accumulation or conservation of some sorts of capital rather than others and to improving his or her position by focusing on some forms of capital and disregarding others. Strategies of valorization also bring us to an important capacity of capital which is mutual convertibility between various forms of capital (Bourdieu 1986: 54). So, one could convert *party capital*, acquired by becoming an active member of the Communist Party, into *academic capital* as the membership could grant one a permit to have a better prospect to become a university lecturer or getting a stint at a foreign university and hence have more opportunities to come by to books which were unavailable in Czechoslovakia as the Communist Party rule did not enable a free intellectual exchange across Czechoslovak borders.

Forms of capital can be converted into other forms, but at different rates of conversion (Bourdieu 1986: 53–55). And some forms cannot be converted into others at all. A good example will suffice to illustrate the mechanism here. If one wanted to maximize *academic capital*, one had to consider the accumulation of *party capital*. It was *party capital* which was easily convertible into further *academic capital* and into strengthening and improving on one’s position in the field. Conversely, even by being an amazing lecturer, excellent researcher or famous populariser of science, one could not much improve his or her position in the field by crowning the career by becoming head of a department, unless one was also a Communist Party member. This convertibility or inconvertibility of some forms into others sheds further light on the degree of autonomy of the field.

When speaking of the socialist Czechoslovakia, we can view the pursuit of *economic capital* and *party capital* as having the same or similar efficacy across different fields. *Social capital* can refer to relations within the field as well as to relations outside the field. The remaining forms of capital, *cultural*, *symbolic* and *academic* will be approached as field-specific. The existence of specific forms of capital is granted by the very fact that the field of ethnography and folklore studies had *some* degree of autonomy. Hence, contrary to Skalník, the discipline of ethnography and folklore studies was not a mere appendage of the state power providing ideological fodder for the Communist Party (Skalník 2002b: 101). Even though that there existed “generalized production”, which means that ethnography provided ideological services to the state, there also existed “restricted production”, that is, ethnography knowledge was produced to be consumed solely within the confines of ethnography (Bourdieu 1991a: 19). This means that ethnographers could still employ strategies that were aimed at acquisition, accumulation and conservation of forms capital internal to the field and pursue the image of accomplished scientists or experts in their respective specializations.

Acquiring and accumulating capital is not a straightforward process. Bourdieu resorts to a ludic metaphor and says that agents play games in the field. These games involve investment in the form of stakes which become the object of competition. Strategies can prove successful and agents profit from their investments or their strategies can misfire and agents lose their stakes. Apart from partial investments, “the definition of the stake of the struggle is also a stake in the struggle” (Bourdieu 1991a: 14). This in other words mean that agents, for example, try to impose on others what counts as science, or alternatively, what counts as ethnography proper. This also involves what theories or methods are relevant or even what research orientations count and which do not. If some definition prevails then it divests those who do not fit into the definition of their capital and of further possibilities of accumulation and conservation.

At this point, it would be appropriate to introduce one distinction important to Bourdieu’s sociology. Coining an elegant definition of what one’s field of enquiry is and what it is not, pointing out to similarities to and differences from kindred fields of enquiry as well as providing a critique of someone else’s definition can be perceived as *position takings* in the *field of possible stances* (Bourdieu 1991a: 10–11). The *field of possible stances* is not just limited to questions of definitions. It is a broader

... universe of legitimate problems and of objects, questions to be resolved, theories to refute or surpass, experiments to verify or invalidate, insistently captures the attention of all those who claim to assert their existence in the field, and who have the specific competency necessary for knowing and recognizing these insistent virtualities. (Bourdieu 1991a: 10)

The *field of possible stances* should not be confused with the *field of positions* actually occupied by agents. By taking a position in the field of possible stances agents try to invest in the game and capitalize on the capital they possess at the moment and hence strive to improve on (or at least conserve) their position in the *field of positions*. These strategies can be either successful or unsuccessful and agents might lose or gain capital and hence “move” in the *field of positions* accordingly. The whole debate which was introduced in the Part 1.2 can serve as an apt example of agents trying to valorize on their capital by investing themselves in definitional struggles and hence to improve their position by means of offering elaborated arguments of what some discipline is and what it is not. Similarly, this thesis can be understood as an attempt to change the universe of legitimate discourse by offering a critique of intellectualist approaches and by developing an alternative view on what constitutes scholarly disciplines.

Accepting this view on actors’ strategies, we can now understand the strengths of Bourdieu’s approach especially if we compare it to the intellectualist approach of Kuhn and its derivations. Bourdieu’s sociology perceives the field of science as a space of many different activities. Not all academics aim to pursue the questions of science and hence the achievement of the “Kingdom of Ends” in science (Bourdieu 1991a: 8) is at mercy of these agents too. Moreover, even those agents interested in pure science must secure for themselves non-scientific means of practicing science, that is positions, economic means or fame for it is exactly these means which allow them to get closer to the Kingdom of Ends. Since all means are scarce, achieving these means implies achieving them at the expense of someone else. Therefore pursuing these means also entails domination, that is reduction of someone else’s possibilities to act.²⁰ As Bourdieu says agents’ strategies are

²⁰ One could argue that the situation in science under socialism was different from science practiced under current conditions. Although there is neither the Communist Party encroaching on the field of science, nor is science currently harnessed to some direct national ends, which suggests that academics are freer to pursue their research interests today, it does not follow that academics do not to pursue strategies aimed to secure other-than-strictly-academic forms of capital. For example, we can discern strategies that aim to maximize academic fame and hence more possibilities by writing for the general public, strategies that aim to enhance one’s CV and hence one’s possibilities by rapid writing. Similarly, some forms of capital are hard to convert

always two-sided in these respects (Bourdieu 1991a: 16) without actors necessarily being aware of this. The fields are dynamic sites and it is actors who partake on the distribution of forces by their very actions.

Here, it is necessary to emphasize that Bourdieu's sociology is intrinsically political. It approaches people as agents who, by means of various strategies, try to secure their position within the field by accumulating or conserving various forms of capital. In Bourdieu's view scholars' actions are intrinsically political regardless of whether scholars themselves are aware of the political dimension of their action and regardless of other dimensions their actions have. The major advantage (and at the same time its weakness) of the concept is that it provides some logic to human action where no direct evidence can be found, be it for the lack of sources or for the narrators' unwillingness to speak or their more or less conscious omissions of certain topics. This is important to state beforehand as the ensuing account works with these presuppositions and is hence open to further criticisms.

1.3.4 Ethnographers' Habitus

Finally, we arrive at the last and, for the main argument of this thesis, crucial conceptual component. It is the concept of *habitus*:

The conditionings associated with a particular class of conditions of existence produce habitus, systems of durable, transposable dispositions, structured structures predisposed to function as structuring structures, that is, as principles which generate and organize practices and representations that can be objectively adapted to their outcomes without presupposing a conscious aiming at ends or an express mastery of the operations necessary in order to attain them. Objectively 'regulated' and 'regular' without being in any way the product of obedience to rules, they can be collectively orchestrated without being the product of the organizing action of a conductor. (Bourdieu 1990: 53)

The thesis is mainly interested in ethnographers' and folklorists' *habitus* that is structuring structures and dispositions which produce academic practices. *Habitus* originate

into forms of capital pertaining to the field of power. Politicians or civil servants are not usually experts in scientific fields and one can make much more impression on them by showing off the number of publications, stressing the symbolic capital of journals in which the publications appeared or by mentioning a stint at a prestigious university abroad.

in particular conditions, which, in our case, are mostly defined by the field of ethnography and folklore studies in the late socialist academia, but they are not limited to them.

Bourdieu distinguishes two types of *habitus*, *primary* and *secondary*. The concept of *primary habitus* refers to the dispositions acquired in the earliest phase of upbringing, that is dispositions that form a basis for subsequent formation of any other *habitus* (Bourdieu & Passeron 1990: 42). Although I am not concerned with *primary habitus* here, it is necessary to consider that differences between scholars and their ways are to certain extent determined by their different upbringing and that making references to their *primary habitus* might account for these differences. Nonetheless, this work is mainly interested in *secondary habitus*, or its components, formed especially during university training and in the course of academic careers (Bourdieu & Passeron 1990: 42–43). Here the concept reconnects to the concept of field. *Field* conditions and shapes *habitus* that appertain to it to an important degree. *Field* is a structure that structures *habitus* and is a structure that is in turn structured by *habitus* (Bourdieu 1990: 53).

The concept of *habitus* possesses one crucial component which is *durability*. This thesis explores the *durability* and bases its main argument on it. When the situation in the field of ethnography and folklore studies changed after 1989 and brand-new possibilities opened for ethnographers and folklorists, not much substantial change occurred as the agents remained pretty much the same and continued their academic work in ways not so different from what they had been doing throughout the late socialist period. Bourdieu calls this mismatch between the newly available objective possibilities in the field and subjective orientations *the hysteresis effect*: “there is an inertia (or *hysteresis*) of habitus which have a spontaneous tendency... to perpetuate structures corresponding to their conditions of production.” (Bourdieu 2000: 160).

Another crucial component of the argumentation of this thesis is the reproduction of *habitus*. Bourdieu was originally interested in the reproduction of inequalities within society. He noted that the unequal distribution of *cultural capital* (that is various dispositions) between various classes of the society is reproduced due to the schooling system which, although allegedly based on meritorious principles – to each according to their abilities – actually reinforces unequal distribution of *cultural capital* among social classes (Bourdieu 1998b: 20). Here, I am not interested in the reproduction of inequalities, but in the reproduction of specific academic and scholarly practices which endured changes of the field under scrutiny in the 1990s.

Lastly, habitus outwardly manifests as bodily *hexis*. According to Bourdieu, hexis is a “pattern of postures that is both individual and systematic, because linked to a whole system of techniques involving the body and tools, and charged with a host of social meanings and values” and covers bodily ways as diverse as “a way of walking, a tilt of the head, facial expressions, ways of sitting and using implements, always associated with a tone of voice, a style of speech” (Bourdieu 1977: 87). In the context of this thesis, it is to say that scholars have specific bodily ways of being, acting and behaving in the world, ways of appearing to others in the world.

Focusing on ethnographers and folklorists’ *habitus* I am only attempting to offer a comprehensive view on a theme which has been around for some time in anthropology. Apart from works of some classic authors who also wrote on how anthropological practice should be taught, that is, how practical proficiency should be reproduced (Evans-Pritchard 1951; Lévi-Strauss 2006: 310–318), probably the first one who came closest to studying something like anthropologists’ habitus was Tamás Hofer who back in the 1960s offered some notes on differences between *professional personalities* of Hungarian and American anthropologists (Hofer 2005). Other good examples include similar comparisons provided by Bea Vidacs (Vidacs 2005) and Chris Hann (Hann 2009). More focus on the educational aspects of forming professional personalities can be found in articles by Peter Hervik (Hervik 2003) and Thomas Fillitz (Fillitz 2003). These accounts, though scarce, nicely exemplify what Stocking made his goal in his work on the history of anthropology: “the development of anthropology as a form of disciplined inquiry” (Stocking 1995: xv). Stocking’s goal is also the goal of this thesis, yet this thesis will be paying attention to different aspects of scholarly work than Stocking did.

1.3.5 Material

This work is necessarily historical because it mostly deals with what happened in the remote past. This made the research difficult for I could not study the problems *in situ*. This is especially important in relation to the reproduction of scholarly habitus which is a matter of day-to-day activities and interactions between ethnographers. This day-to-day transmission must be presupposed. Owing to this methodological difficulty, I had to look for the testimony of practices elsewhere. The main source of material is represented by scholarly writings of ethnographers. My research did not start with some neatly defined pool of writings and as my research progressed it became harder to find any rationale for

the selection of all the texts which influenced the construction of my thesis. The sources which I used are listed in the Bibliography at the end of this thesis.

In this thesis, I approach ethnographers' writings not only as textual sources and I am less interested in the poetics of ethnography and folklore studies (Clifford & Marcus 1986; Fabian 1983; Lass 1989), that is, how ethnography and folklore studies constituted its object of study by textually representing it. On the contrary, I am mostly interested in various extra-textual features of these writings such as their numbers, length, quantity, uses of citations or quotations, uses of figures, page layout and material, bookbinding etc. I approach these extra-textual features as indexes of former practices. Especially Chapters 4 to 6 employ this methodological approach. I did not follow any precise methodology, but I attempted to interrogate my material as much as possible to obtain maximum possible information related to the questions which I pursue in this thesis. Some information was elicited not from singular sources, but from a comparison of various sources.

Equally important source of this thesis are works on the history of Czechoslovak ethnography among which the most important is a collective monograph *Ethnology in a Confined Space* (Woitsch et al. 2016), but there are several other books, collections, almanacs and articles (Hlaváček 2017; Holubová et al. 2002; Petráňová 2017; Thořová et al. 2005; Válka 2006, 2013; Válka et al. 2016; Woitsch 2013). Additional sources of information are several books on the history of Czech and Czechoslovak higher education and the history of the Faculty of Arts of Charles University in Prague of which the Ethnography Department was a part (Holý 2010; Jareš et al. 2012; Petráň 2015; Šima & Pabian 2013). Biographical information about ethnographers is taken from an encyclopaedia of traditional culture, which was, fortunately enough, published before the GDPR times (Brouček & Jeřábek 2007).

Four more sources helped me to obtain further information. First, I made use of several documents available in the archives of the State Security (*Státní bezpečnost*). This source presented a minor source of information and the exact list is included before the list of conventional sources. Second, I utilized an annually published list of lectures called *Karolínky* to get a grasp of personal changes at the Ethnography Department between 1970 and 1999. These lists are accessible in the Archive of Charles University. Third, I made use of several bibliographies which helped me to trace ethnographers' publication output (Drápala & Nosková 2003; Zajonc et al. 2013). In the case of other journals and media, I

created the bibliographies myself.²¹ Fourth, I gleaned some information from interviews with ethnographers, previously published in a compendium by Jiří Hlaváček and Hana Bortlová-Vondráková (Hlaváček & Bortlová-Vondráková 2018). Besides, for the purposes of obtaining information, I conducted several interviews myself and the methodology is described in the adjoining section.

1.3.6 Oral History Method

This thesis borrows inspiration from the method of oral history for the purposes of eliciting information about past practices from scholars who began their careers between the 1950s and the 1990s. It is particularly inspired by the mode of oral history which Lynn Abrams calls “recovery history”, which stands for “the practice of interviewing people to provide evidence about past events which could not be retrieved from conventional historical sources, usually written ones, or to uncover the hidden histories of individuals or groups which had gone unremarked upon in mainstream account.” (Abrams 2010: 5). This thesis thus downplays, but not completely, other modes of oral history.

This method naturally faces objections which concern the trustworthiness of interlocutors’ recollections as to their relation to the past events. A variety of good reasons casts doubt upon the relationship between memories and the past events. Human beings generally tend to forget, their memories get mixed up and the longer the temporal distance dividing the interview and the original events, the hazier the recollections are. Besides, human beings might be occasionally interested in withholding information, in manipulating the impression of their stories or in telling lies. And regardless of whether we account for the inaccuracies in terms of the inevitable natural processes of the brain or in the terms of social forces, interlocutors’ stories cannot be approached as representing the past. Therefore, the recovery mode of oral history is put into jeopardy. No doubt that some researchers switched to different modes of oral history which instead focus on the present construction of memory stories or individual subjectivity (Abrams 2010: 7). My research acknowledges the seriousness of all the objections listed above, but I believe that a consistent application of concomitant scepticism would seriously undermine any historical inquiry based on the recollections of interlocutors. Moreover, such a consistence would

²¹ These self-made MS Excel bibliographies include journals *Český lid*, *Zpravodaj KSVI* and *Národopisný věstník československý*. They also include the edition *Národopisná knihnice*, two series published by Moravian ethnographers (*Socializace vesnice* and *Lidová kultura a současnost*), and other relevant volumes which are mentioned in Chapter 5.

undermine and render impossible many social science approaches which are based on historical research, including Bourdieu's sociology.

There are two possible ways of saving the historical, or recovery, mode from these objections. The first concerns the general claim about the connection of the recollections with the past. If it is claimed that the statements of interlocutors are unrelated to what happened in the past and relate only to the present then this claim does not constitute a proof of the impossibility of the relatedness. Possibly more fruitful is the second way which leads us from general statements about the relation to more practical questions for which a discriminating between various interlocutors' statements and utterances is crucial. In the course of my research I confronted my interlocutors with many questions which concerned the past practices. These, for example, involved their career choices, experience with language courses, peer review process, writing of books, duration of fieldwork and others. Some peculiarities of these topics were brought to the light of day by my interlocutors. One of them brought my attention to the existence of a journal called *Demos* of which I was ignorant and in which ethnographers had used to publish. Another brought forward the issue of remuneration for the articles written to *Český lid*, the most important disciplinary journal of the times. I attempted to corroborate these statements by asking other ethnographers about these matters too as well as by looking for corroborating evidence in written documents. As I learned later, the journal which one of my interlocutors recalled, indeed existed. What I also learned was that my interlocutors were quite able to recall the names of their erstwhile classmates, a recollection which can be contrasted with the on-line records of the Library of the Charles University which stores the diploma theses which include the year of submission. Acknowledging shortcomings of this procedure, if we assume that the same year of submission also entails the same class, then we can deduce the erstwhile classmates and then compare it to the recollections about former classmates.

Of course, the more detailed a particular recollection is, the more scepticism it invites. For example, when it comes to the issue of remuneration, several of my interlocutors recalled the remuneration independently on each other, but were uncertain as to the precise amount or as to the kinds of articles for which they were remunerated. Such a recollection would require a corroboration from archival materials. Some scepticism is also invited by the difference in accounts.²² One essential topic of this work is the role of

²² A few arguments of this thesis rest only on singular statements. I did so in cases which I find worthy of interest and which other future accounts might corroborate or disprove.

Antonín Robek, who is especially important for Chapter 3. As we shall see, there are individuals who present diametrically opposed views on his personality and his place in the history of ethnography. However, as I attempt to show it in the chapter, these contradictory views should not lead to a conclusion that some of these views are right and that the rest is wrong. In fact, I believe that these differing views are linked to different social positions of various actors and their concomitant experience. Some contradictions cast doubt on the recollections, but some others do not.

Scepticism also surrounds more intimate issues such as the relation of my interlocutors to the rule of the Communist Party. I cannot say that any of them denied their former membership in the Communist Party in spite of the fact that after 1989 the membership has become to be viewed as morally reprehensible. Although several of my interlocutors had some funny stories to tell – stories which tended to downplay their engagement with the Party, they were at the same time frank about their membership. This was perhaps possible because of the anonymity guaranteed to my interlocutors.

What requires to be underscored is that the recollections represent only one of the hinges on which my argumentation rests. Backed by the theoretical orientation of this work and alongside other sources they create a network of mutually supporting links. I do not claim that any of these links is unbreakable and this should serve as an invitation to future researchers in the field to cast doubt on the links which I have forged and reinterpret them in the light of novel evidence or reasoning. Apart from methodical carefulness, the sceptic's argument of impossibility does not have much to contribute to this research.

The practical dimension of the procedure was informed by a handbook of oral history (Mücke & Vaněk 2015). According to both authors, one of the key principles is to conduct at least two interviews with a single interlocutor. The first one lets the narrator follow his or her narrative, whereas subsequent interviews can involve questions or even some polemics (Mücke & Vaněk 2015: Chapter 5). Most interviews of mine did not follow this two-stage rule and I also did not hold to all the principles for the fact that the bulk of my material consists of material mentioned in the previous section. Interviews were useful for obtaining information and also for giving me directions in my research.

The oral history inspiration ended up with forty-five interviews with thirty-six narrators who had had experience with Czech *národopis*, ethnography, ethnology and anthropology between the 1950s and 1990s. As the initial focus of the research was on the 1990s, sixteen interviews covered narrators who did not begin their studies before 1990, and I transcribed only some of these interviews. The shortest interviews were only some

forty-five minutes long and the longest one took one hundred and eighty minutes. For each interview, I had a set of questions which I asked my interlocutors. The set of questions was gradually adjusted as the research progressed, because some of the questions turned out to be naïve or ill-posed. It might appear that the interviews were conducted in the mechanistic spirit of exchange of my questions and interlocutors' answers, but it was not so. Actually, some questions were designed as to give my interlocutors enough space to express themselves and many interesting information were not elicited as direct responses to my questioning. Several interviews proceeded in a more dialogical way as I departed from my questions and followed on themes broached by my interlocutors.

The interlocutors' names have been encoded for the purpose of this thesis and the interviews are referenced in footnotes. The codes are alphanumeric, begin with P0001 and end with P0036. Every such reference also includes a number of the page of the transcript of the referenced interview, but for the reasons of anonymity, neither the original recordings nor the transcripts will be published. Some interlocutors' statements which appear in this thesis refer directly to the concrete person rather than to the encoded interview, but in all such cases interlocutors agreed with it and were provided with a context in which their statements would appear. These non-anonymized references are not linked to any of the anonymized interviews.

In a way, oral history method is similar to grounded theory since it follows a bottom-up direction beginning with empirical material (interviews) and ending with forming concepts, theories and eventually conclusions that are grounded in the very empirical material (cf. Strauss & Corbinová 1999). Neither the oral history method nor grounded theory begins with theories that are further tested by empirical material. This thesis diverges from these approaches as its main frame is adopted from the sociology of Pierre Bourdieu which was subsequently moulded according to the degrees of resistance of the original material which I obtained during my research.

1.3.7 Comparative Method

This thesis also draws inspiration from Matei Candea's work on comparative method in anthropology. (Candea 2019). Candea claims that comparison has been a central method to anthropology and has been utilized even by those anthropologists who openly opposed the method. In this thesis, comparison serves to assess and compare multiple sets of partly resembling and partly differing constraints and their similar or divergent effects.

Comparison helps to reveal why some phenomenon exists under one set of constraints or conditions, but why the same phenomenon does not exist under another. Comparison is not tantamount to causal explanation and it is important to stress that it can only serve as a means of testing the convincingness of our explanations.

To put it in more concrete terms, comparison firstly serves as a method for tracing continuities and discontinuities in the historical development of Czechoslovak ethnography and folklore studies between the 1940s and 1990s. Secondly, in several chapters, ethnographers' practices are compared to those of anthropologists. These two comparisons allow me to show similarities as well as differences between the two disciplines. Comparison does in no way serve some flight of fancy which aims to show the superiority of one discipline over another. By the means of comparison of these disciplines, we can attain a better understanding of both. Furthermore, this thesis pursues two minor comparative tasks. The first of these traces the development that begins with the nascent of Czechoslovak ethnography and ends with its transformation into its successor disciplines, Czech anthropology and ethnology. Again, the comparison strives to capture development in terms of continuities and discontinuities. Finally, the second minor comparison involves pointing out to some differences between the post-Socialist Czech anthropology as an inheritor to Czechoslovak ethnography on the one hand and Western anthropology on another.

These four comparative tasks form a basis for the response to the discourse on anthropology in postsocialist Europe which I discussed in Part 1.2. As we have seen, these works worked with a comparative dimension too. An exemplary case is Skalník's short paper titled *Can We Create Anthropology out of Národopis?* (Skalník 2005b). The article implicitly works with all the four comparisons. It asks the question how to create a Czech tradition of anthropology by transforming the heritage of *národopis* and ethnography into sociocultural anthropology of the western fashion. Countless other articles are framed in the same comparative dimensions (Buchowski 2004, 2012b; Čapo 2014; Červinková 2012; Hann et al. 2007; Jakoubek 2012; Lozoviuk 2005; Nešpor & Jakoubek 2004; Scheffel & Kandert 1994). This thesis is only a more nuanced response to the comparative perspective which has been around for some time.

It is important to stress that what is being compared in the first place here are not disciplines as such, but disciplines as composites or sets of various features and practices which appear under certain conditions. Disciplinary labels such as anthropology, ethnology or ethnography enter the comparison only in the second place as labels that designate

composites of various practices some of which are more durable and persistent than others. For the purpose of tracing continuities and discontinuities in academic and scholarly practices, these labels as such are secondary as well as their “spatial” (Czechoslovak, Western, Franglus) or “temporal” (Socialist, late-socialist, post-Socialist, 1980s’) designations.

1.3.8 Notes on Translations

Some dilemmas of this thesis stem from the fact that its language is English, but its substance is related to a Czech and Slovak speaking world. For this reason, this thesis had to find some balance between being forthcoming to readers not versed in Czech or Slovak languages and the necessity to respect the original sources.

As regards the names of periodicals, they are not translated to English. Their approximate English translations are provided in a glossary inserted at the end of this thesis. Titles of books and articles appearing in the text were translated to English and their original Czech or Slovak titles can be looked up in the sources. Names of institutions, organizations, movements, events or some exceptional documents were translated to English and their first appearance is followed by the original name in brackets. Some of the most important names are also included in the glossary.

There are countless direct quotations from works of Czech and Slovak scholars past and contemporary, including many ethnographers, historians and anthropologists. Some quotations are from works translated to Czech or Slovak languages from Russian (e.g. Bromlej 1980; Potěchin 1953). Also included are English translations of quotations from interviews with interlocutors which I conducted or which were conducted by other researchers (e.g. Hlaváček 2017; Hlaváček & Bortlová-Vondráková 2018). I originally intended to supplement each translation with an affix indicating that I provided the translation to English. Unfortunately, the recurrent affix would be at the detriment of the smooth flow of the text in some chapters which make an intensive use of direct quotations.²³

²³ Some translations of mine have their origins in a collaboration with Robin Cassling who proofread my earlier article as a part of peer review process of my earlier article in *Sociologický časopis / Czech Sociological Review*. Here, I would like to express my gratitude once again.

1.4 Chapters

Having inspected all the gears and cogs of the theoretical and methodological dimensions of the work, it would be appropriate to present some outline of the chapters to follow. Each of the chapters dissects a specific part of ethnography and can be, to a certain extent, read separately. Nonetheless, there is a cumulative progression and a reader is advised to proceed in a linear way as some arguments appearing in later chapters build on arguments of earlier chapters.

Chapter 2 presents a sort of digression from the main focus of the thesis. While this work is mainly directed against the prevalent intellectualist approaches in the history of ethnography and anthropology, it cannot wholly repudiate them. If it is true that the life within the groves of academe is not only about matters intellectual, then it is equally true that intellectual matters are just an essential part of that life. Hence Chapter 2 will adopt an intellectualist standpoint and will conform to the intellectualist rules of the game. It explores two theoretical approaches – Marxism-Leninism and nationalism – which I have identified as dominant in ethnography. The chapter also tries to determine the extent to which ethnography and folklore studies was a positivistic science, an accusation levelled against ethnography especially by authors who wrote about ethnography after 1989.

Chapter 3 departs from the intellectual considerations and tries to approach ethnography and folklore studies as a specific field. The introductory part of the chapter offers some insights into the development of ethnography between the late 1940s and 1960s, but the main part of the chapter is devoted solely to the late-socialist period. It is especially interested in the Czech subfield of ethnography and its two major institutions in Prague, that is the Ethnography Department and the Ethnography Institute. Nonetheless, the chapter will also include the whole picture of the Czechoslovak field of ethnography and folklore studies and the Moravian and Slovakian subfields. The chapter will consider the diminished autonomy of the field and consider the political role of the state, the Communist Party and the dominant ideology. The chapter, among other things, addresses the oft addressed topic of intellectual freedom under the state Socialism.

While Chapter 3 attempts to track the forces which constrained and structured the field of ethnography mostly from the outside, Chapters 4 to 6 focus more on forces immanent to the field and focus more on actual scholarly practices of ethnographers in Prague in the late socialist period. Chapter 4 proceeds as a description of some basic features of ethnographers' daily bread. It inspects academic hierarchies, says something

about ethnographers' research and academic practices and language competences. Chapter 5 is particularly interested in ethnographers' attitudes to writing and their publication strategies. It adopts a quantitative approach and asks questions of how much ethnographers wrote, how many articles and how many books, and eventually asks which forces constrained and shaped ethnographers' publication strategies. Chapter 6 inspects ethnographers' attitudes towards discussing and mutual criticism. The chapter does so by resorting to a concept of the culture of contention and attempts a fresh look on the question whether ethnography stalled theoretically. These three chapters make an occasional use of the comparative perspective.

Finally, Chapter 7 brings the reader to conclusions. It attempts to resolve the central questions pursued in this thesis by pointing out to possible continuities and discontinuities between pre-1989 Czechoslovak ethnography and post-1989 Czech anthropology (and ethnology). Among other things, it offers some insights into the complicated relations which emerged between local ethnographers and those who came from the West in the early 1990s to preach sociocultural anthropology in Czechoslovakia.

2. Ethnography and Folklore Studies as an Intellectual Tradition

2.1. The Czech Case: The Three Accusations

The discipline of ethnography and folklore studies has been repeatedly accused of having committed three intellectual errors. It conformed to the ideology of Marxism-Leninism,²⁴ it worked with a more or less implicit nationalist framework and lastly, its research rested on positivistic foundations. I will consider these three accusations in three consecutive sections of this chapter, introducing each section with specific instances of the accusations. As we shall observe, some authors make do with just one accusation, others level more than one. I will discuss only accusations which are relevant to Czechoslovak ethnography and folklore studies and its successors and I will disregard the pre-socialist era of *národopis*, which is also understood as having been dominated by nationalism and positivism (Grill 2015: 3–4; Uherek in Hann et al. 2007: 45; Scheffel & Kandert 1994: 16; Skalník 2005b: 12), as well as accusations related to ethnography traditions from across the CEE region in general (Čapo 2014: 52, 56; Hann et al. 2007; Kuper 1996: 192).

All these accusations represent specific instances of the intellectualist approach as their primary concern is with intellectual issues, that is with theories and concepts as well as knowledge resulting from their application. Besides, these accusations have one more common denominator. Apart from a few partial accounts, they do not elaborate on what Marxism-Leninism, nationalism or positivism in ethnography supposedly stood for. These accusations usually limit themselves to inferring intellectual content of ethnography from the general political situation, historical context or the leading figures in ethnography. This chapter attempts to fill the gap by offering a comprehensive picture of these three theoretical wholes which allegedly influenced Czechoslovak ethnography.

One important issue must be addressed from the outset. If we are to assess whether ethnographers thought as proper Marxist-Leninists, navel-gazing nationalists or incorrigible positivists, we sooner or later discover that there is no systematic and elaborated treatise such as *The Rules of Ethnographic Method* or *Rethinking Folkloristics* which would help us understand the three theoretical currents. This situation probably also

²⁴ We mostly encounter the label Marxism, although the official doctrine was Marxism-Leninism. Marxism, I suppose serves as a convenient shorthand for Marxism-Leninism (cf. Kornai 1992: 360–361).

accounts for the fact that the contemporary critics of ethnography have not offered an elaborated criticism of Marxism-Leninism, nationalism or positivism in ethnography yet. Since no comprehensive theoretical treatise in ethnography exists, the critics of ethnography find themselves in an uneasy position as they have not much on which to rest their critiques. There are no foundational theoretical treatises within socialist ethnography which could be pinpointed as important sources for later intellectual developments of ethnography.

The absence of comprehensive and systematic theoretical treatises naturally poses a challenge for this chapter, but it should not lead us to a conclusion that ethnography was devoid of theory. In fact, reading ethnography books and articles today, I find them replete with various statements which derive their explanatory power from some theoretical background, however implicit and submerged that background is. This chapter seeks to garner variously scattered bits and pieces of argumentation and its goal is to seam these parts together to reconstruct the three theoretical complexes in question. This means that the resulting picture might be imprecise to a certain degree. It will discount differences in formulations and favour similarities. These similarities will not be traced only between authors, but also across time.

It is crucial to add that even if this chapter frequently speaks of Marxism-Leninism, nationalism and positivism as of “complexes”, “theories”, “doctrines”, “worldviews”, “currents” or “wholes”, these terms are used only for the sake of exposition. It would be perhaps more precise to say that ethnographers repeatedly resorted to explanatory principles, which appeared in various combinations in their writings and which can be shown to have been genetically linked to one of these three complexes. Talking of theories, it is also important to stress that not all of that which was theoretical in ethnography can be exhausted by reference to Marxism-Leninism, nationalism and positivism. There were other important intellectual strands inspired by structuralism, sociology, theory of art, linguistics, aesthetics or even anthropology. However, these intellectual currents will not be considered here as they are not targeted by the contemporary discourse on the history of ethnography.

As regards the method, the chapter will give precedence to writings of ethnographers who were important in terms of power (cf. Bourdieu & Wacquant 1992: 243). Therefore, it will focus on Otakar Nahodil and his Marxist-Leninist circle in the 1950s and on Antonín Robek in the late socialist period, but writings of other Czechoslovak

ethnographers from Czech and Moravian subfields will be included as well as writings of several non-ethnographers whom ethnographers referred to.

The subsequent three sections will offer an intellectual detour through books and articles written by Czechoslovak ethnographers and an attempt to distil a comprehensive picture of the three intellectual wholes. It will begin in the late 1940s and progressively move toward the late socialist period. By means of describing intellectual attitudes of ethnographers, the section can be also read as a more comprehensive introduction to Czechoslovak ethnography.

2.2 The First Accusation: Marxism-Leninism

When it comes to Marxism-Leninism and its presence in ethnography, contemporary authors tend to adopt a straightforward approach. They view Czechoslovak ethnography between the late 1940s and the late 1980s as having wholly or almost wholly conformed to the ideology of Marxism-Leninism. It is not surprising since Marxism-Leninism, the official doctrine of the socialist Czechoslovakia, permeated all spheres of public life, sciences and humanities having been no exception.²⁵ In Petr Skalník's words: "The power monopoly of the Communist Party of Czechoslovakia imposed Marxism-Leninism and historical materialism as the only framework for scientific discourse" (Skalník 2018: 6). This had without any doubt consequences for the discourse of ethnography and folklore studies: "Marxism-Leninism made ethnography an auxiliary branch of knowledge the goal of which was to supply suitable data to the theory of Marxist historical materialism." (Skalník 2018: 4) (see also Skalník 2002b: 101).

Although they are less categorical than Skalník, David Z. Scheffel and Josef Kandert agree with Skalník on the point that Marxism-Leninism had "serious consequences for the discipline and its practitioners." (Scheffel & Kandert 1994: 15) and that ethnography had "always been a willing servant of ideology" (Scheffel & Kandert 1994: 16). In a more recent article, Kandert admits an exception and writes that two groups of ethnographers worked under the big brother of Marxist-Leninist ideology. Members of the first group refused to serve the official doctrine, but they were marginalized as a result and chose to

²⁵ This chapter does not address the pertinent question of the sincerity of the usages of Marxism-Leninism as it will be considered in the following chapter.

retreat to “the niche of pure positivism” (Kandert 2005: 238).²⁶ The other group willingly accepted Marxism-Leninism (Kandert 2005: 239). The idea that the state socialism directly influenced the discipline in an ideological way by the means of its chief ideology can also be found in articles authored by Petr Lozoviuk (Lozoviuk 2005: 230), Václav Hubinger (Hubinger 2015: 68) or Blanka Soukupová (Soukupová 2013: 19).

It is beyond doubt that Marxism-Leninism was established as an important feature of Czechoslovak socialist scholarship. The spectre of Marxism-Leninism began to haunt Czechoslovak *národopis* close of the 1940s as a group of young Marxist-Leninists ushered in a paradigmatic ethnographic revolution. This group which came to be known as the Marxist Circle intended to remodel the discipline of *národopis* in the Soviet fashion and conform it to the principles of Marxism-Leninism. It was led by Otakar Nahodil (1923–1995) assisted by his colleagues of whom the most notable were Jaroslav Kramařík (1923–1974) and Olga Skalníková (1922–2012).²⁷ As it is clear from their writings which appeared in the leading disciplinary journals of the time, the break which the young generation wanted to accomplish was intended to be total – not merely intellectual, but political as well, provided that we can even make a clear distinction between political and intellectual dimensions in the Marxist-Leninist worldview. Supported by the Communist Party which has just acquired unlimited powers in Czechoslovakian politics, these young radicals harshly dismissed the previous generation of the scholars of *národopis* represented by professors Karel Chotek (1881–1967) and Antonín Václavík (1891–1959), and to a lesser degree by docents Vilém Pražák (1889–1976) and Drahomíra Stránská (1899–1964). The older generation was denounced for their ideas conforming to the bourgeois tradition of *národopis*. They had no place in the new socialist order built on the principles of the invincible teaching of Marxism-Leninism.

²⁶ Although he does not refer to himself in his article, Josef Kandert probably capitalizes on his own experience during the late socialist period when he was academically marginalized and could not pursue a career as a lecturer or researcher and was employed in Náprstek Museum, which was a museum specializing on material culture of non-European societies (Brouček & Jeřábek 2007: 107).

²⁷ The Marxist Circle (*marxistický kroužek*) is not a recent label. Nahodil used it himself in the 1950s (Petráňová 2016: 85), but the accounts vary as to who was a member (cf. Nahodil 1995: 10–11; Robek 1972c: 32–33; Scheffel & Kandert 1994: 16; Skalník 2005a: 58; Skalníková 2005: 171–172). There are two reasons for the fuzziness concerning the membership. First, the Circle was very likely an informal group with no neat boundaries. Second, Nahodil and other scholars sought to rehabilitate themselves in the process of rehabilitations after 1989. In the process, any activities supportive of the Communist Party rule in Czechoslovakia (which included the activities in the Marxist-Leninist Circle) counted against the candidates (including Nahodil) who, as a consequence, were very likely motivated to deny or play down their erstwhile membership (cf. Nahodil 1995; Petráňová 2017: 5–6; Skalník 2002b: 103).

The first fruit of this paradigmatic revolution came with an intent to rename the discipline. Further on, the discipline was not to be referred to as *národopis* but as ethnography, though in one of his papers Otakar Nahodil stated that ethnography, a word of Greek origins, translated to Czech as *národopis* or *lidopis* (Nahodil 1951: 52). The idea of renaming came from the Soviet Union as the archetypical Soviet discipline was called ethnography (этнография). Apart from the name, Marxist-Leninist ethnographers introduced other means of the total break with the bourgeois past – specific theoretical and ethical postulates which I explore here one by one.

2.2.1 Subject Matter

Marxist-Leninist ethnographers also intended to refocus the discipline on a different subject matter. According to Kramařík, the bourgeois tradition of *národopis* disregarded the working classes, their modes of livelihood and culture (Kramařík 1955: 98) because it was too preoccupied with “the richest strata of the rural areas, focused on the veneer of their material culture, especially folk costumes excessive in colours, richly decorated interiors, and other phenomena which were typical only for the class of rural moneybags” (Kramařík 1954: 49). From then on, ethnography was supposed to focus much more on the “progressive traditions of our people in the past, contemporary [cultural] production of our working classes and cooperative societies in our village” (Kramařík 1953: 108). For Marxist-Leninist ethnography, it was the working classes which under the conditions of capitalism became the vanguard of progress and it was the working classes that had an important historical mission (Kramařík 1953: 108). From the 1950s onwards, the dominant topics in ethnography and folklore studies became the livelihood of the working classes both rural and urban, their history, custom and material culture. This orientation was soon to be epitomized in two grand works on the ethnography of the mining regions of Kladensko and Rosicko-Oslavansko (Fojtík & Sirovátka 1961; Skalníková et al. 1959).

2.2.2 Historicism

The central theoretical pillar of Marxism-Leninism which supported the whole intellectual edifice was the maxim of historicism (*historismus*). This encompassed four interconnected commitments. The first of these commitments, plainly speaking, meant that ethnography was considered a historical discipline. This position was neatly stated by Nahodil.

According to him, ethnography “studies objects, events and phenomena in their origins, development and death in the context of particular historical conditions.” (Nahodil 1951: 55).

The second component of historicism is also present in the quote by Nahodil. We can call it “proper historical contextualization”. In order to understand some object of ethnographic enquiry, let us say, a folk song, ethnographers had to contextualize it properly. They had to understand the historical period, the specific historical conditions in which the folk song had appeared and existed. As Kramařík wrote about folklore phenomena: “these facts are a part of life, ... fairy tales, legends and songs are something that is intimately related to the life, that they mirror the life of the people and that they give expression of certain ideas” (Kramařík 1953: 102). Anyway, the method of proper historical contextualization was not limited to folklore material. The method would work well regardless of whether ethnographers faced a folk song, a decorated cupboard or a religious custom.

2.2.3 Laws of History

There is nothing unique about the two components of historicism. It is likely that they present some general agreement among historians on what the task of history is. History studies the past and when it wants to understand events, individuals, institutions, object, ideas or else it should abstain from a presentist perspective and try to understand its objects of inquiry in their proper historical context (cf. Kutnar 1947: 1). However, for Marxist-Leninist ethnography, there was a third component of historicism which not all historians would share. This component was also present in Nahodil’s quote in which he spoke of the study of development. For Marxist-Leninist ethnography, it was not just any temporal development, but as Nahodil put it elsewhere, the concern of ethnography was “pinpointing certain laws [*zákonitosti*] in the development of human society” (Nahodil 1951: 54). Marxism-Leninism did not understand the development simply as a succession of unique events in time, but as a succession driven by regularities and laws which ethnographers could discern to have been at work in the historical process.

2.2.4 Base and Superstructure

As understood by ethnographers, the process of history manifested in a temporal sequence of the stages of socioeconomic development. These stages presented successive and specific socioeconomic formations: primitive communism, slave society, feudalism, capitalism and socialism. These formations among other things differed by the specific arrangements of the means of production and the relations of production in each of the formations (Nahodil 1951: 55, 1954a: 14–17). This is what ethnographers also referred to as the base which they opposed to the superstructure. The superstructure on the other hand consisted of political, legal, religious, artistic and philosophical ideas and the corresponding political, legal and other institutions. Each base was approached as having a corresponding superstructure (Nahodil & Kramařík 1951: 8).

Marxism-Leninism perceived a sort of causal connection between the base and superstructure, the former was understood as being formative to the latter. Here we also return to the requirement of the proper historical contextualization – to understand a phenomenon in its proper historical context was to present it as a product of a particular base. For this reason, historical contextualization overlapped with causal explanation. For to explain a phenomenon it was important to show its causal relatedness to a corresponding base. A good example of this method can be found in Nahodil's book on superstitions in which he contextualized religious and superstitious beliefs in terms of corresponding bases. For example, Christianity was viewed as a product of the social conditions of the slave society. It was viewed as a social fantasy which had originally emerged as a consequence of the powerlessness of the exploited slaves who had hoped for a better world. Later in the age of feudalism, it had become a tool for the exploitation of serfs by their feudal masters (Nahodil 1954b: 96–97).

2.2.5 Against Idealism

The proper contextualization was actually a process whereby ethnographers contextualized phenomena especially in relation to a specific socioeconomic base in which they had appeared. The specific explanation which followed causal relations between the base and superstructure was opposed to theories and approaches which advocated a reverse causality and therefore a reverse explanation. In other words, Marxism-Leninism was a species of a broad family of materialist theories and was opposed to various currents of idealism, which Marxist-Leninist ethnographers frequently assaulted in their writings.

When ethnographers spoke of idealism, they often invoked the name of the Czech conservative historian Josef Pekař (1870–1937). Sometimes they did not refer to him directly, but contemptuously spoke of the Pekař tradition (*pekařovská tradice*), or to coin an Anglicism – Pekařianism, (*pekařovština*). Accusations of following the Pekař tradition frequently appeared alongside accusations of idealism (Kramařík 1951a: 132, 1954: 49, 1955: 98; Nahodil 1951: 52; Nahodil & Kramařík 1951: 15; Skalníková 1951a: 1).

Ethnographers did not specify what the idealism represented by Pekař stood for and they did not offer a comprehensive summary of this intellectual orientation. For a better understanding of these accusations we may fill the gap by using a book written by the Czech historian Jan Pachta (1906–1977), which Nahodil and Kramařík approvingly quoted (Nahodil & Kramařík 1951: 15). In his book, Pachta opposed idealism to the conception advocated by Marx and Engels which extrapolated ideas from particular living conditions. Contrary to materialism, Pekař as an idealist scholar tried to track “a creative spiritual tendency which is the source of all being and which is the source of the religious, legal, economic and social development” (Pachta 1950: 47). Scholars could discern particular manifestations of this spiritual development in different eras. This is what justified their uses of periods such as the Gothic period, the Renaissance or the Baroque as these were understood by idealists as different manifestations of the developing spiritual tendency (Pachta 1950: 48).

Whereas Marxist-Leninists looked for different sociohistorical arrangements and the lawlike development thereof (the human history beginning with primitive communism and ending in socialism), idealists looked for particular eras defined by their moral, intellectual or cultural climate (*duch doby*)²⁸ and national individualities (*národní individualita*) and sought to establish psychical laws of the development of national histories (*psychické zákony vývoje dějin národních*) (Pachta 1950: 47). According to Pachta, idealism presupposes that besides the mundane world there exists a more substantial supramundane world of values and ideas, supposedly independent on socioeconomic conditions (Pachta 1950: 97).

While Marxist-Leninist ethnographers used the word idealism with denunciatory intentions and rarely bothered to specify it, it is also likely that they had some concrete content on mind when it came to the discipline of *národopis*. If we look for a possible

²⁸ I follow the definition of the German word *der Zeitgeist* (*duch doby* in Czech) provided by the Merriam Webster dictionary as there is no appropriate equivalent for the word in English <https://www.merriam-webster.com/dictionary/zeitgeist> (cf. Jung 2014).

manifestation of idealism in *národopis*, we can single out a conception of Antonín Václavík who in 1940 defined *národopis* as “a science of the people and its culture, its intellectual capacities and spiritual powers, which, in the end, more than social and economic conditions determine the lot of a larger whole which we call the nation.” (Václavík quoted from Válka et al. 2016: 22).

In the context of the materialist-idealist divide, this was obviously an idealist formulation. In it, Václavík did not give causal precedence to social and economic conditions as Marxist-Leninists did, but it rather gave causal powers to intellectual capacities and spiritual powers (*duchovní schopnosti*)²⁹, economic system and social relations being only derivative of these.

It is apt to note that Václavík figured as one of those subjected to a heavy criticism by Marxist-Leninist ethnographers in the 1950s, but in the texts, he was not explicitly related to idealism as were, for example, his colleagues Václav Davídek and Karel Chotek. Writings of the latter of the two evince idealist traits, however, Chotek’s work can better be described as eclectic (Chotek 1946: 10, 1949: 23–24).³⁰ Nevertheless, it was Václavík whom young ethnographers criticised for his “eclecticism which connected all bourgeois theories of particularly reactionary nature into a certain synthesis” (Nahodil & Kramařík 1951: 14). Nahodil and Kramařík did not specify the “bourgeois theories of particularly reactionary nature”, but it is likely that idealism was one of them.

The materialism-versus-idealism opposition manifested in ethnography on two other levels. In a specific conception of ethnography method and in a conception of human thought and action. When it came to the method, idealism was chided for its irrational intuitivism and the method of historical empathy. It was opposed to a more scientific approach which was supposed to start with tangible historical conditions of living and expected to end with the discovery of objective laws operating in history (Pachta 1950: 97). The other level of the opposition can be found in a discussion related to the actions of the primitive man in the works of Edward Burnett Tylor and Lucien Lévy-Bruhl. Did the primitive man in the fashion of a “primitive philosopher” first contemplated the world in order to act in it? Or was he, in the first place, practically oriented? Nahodil and Kramařík criticised both scholars for approaching the mind of the primitive man as “shrouded in an

²⁹ The word *spiritual* does not entail supernatural powers in this context, but rather capabilities which can be understood as mental, psychological or intellectual.

³⁰ In Chotek’s work we can find traces of evolutionism, ecological determinism, diffusionism, structuralism and materialism (Chotek 1949).

idealist, mystico-religious cloud”.³¹ Nahodil and Kramařík did not deny the importance of studying primitive thought, but appealing to L. H. Morgan and invoking the necessity to study thinking in its socio-economic context, they insisted that the primitive man was practically oriented, something which we know from the primitive man’s “inventions and discoveries”. They especially chided the French scholar for having created an unwarranted qualitative divide between us and them, between the rational action of the modern man and the irrational action of the primitive man (Nahodil & Kramařík 1951: 12–14).³²

At the same time, it is important to add that there existed an important concession to the otherwise strictly materialist view. Ethnographers opposed the so-called vulgar materialism and espoused a view that ideas were not just a mechanical product of the base and that even ideas could play an active role in historical process. Nahodil and Kramařík credited Marx and Engels with the view that once the masses acquired some idea, the idea became a material force (and hence a causal power) on its own (Nahodil & Kramařík 1951: 8). Yet even the ideas acting as material forces should have been in the first place properly contextualized and related to material conditions of the appertaining base.

2.2.6 Historical Teleology

After a digression to the idealism-materialism dispute we finally return to the Marxist-Leninist historicism to present its fourth and last component. The laws which operated in history were not viewed as some blind and mindless laws that drove the purposeless flow of history. They were viewed as laws that drove the history towards its inevitable and happy conclusion in its end time, towards the ultimate victory of communism. For ethnographers, this was an integral part of the Marxist-Leninist historical teleology (Nahodil & Kramařík 1951: 16). According to Marxism-Leninism, history had its own agenda to follow.

This was the idea of progress which Marxism-Leninism shared with evolutionism of the nineteenth and early twentieth centuries. It should not surprise us that Marxist-

³¹ It is pertinent to add that although Nahodil and Kramařík criticized Tylor and Lévy-Bruhl, they also presented their works in a positive light. Tylor was praised for his theory of animism and Lévy-Bruhl for his focus on the so-called primitive thinking (Nahodil & Kramařík 1951: 12).

³² Nahodil was not always consistent with his Marxist-Leninist gnomes. If we read his booklet on superstitions, we cannot fail to note that as regards the so-called primitives, Nahodil described them in Frazerian terms as primitive philosophers, not forgetting to mention their powerlessness and fear (Nahodil 1954b: 26, 33). Nahodil did not relate this view to the supposed socioeconomic base of societies in the stage of primitive communism. Moreover, Nahodil even used Frazer’s terms sympathetic magic (*čáry soutrpné*), contagious magic (*čáry styčné*) and imitative magic (*čáry napodobivé*) without crediting or mentioning Frazer (Nahodil 1954b: 26–27).

Leninist ethnography saw in evolutionism a very close yet somehow imperfect relative (Nahodil 1954a: 17). Nahodil himself admired “the progressive scholar” Lewis Henry Morgan (Nahodil 1951: 53) and wrote a laudatory preface for the Czech translation of *The Ancient Society* published in 1954 (Nahodil 1954a). Nahodil repeated the stance known from Marx, Engels and other scholars who had considered Morgan an important precursor of materialism in historical science (Nahodil 1954a: 17). A few years earlier in 1948, Friedrich Engels’s *The Origin of Family, Private Property and State* was republished³³ and it credited Morgan with a rediscovery of materialism (Engels 1950: 5).

2.2.7 Partisanship and Value Partiality

Any science, ethnography not exempting, was expected to contribute to the building of the socialist society and its wellbeing (Kramařík 1953: 108; Nahodil 1951: 52; Nahodil & Kramařík 1951: 17). As Nahodil and Kramařík wrote, ethnography with its findings “must not only be a science of the people, but a science which serves the people” (Nahodil & Kramařík 1951: 16). For this reason, ethnographers rejected the notion of value neutrality and promoted the principle of scientific partisanship (*stranickost vědy*) or value partiality (Nahodil 1951: 57). Value neutrality was rejected as a normative requirement of scientific practice since ethnographers had a moral duty to contribute to the building of the socialist society (cf. Hubinger 2015: 68). Moreover, value neutrality was also rejected on a descriptive basis, because every research was regarded as driven by the researchers’ political bias even if the researchers were unaware of the fact. And if any scientists claimed value neutrality to have been the absolute principle of their work, in reality, they only contributed to maintain the *status quo* which suited the ruling classes (Robek 1964: 7).³⁴

While we can find the principle of partisanship mentioned frequently in their writings, it is actually less clear how ethnographers followed this maxim in practice as it could be argued that any ethnographic writing could in principle contribute to the wellbeing

³³ It was originally published in 1920 in Czech translation. After its republication in 1948 it continued to be published in enormous print runs as we can find copies from 1949 and 1950.

³⁴ This is the exact opposite of what D. H. Price shows was the case in US anthropology in the late 1940s and throughout the 1950s. The situation in the United States was distorted by politics in a way that almost precluded the practice of any form of engaged anthropology, such as fighting for social justice and racial equality. Many progressive and liberal leaning anthropologists were oppressed for their political views and actions (Price 2004). On the contrary, beginning with the 1950s Czechoslovak ethnographers were by default expected to serve state interests in building a better society. Value neutrality in science was considered a sign of reactionary views. Very likely for these reasons, ethnographers often referred to various resolutions of Czechoslovak Communist Party conventions in their scholarly writings.

of society. As we shall see later, some of the research topics emerged as a consequence of the state interest, but the question of how particular findings contributed to the wellbeing remains. One of the possible manifestations of partisanship was the harsh criticism to which ethnographers subjected non-Marxist-Leninist theories such as structural-functionalism, diffusionism or *die Kulturkreislehre*. Ethnographers did not employ any genuine intellectual critique in their diatribes directed against the non-Marxist-Leninist theories and rather focused on their political dimensions. So, the synchronic method of structural-functionalism was not criticised for the demerits of synchronic methods, but for its political implications, for its bourgeois and reactionary nature (Nahodil 1951: 52), for its having been a servant of imperialism and colonialism (Nahodil & Kramařík 1951: 14–15, 17) and its having been a tool of war agitators (Nahodil & Kramařík 1951: 15).³⁵ Diffusionism (migration theory) was also accused of being ahistorical to which was added an accusation of cosmopolitanism (Nahodil & Kramařík 1951: 14). *Die Kulturkreislehre* was criticised of presenting a too static view on cultures which supported the bourgeois view on the private property, state, classes or exploitation (Nahodil & Kramařík 1951: 14). In some of the cases, the link between theoretical and political dimensions of theories subjected to critique appears to be evident, but in most cases, we can only guess at the links for ethnographers did not always make them clear. In this case, a better elaboration on the links between politics and theory of Western scholars can be found in a collection of essays written by Soviet ethnographers under the title *Anglo-American Ethnography in the Service of Imperialism* which was translated by Nahodil and his colleagues and published in 1953 (Potěchin 1953), but again, theoretical considerations espoused by its Soviet contributors give way to political ones.

The above examples of partisanship refer rather to the writings of ethnographers and not to their direct personal engagement as in, for example, applied, advocacy or engaged anthropologies. I was able to glean only several examples of what apart from writing possibly constituted engaged attitudes or activities. The example which is especially important to the discussion of Marxism-Leninism is the promotion of scientific atheism and the struggle against religious and nonreligious superstitions.³⁶ If there was anything that

³⁵ As Skalník observed (Skalník 2005a: 68), these accusations became commonplace in anthropology only some twenty years later (see Asad 1973), needless to add that these accusations in anthropology were developed into coherent and persuasive accounts which are far better elaborated than the accounts written by Marxist-Leninist ethnographers.

³⁶ I am only aware of two further examples of ethnographers' engaged activities. According to the Czech ethnographer Antonín Jiráček, one ethnographer (whose name Jiráček did not disclose) had tried to persuade

ethnographers could do to complete the socialist cultural revolution at least at home, they could become involved with the promotion of scientific atheism and help to set the people free from “mental chimeras which restrain the people’s powers and capacities.” (Nahodil & Robek 1959: 6). As the socialist society at least nominally adhered to the freedom of religion, it had to find other means of uprooting immoral, obscure and superstitious beliefs than by closing the churches or mocking the believers (Nahodil 1954b: 107–108).³⁷ The proselytization of scientific atheism was achieved by publishing semi-popularizing books, pamphlets and articles (Nahodil 1954b, 1956, 1961; Nahodil et al. 1960; Nahodil & Robek 1959, 1960a, 1960b, 1960c, 1961) or by organizing lectures for the lay public. These lectures were intended as a way of convincing the people that it was in their paramount interest to abandon their unreasonable superstitions (Nahodil 1961: 5).³⁸ In the eyes of ethnography, superstitions did not conform to the objective truth (Nahodil 1954b: 9) and as a result had malign consequences for agriculture (Nahodil & Robek 1959: 161), public health or medical treatment (Nahodil 1961: 3; Nahodil & Robek 1959: 183). If the Party wanted to build a better society, ethnographers could contribute at least by convincing people to cast their superstitions off.

2.2.8 Troubles with Survivals

Let us now return to the previously mentioned evolutionism and focus on one last important ingredient of ethnography. We have already seen that the young generation of ethnographers held Lewis Henry Morgan and his work in high esteem. This was not only for Morgan’s pioneering materialist thinking or for his idea of progress, but also for the evolutionist notion of survivals which can be found in Morgan’s work. When it came to survivals, ethnographers did not credit any concrete scholar in particular be it Maine, Tylor

villagers of the benefits and necessity of the agricultural collectivization in the 1950s (Jiráček 1991: 295). The second example concerns Antonín Robek who produced several classified documents in the 1980s. These documents were related to the Czechoslovak emigration (Olšáková 2016: 142). One of my interlocutors mentioned that Robek had a vision of suppressing political aspirations of the Czechoslovaks living in emigration by promoting Czech folklore among them (P0035: 10–11). Nevertheless, as regards any of the two topics, I have no more information.

³⁷ This statement concerns only what was written and does in no way deny the reality of the religious persecution under Socialism.

³⁸ Nahodil in one of his articles mentions some one hundred and thirty public lectures which he supposedly gave across Czechoslovakia. The lectures were about religion and atheism and Nahodil in his article writes about pitfalls of attempting to convince people with ties to religiosity to give their beliefs up (Nahodil 1961: 5). Moreover, his *The Origins of Religion* was originally a series of lectures designed to instruct public enlightenment campaigners (*osvětoví pracovníci*) (Nahodil 1956: 2). The struggle against religious and superstitious beliefs was probably not only a matter of writing but of an active campaigning too.

or Morgan, but it is likely that they had especially Morgan's contribution in mind when speaking of survivals.³⁹ Sometimes ethnographers explicitly mentioned the doctrine of survivals, sometimes they did not mention it explicitly but provided concrete cases. One of them was Morgan's discovery of a divergence between kinship terminology and actual kinship relations. According to Morgan, kinship terminologies are fitted to describe an earlier evolutionary state of kinship relations and do not fit the present state thereof (Nahodil 1954a: 13). While George W. Stocking later saw in the rejection of the doctrine of survivals a precondition for the emergence of anthropological functionalism (Stocking 1995: 320), the first modern anthropological school, for Czechoslovak ethnographers, the same doctrine continued to play an important explanatory role. I will demonstrate the doctrine of survivals on the problem of superstitions.

Czechoslovak ethnographers faced a serious problem. How to explain the persistence of superstitious and religious beliefs in a society? We know that ethnographers should in the first place properly contextualize the superstitions which they encountered in the field. This is what Nahodil indeed did when he showed how patches of superstitious beliefs fitted seamlessly into the socioeconomic fabric of the base. I have already mentioned his examples of the slaves for whom Christianity had been a flight of fantasy from the oppressive bonds of ancient slave societies (Nahodil 1954b: 93–96), and later on, Christianity had fitted the arrangements of the feudal society where it had underpinned the rule of the feudal lords (Nahodil 1954b: 96–97). So did superstitions and religions help to maintain the order imposed by the ruling bourgeois classes all around the world or by the colonial overlords among their subjects (Nahodil 1954b: 95–97, 100–101, 103, 105).

When Nahodil got to explain the persistence of superstitions in the socialist society, he seemed to follow the same procedure. The socialist society was for various reasons understood as not particularly conducive to superstitious beliefs. First, compared to its predecessors it allowed its citizens to live in an unprecedented affluence. Second, it was underpinned by scientific atheism which demonstrated that superstitions were clearly false. Last but not least, it was a classless society and there was no question of religion serving as a useful means of oppression for the ruling classes. With a pinch irony we could add that even if such ruling classes had existed, their aim would not have been to dominate anyone. Hence Nahodil noted in an optimistic vein that the numbers of nonreligious individuals

³⁹ A very detailed account of the doctrine of survivals and its changes throughout the nineteenth and twentieth centuries can be found Stocking's works on the history of anthropology (Stocking 1987, 1995).

were increasing and that a nonreligious epoch was about to come in the future (Nahodil 1954b: 20–21). At the same time, however, Nahodil admitted that superstitions persisted. But how could he explain their persistence? Here we only have to remind ourselves that changes in the superstructure do not immediately reflect the changes in the base (cf. Engels 1950: 32, 124–125; Kramařík 1953: 104; Nahodil & Kramařík 1951: 8). It was at this point when the notion of survivals came into play.

Survivals were approached as elements which become disconnected from the base at some point of time and begin to live a life on their own. This also entails that survivals cannot be properly contextualized. As Kramařík wrote of understanding customs: “Customs are not isolated phenomena; their existence is always connected to particular economic and social relations and to certain ideology. It is important to know, if some custom is representative of some particular period or whether it is a mere survival” (Kramařík 1953: 107). Here Kramařík seems to say⁴⁰ that ethnographers during their fieldwork usually encounter two kinds of elements. They either face elements which can be accounted for by a proper historical contextualization or they encounter elements which resist any such contextualization. When the second option is the case, an ethnographer knows that she is facing a free-floating survival, that is, an element unconnected from its original base – a base which is no longer existent. Thus, survivals defy any contextualisation other than a mere spatiotemporal localization. This scenario perfectly explains the persistence of superstitions as well as religious beliefs in the socialist society. Surviving superstitions could not be explained by the method of proper historic contextualization as their existence did not make any sense in the context of socialist society. Instead, superstitions were ascribed the status of survivals hopefully destined to extinct.

Unfortunately, this scenario also provokes questions which ethnographers did not address or did not dare to address. Why do survivals exist at all? What is the concrete mechanism which allows some elements to survive outside of their original socioeconomic context? A possible answer to our fault-finding questioning was provided by Nahodil. When he was trying to give an answer to why superstitious beliefs, such as the avoidance of number thirteen, survived even in his day his answer was rather simple:

⁴⁰ Kramařík’s statement is equivocal as it can be understood in two ways. First, customs (or any other objects of ethnographic enquiry) are always connected to particular economic and social relations and then are *either* representative of that relations *or* mere survivals. This reading is underpinned by the wording of Kramařík’s sentences. However, then it would not do much sense to speak about survivals whatsoever. A second reading views customs as either connected to particular economic and social relations or as being mere survivals. This reading is underpinned by the way that ethnographers generally wrote on survivals (see below).

Well, it is a superstition that people gain in their childhood from their parents and environs. It becomes an inseparable part of their thinking, their custom and their life. People do not usually think about superstitions. They are driven by superstitions and do not ask the question of whether their superstitious belief is valid or not. (Nahodil 1954b: 5–6).

This answer apparently creates more problems than it solves. Not only does it seem to imply that there exist survivals in the form of superstitious beliefs, but also survivals in the form of socioeconomic institutions. For from Nahodil's answer it is clear that it is not only a survival which does not change in the process of reproduction. It seems that it is also the family mechanism, which reproduces the survival and which does not change. The inquiry could go further on and we could ask under what changing conditions is the unchanging family mechanism reproduced. If the base changes, why not the family life or at least some of its features? What else does not change in the history and why? Unfortunately, these questions were not even raised when it came to the status of survivals.

Instead of sticking to the doctrine of survivals, ethnographers could of course follow a different line of inquiry. They could wholly discount the doctrine of survivals and take the sole path of the proper historic contextualization and attempt to explain superstitions in the terms of the base of the socialist society. However, this method contained a seed of trouble which could eventually cast socialism, or rather, the audacious ethnographer, in a bad light. If we follow this path which the Marxist-Leninist ethnographers did not dare to take, it could lead us to a conclusion that religious beliefs under socialism existed as a fantasy of the dominated classes – classes, whose existence was candidly denied – and that the role of this fantasy was akin to the role of Christianity among the slaves in the Roman Empire (Nahodil 1954b: 93).

Here we perceive a possible tension within the theory of Marxist-Leninist ethnography. The doctrine of survivals seems to be at odds with the requirement of proper historic contextualization, but nowhere in their writings did ethnographers dare to delimit their respective reach and the two somehow coexisted side by side. This leaves us rather with a banal conclusion that some things change and some do not. It seems that the method of proper historic contextualization was the main explanatory device and the doctrine of survivals served as a useful *ad hoc* explanation. Problems stemming from this tension went unperceived in ethnographers' writings and did not give rise to any controversy between

“Contextualists” and “Survivalists”, a problem which this thesis addresses only later. And as we shall see in the course of this chapter, the problem of survivals was not the only weak point of Marxist-Leninist ethnography.

2.2.9 Marxism-Leninism in the 1970s and 1980s

As Nahodil asserted in his programmatic article from 1951 titled *For the New Conception of the Science of Ethnography*, Marxism-Leninism was introduced with a strong backing from the Communist Party of Czechoslovakia (Nahodil 1951: 52). The postulates of Marxism-Leninism began to dominate ethnography no later than in the early 1950s and remained ethnography’s perennials for the upcoming four decades of the undivided rule of the Communist Party of Czechoslovakia. The status of Marxist-Leninist principles in ethnography was upheld in the beginning of the 1970s after the Communist Party consolidated its power after a period of political and social liberalization in the 1960s. In 1972, Antonín Robek, who at the time assumed his dominant position as head of the two most important ethnography institutions, orchestrated a monothematic and programmatic issue of *Český lid* (Hynková 1972; Klímová-Rychnová 1972; Kramařík 1972; Robek 1972a, 1972b) and wrote two more articles for *Acta Universitatis Carolinae* in a similar spirit (Robek 1972c, 1972d). These articles blazoned abroad Robek’s unshakeable position in ethnography and, taken together, they can be likened to the *Lessons of the Crisis (Poučení z krizového vývoje)*, an official political manifesto of the Communist Party of Czechoslovakia from 1970 which dismissed the liberal course of the 1960s and set a tighter and a more ideologically principled course.

The monothematic issue of *Český lid* upheld the very same principles of Marxist-Leninist ethnography which had been stressed in the texts from the 1950s. Ethnographers emphasized the importance of value partiality and partisanship (Hynková 1972; Klímová-Rychnová 1972: 213; Robek 1972b, 1972a), the importance of proper historical contextualization (Hynková 1972: 196) and these two appeared in Kramařík’s article alongside others previously mentioned: ethnography as a historical science, the proper historical contextualization, the focus on historical development and on the uncovering of the laws in history, historical teleology, the focus on the working classes, the revolutionary

approach of evolutionism, the definition of survivals, the distinction between the base and superstructure and the critique of idealism (Kramařík 1972).⁴¹

The same principles continued to appear in ethnographers' writings throughout the 1970s and 1980s. It would be appropriate to demonstrate here the actual uses of Marxism-Leninism on ethnography texts from the 1970s and 1980s. This will help us to see that Marxism-Leninism was not just a set of empty proclamations, but a set of postulates that were actively employed in ethnographic explanations. For the sake of this task, we can pick a monograph written by Antonín Robek in 1964, at times when he did not hold any managerial post in ethnography. The publication's original title was *An Outline of the History of Czech and Slovak Ethnography* (Robek 1964). The book was republished with some omissions later in 1976 under the title *The History of Czech Ethnography I* (Robek 1976). At the time of its republication, Robek was an undisputed head of Prague ethnography and one of the major players and power holders in the field of Czechoslovak ethnography and folklore studies. Robek's book is remarkable for its strict adherence to Marxism-Leninism and can be used as a perfect example of the influence which Marxism-Leninism exerted over ethnography since most of the major ingredients of Marxism-Leninism are represented, although the links between Robek's Marxist-Leninist postulates and his empirical material are rather weak and although he does not usually reveal the source of his empirical assertions.

The aim of the book was to provide an account of the historical development of Czech ethnography⁴² beginning with the era of the Czech National Revival of the end of the eighteenth century and ending with the 1950s. Besides its historical scope, there were other ways in which Marxism-Leninism manifested throughout the book. First, Robek linked the origins of ethnography to the decline of feudalism and the emergence of capitalism: "Ethnography as a social science emerged in the era of developing capitalism which grew in the womb of the old feudal society." (Robek 1964: 8, 1976: 9). In order to understand the role of ethnography, "it is important to study its history as a reflection of contemporary social processes" (Robek 1964: 7, 1976: 8). This was a perfect example of

⁴¹ All of these features were mentioned in Kramařík's article though with some minor differences from his articles from the 1950s and with some new ideas. For that time, he seemed to prefer the term culture when he spoke of superstructure (Kramařík 1972: 200), or he mentioned recent works of Soviet ethnographers who also considered ecological explanations or intercultural influence, which makes the impression the diffusionism in disguise (Kramařík 1972: 201) and lastly, Stalin did not appear alongside the remaining three classics of Marxism-Leninism.

⁴² To repeat, Robek used the term ethnography retrospectively to cover scholars who had not called themselves ethnographers.

the requirement of proper historical contextualization in which a phenomenon was explained by a reference to the corresponding base.

Robek was not concerned with providing his readers with an intellectual history of ethnography. He restricted any intellectual considerations to encyclopaedical descriptions of trends, schools and currents including names of representative scholars. His readers could learn about the early Czech revivalists, about the conservative Catholic ethnographers or about the Athenaeum school, but they could learn a little about their intellectual achievements. Instead of that, Robek showed how various trends and schools had emerged as a result of the changing socio-economic conditions and commented on the role which ethnographers and various schools had played in the developing class struggle. As he stated it in the beginning, it had been ethnography which purposefully or inadvertently served to various group and their class interests. Robek's book was mainly and self-avowedly designed as a book about the politics in the history of Czech and Slovak ethnography (Robek 1964: 7, 170, 1976: 7–8).

According to Robek, ethnography in its beginnings had contributed to the social emancipation from the old feudal order. At the time, ethnography had been an activity cultivated by the bourgeois strata and had played an important and progressive role. For example, the idea of Pan-Slavism championed by ethnographers in their writings, had after all been a political weapon which had served the emerging bourgeoisie in its struggle against the nobility (Robek 1964: 9, 12, 28–29, 1976: 10, 15, 36–38). Robek also linked the emergence of the so-called exotic ethnography to the aims pursued by the Czech bourgeoisie. Czech ethnographers specializing in non-European societies had linked the fates of peoples toiling under colonial systems to the fate of their own nation under the Habsburg yoke of late feudalism (Robek 1964: 33, 1976: 42). It was for their progressive role in their struggle against the feudal order that Robek praised the bourgeois currents within ethnography.

Nevertheless, with the steady development of capitalism in the nineteenth century, there had also emerged reactionary trends within ethnography. As Robek put it, the growing capitalism had given a rise to new contradictions in society and ethnography had become a useful tool for disguising these contradictions by forging nationalist sentiments (Robek 1964: 63–64, 1976: 75–76). The major change had come by the 1890s when Czech capitalism had been fully developed and when the working class in the Czech lands had fully emerged. From this point onwards, the development of ethnography had been less a story of the progressive bourgeois ethnographers fighting against the backward feudal order

and more about a struggle between ethnographers who had represented the reactionary bourgeois order and those who had represented the progressive socialist trends (Robek 1964: 77–78, 1976: 91–93).⁴³

Robek's Marxism-Leninism has a comparative dimension as Robek continuously compared Czech ethnography to Slovak ethnography. He repeated that the Czech ethnography had been ahead of the Slovak one all the time. Robek explained the deficiency of the Slovak ethnography by the fact that the Slovak society had continued to be rooted in the stage of agrarian feudalism in which the progressive bourgeois and later socialist trends (with their corresponding developments in ethnography) had been unable to emerge whereas the Czech ethnography had developed as a consequence of the rapid industrialization of the Czech society (Robek 1964: 38, 58, 76).⁴⁴

Robek's book almost unconditionally confirmed the Marxist-Leninist nature of Czechoslovak ethnography. Robek's history of the Czech and Slovak ethnographies and of the Czech and Slovak societies presented a backbone to a host of divergent studies by his colleagues who provided the backbone with additional flesh. We can adduce Bohuslav Šalanda's studies focusing on folklore of various strata in the late eighteenth and early nineteenth centuries (Šalanda 1980a, 1980b, 1984, 1986), studies by Ludmila Sochorová who focused on the role that the popular theatre had played in the social emancipation of rural areas (Sochorová 1981, 1983, 1987a, 1987b) or in an article by Oldřich Kašpar on the image of slaves in Americas in the nineteenth century Czech press (Kašpar 1982). And there were other pertinent issues related to the history of the social emancipation from feudal fetters. To what extent had the emancipation been a project of the bourgeoisie and intelligentsia or to what extent had the emancipation had strong plebeian origins? Robek provided an affirmative answer to both possibilities. In his book on the history of ethnography, he stressed the role of the middle classes as it had been especially the middle classes from which the precursors of modern ethnography had recruited. In other books of his, Robek stressed the popular roots of the social emancipation (Robek 1974a, 1977). Speaking of the popular roots of the Czech National Revival, Robek's position was echoed in the work of the Section of the Ethnography of the Working Classes (*oddělení etnografie dělnictva*) of the Ethnography Institute, which was active in the late socialist period and

⁴³ Ethnographers writing on the same era, usually mentioned that the 1890s presented a fault line in the development of Czech society. We can find it in earlier ethnography writings (Skalníková 1955: 106, 110) as well as in writings published later (Brouček 1979).

⁴⁴ Seven chapters which outlined the history of Slovakian ethnography as well as the chapter concerning the development of ethnography after 1945 were omitted in the 1976 version.

which researched on the livelihood of the working classes. Among a host of studies, the members of the Section collaborated on a large research project about the working classes in Prague in the nineteenth and twentieth centuries (Robek et al. 1981).

By and large, the contemporary commentators who link Czechoslovak ethnography to Marxism-Leninism seem to be right even though they do not usually elaborate on various features peculiar to the Marxist-Leninist framework. Even if this chapter dealt only with a fraction of Marxist-Leninist inspired writings, a reader versed in ethnography production will sooner or later find out that other writings mostly repeat and differently stress the same themes which have been described in this section.

2.3 The Second Accusation: Nationalism

As a matter of fact, accusations of ethnography for its nationalism are far less frequent among the contemporary Czech and Slovak scholars than the accusations of Marxism-Leninism. The accusation of nationalism appears in Skalník's harsh criticism of the late socialist period which he calls a twenty years of hegemony of "Robek, a paranoid nationalist communist" (Skalník 2005b: 14), whom he describes elsewhere as "a staunch communist and isolationist nationalist" (Skalník 2005a: 74). Nevertheless, far more frequent are various diagnoses of nationalism voiced by foreign scholars. Such observations can be found in the work of Chris Hann (Hann et al. 2007: 9), Katherine Verdery (Verderey in Hann et al. 2007: 49) or in the already mentioned observations by Jasná Čapo and Adam Kuper. Since the word nationalism can obtain various meanings ranging from political to strictly epistemological, readers might require me to make it clear what is meant by nationalism. Here, however, I limit myself to saying that contrary to Marxism-Leninism whose theoretical ingredients were more explicit, nationalism presented a more submerged and less visible framework which exerted its influence over ethnography. It presented a mixture of political, epistemological and moral stances and the best way of presenting these will be by going through ethnography writings in the same way as in the previous treatment of Marxism-Leninism. We can distinguish two nationalist strands in the late socialist Czechoslovak ethnography which overlapped no later than in the 1980s. The first of these strands predates Czechoslovak ethnography, reaches back at least to the first half of the twentieth century and its origins are inextricably linked to the era of the Czech National Revival. The second is of more recent origins, it is related to Soviet ethnography and its interest in ethnic groups.

2.3.1 Ethnography of the Czech National Revival

The first strand was represented by a specific research orientation within the late socialist ethnography of the 1970s and 1980s. It was the ethnography of the Czech National Revival. The term Czech National Revival referred and still refers to an era dated to begin in the late eighteenth century and continuing throughout the nineteenth century. It was, among other things, a national emancipation movement and like similar movements in the nineteenth-century Europe it aimed to achieve political, cultural and linguistic emancipation and independence on the European empires of the time (Hroch 2009: 149–150). The Ethnography Institute had a section focused to study the period (Thořová et al. 2005: 65) and the orientation was supported by the state plans set up for Czechoslovak ethnography in the 1970s and 1980s (Olřáková 2016: 146–147). The ethnography research on the Czech National Revival included two interconnected topics: the formation of the modern Czech nation and the history of ethnography for the pioneers of ethnography in the eighteenth and nineteenth centuries had also been scholars who had actively participated in the Revival. We have already found both these topics interconnected in Robek's book on the history of ethnography. For this reason, we will return to the book and see what else it offers apart from a Marxist-Leninist understanding of the history of ethnography.

Subjecting the book to a Marxist-Leninist reading, it seems that its central theme is derived from the historical development between the late eighteenth and mid twentieth centuries. Tensions within the late feudal society gave rise to capitalism which later toppled the old feudal order (Robek 1964: 8–9, 1976: 9–11). As capitalism burgeoned and grew stronger, new class tensions appeared and progressively intensified. These tensions then led in the direction of establishing a socialist society. Robek offers a view which understands the dynamics of the socioeconomic development as having been driven by struggles between the ruling and dominated classes – first between the feudal lords and the serfs and subsequently between the bourgeoisie and the working classes; and this struggle necessarily influenced the development of ethnography. Socialism was considered to be the final stage of the socioeconomic development and so the ethnography practiced under socialism was considered as entering its final stage. It was only under socialism freed from the contradictions of class-based societies when ethnography was able to yield objective knowledge and serve the real needs of the society at large (Robek 1964: 160).

The emergence of the Czech nation alongside its nationalism in the era of the National Revival are described as a project of the bourgeoisie in the book. This conforms to the Marxist-Leninist point of view according to which nationalism (similarly to religious superstitions under capitalism) served as a useful tool for political ends of the bourgeoisie as its purpose was to obscure class conflict (Robek 1964: 63–64, 1976: 75–76). Robek stresses that the original tensions within the Habsburg monarchy were class tensions. Only subsequently and as a result of political manipulations, they began to be understood as a product of national struggles between the Czechs and their rivals, the Germans and Hungarians (Robek 1964: 25, 1976: 32). To underscore this view whereby Robek distances himself from the national stakes of the nineteenth century, it is suitable to mention that Robek also reprehends older generations of ethnographers for their romanticizing vision of the Czech nation and for their indifference to conflicts within national groups, including the Czechs (Robek 1964: 24–25, 1976: 31). So, in the midst of the unsparring struggle between the classes, ethnography was created as a useful tool for the pursuit of the victory in politics: “The bourgeoisie needed to study its own people, its own nation and win its political favour... and so appears the ethnography of the nation. It is entrusted with the task to affect the self-confidence of the nation and the growth of its national consciousness.” (Robek 1964: 9, 1976: 11). The conformity of the bourgeois ethnography to the national worldview appears even stronger in another formulation:

It was important to remind the nation that there had been times in history when the Czech state stood at the helm of the spiritual and political life in Europe... It was, however, important to show the nation its contemporary virtues, what its contemporary role in the history of the world [světodějnost] was. It was important to demonstrate that the contemporary Czech nation was equal or even superior to other nations. (Robek 1964: 21, 1976: 26–27)

So far, Robek’s position does not appear to diverge from Marxism-Leninism. However, this is not the only story that we can read in Robek’s book. Once in a while a different and obviously a more important story emerges from its narrative. The book sometimes seems to say that the modern history stages a more significant struggle than the class struggle resulting in social emancipation. It is the struggle of the Czech nation for a national emancipation. In some formulations, the class struggle recedes to the background or is even harnessed to the ends of the national struggle. Some warrant for a nationalist

reading of Robek's book is already contained in the last two quotations. However, there is certain ambiguity and most of it stems from Robek's choice of words. In many formulations of his, Robek employs a narrative device known as the historical present which Czech language achieves either by using present tense when speaking of the past or by mixing present with past tense, which is not considered to be a grammatical error. In Robek's book we encounter both ways. For example, one of the sentences quoted above reads: "It *was*, however, important to show the nation its contemporary virtues, what its current role in the history *is*." By using the historical present, Robek catapults his audience in the midst of the story, but at the same time, this blurs the difference between his own point of view of the second half of the twentieth century and that of the ethnographers and revivalists of the nineteenth century. What adds to this ambiguity is the fact that nowhere in the book does Robek make it clear what his specific vision of the nation is and nowhere do we find it explicitly stated in what sense Robek's own understanding of the nation differs from that of the revivalists. If Robek spoke only "as if from the point of view of the revivalists", then it would be likely that Robek credited the revivalists with the conviction that the nations are entities endowed with existence and at the same time it would mean that Robek understands the nation simply as something what we could call a discursive weapon in the late feudal and early capitalist class struggles (but the revivalists could not or did not want to see it). This line of interpretation, in spite of ambiguous formulations, would still portray Robek as a Marxist-Leninist.

Nevertheless, there are some passages which sideline textual ambiguities and highlight the nationalist thread interweaved through the narrative. In these passages, Robek asserts that the nation can be more or less self-confident, the national consciousness of the Czechs can be ill- or well-developed and that the Czech nation is an entity which exists independently on any struggle of the bourgeoisie. This becomes evident when Robek states that specifically class ends pursued by the young Czech bourgeoisie were overlaid by the national ends pursued in a struggle of the dominated Czechs against the ruling Germans. The struggle resulted from "the specific status of *our nation* in the Austrian monarchy" (Robek 1964: 20, 1976: 26, emphasis mine). Here Robek provides an explanation of a historical situation that is based on an estimation of the socio-politico-economic situation of the Czech nation within Habsburg Monarchy in the nineteenth century. The whole passage says nothing of a false national consciousness resulting from political manipulations of the bourgeoisie.

Moreover, Robek's book is full of praise for the revivalists of the nineteenth century regardless of their mostly bourgeois origins.⁴⁵ Even today, the revivalists are understood as prominent Czech patriots who significantly contributed to the Czech National Revival, to Czech culture and, last but not least, to the creation of ethnography: Karel Jaromír Erben (1811–1870), a Czech poet and folklorist, Božena Němcová (1820–1862) an early Czech feminist writer and also a folklorist, or Karel Havlíček Borovský (1821–1856), a Czech journalist, poet and national martyr. Of Božena Němcová's writings, Robek writes in almost an eulogising tone: "They lead the people from becoming conscious of their nationality to active political participation, from the nation in itself to the nation for itself." (Robek 1964: 43, 1976: 53). Anyway, the list is by no way complete and many more figures could be added to it.

The nationalist framework in ethnography is even more conspicuous in a book by Stanislav Brouček, Robek's colleague and another specialist on the topic of the Czech National Revival. The main topic of Brouček's book is the Czechoslavic Ethnography Exhibition of 1895. The exhibition became a milestone in the development of Czech ethnography and Czech politics of the late nineteenth century. The exhibition was organized by the first professional generation of Czech *národopis* scholars such as Lubomír Niederle, Čeněk Zíbrt, Jan Jakubec, Otakar Hostinský or Emanuel Kovář, who importantly contributed to an institutional and intellectual development of Czech *národopis* and it should not come as a surprise that these personalities also prominently figure in Robek's book.

When it comes to nationalism, Brouček's book on the exhibition is far less ambiguous than Robek's. For example:

Nation as a community of various classes and social groups did not live only by questions of its language, territory, and its own culture, it was also being crushed by capitalism, which, engulfed by the growing imperialist conflicts, which were amplified and deepened by many feudalist survivals, escalated the basic social

⁴⁵ Robek even praises some representatives of Catholic and conservative currents for their ethnographic work, although it is obvious that he strongly disagrees with their views (Robek 1964: 83–91, 1976: 100–110). In his praise of bourgeois, conservative and Catholic currents, Robek uses a strategy which seems to soften the Marxist-Leninist approach which works out from the inseparability of political views from scientific work (see the part on partisanship and value partiality above). The praise probably stems from the fact that Robek viewed the Catholic and conservative currents as contributing to the knowledge about the Czech nation.

conflict which resulted into an irreconcilable struggle of the proletariat against the bourgeoisie. (Brouček 1979: 77)

Brouček's book also devotes one part to one of the thorny issues that is related to the participation of the working classes in the exhibition. The topic was intensely debated and caused a heated controversy among the members of the organization committee. Eventually, the organizers decided not to allow the working classes to participate (Brouček 1979: 131–139). For this reason, Brouček repeatedly criticises (sic!) the organization committee:

The bourgeois nature of the ethnography movement prevented the strongest part of the nation – the proletariat – from participating. The mobilization for the ethnography exhibition was supposed to create a sphere of activity of the new ideology of the bourgeois national movement. (Brouček 1979: 18)

In this passage, Brouček does not criticise the organization committee for its nationalism, as many would expect from a Marxist-Leninist, but actually for an insufficient nationalism. In his eyes, the exhibition failed for a lack of a more inclusive vision of the nation. In Brouček's view, the nation is not only composed of the middle classes, but also of the proletariat, a fact to which the organization committee were blind to because of the social background of its members. As the organizers recruited from the bourgeoisie, the exhibition was in principle unable to step over its own class shadow and become truly inclusive of all the parts of the nation (Brouček 1979: 99–100, 142–143).

The view on the nation as an objective-existing entity seems to have been widely shared by the late socialist ethnographers. For we also find it in works of Robek and Brouček's colleagues who researched on the topics related to the Czech National Revival and whose writings do not evince any ambiguity as regards the objective status of the nation. Writing about the nineteenth century, Irena Štěpánová writes of the "the young Czech nation." (Štěpánová 1983: 194), Mirjam Moravcová of the "modern Czech nation" (Moravcová 1986: 5,7) and so does Ludmila Sochorová (Sochorová 1987b: 24, 1987c: 71). Oldřich Kašpar writes of the nation which in the nineteenth century just entered the arena of international politics (Kašpar 1982: 199) and Vlasta Matějová of a process of a growing national awareness (Matějová 1982a: 15).

It seems that we have to considerably broaden our view on ethnography resulting from the discussion of Marxism-Leninism. It seems to be the case that not only did ethnographers hold bourgeois ethnographers in high esteem for their progressive role in fighting the remnants of feudalism, that is for their positive role in the class struggle and the struggle for social emancipation. They also praised them for their positive role in the national emancipation of the Czech nation. Nonetheless, the praise had its limits as the nineteenth century scholars were generally perceived as restrained by their social or class origins and were considered to have been unable to step over the shadow cast by their social pedigree. As the nineteenth century was nearing its end, it became increasingly evident that the bourgeois worldview also supported the reactionary forces of capitalism and was detrimental to the national cause as the bourgeois conception of the nation did not include the proletariat. This view on the nineteenth-century ethnographers was a part of a greater historical narrative according to which the world history in general and the Czech history in particular staged not one, but two struggles: that of the social emancipation and that of the emancipation of the Czech nation (side by side with the Slovak nation and vis-à-vis their German and Hungarian antagonists). Both historical processes were seen as completed in the socialist era. It was only later during the socialist era that the nation as a whole could be fully liberated, nationally and socially (Robek 1964: 160). The importance of these two processes was variously stressed in ethnographers' writings. In some passages, ethnographers wrote of the struggle for social emancipation as of the most important, sometimes this struggle gave way to the other one, to the fight of the Czech nation for its rightful place in the international community of nations.

There remains only some ambiguity as to what classes contributed more to the building of the modern Czech nation. As far as I am aware, in some of their writings, ethnographers stressed the contribution of various Czech ethnographers who recruited from the middle classes. This view almost naturally stems from the focus on the history of ethnography. However, it was especially Robek who devoted some of his writings to the exploration of the popular origins of the national emancipation (Robek 1964: 21, 1974a: 8, 1976: 26–27). This mirrors the situation with the origins of ethnography mentioned above.

As regards the status of the nation, from ethnographers' formulations, it is evident that we can recognize several notions which were not explicitly distinguished by ethnographers. To use a contemporary term, we have seen that Robek understood the nineteenth century nationalism as a *discursive weapon* of the bourgeoisie which had used it as a sort of disguise, as a cunning ploy to beguile the masses. Also, ethnographers

understood the Czech nation as an objectively existing entity which existed amidst other European nations of the time. What is also of interest here are the adjectives “young” and “modern”. These two refer to a specific constructivist conception which was present in Marxism-Leninism which approaches nations as modern phenomena emerging under the socioeconomic conditions of capitalism. This view was clearly elaborated in a book by Miroslav Hroch, a Czech historian of European national movements of the nineteenth century. Hroch, who also collaborated with Prague ethnographers in the 1980s (Hroch 1989), made use of an important distinction which he credited to J. V. Stalin. According to Hroch, Stalin had distinguished between two terms: “nation” and “nationality”. He had understood the former as modern formations which appeared only under the conditions of developing capitalism, the latter as pre-modern precursors on whose basis some future modern nations developed (Hroch 1986: 39–43).⁴⁶ In ethnography writings published in the 1970s and 1980s, we do not find Stalin’s name mentioned at all, but the writings seem to conform exactly to this view.

So, ethnographers did not only understand the nation as a discursive weapon of the bourgeoisie in the nineteenth century class struggle. They also understood it as an objectively existing entity on creation of which the bourgeoisie had eagerly participated. Perhaps the last question which needs to be addressed is the matter of this creation itself. The works on the topic of Czech National Revival never disputed the idea of the revival, which literally means that the dormant nation is revived. Whereas some authors claimed that the era might have entailed a great deal of manipulation, fiction or *ad hoc* invention (cf. Hobsbawm & Ranger 1983), some others even went as far as to claim that nations had been novel inventions that had emerged (and could only have emerged) as a result of specific socioeconomic changes (cf. Gellner 1993), ethnographers on the contrary stressed historical continuity. “The modern” or “the young” Czech nation was not built out of nothing, it was built on the foundations of a pre-existing Czech nationality. The constructivist standpoint which approaches the Czech nation as a modern invention is thus attenuated by a view which credits the modern Czech nation with a cultural continuity that reaches back at least to the Middle Ages. Ethnographers viewed the work of the revivalists

⁴⁶ One cannot avoid the feeling that Hroch portrayed as more valuable those nations whose history had reached back to the Middle Ages, as opposed to the nations which had emerged only in the nineteenth century. As Hroch included the “small Czech nation” in the former group alongside the large nations (the British, the French, the Spanish) and several other small nations (the Norwegians), his scholarly work indirectly promotes some sort of Czech historical exceptionalism – as a small entity endowed with rich history (Hroch 1986: 52–53).

not as a construction (or not only as a construction), but also as a cultural work which had aimed to revive and emancipate the Czech nation, its culture and language whose continuity had been severed by Habsburg Monarchy and its politics of recatholization and Germanization which had followed the battle of the White Mountain in 1618 (Kašpar 1982: 199–200; Šalanda 1986: 23–24). This is what Brouček referred to in his book as to “the three-hundred-year servitude” (Brouček 1979: 17–18), a phrase that is still well-known among the contemporary Czechs.⁴⁷ From the perspective of ethnography, we can say that the revivalists of the eighteenth and nineteenth centuries had revived the cultural content and given it a cut suitable for the modern era.

To summarize the situation so far, in ethnography writings we find several interconnected concepts of the nation. The first notion views the nation as a discursive weapon in the class struggle, a useful tool for the ruling strata to fool the subjected masses. Coexisting with this notion was another one, which approached nations as objectively existing phenomena. This view had three main ingredients: (1) the nation had emerged only under specific socioeconomic conditions of capitalism, (2) the nation had mostly been a creation of the bourgeoisie although it had also had an important popular component and (3) the nation had not been created out of nothing as it had been built from a pre-existing cultural substrate, the origins of which could be traced back to the times of the Czech feudal society.

It is likely that the general nationalist lens which we encounter in the late socialist ethnography writings did not originate in that era. It had been present among the foremost scholars of *národopis* (cf. Chotek 1946) and, as according to Jan Grill, the circle of young Marxist-Leninists ethnographers had been also burdened by the strong feeling for their nation (Grill 2015). It should not then come as a surprise to find that Robek and Brouček’s views did not essentially differ from what Olga Skalníková had written two decades earlier on the Czechoslovak Ethnography Exhibition. Although the nationalist ingredient of her account had been less pronounced than Robek or Brouček’s, Skalníková can also be

⁴⁷ Ethnographers did not explicitly mention the battle, the Habsburg oppressive rule with its language and religious policies as this would amount to bringing owls to Athens. The knowledge of these events as well as others (the rule of Charles IV, the Hussite Wars, the enlightened absolutism of empress Maria Theresa and her son emperor Joseph II, to mention just a few others), is part of a common stock of historical knowledge that can be found in the Czech population. As a student of elementary school in the 1990s, I acquired this knowledge during compulsory lessons in history (cf. Ingrao 2009).

credited with the view that if we want to capture our national being we must include the working classes in the picture (Skalníková 1955: 110).⁴⁸

It is very likely that this synthesis of nationalism with Marxism-Leninism did not originate in ethnography, but resulted from a fermentation in Czech intellectual life which went back at least to the first Czechoslovak president T. G. Masaryk who articulated a specific version of Czech nationalism, or more precisely, envisioned a role that the world history had bestowed on the Czech nation. According to Ernest Gellner, Masaryk saw the origins of the Czech nation in the Hussite proto-Reformation movement of the fifteenth century, a movement which Masaryk understood as a popular proto-democratic movement. In Masaryk's view, the Czechs had an important historical mission as the vanguard of world democracy and social equality (Gellner 1994: 116–118). This idea was later taken up by Czech Communists, among whom the most representative was Zdeněk Nejedlý (1878–1962), Czech historian, musicologist and a prominent Communist intellectual who later held several important offices in the 1940s and 1950s Czechoslovakia. According to his biographer, Nejedlý created a synthesis of nationalism and socialism in a work partly indebted to Marx and partly to Masaryk (Křesťan 2012: 164, 328). Nejedlý was probably only a prominent spokesman for an otherwise widely shared mood which affected the Czechoslovak society in the years following the end of WWII (Křesťan 2012: 331–332).

Ethnography writings seem to stem directly from this tradition that weaves together the thread of Marxism-Leninism with the thread of nationalism. Some ambiguity only results from the fact that ethnographers unevenly stressed these two themes in various writings and were not always clear in their exposition. Sometimes they stressed the struggle for social emancipation which seems to have been strong especially in the 1950s, sometimes they stressed the struggle for the national emancipation, a theme which began to be more visible during the late socialist period. It is hard to decide which of these two themes was more important or more genuine – it seems that both themes were equally momentous and peacefully coexisted side by side.

⁴⁸ So, when Marta Edith Holečková, writing on the University of 17th November in Prague, says that *ad hoc* vindication had to be provided in cases in which the university, designated to educate future elites from the third-world countries, hosted students from well-to-do bourgeois third-world families, she is not entirely right. According to Holečková, these accounts invented an *ad hoc* vindication for the fact that the revolutions against colonialism were carried out by the bourgeois classes (Holečková 2013: 163). This appreciation of bourgeois classes was by no means an *ad hoc* invention, because it had been already programmed into Marxist-Leninist thinking.

2.3.2 Ethnography of Ethnic Processes

As it was stated in the beginning of this section, there coexisted two strands of nationalism in ethnography. I have just described the one related to the topic of the Czech National Revival. The second strand covered the research on ethnic groups, but was better known as a research on ethnic processes for ethnographers usually focused on the dynamics of coexistence between various ethnic groups. The research topic was not itself novel as it had existed in Czechoslovak social sciences and humanities before the late socialist period. Ethnographers partook on the research of Czech enclaves outside of Czechoslovakia (cf. Heroldová 1958, 1964; Skalníková & Scheufler 1963) and no less important was the burning issue of the integration of the Roma ethnic minority to the Czechoslovak society (Hübschmannová 1970).⁴⁹

By the late 1970s, the research on ethnic groups gained a new impetus due to an intellectual intervention from the Soviet Union whose most prominent exponent was the leading Soviet scholar Yulian V. Bromley who was then known as the author of a 1973 book called *Ethnos and Ethnography*. The book erected a theoretical framework for the research on ethnic groups and ethnic processes and, as Ernest Gellner observed at the time, the book caused a minor revolution in Soviet ethnography (Gellner 1977; see also Šnirelman 1997). I am not in a position to judge Gellner's claim, but as we shall see, the book undeniably caused an intellectual revolution in Czechoslovak ethnography, especially among the ethnographers based in Prague. Prior to its publication in Slovak translation in 1980 (Bromlej 1980), Czech ethnographers had already started to publish (Robek et al. 1977) and research (cf. Nosková 1984a: 75) on the topic. The restoration of the interest in ethnic groups was crowned by the end of the 1970s when the Czechoslovak Academy of Sciences signed a bilateral agreement with the Soviet Academy of Sciences related to the research on ethnic processes. After the agreement was approved by both parties, the Ethnography Institute set up a new section dedicated to the study of ethnic processes (Olšáková 2016: 140; Thořová et al. 2005: 67–68) and Iva Heroldová was appointed as the section's head. The topic officially entered the state planning in 1981 and the two five-year plans set up for ethnography and folklore studies in the 1980s were accommodated to the topic accordingly (Olšáková 2016: 147).

⁴⁹ The original interest in Czech groups living abroad can be dated at least to the inter-war period (cf. Brouček 1985b).

The research on ethnic groups included several sub-topics. The major of these was a research on groups settled in the Czechoslovakia in the borderland villages and towns from which German population had been expelled after the World War Two. The groups of the new settlers had come from various countries such as Romania, Ukraine, Hungary, Slovakia or Poland and mostly, but not only, had composed of various groups of Czech and Slovak repatriates (Heroldová 1984, 1985, 1986, 1988a; Nosková 1984b, 1984a, 1989a; Robek & Heroldová 1983; Robek & Kadeřábková 1982; Secká 1988a; Valášková 1982, 1987b, 1987a, 1989). Several studies contributed to the research on the groups of *Gastarbeitern* from Vietnam or Cuba (Heroldová & Matějová 1987; Kašpar 1986a; Secká 1987). The study of ethnic processes also accommodated for the study of the Roma minority in Czechoslovakia (Grulich & Haišman 1986; Haišman 1989)⁵⁰ and a few articles explored Czecho-Slovak relations in the interwar period (Nosková 1987, 1989b) or population transfers of Czechs and Germans during the WWII (Šisler 1984, 1989). Lastly, ethnographers also studied groups of Czech compatriots living abroad, mostly those who had left the Czech lands between the nineteenth and mid-twentieth centuries and had settled in various countries in Europe, the United States and elsewhere, but contrary to the other sub-topic this kind of research was mostly based on the study of written documents (Brouček 1982; Brouček & Vasiljev 1985a, 1985b; Kristen 1986). Ethnographers from the Ethnography Institute also collaborated on a creation of an edited volume on the Polish minority in Silesia (Robek et al. 1989).

Ethnographers from the Ethnography Department, although fewer in numbers, also contributed, mostly by articles on Lusatian Sorbs or on Czech expatriates living in the United States and Slovakia (Dubovický 1989; Kašpar 1986b; Šatava 1981a, 1985, 1986, 1989). Moravian ethnographers working for the Ethnography Institute branch in Brno did not lag behind and contributed by writing a collection of essays on ethnic processes in Moravia (Navrátilová et al. 1986).

Apart from providing description of the livelihood of particular groups, Bromley's work inspired ethnographers to contribute with a series of theoretical articles that appeared

⁵⁰ According to one of my interlocutors, the research on the Roma minority and the groups of *Gastarbeitern* was directly enjoined by the Central Committee of the Communist Party. The research on Roma was motivated by the "Roma question" (cf. Hübschmannová 1970). The research on *Gastarbeitern* was partly motivated by a spread of stereotypes and stories which could today be called as urban legends. The Cubans were seen as a lazy workforce, and bizarre stories were related to the Vietnamese. Some of the Vietnamese supposedly wore dangerous knives hidden in shoes or carried infectious diseases. To the latter is related a story of a Vietnamese man who went to a dentist who found worms in the former's tooth (P0032: 5–6).

in *Český lid* by the end of 1980s and which resulted from a series of debates organized by the Ethnography Institute and which related to a never-finished publication of *The Encyclopaedic Dictionary of Ethnic Processes (Slovník etnických procesů)*. These articles aimed to provide precise analytic definitions of several crucial terms such as *ethnos*, *ethnic group*, *ethnic community*, *ethno-social organism* or *ethnic process*, and were intended as encyclopaedic entries (Brouček et al. 1987; Brouček 1988; Brouček & Vasiljev 1988a; Hubinger 1988; Vasiljev 1988). For this reason, we can call this strand *analytic nationalism* as it worked with an assumption that it was possible to analytically define and refine the cluster of interlinked concepts of which the most important were *nation* and *ethnos*.⁵¹

The revolutionary import of Bromley's ideas was not only in the soaring numbers of articles and books on the ethnic theme. His ideas also gave the nationalism in ethnography a specific and unpredicted twist. Not only did they strengthen ethnography's nationalistic dimension, they also strongly supported an unperceived return of idealism which the first strand supported rather indirectly. Even though this idealistic twist did not give rise to any controversy, it further highlighted contradictions immanent within the epistemological framework of ethnography. To understand its revolutionary import, it will be firstly necessary to outline the basic line of argument of Bromley's book.

2.3.3 Idealism Comes from the Soviet Union

Bromley divided his book *Ethnos and Ethnography* into two parts. I will only discuss the first part, in which Bromley offered a theory of *ethnos*. The second part, which tries a delineation of the science of ethnography, need not concern us here. In his book, Bromley postulated the existence of *ethnoses* (sg. *ethnos*). The term *ethnos* obtained two distinct meanings: In a wider sense, the term referred to any group of people with shared traits.

⁵¹ I draw inspiration from an interview in which Ladislav Holý expressed an opinion that he did not see much point in trying to define the concept of nation as an analytical concept, a definition that Czech ethnographers sought in the 1980s when working on *The Encyclopaedic Dictionary of Ethnic Processes* (Holý in Chorváthová 1990: 350). The attempts to provide an analytical definition of the subject matter of ethnography, have their origins in Bromley's work (Bromlej 1980: 11–12). However, it would be equally plausible to speak of ontological nationalism because ethnographers did not question the existence of nations or ethnic groups, which were perceived as existing entities. The term “analytical” is supposed to stress the theoretical and conceptual aspect of ethnographic enquiry. To put it in a different way, ethnographers viewed the world as populated with nations and ethnic groups, but it was up to ethnographers to attempt an exclusive definition of entities of this kind.

I shun the term “methodological nationalism” proposed by Nina Glick-Schiller and Andreas Wimmer because the term does not explicitly distinguish an ontological level (nations as natural forms) which gets confused with the question of methodology (nation as an explanatory concept). The “methodological” in their term is hence misleading as it covers non-methodological aspect of thinking about nations (Wimmer & Glick-Schiller 2002).

Such a group might consist of several smaller groups which can live isolated from each other. These particular groups then represented what Bromley called ethnos in a narrower sense, or also called *etnikos* (sg.). The members of an *etnikos* not only share common traits, but also live as a compact unit. To make his classification clearer, Bromley gave an example of Ukrainians living in the USSR and Ukrainians living in Canada as examples of two different ethnoses in this narrower sense. By implication, the Ukrainian ethnos in the wider sense included both groups of Ukrainians. As Bromley said, the basic traits of any ethnos rested in its specific cultural traits, particular ethnic consciousness, ethnic psychology, unique ethnonym and the knowledge of common origin. It was these traits which connected both groups of Ukrainians and pointed out to their shared origins (Bromlej 1980: 47–48).

As a scholar steeped in Marxist-Leninist materialism, Bromley insisted that ethnos with its traits was a product of the socioeconomic base. As Bromley did not forget to remind his readers that it is socio-economic factors which are constitutive of other phenomena such as culture, psychology or language (Bromlej 1980: 50), not the other way around. Bromley then introduced another term – *ethno-social organism*. Ethno-social organisms were understood as formations arising from a synthesis of socio-economic conditions with specific ethnos (in the narrower sense) and its traits. Ethnographers could thus speak of various groups of Ukrainians as well of other groups as existing under consecutive stages of feudalism, capitalism and socialism and each of these could be approached as a particular ethno-social organism (Bromlej 1980: 50). This dual view maintains that any society is a synthesis of two components: an ethnic one (the superstructure) and a socioeconomic one (the base).

However, there is more to Bromley's theory. Although he viewed ethnic components as originating from socioeconomic components, Bromley also advocated the view that ethnic components could be seen as achieving independence of the original socioeconomic components and, moreover, that they could be credited with autonomous causal powers. So, while the base of a particular society changes throughout the history, the ethnic identity or ethnic psychology (or some other ethnic features), which emerged in an earlier stage of the development of that society, might remain unchanged (Bromlej 1980: 78–79, 106). Yes, all ethnic features originally emerged in some sociohistorical conditions, but once they come into existence, ethnic features persist in spite of the changes in the material base of the society.

Bromley further stated that ethnographers could discern two mutually independent types of processes in history – socioeconomic processes and ethnic processes. He

understood the former processes as having greater volatility and a faster rate of change, the latter on the contrary, as being stabler and taking longer time to change. The two to a certain degree independent processes merge together in history and give rise to various ethno-social organisms. To support his view, Bromley gave an extreme example of the Armenians who had subsisted, that is, had remained Armenian through and through, under four consecutive socioeconomic stages (Bromlej 1980: 172).

Now, it should be clear that Bromley introduced profound changes in the Marxist-Leninist perspective. It was of no help that Bromley appealed to the authority of Marx and Engels and Lenin and returned to their observation (which I have discussed earlier in this chapter and which is linked to the concept of survivals) that the superstructures tend to persist in spite of the changes of their corresponding bases (Bromlej 1980: 89–90). It seems that Bromley explicitly granted the ethnic superstructure far more independence than usual in Marxist-Leninist ethnography and pushed ethnography nearer to idealism. Moreover, he introduced two terms – culture and national psychology – which at least since the 1950s had been viewed with suspicion by Marxism-Leninism, given their idealist, racist and colonialist overtones (Nahodil & Kramářik 1951: 13, 17).⁵² Czechoslovak ethnographers who researched on the topic of ethnic processes and who found inspiration in Soviet ethnography hence unwittingly jumped on the bandwagon of idealism which was distributed under the label of the latest advances of Soviet science and which was at odds with Marxist-Leninist notions prevalent since the 1950s.

What was also likely new in Bromley's conception was the conceptual apparatus of cultures and psychologies, nations and ethnic groups, or of ethnic processes. While the former strand spoke of the nation and national culture mostly in the context of Czech history, Bromley's work introduced a more general terminological apparatus which allowed ethnographers to speak about nations, cultures and psychologies in plural and with a greater ease. Although they did not always mention Bromley, his outlook began to dominate their thinking on ethnic groups in Czechoslovakia (Haišman et al. 1983: 31, note no. 8; see for

⁵² It is appropriate to add that such a view was present (although in a suppressed form) already in the 1950s' ethnography. For example, Nahodil in one of his articles defines ethnography as a science that explores specific features of various cultures (Nahodil 1951: 55). Since there are only five types of the base (primitive communism, ancient society, feudalism, capitalism and socialism) and many times that number of actually existing societies, it is clear from Nahodil's wording that the specific cultural features cannot be approached solely on the basis of differences between socioeconomic bases and that Nahodil's conception presupposes some differences among cultures, differences which are not directly caused by the socioeconomic base (Cf. Nahodil 1953: 6).

example Haišman 1989; Nosková 1984a: 75, 1984b; Šatava 1981a; Secká 1987, 1988a; Valášková 1982: 153–154, 1987b: 25).

2.3.4 Ethnic Processes in Action

Let us see what fruits Bromley's ideas yielded in Czech ethnography. Bromley's book equipped Czech ethnographers with an explicit ethnonational framework, or even ethnonational ontology. According to this ontology, the world is populated with ethnic groups which are approached as something given, almost unchanging and endowed with durable and peculiar ethnic features such as language, culture, custom, psychology or consciousness. Such a view also implied that it was possible, at least to a certain degree, draw firm boundaries between the distinct and discrete ethnic groups.

The goal of ethnographers going to the field was to capture specific processes that resulted from interactions among different ethnic groups, mostly between the Czech majority and one or more new-coming groups of immigrants, depending on the situation in a particular locality. Ethnographers conformed to the historical nature of their discipline and described ethnic processes as they had unfolded in the decades following the settlement of the newcomers after World War Two. Key was a concept known as the *inter-ethnic equalization* (*mezietnické vyrovnání*).⁵³ Broadly speaking, this meant that there had been two or more distinct groups in the beginning – the Czech majority which in several decades to follow assimilated the newcoming minority groups. The process of assimilation had not always been an easy one and ethnographers provided vivid details of hardships of mutual coexistence of the minorities with the majority, especially in the first years following the settlement. However, the story which ethnographers offered had a happy ending. The immigrant groups successfully adapted and, in several generations, assimilated while at the same time they retained something of their cultural distinctiveness.

Ethnographers' explanation was framed in evolutionary understanding of the situation too, for they spoke of the cultural backwardness of the immigrants compared to the level of development of the socialist Czechoslovakia and its citizens. We encounter formulations such as “the different levels of development of the two cultures” (Valášková 1982: 153), “anachronisms” (Valášková 1984: 37), “survival” (Secká 1988a: 166) or “in more primitive conditions” (Secká 1988a: 167), “making up for the delay in the

⁵³ The term and its variations appear quite frequently in ethnography articles (cf. Haišman 1989: 35; Haišman et al. 1983: 28; Heroldová 1985; Kadeřábková 1985: 5; Nosková 1984b: 163; Secká 1988a).

development of economic-social conditions” (Heroldová 1985: 58, 59) “primitivism” and “cultural backwardness” (Heroldová 1986: 226, 229) or “the stagnation of ethnic features on the feudal level” (Heroldová 1988a: 222). These widespread evolutionistic premises manifested also in texts which dealt with Czech migrants to the USA (Šatava 1989: 108)⁵⁴ and in texts which did not deal with ethnicity at all (Valášková 1984: 37).

Apart from the evolutionary outlook which was a part of Marxism-Leninism in ethnography, the explicit references to Marxist-Leninist concepts were rather scarce in articles dealing with ethnic groups and ethnic processes. The whole explanatory weight began to rest on the concepts of ethnos and its derivatives such as culture or psychology. Various groups differed as to their ethnic and corresponding cultural and psychological traits which ethnographers could observe. Some of these features were actively used by the members of the groups for the purposes of differentiation *vis-à-vis* other groups. Ethnographers called these features “ethno-differentiating markers” (Valášková 1987b: 25). The particular cultural and ethnic features of the groups in question, alongside some objective features such as the relative size of the groups, helped ethnographers to explain the different dynamics of the inter-ethnic equalization between various localities where two or more culturally distinct groups had coexisted (Heroldová 1984, 1985; Secká 1988a).

Bromley could truly be credited, if not with planting another seed of contradiction in Czech ethnography, then at least with a generous watering of its seedlings. When we return to the 1950s when ethnographers noisily announced their departure from idealism and to later ethnographers’ proclamations which encouraged this departure, we can see that thanks to Bromley, idealism returned to ethnography through the back door under the guise of ethnos. The persuasiveness of Bromley’s idealism was perhaps not only thanks to the fact that he was perceived as one of the aces of the progressive Soviet ethnography, but also due to the fact that the crucial Marxist-Leninist concept of base was useless in the study of ethnic processes as the groups under scrutiny shared the socioeconomic base of the Czechoslovak society with the Czech majority. It would have made no sense to explain any group differences by the reference to corresponding and mutually different bases if workers of various ethnic pedigree had lived in the same village and spent their working hours in the same factory or agricultural cooperative. It seems, after all, that cultures and psychologies, or in another words, intellectual capacities, determine the lot of nations. But

⁵⁴ In Šatava’s book, we thus find almost side by side ethnocentric presuppositions derived from an evolutionary point of view and a criticism of thereof (Šatava 1989: 40).

then, Bromley's theory does not substantially differ from Václavík's or idealist conception of *národopis* quoted earlier.

If we accept that ethnographers felt strongly for their nation and prided themselves of its national culture as is evident from their many value judgements which can be collected from their writings on the Czech National Revival, then Bromley's analytic nationalism with its conceptual arsenal perfectly fitted ethnographers' preconceptions about their own nation and national culture. Bromley further strengthened ethnographers' ways of writing about ethnic groups and nations, which were perceived as something given, endowed with a particular and temporally durable culture, mentality, value orientations or psychology. The Czechs had always been there and Germans had always been their nemesis. It should not come as a surprise that when Robek reflected on the lot of the Germans expelled from Czechoslovakia after World War Two, he tossed away his Marxist-Leninist sensibilities and explained the causes of the war in nationalist terms: "After displacing the German minority, which betrayed the republic of Czechoslovakia, and which under the banner of fascism brought about a destruction of the Republic in the years prior to the Munich agreement..." (Antonín Robek 1981: 193).

Notwithstanding their enormous influence, Bromley's ideas were not powerful enough to wholly displace Marxist-Leninist materialism as this was impossible until 1989 when the socialist regime in Czechoslovakia fell and Marxism-Leninism lost its main political backing. Up to then, the Marxist-Leninist materialism existed in an intellectual connubium with nationalism. The trouble is that these two approaches which existed side by side can be conceived as antagonistic too. It is one thing to explain a phenomenon as a product of the socioeconomic base and quite another to explain it as a product of the national culture which is conceived independently on the socioeconomic base. There was no one who pointed out to this contradiction, no one who dared to delimit the explanatory possibilities of the two approaches or find a way to reconcile them. For nobody pointed that out, no intellectual discussion ensued. Whilst some ethnographers continued to acknowledge Marxism-Leninism as the fundamental explanatory scheme, they also began to utilize the competing explanatory terms such as national psychology or ethnic consciousness (cf. Šalanda 1989a). These two explanatory strategies appeared cheek by jowl in a way all too similar to the two doctrines we talked about earlier – the doctrine of survivals and the requirement of proper historic contextualization.

The two frameworks were able to coexist without much fuss in a way not dissimilar to the coexistence of the same two frameworks in Bromley's theory of ethnos. This peculiar

synthesis of Marxism-Leninism and nationalism which was to be found among Czech ethnographers and also historians was actually noted by Ernest Gellner in his review of a book by the Czech historian Miroslav Hroch, *The Social Preconditions of the National Revival in Europe* (Hroch 1986), to which I have referred earlier.⁵⁵ Gellner noted that Hroch used two mutually irreducible explicanda – nations and classes – and viewed the history of modern nations as a story of two emancipatory processes – national and social. In Gellner’s opinion, Hroch tried to salvage the two great myths of the nineteenth and twentieth centuries, the myths of nationalism and Marxism, by reformulating them in a scholarly manner (Gellner 1994: 199).⁵⁶ Though Gellner disagreed with Hroch’s view, he expressed his appreciation for Hroch’s “outstandingly well-documented argument” (Gellner 1994: 182). What Gellner probably did not know was that Hroch’s argument was in principle not that outstanding if we judge it relative to the standards of Czechoslovak ethnography. Hroch shared his views with ethnographers whose thinking was buttressed by the very same intellectual currents. This also shows that Marxism-Leninism and nationalism, at least in the Czechoslovak case, did not represent two antagonistic wholes. This is a point worthy of stressing since, as Ladislav Holý and Katherine Verdery were to argue later, some authors tended (and still tend) to portray Marxism-Leninism as inimical to nationalism (Holý 1996: 7; Verdery 1996: 83–84).

2.3.5 Nations, Cultures and Traditions as Survivals

Marxist-Leninist ethnography was riven with contradictions as ethnographers wanted to eat their cake and have it too. They insisted on the proper historic contextualization, yet they also wanted survivals. They wanted class struggle, but they also wanted national emancipation. They saw socioeconomic base as causing other phenomena, but they also acknowledged culture and national psychology as causes. And such contradictions can be found even in works unrelated to the topic of ethnic groups (Pargač 1988; Šalanda 1989a). Another variation on the same theme was the concept of tradition which in ethnographic explanation played a role similar to the concept of nation or ethnos. In any tradition,

⁵⁵ The English version was published in 1985 in Cambridge University Press.

⁵⁶ To underscore the book’s nationalist dimension, Marx, according Hroch, was wrong when he refused to acknowledge the right of the Czech nation to its self-determination. Marx failed to acknowledge that there existed a Czech bourgeoisie which was a necessary precondition to the self-determination. This critique even involves Engels to whose earlier statement Marx appealed (Hroch 1986: 34). Hroch’s criticism is interesting since it puts the nation above the Marxist-Leninist materialism, which otherwise provides Hroch with a basic framework for his historical inquiry.

ethnographers could recognize features which had changed under changing socioeconomic conditions as well as features which had not. And it was up for ethnography research to decide which of the unchanging traditional features were vital even for the life in the socialist society and which could be cast off. The concept of tradition was utilized by ethnographers based in Prague (Pargač 1988; Šalanda 1989a) as well as by ethnographers from Brno and Bratislava (Frolec 1984; Leščák & Sirovátka 1982).

Nations, ethnoses, cultures and traditions seem to be conceptual variations of survivals. As well as survivals do, nations, ethnoses, cultures and traditions emerge under particular socioeconomic conditions, and prove to be durable enough as to outlive the original socioeconomic base which gave them birth. However, there is one important difference. Ethnographers would have surely spoken of survivals as of parts of traditions, cultures or nations, but no ethnographer would have spoken of a tradition, culture or nation as of survivals as the meaning of term survival (*přežitek*) ranges from slightly to considerably negative connotations in Czech language. A survival is something that has been outlived. It is something dated, or even useless, a non-functional atavism. Nation, culture or tradition were not usually spoken of in a similarly negative manner. Yet as concepts they shared the same structure with survivals. All of them were viewed as something durable and longevous and less amenable to change.

2.4 The Third Accusation: Positivism

We have seen that the accusation of nationalism is not wide off the mark for a nationalistic vision permeated ethnographers' work. Let us now turn to the accusation related to positivism. The task is troubled from the outset, for all the accusations of positivism are lacking in a precise definition of positivism and authors usually use the label positivism in a self-evident manner. So, while some commentators agree that Czechoslovak ethnography was if not wholly then at least considerably permeated by positivism (Uherek in Hann et al. 2007: 47; Janeček 2017: 149; Kandert 2005: 238; Skalník 2005b: 12–14, 2005a: 56pp), they do not usually tell the readers what positivism is. It is hard to resist the feeling that the accusation of positivism serves rather as an all-purpose beating stick (cf. Candea 2019: 135; Roscoe 1995).⁵⁷

⁵⁷ Moreover, here we arrive at one problem which has been obvious at least since the publication of Karl Popper's *The Logic of Scientific Discovery*. If nothing as a theory-free science is possible (Popper 1997: note 1, 41), then positivism with its inductive inference and verification is itself a theory and alleged positivists

If we disregard the abuses of the term, positivism for the abovementioned critics of ethnography obtains two major meanings: descriptive mode and atheoretical character. The trouble is that usual accounts accusing ethnography of being descriptive and/or atheoretical do not explicitly link these accusations to being positivistic. However, this is what people usually disclose when the conversation turns to the demerits of ethnography. Let us then see how various contemporary critics of ethnography specify description and atheoreticity.

2.4.1 Descriptivism

The attribution of descriptivism to ethnography can be found in a text by Lozoviuk: “the methodology of Czech ethnography in the socialist era continued to focus on collection, description, and classification of traditional ethnographic materials,” (Lozoviuk 2005: 233) and also makes appearance in accounts by Skalník (Skalník 2002b: 101, 103), Michael Stewart (Stewart in Hann et al. 2007: 39), Verdery (Verdery in Hann et al. 2007: 49), Holý (Holý in Chorváthová 1990: 348) or Podoba who speaks of an “archaic, pre-scientific, descriptive field of ethnography, with no theory on its own” (Podoba in Hann et al. 2007: 29). In Jakoubek and Nešpor’s account, the diagnosis of descriptive ethnography (Nešpor & Jakoubek 2004: 58) turns into a general characterization of ethnography:

...the essence of ethnography [etnografie] is a collection of primary sources..., their comparison and description. Ethnographic disciplines [národopisné disciplíny] are well-armed to perform these tasks, but we should not require them to venture into a general interpretation of their material just because they do not possess proper theoretical means to do that. (Nešpor & Jakoubek 2004: 62).⁵⁸

While Nešpor and Jakoubek do not clarify what exactly they understand by comparison, they probably have on mind some simple charting in the sense of charting differences and similarities between different kinds of objects such as ploughs, pottery, costumes, narratives etc.

Although Nešpor and Jakoubek’s text does not work with the label *positivism* or *positivistic*, the label appears in Jakoubek’s solo article published later. In it, Jakoubek

are no positivists at all. Positivism is nothing but a fanciful *contradictio in adjecto*. Without delving deeper into the problem, positivism in ethnography equals rather a research attitude attributed to ethnographers from the outside instead of positivism as a hard methodological, epistemological or philosophical commitment.

⁵⁸ Note the equalization of *etnografie* with *národopis*.

stresses the “paradigmatical differences” between anthropology and ethnology (Jakoubek 2012: 72). Not only does Jakoubek presupposes the continuity between ethnography and ethnology for the purposes of his argument (Jakoubek 2012: 71), he also establishes the continuity in a different way. His article singles out Iva Heroldová and her works as paradigmatically representing both ethnography and ethnology. What Jakoubek does not say is that Heroldová began her career in ethnography at the Ethnography Institute in the 1950s where she remained until the 1980s (Brouček & Jeřábek 2007: 72), but she continued to publish in the 1990s when the label ethnology was given priority over ethnography. Jakoubek even quotes her works from the latter era. However, these facts remain hidden to the scholars unfamiliar with the history of ethnography.

Jakoubek’s text has been subjected to a critique (Balaš 2016), but at least, the text can be considered as an attempt, as his earlier one with Nešpor, to provide an account of differences understood as paradigmatic. Jakoubek’s intention does not seem to be to replace ethnology with anthropology, he only aims to define anthropology and demonstrate its differences from ethnology. This goal of his follows on from his earlier attempt to demarcate anthropology from bordering disciplines (Jakoubek 2012: 71–72; Nešpor & Jakoubek 2004: 66). According to Jakoubek, while ethnography/ethnology aims to uncover the objective truth, anthropology renounces any search for the objective truth and rather strives to interpret, that is understand actions, thoughts and words of individuals and peoples from their point of view, at the same time insisting on the partiality and subjectivity of such interpretive attempts. Ethnographers/ethnologists on the contrary strive to discover the objective truth which is often at odds with the native conceptions. For example, what informers say about their kinship ties might be at odds with the objective reality of biological reproduction (Jakoubek 2012: 95). The idea of the objective truth that the ethnographer/ethnologist seeks to uncover evokes the supposedly positivist tools such as observation, experiment and inductive inference by which we arrive at natural laws which govern the course of the world and which structure the real kinship links.

Compared to other texts, Jakoubek’s text is probably the only text which offers some definition of positivism for the purposes of tracing differences between anthropology and ethnography/ethnology. Unfortunately, his approach is too crude as it operates with an untenable binary distinction. For Jakoubek, ethnography/ethnology is positivistic, because it searches for some objective truth which can only be attained by the researcher. On the contrary, anthropology is interpretive as its aim is to understand the world from the native’s

point of view. For the sake of this distinction Jakoubek, and also Nešpor, use the dichotomy *etic* versus *emic* (Jakoubek 2012: 94; Nešpor & Jakoubek 2004: 69).⁵⁹

2.4.2 Atheoreticity

Let us now focus on the second issue, that of atheoreticity. According to Andrew Lass, Czechoslovak ethnography was governed by “a resistance to theory” and that it “has been the least open to theoretical developments that have occurred in the social sciences and in the humanities” (Lass 1989: 10). When speaking of *národopis* and ethnography⁶⁰ Lass uses the term *description* which evokes a common-sense understanding of positivism. It is fair to say that in his article, Lass also speaks of ethnography’s resistance to reality and that he depicts the discourse of ethnography as driven by inner tensions between the idealisation of the folk culture on the one hand and the need for scientific descriptions footed in reality on the other, the former of which seems to be at odds with the claim regarding the nonexistence of a theoretical framework, a feature which Lass also stresses (Lass 1989: 10).

Accusations of atheoreticity or accusations of deficient theorizing also appear in other accounts (Nešpor & Jakoubek 2004: 54–55; Scheffel & Kandert 1994: 20; Skalník 2002b: 101, 2005b: 14, 2005a: 76), and to repeat Podoba’s words: “archaic, pre-scientific, descriptive field of ethnography, with no theory on its own” (Podoba in Hann et al. 2007: 29). Lozoviuk adds an explanation to this atheoretical character of ethnography and says

⁵⁹ While it is certainly the case that “the native’s point of view” or “putting oneself in someone else’s shoes” was underdeveloped in ethnography and ethnology, the trouble is that Jakoubek seems to be unaware that many anthropological approaches tend to discount the native’s point of view too. Various Marxist, evolutionary, ecological, materialist, critical, psychoanalytic, structural or functional approaches are the case in point. Conversely, the criticism of ethnography for its descriptivism makes it almost seem that anthropology dispenses with any description, which is an equally untenable implication.

As to the crude distinction between *emic* and *etic*, there are several points of objection of which I mention two. Firstly, in the process of its adoption from linguistics, Marvin Harris, thanks to whom the terms became popular among anthropologists, changed, not to say impoverished, the original meaning of the distinction, which had originally offered a subtle analytical device (Fisher & Werner 1978). Secondly, the impoverished version makes a distinction between two anthropological approaches: the anthropologist can either proceed from the natives’ statements (*emic* analysis), or the anthropologist can discount the native point of view and use only anthropological concepts for explanation (*etic* analysis). The distinction did not present much trouble for Harris who championed the *etic* analysis and strove to discount the native’s point of view. Unfortunately, Jakoubek, who considers himself to be a champion of the *emic* analysis, does not further state what he means by the native’s point of view. Is it what the natives tell us or is it what the natives think? Moreover, Jakoubek seems unconcerned about many other problems such as lying, forgetting, ignorance, silence, irony, universalising strategies or impression management which further compromise the facile distinction between *emic* and *etic* analysis.

⁶⁰ Lass gives examples from the late 19th and early 20th century *národopis* (but refers to it as ethnography). And although he does not name ethnographers who were his contemporaries when he was writing his article in the late 1980s, he certainly saw a continuity. The continuity in his article is achieved by his using present perfect tense.

that as a consequence of the socialist rule, ethnography and folklore studies fell behind theoretically and methodologically (Lozoviuk 2005: 233).

There is a great deal of irony here as the critiques of ethnography for its atheoreticity and positivistic descriptivism began to regularly appear within the discourse of ethnography at least since the early 1950s, but none of the contemporary commentators seems to have noticed that. Already by the 1950s, Nahodil criticised “formalistic factology” (Nahodil 1951: 55), Kramařík criticised the positivistic and formal analyses and comparisons of folklorists Václav Tille and Jiří Polívka (Kramařík 1953: 101–102) and their descriptive mode which was destined to fail as it did not employ the method of proper historic contextualization (Kramařík 1954: 49; see also Nahodil 1961: 2).⁶¹ Later we find Karel Chotek writing that the proper goal is not description, but explanation (Chotek 1966: 278, 284). Countless other examples witness to the fact that ethnographers throughout the socialist era were acutely aware of the need of theoretical development of their own discipline (Anon 1965: 129; Holý & Stuchlík 1964: 228; Kramařík 1968: 6; Krupková 1991: 55; Nahodil 1951: 55; Robek 1972a: 232; Tůmová 1964: 45).

2.4.3 Was It Really the Case?

Descriptivism and atheoreticity are just two sides of one coin, but accentuate different aspects of ethnography. The first presents ethnography as a bug-collecting pastime, the other one as an intellectually insipid enterprise. Concerning the purely intellectual focus of this chapter, it is hard to nod to either of these accusations, especially after such a lengthy description of Marxist-Leninist and nationalist frameworks. These two complexes can be viewed as two theoretical frameworks that theoretically supported the edifice of ethnography. Yes, ethnography was a descriptive science which began, to employ a positivistic formulation, with perceivable phenomena. But what then to make of an array of more or less explicit theoretical concepts which permeated ethnographers’ writings? We can return to the troubles connected to the competing methods of historic contextualization and survivals. Even in Kramařík’s original account, survivals were not approached as objects with some survival-like quality existing independently on the observer; and the

⁶¹ It is possible that Lass in whose article the atheoreticity is articulated most clearly acquired this idea during his own ethnography education in the late 1960s and early 1970s when he lived and studied at the Ethnography Department in Prague. Another source of inspiration for Lass’s accusations of atheoreticity might possibly be a book on women under socialism written by his mother Hilda Scott. Scott in her work notices a deficiency in Marxist-Leninist theorizing related to the thinking about family (Scott 1974: 41–43).

same was true of the properly contextualizable phenomena, since it was up to the ethnographer to provide an appertaining context, or in other words, it was up to the ethnographer to reason out which of the two possibilities was the case. Similarly, ethnographers working on the issue of ethnic groups, provided their readers with interesting explanatory schemes which went beyond conventionalized descriptions. As I have noted earlier, ethnographers proposed variables which could account for different rates of assimilation and adaptation in ethnically heterogeneous settlements. These variables usually covered the relative size of various groups or their particular cultures that might or might not be inclined towards assimilation.

To say that ethnography was merely descriptive and devoid of any theoretical considerations is to grossly misrepresent it. It does not make much sense to maintain this accusation *vis-à-vis* the Marxist-Leninist and nationalist frameworks which represent ethnography's materialist and idealist currents. Contrary to some of their accusations of positivism, some authors actually seem to be aware of the fact that ethnography was not wholly positivist, but do not develop the insight into a coherent picture. Lozoviuk mentions the topic of ethnic processes which according to him went beyond mere description (Lozoviuk 2005: 231) and similarly Podoba mentions "ethnic theory" and sporadic forays into structuralism or theories adopted from the arts (Podoba in Hann et al. 2007: 29). So, it cannot be right at all to claim that ethnography was both Marxist-Leninist and positivist or that it was at the same time nationalist and positivist as some other commentators maintain.

2.5 Partial Conclusions and Other Considerations

To summarize the story so far, the least persuasive is the accusation of descriptive and atheoretical positivism since both Marxism-Leninism and nationalism presented characteristic theoretical frameworks or epistemic commitments which in their particular ways disciplined the empirical material garnered by ethnographers. Ethnographic description was surely a device used by ethnographers and we encounter more and less descriptive works. Nonetheless, as regards the claim that ethnography and folklore studies was solely a descriptive enterprise devoid of any theory, we can argue that this claim is hardly tenable.

While the accusation of positivism can be easily dismissed, the accusations of Marxism-Leninism and nationalism withstand. Marxism-Leninism is strongly present in some texts and in hindsight it does not seem that invoking Marxist-Leninist principles or

quoting Marx and Engels was always an insincere strategy. Moreover, Marxist-Leninist framework manifested even in articles in which ethnographers did not quote any Marxist-Leninist figures or in articles which did not employ any Marxist-Leninist jargon.⁶² Nationalism was equally, if not more, vigorous and it is important to bear on mind that the frameworks of nationalism and Marxism-Leninism complemented each other in the late-socialist ethnography.

Our consideration of the three accusations also allows us to point to a deficiency in the statements insisting on intellectual or even paradigmatical differences between Czech ethnography with its successors and the strands of American, British or French anthropology. The specific conception of the nation which we have encountered in ethnography overlaps with notions that we encounter in the works of anthropologists whose names went down in the history of anthropological research on nation. In ethnographers' eyes, the nation was seen as a modern phenomenon, a notion which tallies with the constructivist standpoints of Ernest Gellner and Benedict Anderson (Anderson 2008; Gellner 1993). At the same time ethnographers saw modern nations as having antecedent traditions the roots of which had gone back at least to medieval times. This view brings ethnography closer to Anthony D. Smith and his notion of *ethnie* (Smith 1988). Seeing nations as entities on their own is very close to the so-called epochalism and essentialism, terms used by Clifford Geertz in his article on the newly formed nations (Geertz 1973: 234–254). Moreover, we have witnessed the 1950s' ethnographers introducing a break which was mostly argued on the basis of materialist versus idealist divide. Though considerably reductive and sweeping, these labels point to different explanatory strategies which we generally find in social sciences and which trace their origins to the nineteenth century when Marx dared to place Hegel upon his feet. The difference between materialism and idealism and their rich variations can be also found in sociocultural anthropology (cf. Keesing 1974; Ortner 1984; Sahlins 1976). Lewis H. Morgan who was appropriated by ethnographers as a godfather of materialism, was also viewed as a crucial figure for the development of the twentieth-century anthropology. According to Marshall Sahlins, the figure of Morgan represented one of the two central paradigms within anthropology (Sahlins 1976: 57). Again, the criticism of Tylor and Lévy-Bruhl from the pen of the young Marxist-Leninist ethnographers cannot be taken as a proof of radical disjunction as the

⁶² I will focus on the presence of Marxism-Leninism and its non-intellectualist dimensions in ethnography texts in Chapter 3.

arguments of Nahodil and Kramařík partly resemble the view of Malinowski who saw the primitive man as the man of practice, not of theory (Malinowski 1992: 28). Not even the difference between the supposed anthropological synchronic method and ethnographical historicism can be used to produce a sharp distinction between the two disciplines. At least from the 1970s, anthropology reappropriated the importance of historical analysis (Ortner 1984: 142–143), not to mention earlier uses of history in anthropology. And to add one last example of the mutual compatibility, the method of proper historic contextualization is not that different from functionalist and sociological maxim of explaining a phenomenon by the context in which it appears (Candea 2019: 64–67; Kutnar 1947: 1). As the philosopher Ian Hacking noted: “If two people genuinely disagreed about great issues, they would not find enough common ground to dispute specifics one by one.” (Hacking 1983: 5). It seems that ethnography and anthropology after all shared a vast common ground.

It is not to say that the two traditions are identical. Theoretical currents such as structural functionalism, the so-called symbolic anthropologies, psychoanalytic inspirations, Geertz’s interpretive project and other hermeneutic ventures, postmodernism or structural Marxism could not easily enter ethnography in the times of socialism. There is a striking theoretical variety in anthropology, while ethnography, though it was certainly not uniform, could not offer the same intellectual richness. Nonetheless, the main point should not be forgotten. The two traditions cannot be conceived as radically different for they agree on many important points. So, the wide divide separating the two traditions, which we encountered in the beginning of this thesis, now disappears. We are one single step from acknowledging the existence of just one tradition and its varieties, but let us postpone any conclusions for later.

2.5.1 A Point of Departure

In the introduction, I mentioned several opinions that concern the intellectual deficiency of local traditions (be they ethnographic or anthropologic) *vis-à-vis* Western sociocultural anthropology. This idea is lucidly phrased by Červinková who in her already mentioned article asks herself a question: *So why is it that we as east-central European anthropologists rely on western paradigms and seem incapable of developing original concepts that would be as intellectually powerful and empowering...?* (Červinková 2012: 158–159; emphasis mine).

It is perhaps unwittingly that Červinková here reaches the limits of the intellectual approach. She is aware of the fact that contemporary CEE anthropologists make use of Western concepts, but seem incapable of developing their own. The framing of the problem to which she subscribes cannot but show the situation as a difference between intellectual powerfulness and intellectual deficiency, the advanced them and the backward us.

We do not have to go far for a proof which vindicates Červinková's point. When we, for example, compare anthropological, sociological and historical literature which emerged around the problem of ethnic groups and nationalism in the 1980s and 1990s, we cannot fail to notice that this discourse abounded in a variety of often antagonistic approaches which were usually formulated as rejoinders to previous works. We see that there existed constructivist approaches which historicized the emergence of nations and placed their origins in the early modern or modern periods, but differed as to what occasioned the creation of nations. Was it the industrial revolution (Gellner 1993 [1983]) or was it the invention of the printing press and a subsequent development of capitalism (Anderson 2008 [1983])? Another approach endued nations with a more ancient pedigree (Smith 1988). Yet others avoided these macro-questions and tried to show how nationalist feelings are created and maintained in day-to-day interactions of individuals and institutions (Holý 1996; Kapferer 1988).

When we encounter such a striking variety of positions, the intricacy of arguments presented and disagreements voiced, we cannot but help to admit that sociocultural anthropology is a far more stimulating discipline than the local Czech tradition under whichever name it appears. And note that I have only mentioned a handful of publications which relate to just one topic among a host of others. Witchcraft and sorcery, religion and rituals, economics and exchange, embodiment, gender, healing and medicine, transnationalism, family and kinship, animal symbolism, politics... The argument from intellectual deficiency seems to carry something relevant about it, but there appears to be something wrong with the explanation. This is a point from which the recent commentators begin and which I also touched when I was showing the problematic cohabitation of some theoretical postulates. While these contradictions would in anthropology give sooner or later a birth to innumerable controversies, ethnographers did not even perceive the contradictions which were inherent in their work. Or maybe they did, but they did not enshrine them in writing. How to explain their ethnographers' scholarly attitudes?

Unfortunately, I believe that the contemporary commentators which are bound by the limits inherent in the intellectualist vantage point cannot offer us a satisfactory solution.

Their approach rests on an implicit assumption that only “ideas beget ideas” (Voříšek 2012: 16). If a tradition is intellectually deficient then we cannot expect it to produce intellectually stimulating and empowering ideas and *vice versa*. In the remaining chapters, I will try to find a way around such a conclusion and account for the intellectual differences between anthropology and ethnography by a recourse to non-intellectual components of scholarly work. For this reason, we have to start over in the 1950s.

3. The Field of Ethnography and Folklore Studies in the Late Socialist Period

3.1 A Problem

This chapter aims to capture the development of the subfield of Prague ethnography during the late socialist period, that is the period following in the wake of the 1968 and ending with the Velvet Revolution in 1989. However, for the sake of exposition, the chapter opens with an outline of the history of ethnography between 1948 and 1968, for the solutions of some dilemmas in ethnography in the late socialist period have their origins in the situation before 1968. While this chapter will deal mostly with the Prague subfield, some things will be considered in relation to the field of Czechoslovak ethnography as a whole.

When it comes to the analysis of the situation in the late socialist Czechoslovak ethnography, we have to focus on the role of Antonín Robek, both a prominent figure in Czechoslovak ethnography and the undisputed sovereign of the Prague sub-field of ethnography during the late socialist period. Even thirty years after the fall of socialism, Robek's personality continues to occupy the minds of Czech scholars. For example, in his ruminations over a recent collection of interviews with former ethnographers (Hlaváček & Bortlová-Vondráková 2018), the Czech historian Martin Nodl complains about the absence of a critical analysis of Robek's role (Nodl 2019). Nodl furthermore observes that the interviews offer two incommensurable narratives. Some interviewees praise Robek as a figure who salvaged ethnography and helped it to survive during uneasy times, others denounce him as a skilful manipulator who contributed to ethnography's decline. At one point, Nodl suggests that out of these two contradictory narratives only one can be true and the trouble is, according to Nodl, that no one has so far satisfactorily attempted to deal with the contradictory assessments related to Robek and his role (Nodl 2019: 234). Two years prior to Nodl's lamentation, Jiří Hlaváček attempted to handle the contradiction and offered a more-than-satisfactory account grasping the complexities surrounding Robek's personality, his role and activities in the late socialist ethnography. The goal of this chapter is to pick up the gauntlet, follow up with Hlaváček's account as well as with several other

accounts and push the analysis of the late socialist ethnography even further with the help of the sociology of Pierre Bourdieu.⁶³

3.2 The Sovietization of *Národopis* in the 1950s

The Communist coup d'état in February 1948, or the Victorious February, brought about a radical political change to the post-war Czechoslovakia. The Communist Party of Czechoslovakia became *de facto* the only ruling party in the state and drew Czechoslovakia under the sphere of influence of the Soviet Union. For another forty years, Czechoslovakia became an inseparable part of the Eastern Bloc and its integrative agents – the economic organization Comecon and the military organization Warsaw Pact. The changes which the newly established socialist regime introduced were meant to be total and, not surprisingly, had consequences for research and higher education in Czechoslovakia.

In the previous Chapter, we have witnessed the young Marxist-Leninists speaking out strongly against the older generation of *národopis* scholars. This disagreement was not just a result of a generation gap between the older and the younger, or merely an academic disagreement between the idealist old-guard and the revolutionary materialists who, by the time, were in their early thirties, but a change that was fostered and intensified by political transformation. So it is by no means an overstatement when Nahodil wrote that ethnography greatly benefitted from the help of the state (Nahodil 1951: 52). The state intervened in ethnography in several ways.

To begin with, Czechoslovakia established the state-funded Czechoslovak Academy of Sciences (*Československá akademie věd*) after the Soviet model in 1953 (Nešpor 2014: 305). The main aim of the Academy was research in many different fields, including ethnography. The Institute for Ethnography and Folklore Studies was established as a part of the Academy in January 1954 when two institutions within the Academy were merged together – the Section for *Národopis* (*Kabinet pro národopis*) and the Section for Folk Song (*Kabinet pro lidovou píseň*) (Macková 2016: 332). From its very beginnings, the Ethnography Institute had a branch in Prague and a branch in Brno (Macková 2016: 345). It is to this year that the origins of the phrase “ethnography and folklore studies” can be dated. Before 1954, ethnographers in their articles did not use the overarching label which

⁶³ More specifically, the following account is partly indebted for inspiration to the work of Katherine Verdery's who employed Bourdieu's sociology for the purposes of analysing the struggles for domination among Romanian scholars (Verdery 1991).

connected the two. In one of his programmatical articles, Nahodil explicitly wrote about folklore studies as a discipline independent on ethnography (Nahodil 1951: 57). This division was again upheld by Kramařík two years later when he wrote about recent trends in ethnography and called for a cooperation of folklore studies with ethnography (Kramařík 1953: 106), implying that the two disciplines were distinct, and perhaps also foreshadowing their near merging. Though scholars continued to maintain some distinction between ethnography and folklore studies (cf. Kramařík 1955), the two fields of inquiry began to be mentioned cheek by jowl (Kramařík 1954: 49). A decade later, the two were almost naturally considered as two branches of one overarching discipline of *národopis* (Frolec & Holý 1964: 7). Had it not been for the institutional merger, ethnography and folklore studies would have probably remained independent on each other.

The Ethnography Institute issued two journals, *Český lid* and *Československá ethnografie*. As the front matters testify, the Ethnography Institute acquired *Český lid*, from the publishing house Brázda which had issued the journal between 1946 and 1951. Beginning with 1952, the journal became to be published by the Government Committee for the Construction of the CSAS (*Vládní komise pro vybudování ČSAV*), a provisional organization which had been assigned the task of creating the Czechoslovak Academy of Sciences. However, the editorial board of the journal had changed already in 1951 when Olga Skalníková had become its chief editor. The journal passed to the Ethnography Institute after the Institute's establishment in 1954. The new purpose of the journal was popularization of ethnography (Secká 1991: 146) whereas its scientific ethos was bequeathed to *Československá ethnografie*, which was established in 1953 and became a professional platform for ethnographers. It was named after the model ethnographic Soviet journal *Sovietskaya ethnografiya* (*Soviet Ethnography*) and its principal purpose was the theory and method in ethnography and, apart from Czechoslovak topics, it also devoted a considerable amount of space to research on non-European societies. Otakar Nahodil became the chief editor from the journal's inception (Bahenský 2016: 154).

Not only did ethnography receive an autonomous institute at the Academy, but it also continued to exist at the three major universities in Czechoslovakia – at Charles University in Prague, Masaryk University in Brno and Comenius University in Bratislava. Contrary to sociology, which was prohibited as a bourgeois pseudoscience after 1948

(Nešpor 2014: 15), ethnography and folklore studies did not face any similar prohibitions.⁶⁴ Some would probably contest such a claim as it obscures the fact that it was the predecessor of ethnography and folklore studies – *národopis* – which was repressed personally and ideologically, a fact that signifies a disciplinary discontinuity. We can see that in the early 1950s many scholars of *národopis* were actually subjected to persecution of some sort or another and it could be further argued that this persecution helped to transform *národopis* into ethnography and folklore studies.

However, unlike the arena of politics or military, no high-profile⁶⁵ *národopis* scholar was executed or imprisoned during the Sovietisation of *národopis* after the Communist coup in 1948. Despite the many hostile accusations levelled against *národopis* scholars, none of them was dismissed from their university posts. There were two exceptions to this – Josef Voráček and Závěš Kalandra, two authors whose intellectual preoccupations were close to *národopis*, but who did not fall in the ranks of *národopis* quite neatly. Josef Voráček (1910–1980), a graduate in sociology, had originally studied in France where he had learned about the French School of Sociology from its prominent

⁶⁴ Some issues that concern the continuity of these Departments need to be made clear from the onset. There were times during which ethnography and folklore studies did not have autonomous departments. Immediately after WWII, Chotek established an independent *Národopis* seminar in Prague and Václavík the Ethnography and ethnology seminar in Brno. In 1950, universities were reorganized on the basis of departments (*katedry*, sg. *katedra*), another invention coming from the Soviet Union. The seminar in Prague was incorporated into the Department of *Národopis* and Prehistory, the seminar in Brno into the Department of History. In Prague, an independent Department of *Národopis* was created in 1953 only to be reduced to the Section of Ethnography and Folklore Studies of the Department of Czechoslovak History and Archival Science in 1958. In 1960, an autonomous Department of Ethnography and Folklore Studies was created and it kept its name until the early 1990s. Concerning the situation in Brno, the original Seminar created by Václavík was subjected to even more reorganizations. In 1954 it moved under the Department of Prehistory and *Národopis*, in 1961, under the Department of Art History and *Národopis* and in 1962, under the Department of Art History, Creative Arts and Ethnography. An independent Department of Ethnography and Folklore Studies was created in 1964. A further reorganization took place in 1970 when the Department was incorporated into the Department of History and Ethnography of Central, Southeast and East Europe. Even further reorganization took place in 1986 when the Department of History, Archival Science and Ethnography was established. This last socialist incarnation of the Department existed until 1991 (Janeček 2017: 143–147; Pavlicová 2017: 165; Válka et al. 2016: 24–44). This overview contradicts Skalník's recent flat assertion that beginning with the 1950s, departments uniformly changed to departments of ethnography and folklore studies (Skalník 2018: 6).

Two lessons should be taken from this overview, one factual, the other formal. First, these reorganizations did not necessarily entail personal changes let alone a disciplinary discontinuity (see below). Secondly, for the sake of clarity, both institutions will be further on referred as to the Ethnography Department (in Prague) and the Ethnography Department in Brno (or the Brno branch).

One last remark concerns the existence of the Seminars in Prague and Brno. None of the accounts is actually precise on the point how and when exactly the seminars ceased to exist.

⁶⁵ As far as I know, the only *národopis* scholar who was imprisoned was Klára Freislebenová. Born in 1921, she was given an eleven-year sentence for treason in 1950 (*Klára Freislebenová – odsouzena na 11 let*, online). Although she was active in the institutions of *národopis*, she was still in the beginning of her career. Her case hence contrasts with the post-1948 fates of the high-profile representatives of *národopis*. I am grateful to Václav Hubinger for drawing my attention to Freislebenová's case.

representatives (Nešpor 2014: 598). He later published a book on general anthropology and a book on family and marriage (Voráček 1940, 1941). Both books evince Voráček's close acquaintance with French, German, British and American scholars and reflect his wide-ranging interests in human biology, anthropology, ethnology, sociology and psychology. Of Czech scholars of the time, he was very close to topics developed by British and American anthropologists and the French school of sociology. After 1948, Voráček began to be progressively marginalized at the Faculty of Arts at Charles University where he lectured at the time. He was dismissed in the early 1950s, two years after a ban on his lecturing (Petráňová 2016: 84),⁶⁶ and would never return to academia. Závaš Kalandra (1902–1950) was a Czech Marxist author and journalist known for his work on interpretation of Czech medieval legends and myths (Kalandra 2018a, 2018b [1947]). In his books, Kalandra utilized some ideas developed by anthropologists, folklorists and Czech scholars of *národopis*. Kalandra became a victim of a staged trial, faced charges of being a part of a Trotskyist conspiracy, and was eventually hanged alongside the Czech politician Milada Horáková and others in 1950 (*Proces s vedením záškodnického spiknutí proti republice* 1950: 20–21).

Quite surprisingly, neither Kalandra nor Voráček figured in the texts of the young Marxist-Leninists as targets of their ideological fusillades. The fate of these two intellectuals was incomparably harsher to the fates of the prominent representatives of the previous era of the allegedly bourgeois *národopis*, Chotek and Václavík, Horák, Pražák and Stránská, all of whom survived the years of political turmoil relatively unscathed albeit they frequently faced criticisms for the bourgeois pedigree of their ideas. Especially Chotek and Václavík, as we have seen, had to face denunciatory criticisms in print and in person (cf. Petráňová 2016: 86). For Antonín Václavík, who continued to work at the Department of Ethnography and Ethnology at the Faculty of Arts at the university in Brno until his death in 1959, the situation was more severe. Václavík wrote a self-denunciatory critique which was published in *Český lid* (Václavík 1952) and according to Richard Jeřábek, his works continued to receive an undeserved degree of mostly unjust criticisms even after his death (Jeřábek 1991: 219–220). Some measures against the senior generation proved to be a bit more effective than the pen, ink and the print. Those lecturers at Charles University who were sixty-five or older and were not members of the Communist Party were retired in 1956 (Petráňová 2016: 87–88). Anyway, it seems that Chotek, who had reached sixty-five in

⁶⁶ Unfortunately, from the source quoted it is unclear whether Voráček was dismissed in 1951 or 1952.

1946, could lecture as an external lecturer until the academic year 1957–58 (Petráňová 2016: 88, note 42). It also seems that the older generation was not so powerless against the young revolutionaries. Stránská, Chotek and Pražák wrote negative assessments on Nahodil's *habilitation* thesis in 1953 and Nahodil, the leader of the Marxist Circle, was denied his *habilitation* as a result (i.e. he was denied the rank of *docent*), which was the second highest academic rank in Czechoslovakia (Petráňová 2016: 86).

In the retrospect, it can be argued that the Marxist-Leninist revolution in *národopis* and its transformation to ethnography was underdone and went only halfway even if it was supported by state power. The half-baked revolution can be attested to by several jubilees published in *Český lid*. In the very volume which published Nahodil and Kramařík's articles denouncing their senior colleagues, readers could also read a short jubilee written by Kramařík in which he congratulated Karel Chotek to his seventieth birthday. In it Kramařík somehow weakened the earlier fierce criticisms and concluded his congratulations: "In the struggle for the new and progressive science of ethnography, we therefore offer a critique of many of Chotek's ideas, but it does not mean at all that our criticism equals denouncing all his works." (Kramařík 1951a: 132). Similarly, we find later laudatory jubilees or obituaries written to the memory of Vilém Pražák (Kramařík 1959), Drahomíra Stránská (Johnová 1965), Jiří Horák (Kunz 1959) or even Josef Voráček (Vařeka 1970). As a matter of fact, ethnography never erased these authors from its own disciplinary history. They were allowed to lecture and publish⁶⁷ and continued to be quoted and mentioned even after they passed away. So, when Robek later wrote his book on the history of Czechoslovak ethnography (Robek 1964, 1976), he also included the names of Chotek, Stránská, Pražák and Václavík, although he was largely, but not always, critical of their professional accomplishments. Their works continued to be referenced without much ideological ado by ethnographers active from the 1950s (Baláš 1970: 12–13; Karbusický 1963: 298; Kramařík 1968: 14–16) as well by ethnographers who began their academic careers some twenty or thirty years later (Pargač 1988: 63, 65; Secká 1988a: 168; Štěpánová & Märzová 1984: 31, 32, 33–34; Valášková 1984: 42).

It is not to deny the reality of the victimization, but it is important not to downplay the fact that the discipline of *národopis* and the older generation who represented it successfully endured in their positions despite adverse conditions they had to face. If we

⁶⁷ Václavík could even publish a book about folk embroideries in German, English and French translations in 1956 (Jeřábek 1991: 219, 221).

disregard the labels which allegedly stand for different disciplines and if we cast away the black-and-white view on the history of Czechoslovakia and focus on personal and institutional continuities instead, then the resulting picture is quite different. The fact that the term *národopis* was not abandoned is itself an interesting testimony of the situation.

It is, of course, a question which forces played role in the fact that the bourgeois generation of *národopis* scholars survived the political changes backed by the ideological diatribes of the young Marxist-Leninists. One reason was probably the high scholarly and symbolic status of the older generation which was acknowledged by the younger Marxist-Leninists in the jubilees and obituaries and which counterbalanced the assumed intellectual and ideological failings of the older generation. Another possible reason is provided by Lydia Petráňová who mentions that Chotek was on friendly terms with Zdeněk Nejedlý whom he knew from his youth (Petráňová 2016: 88) and who at the time held various ministerial posts in Communist governments and who also acted as chairman of the Czechoslovak Academy of Sciences (Křesťan 2012: 438–439).⁶⁸

Hence, regardless of the relabelling and intellectual reorientation of the discipline, its field evinced a rather large degree of continuity. In the previous chapter, we have seen that Marxist-Leninist ethnographers continued to celebrate the earlier generations of revivalists and *národopis* scholars in spite of their bourgeois origins (Kramařík 1951b; Nahodil 1951: 56; Skalníková 1951a, 1951b). The last bourgeois generation of *národopis* scholars faced a similar fate. They were never to be erased from the history of the discipline, they continued to publish and to teach younger generations of ethnography neophytes in spite of the fact that they were subjected to an unprecedented and merciless degree of criticism to which the earlier generations of representatives of the bourgeois *národopis* were never subjected by Marxist-Leninists.

3.3 The Liberalization of the 1960s

The situation in ethnography changed considerably in the 1960s. Socialist Czechoslovakia witnessed a partial liberalization of political, academic and cultural life in the second half of the 1950s and throughout the 1960s, which was a consequence of the Great Thaw of the Khrushchev era (Nešpor 2014: 306). Apart from augmented and improved possibilities for

⁶⁸ The Czech historian Josef Polišenský indicates a similar possibility that after the 1948 coup, it became more important to be on friendly terms with Zdeněk Nejedlý rather than to profess the correct ideological allegiance (Polišenský 2001: 189).

ethnography research, the liberalization affected Czechoslovak society at large. Some of the highest peaks of the liberalisation policies were economic reforms or an end to the censorship.

Otakar Nahodil, the main proponent of the 1950s' ethnography's shift towards Marxism-Leninism, gradually lost support and power. According to Lydia Petráňová, Nahodil did not get on well with his Party comrades and she holds that the only reason that Nahodil could remain in power until the early 1960s was that he had contacts in the secret services (Petráňová 2016: 86). By 1963, *Československá ethnografie*, the journal run by Nahodil, was dissolved and merged with *Český lid* (Kramařík 1963). Nahodil was expelled from the party in 1962 and removed as head of the Ethnography Department (Petráňová 2017: 21–22).⁶⁹ He then began to work in the Central Institute of Health Education which was a part of the Ministry of Health, but he did not cease to be academically active (see Nahodil 1964, 1965). In 1966 he emigrated to West Germany thanks to a fellowship granted to him by the Alexander von Humboldt Foundation (Petráňová 2017: 5).

The growing liberalization in ethnography was very likely not a result of Nahodil's withdrawal, but resulted from a mixture of several trends which mutually reinforced each other. As a part of the process of decolonization, the Soviet Union sought to exploit new possibilities in Africa, Asia and South America. This geopolitical trend allowed an increased support for the research outside of Europe and this allowed several Czech ethnographers and students of ethnography to visit non-European countries (Bahenský 2016: 155–156). As part of the foreign interests, the Czechoslovak Communist Party established the University of the 17th November which had its seat in Prague and which was modelled after Patrice Lumumba University in Moscow. The university was active between 1961 and 1974 and served to educate students from Africa, Asia and Latin America (Holečková 2013: 159–161).⁷⁰ Czech ethnographers also partook on ethnocartography projects organized by European ethnologists, visited ethnological conferences and

⁶⁹ Petráňová's account is rich on details extracted from archival sources and pretty closely maps the forces behind Nahodil's fall, but she does not mention when exactly Nahodil ceased to lecture at the Ethnography Department.

⁷⁰ There are several theses defended at the Ethnography Institute by students from Africa, the Caribbean or South America. These theses some of which were written in English are currently available in the library of the Ethnology Department. As some of the theses indicate, a few of these students were affiliated to the University of 17th November, but studied at the department. This was the case of Hugh Alwin Skinner from Trinidad whose cousin Elliott Perceval Skinner obtained a degree in anthropology at Columbia University and was later appointed as U.S. ambassador to Upper Volta (Hevesi 2007; online) The State Security subjected H. A. Skinner to a questioning as to why he had chosen to study in Czechoslovakia and not in the USA as his cousin had done (ABS, KR-614375 MV).

established professional links with colleagues across Europe (Woitsch 2016a: 197–200). Sociology was re-institutionalized in the mid-1960s (Nešpor 2014: 299) and at the time, several sociology and anthropology books appeared in their Czech translations. Among these were Malinowski's *Scientific Theory of Culture* or Lazarsfeld's *Language of Social Research*, or two books by C. W. Mills's, *Power Elite* and *Sociological Imagination* (Možný 2004: 613; Nešpor 2014: 332–333; Voříšek 2012: 47–48). Ethnographers could freely look for inspiration in sociocultural anthropology (Skalníková & Fojtík 1969), appropriate the label anthropology (Scheufler 1971), or quote the Austrian economist J. A. Schumpeter without fearing negative consequences (Polednová 1969: 339–340). Also, in the 1960s, several prominent intellectuals of the time visited Czechoslovakia to give lectures as a part of a series of lectures organized by Czechoslovak Society for Sociology: Talcott Parsons, Theodor Adorno, Ralf Dahrendorf or Peter Berger (Nešpor 2014: 317). Ernest Gellner was also one of those who were invited to Czechoslovakia to deliver a lecture to a sociological audience (Hall 2010: 192).

3.3.1 A Note on Czechoslovak “Anthropology”

It was during the more open and liberal era of the 1960s when local scholars could make freer use of anthropological and sociological inspiration coming from the countries from across the Iron Curtain. In relation to ethnography, we can discern two partially intersecting trends. The first trend was related to the Section of Integral Anthropology (*Sekce integrální antropologie*). It was originally an informal club which crystallized around Karel Mácha and was incorporated into the Czechoslovak Society for Sociology in 1966 (Anon 2016: 100). The most prolific of this group was Josef Wolf, originally a biologist, who taught at the Ethnography Department in Prague. At the beginning of the 1970s, Wolf compiled and, with several collaborators,⁷¹ translated to Czech an anthology of anthropology texts written by, with the exception of Richard Thurnwald and Claude Lévi-Strauss, American- and British-based anthropologists (Wolf 1971). No member of this group did ever conduct any empirical research and the writings of these authors were rather theoretical if not speculative (Nešpor 2014: 601). The second trend was related to the activities of Ladislav Holý (1933–1997), Milan Stuchlík (1932–1980), their colleagues and students. Holý was an employee of the Ethnography Institute and Stuchlík worked for the Náprstek Museum

⁷¹ One of the collaborators was Andrew Lass, who, by the time, was a student at the Ethnography Department (P0001: 3).

of African, Asian and American Cultures. Both lectured at the Department of Ethnography in Prague (Janeček 2017: 147) and partook on discussions of Mácha's integral anthropology circle (Anon 2016: 137–144). Holý and Stuchlík represented the so-called specialization in exotic ethnography during the decade. Holý and Stuchlík's early works from the first half of the 1960s were based on secondary sources. Holý wrote on African Interlacustrine area and Stuchlík on Sumatran Bataks (Skalník 2005a: 69).⁷²

What deserves to be stressed in particular is that exotic ethnography did not begin in Czechoslovakia with Holý and Stuchlík. Otakar Nahodil had been interested in non-European societies already in the 1950s and these predilections of his had very likely derived from the Marxist-Leninist concept of primitive communism which had denoted exactly the same groups among which anthropologists of the times had conducted their research. Also, by the mid-twentieth century, Czechoslovakia had already established several scholarly traditions of African, American or Oriental studies linked to ethnography by their interests, but with their independent institutions. Scholars linked to these traditions had been affiliated to the Oriental Institute of the Czechoslovak Academy of Sciences (*Orientální ústav ČSAV*), various departments at the Faculty Arts or to Náprstek Museum. Several scholars embedded in these traditions had also contributed to *Československá ethnografie*.⁷³ The new element in the situation in the 1960s was the vigour of Holý and Stuchlík's enterprise and their attempts to move exotic ethnography closer to the British tradition of social anthropology, or alternatively to establish an independent Czechoslovak tradition of anthropology.⁷⁴

⁷² The translation of the term is slightly misleading as the name of the specialization in Czech language – *cizokrajná ethnografie* – does not imply much exoticism but simply the fact that it is interested in foreign lands (*cizí kraje*). The Czech language knows both words – *foreign* (*cizí*) and *exotic* (*exotický*).

⁷³ Among the contributors to the journal were Indologists Otakar Pertold and Kamil Zvelebil or Americanists Čestmír Loukotka and Václav Šolc.

⁷⁴ It is interesting to note that while Holý and Stuchlík's contribution is today stressed as crucial for the establishment of the short-lived tradition of Czech exotic ethnography, Nahodil's role, on the contrary, is completely downplayed. However, one of the students indicated that Nahodil had a beneficial influence on exciting students' interests in exotic ethnography: "We were absolute enthusiasts. It was Nahodil who inoculated us." (P0023: 20).

This presents an interesting counterpoint to Petráňová who concludes her recent article on Nahodil with an insinuation that Nahodil was rather an insignificant ethnographer whom we remember only because of his political and ideological activities: "his footprints in the history of ethnography would have been negligible" (Petráňová 2017: 22). Petráňová's claim is rather unfair given the fact that she is concerned merely with the political dimension of Nahodil's activities and does not give a shot at providing some account of Nahodil's scholarly work, a deficiency which Josef Kandert attempts to amend in his rejoinder to Petráňová (Kandert 2018). Here I would only add that Nahodil's rich publication profile offers quite a different view on his supposed negligibility. Besides, what also contributes to the many-sidedness of his personality is the fact that he was able to win students' hearts for ethnography, which can be considered an invaluable quality.

Be as it may, the increasing liberty of the 1960s' ethnography research can be nicely demonstrated on the development of Holý's work. Beginning with the 1950s, Czechoslovak ethnographers under the influence of their Soviet colleagues harshly condemned anthropological functionalism and a host of other approaches as handmaidens of colonialism, although it was at the time possible to occasionally commend Anglo-American anthropology for the volume of material which anthropologists had collected (cf. Tolstov 1953: 30–31). We find the same old story in Holý's thesis on the disintegration of the kin group structure in the Interlacustrine area in East Africa from 1963. In the preface to his thesis, Holý blamed anthropologists for an ahistorical character of their approach and for their having been "apologists for the colonial system" (Holý 1963: 3–4), yet he made an extensive use of their works. In his thesis, we frequently encounter references to works of J. H. M. Beattie, Edward H. Winter, Kalervo Oberg or Lucy Mair, that is, anthropologists raised in the British functionalist tradition. Holý justified the extensive use of anthropological sources on the basis that it was possible to discern in these works some information uncorrupted by the general ideological outlook of authors of these works (Holý 1963: 3–5). By the end of the 1960s, Holý seemed to be somewhat lukewarm to ideological preoccupations. In 1968, he published a volume jointly edited with Milan Stuchlík on social stratification in Africa (Holý & Stuchlík 1968a). Its introductory article quotes Lenin and refers to Marx, but also mentions a host of contemporary British and American anthropologists as well as the works of Durkheim, Weber and Pareto without any ideological fuss about these authors' ideas or their bourgeois bias (Holý & Stuchlík 1968b). The volume contains several accounts of social stratification of various African groups written by Holý, Stuchlík and four colleagues of theirs. However, as the editors announce it in the introduction, their accounts are not based on fieldwork but on secondary sources (Holý & Stuchlík 1968c: 5). The volume was published in English, the translation was provided by a former RAF officer Joy Turner-Kadečková⁷⁵ and two years later it was reviewed in *American Anthropologist* (Frantz 1970).

The growing inclinations of the two towards anthropology can be demonstrated also on their reply to their colleague, Vanda Tůmová, who in one of her articles called for a closer cooperation between ethnographers and sociologists. Tůmová's article from 1964 would sound oddly familiar even to contemporary readers as the article argues that while the domain of ethnography is the particular, the domain of sociology is the extrapolation of

⁷⁵ see *Joy Kadečková*; online.

general regularities from particular occurrences (Tůmová 1964: 47). Holý and Stuchlík replied in a fashion which, on the contrary, many would find anachronistic today. According to both, ethnography and sociology are indeed distinct disciplines, but in a different sense. Although they acknowledge that not always was the dividing line been so sharp, there is one crucial difference. Relating to Ferdinand Tönnies's dichotomy of *Gemeinschaft* and *Gesellschaft* without explicitly mentioning the German sociologist, (*pospolitosti*, sg. *pospolitost*), sociology is interested in modern societies whereas ethnography is interested in communities which under modern conditions progressively disappear (Holý & Stuchlík 1964: 230–231). Without any doubt, disregarding mere scholastic concerns related to proper definitions, we can read Holý and Stuchlík's reply as a kind of position taking by which they strove to earn more autonomy on both ethnography and sociology, the cooperation between which was called for by Tůmová.⁷⁶

Nonetheless, their article is remarkable for yet another feature. Even if they did not mention any anthropologist by name in it, they worked with several notions which must have been taken straight from British anthropology of those times. In their article, we find a clear distinction between social structure and social organization (c.f. Firth 1955; Holý & Stuchlík 1964: 229, 231) and they also mention two kinds of functions derived from the respective conceptions of Malinowski and Radcliffe-Brown (Holý & Stuchlík 1964: 229; c.f. Malinowski 1960; Radcliffe-Brown 1952).

In the 1960s, Holý received a funding to conduct a fieldwork in Sudan among the Berti (Skalník 2005a: 69). It was probably thanks to this fieldwork that Holý made acquaintances with several British anthropologists who in the 1960s and early 1970s came to Prague to deliver a few guest lectures: Farnham Rehfisch, Jack Goody, Ian Cunnison, Max Gluckman and Meyer Fortes.⁷⁷ These anthropologists mostly recruited from circles

⁷⁶ The fact that Holý and Stuchlík had to carry through their exotic specialization is supported by what I learned from Olga Kandertová. According to her, there existed diverging opinions related to the proper subject matter of ethnography. She recalled her first meeting Robek on an occasion of her visiting the Ethnography Department prior to her university studies to learn about the curriculum. Robek, having learned that Kandertová had not come from a village, tried to dissuade her from enrolling for ethnography. Nahodil, whose classes she later attended, on the contrary, stressed the importance of research outside Europe and used the derisory soubriquet “žudristi” (sg. žudrista) for ethnographers focusing on domestic topics. The soubriquet is derived from the term “žudro” which refers to a kind of annexe in the vernacular architecture. Focusing on academic struggles, this points out to an existence of a dominant cleavage in Prague ethnography which divided two groups of ethnographers. The first focused on exotic themes and was originally represented by Nahodil, the other focused on domestic themes and was represented by Robek. This story is interesting since, as we have seen, Nahodil collaborated with Robek on several articles and books on religion and superstitions, but either was interested in different regions. It also suggests that Robek tried to dissuade applicants who were less likely to fit in his scholarly interests.

⁷⁷ P0023: 12. See also Eriksen (2015: 86).

related to the Manchester school of anthropology and to one of the two currents in Cambridge and Oxford, leaving the other current inspired by Lévi-Strauss's structuralism and emblazoned by the names of Edmund Leach, Mary Douglas and Rodney Needham unrepresented (Gell 1999: 4; Kuper 1993: 59–60).

During the 1960s, several of Holý and Stuchlík's colleagues and students wrote theses on non-European societies and made their first experiments with fieldwork. To mention a few, Stanislav Novotný wrote a thesis about the material culture of the Numfor of New Guinea, Blanka Kučerová on initiation ceremonies among the Kore, or Věra Hauptvogelová on the social control among the Pueblo. Some of the students' theses were actually based on some previous fieldwork such as Olga Kandertová's (née Pichová) on the Mapuche of Chile.⁷⁸

Thanks to his acquaintances made among anthropologists, Holý then received an offer to serve as director of Rhodes-Livingstone Museum in Zambia. When his stint ended in 1972, Holý decided not to return to Czechoslovakia and emigrated to the UK. Stuchlík made use of an opportunity of a university exchange scheme with Chile and left in 1968 to conduct a fieldwork among the Mapuche and, for some time, he ran a small anthropology department at University of Temuco (Stuchlíková 1997: 47). After the 1968 invasion of Czechoslovakia he decided to remain in Chile, but in the wake of the Chilean coup five years later he emigrated to the UK too (Stuchlíková 1997: 345). Both men continued in their collaboration, Holý at the University of St Andrews and Stuchlík at The Queen's University in Belfast. The fruits of their mutual collaboration were published after Stuchlík's untimely death in 1980 (Holý & Stuchlík 1981, 1983). With the emigration of both, the exotic ethnography lost its two central protagonists and for decades to come, the specialization was peripheral to major preoccupations of ethnography and folklore studies.⁷⁹

⁷⁸ I am grateful to Olga Kandertová for this information. The theses mentioned above can be found in the library of the Ethnology Department of the Faculty of Arts of Charles University.

⁷⁹ As one interviewee recalled Robek's words from the entrance interview: "..., but we do not study Indians, we study the Red Belt around Prague." According to the interviewee, two students of the Ethnography Department in the late socialist period pushed through a research on Native Americans and were even allowed to travel abroad. Nonetheless, the two students exploited the possibility to emigrate and this incident further weakened the possibility to specialize in exotic topics (P0032: 1). The contingency of emigration probably served as one of the reasons behind very limited possibilities of ethnographers to travel outside Czechoslovakia. Some ethnographers continued to specialize on exotic ethnography in the late socialist period (see below), but their works dealt with languages and history and were not based on fieldwork.

3.4 Ethnography in the Late Socialist Period

The brief period of intense liberalization in Czechoslovakia which came to be known as the Prague Spring was abruptly ended by the invasion of the armies of five member states of the Warsaw Pact in August 1968. In the years following the invasion, Czechoslovakia was subjected to widespread changes which came to be known by the euphemism “consolidation process” or the “Normalization era”,⁸⁰ which abolished the recently acquired liberties. A more conservative faction of the Communist Party complying with the USSR demands seized power and began to purge the Party of those who supported the previous liberal tendencies in the 1960s and those opposing the 1968 invasion. The purges affected all levels of society, but they mostly affected Communist Party members active in the 1960s. In spite of the fact that these purges did not aim at violent reprisals, staged trials or even executions like those after the 1948 Coup, these were probably the largest purges in the history of socialism in Czechoslovakia. For example, of six hundred employees of Charles University sixty-seven emigrated, were fired or had to retire prematurely between 1969 and 1971 (Hlaváček 2017: 30; Jareš et al. 2012: 71, 76).

The purges took a form of screenings (*prověrky*) of the current Party members who were subjected to questioning related to their previous activities. The screenings followed a top-down direction. They began in the higher party circles and ended with purges on the rank-and-file level (Jareš et al. 2012: 53–54). Members who were not found trustworthy were not allowed to remain Party members and were either suspended (*vyškrtnutí ze strany*) or expelled (*vyločení ze strany*). The results of the purges served as a pretext for taking further measures. Those who were suspended from the Party could usually remain employed at the Faculty, but the suspension presented an obstacle for further career advancements. Expelled members faced a situation which was more severe as some of them were also dismissed from the Faculty and had to accept jobs outside of their specialization and could not continue their previous academic careers. The purges had also serious consequences for the families of the expelled members as their children could not be easily admitted to universities (Holý 2010: 17; Šimečka 1990: 126).

The screenings of the Party members and subsequent sackings were just one of the many tools of the consolidation process. Other instruments of control were made available by changes in legislation which came with the new minister of education in 1969. New laws

⁸⁰ Sometimes the whole period covering the 1970s and 1980s is referred to as the Normalization period (Kolář & Pullmann 2016).

entitled the minister to remove deans straight from their offices, dismiss lecturers or disband departments (Jareš et al. 2012: 53). The most important way how the state power encroached on the autonomy of universities and academic institutes was that many decisions were subjected to two committees of the Communist Party – Prague Municipal Committee and its subordinate Prague 1 District Committee (Jareš et al. 2012: 171).⁸¹ This is just to provide some general overview of the post-1968 changes which entailed a diminished degree of academic autonomy. Other tools of Communist Party rule relevant to the case of ethnography and folklore studies and its autonomy will be described throughout this section when appropriate.

Antonín Robek (1931–2008) became the crucial figure in the consolidation process in ethnography. If Otakar Nahodil had been the *dramatis persona* of the 1950s' Sovietisation of ethnography, Robek played a similar role in Czechoslovak ethnography after 1968. Robek was no newcomer to the discipline. He had studied history and *národopis* between 1950 and 1955 at the Faculty of Arts where he had become a lecturer already in 1953 (Hlaváček 2017: 28).⁸² By 1968 he was already an accomplished scholar with a formidable publication record. Robek was viewed as a trustworthy person who could carry out the transformation of both institutes for two additional reasons: He was known as a Party member who opposed the liberal tendencies in the 1960s (Hlaváček 2017: 30–31) and independently on the Party purges, Robek with a handful of colleagues from Charles University formed a group of five and attempted an early consolidation of the Faculty of Arts from within after 1968 (Jareš et al. 2012: 57–58). Hence in 1971, Robek was appointed as director of the Ethnography Institute and a year later, he became head of the Ethnography Department (Hlaváček 2017: 31, 33). His appointment was accompanied by a series of articles which were mentioned in the previous chapter and which announced Robek's unshakeable position at the two main stools of Prague ethnography and demonstrated his allegiance to the post-1968 order (Anon 1972; Robek 1972d, 1972c, 1972b, 1972a).

At both institutions, Robek began to pursue personal policies by means of which he aimed to gradually replace and rejuvenate the staff of both institutions (Hlaváček 2017: 31–

⁸¹ It is appropriate to add that the Party hierarchy was rather opaquely interconnected with the university administration and the Ministry of Education hierarchy and it was not always possible to discern various overlapping hierarchies. However, the Party's interests superseded any others (Jareš et al. 2012: 137), but not always did the higher positioned won unequivocally (cf. Jareš et al. 2012: 176–177). This opaque interconnectedness is described as a specific feature of socialist systems by János Kornai (Kornai 1992: 36–40, 98–99).

⁸² A biographical introduction to an excerpt from Robek's diary states 1955 as the year when Robek became a lecturer (Hlaváček & Bortlová-Vondráková 2018: 271).

34).⁸³ The replacement process was gradual and rather protracted as it took some time before young ethnography students could be educated to replace their elder colleagues or fill the gaps. The following section will focus on the transformation from the point of view of personal continuities and discontinuities. Later on, I will discuss other aspects of Robek's reign.

3.4.1 Barriers to Entry: Pursuing Careers in Ethnography

3.4.1.1 Personal (Dis)continuities in Ethnography

When tracing personal continuities and discontinuities in ethnography, two factors played part after 1968. The first was emigration from Czechoslovakia. Vladimír Karbusický, a folklorist from the Ethnography Institute, chose this course of action. Holý or Stuchlík, who had been away when the August invasion of 1968 occurred, decided not to return to the country. The second factor was personal reorganization carried out by Robek and to this factor we turn now.

As regards some concrete cases, Robek dismissed Josef Wolf⁸⁴ from the Ethnography Department in 1973 (Křížová 2016: 317), but allowed the Department's previous head, professor Karel Dvořák, a purged Party member, to remain at the Department, and so was Soňa Švecová allowed to remain. At about the same time, Robek acquired employees of the Centre for Ibero-American Studies (*Středisko Iberoamerických Studií*; abb. CIAS). CIAS was on a verge of dissolution, but thanks to Robek's endeavours it was successfully incorporated into the Ethnography Department and continued to exist under its auspices, although none of the Centre's members was originally an ethnographer or folklorist. As a result of this merging, the department gained various scholars interested in Americas. Among these were historians Josef Polišínský, Josef Opatrný and Oldřich Kašpar and the linguist František Vrhel. The centre retained some autonomy as its employees were not employees of the department. Vrhel and Kašpar later became Ethnography's Department core employees who throughout the late socialist period represented strands within ethnography which focused on more exotic topics (Křížová

⁸³ Unless they decided to quit on their own accord as, for example, did Miloslav Stingl. Stingl, who feared Robek, then continued as a writer of popularizing travelogues and was not persecuted by the state in the late socialist period (Hlaváček & Bortlová-Vondráková 2018: 210).

⁸⁴ As far as I am aware, of all the ethnographers subjected to post-1968 purges, Wolf was the only ethnographer namely attacked in print (Hynková 1972: 196).

2016: 318, 331). Robek also acquired several other non-ethnographers who became members of the Ethnography Department for some time (Petráň 2015: 475).

At the Ethnography Institute, Robek dismissed its previous head Jaromír Jech alongside Vanda Tůmová, Věra Thořová and Petr Novák (Hlaváček 2017: 34). Later in the 1970s, Robek helped Stanislav Šisler, originally a historian and archivist who was after 1968 suspended from the Communist Party (Jareš et al. 2012: 275) and who was later in the 1970s dismissed from the Department of Auxiliary Historical Sciences and Archive Studies at the Faculty of Arts. Robek was able to secure a new place for Šisler at the Ethnography Institute (Motyčková et al. 1997: 167). Olga Skalníková was threatened to leave in 1972, but at Robek's intercession she was allowed to remain until her retirement and even after that she cooperated with the Ethnography Institute. Regarding Skalníková, Robek pushed through his decision against the will of the Party committee at the Ethnography Institute (Petráňová 2012: 235).⁸⁵

When considering personal policies pursued by Robek, we have to bear on mind the particular position in which he was. On the one hand, Robek seems to have been motivated to preserve scholarly continuity in both ethnography institutions. In order to accomplish his goal, he needed to secure his institutions with enough professionals. Several previous accounts of Robek's consolidation of Prague ethnography state precisely this. According to these accounts Robek could benefit from retaining people who were considered as outstanding scholars or lecturers or were prolific authors (Hlaváček 2017: 35; Jareš et al. 2012: 176, 231; Křížová 2016: 319).

However, the major problem was that many scholars who were in position to help to keep ethnography alive had supported the liberal development in the 1960s or had opposed the 1968 invasion and hence their standing was compromised in the post-1968 situation (Jareš et al. 2012: 71–73); and it was precisely for this reason that many institutes and departments faced threats of possible dissolution (Hlaváček 2017: 33, 36; Hlaváček & Bortlová-Vondráková 2018: 279; Thořová et al. 2005: 66). These were no empty threats. At the Faculty of Arts, for example, out of forty-two departments in 1969, the Faculty had only half the number five years later in 1974 as some of the departments were merged

⁸⁵ Proceeding from interviews with his narrators, Jiří Hlaváček writes that Robek aimed to rejuvenate both institutions and he dismissed all those reaching the retirement age allowing no exceptions (Hlaváček 2017: 36). This allegation is problematic for some ethnographers could continue even as part-time lecturers or co-workers after reaching their age of retirement (e.g. Skalníková or Dvořák). Nor does it ponder the idea that Robek's "rejuvenation" was rather an attempt to surround himself with reliable colleagues (see below) and that the talk of retirement could be invoked as a persuasive norm in cases when it was expedient to invoke the norm.

together and others dissolved (Jareš et al. 2012: 71–73). Besides, we also have to weigh up a possibility that Robek was more liable to dismiss people with whom he had not been on good terms,⁸⁶ and at least in one case, it was the State Security which intervened in the matters of dismissal (Jareš et al. 2012: 249, 251).

So, Robek was literally between a rock and a hard place as preserving disciplinary continuity by retaining professional scholars often contradicted the compliance with requests from higher circles. If Robek was to demonstrate his allegiance to the new situation and higher circles, he had to do it by adopting an uncompromising stance towards personal changes at both institutions. This particular position of his resulted into a specific two-face personnel policy.

This policy basically divided ethnographers in two camps depending on which face Robek turned to them. The first group had to leave the Ethnography Department and the Ethnography Institute and usually had to accept positions in minor ethnographic institutions, that is institutions less endowed symbolically with curtailed opportunities to research, publish and with no prospects of lecturing. This was the case of Vanda Jiříková who worked at a regional museum in Central Bohemia (Hlaváček & Bortlová-Vondráková 2018: 96), Petr Novák who then worked as a dramaturge of a national folklore ensemble or Josef Kandert and his wife Olga who moved to work in Námprstek Museum (Brouček & Jeřábek 2007: 161, 107–108). Others, such as Josef Wolf, had to accept jobs outside academia (Brouček & Jeřábek 2007: 259). Whereas the first group could be expected to bear grudge towards Robek, we could expect contrary feelings among the members of the second group consisting of people whom Robek “saved”. Given the fact that career opportunities were uncertain, Robek could create bonds of reciprocity with those whom he did not dismiss and those whom he retained on a full or a part-time basis, even if prospects of their further career advancements became inauspicious.

Nonetheless, judging Robek’s activities by the fact that neither the Ethnography Department nor the Ethnography Institute were dissolved or merged with other institutions and retained their autonomy, they make the impression of having been successful. Moreover, Robek’s ascension to power resulted in an outcome which was similar to that of the revolution in ethnography which had occurred some twenty years earlier: No radical personal revolution took place.

⁸⁶ This motive is suggested by Hlaváček based on information available in Petráň’s book (Hlaváček 2017: 35; Petráň 2015). Hlaváček’s suggestion does not directly portray Robek as having acted on the basis of his personal animosities, but it can be considered as a motive in cases of people sacked by Robek.

3.4.1.2 Newcomers to the Discipline

In the beginning of the 1970s Robek had at his disposal a medley of people with different scholarly backgrounds. Beginning with the early 1970s, Robek began to fill the vacancies and gradually replace those who survived the purges with a fresh supply of alumni of the Ethnography Department (Hlaváček 2017: 36). The barriers to entry which we consider now are related not to ethnographers who began their careers before 1968, but to the those who came after. There were two successive entries which anyone interested in careers in ethnography had to pass through in the late socialist period. The first entry, naturally, was the admission to study at the Faculty of Arts of Charles University from which the future ethnographers recruited. Most of the future ethnographers studied at the Ethnography Department, but we find cases of ethnographers who majored at other departments in different subjects such as cultural theory, library science or history. The second entry was an admission to either the Ethnography Department or the Ethnography Institute the precondition for which was a successful university graduation.

The first entry consisted of two written exams and an interview. Because of the state planning, the Ministry of Education stipulated the maximum number of admitted applicants in guiding numbers set for each university, faculty and discipline (Jareš et al. 2012: 301).⁸⁷ The applicants who successfully passed the admission exams had to wait for a verdict of the exam committee consisting of the staff of the Faculty. However, the exam committee did not have the final word in the process and the possibilities of applicants did not depend solely on the basis of their exam performance and impression they left during the interviews (Jareš et al. 2012: 305). It was further possible to get admitted to the Faculty by informal interventions which were commonplace at the time. One could improve his or her chances through the intercession of an influential relative or friend in party circles (Jareš et al. 2012: 306–307). Unsuccessful applicants could moreover appeal to rector of the University and the ultimate possibility was an appeal to minister of education. To demonstrate the erratic nature of this process, Jareš and his colleagues mention an example of a student applied for a twin degree in history-*národopis* in 1987. Out of the forty-one applicants, the student ranked second and the exam committee recommended her admission. However, the dean refused to admit her and her further appeal to the dean was to no avail. She was admitted

⁸⁷ The Faculty not always adhered to guiding numbers and accepted more students (Jareš et al. 2012: 303).

only after an intervention by minister of education (Jareš et al. 2012: 317). This is important to consider as Robek did not have admissions to ethnography under his sole control.⁸⁸

However, as regards the staffing of both institutions with graduates, Robek seems to have had more control than was the case of ethnography freshers, although both institutes were also limited from the above by vacancies set by state plans (Thořová et al. 2005: 66) and because in the case of the Ethnography Department the admissions had to be approved by the dean of the Faculty (Jareš et al. 2012: 198). Probably the most important tool for Robek was his supervision of students' diploma theses. Out of one hundred and thirty-one diploma theses defended at the Ethnography Department in the 1980s, Robek supervised eighty-two, Iva Heroldová, an employee of the Ethnography Institute, supervised one. The remaining forty-eight theses do not include any supervisors' names. Only three theses supervised by Robek also mention consultants of which only Jitka Nouřová was external to the Ethnography Department (Iva Heroldová being the other consultant).⁸⁹ This allowed Robek to get acquainted with the students from among whom he could carefully choose his future subordinates. Anyway, ethnographers whom I interviewed told me that it was Robek who approached them in final years of their studies and offered them to continue as lecturers at the Ethnography Department or as researchers at the Ethnography Institute. The recurrent scenario indicates that Robek took advantage of the unequal relation between him as the venerable professor and between his students to whom he manifested his trust and whom he must have, in a way, flattered.⁹⁰ The offer probably also derived its efficacy from the fact that there were far more graduates than vacancies at Robek's disposal and this could be viewed all the more so as an exclusive offer. Offer the position in a one-to-one manner

⁸⁸ According to recollections of one of my interlocutors, Robek was in a position to influence the admissions (see 4.5). This comes as a no surprise, since Robek was a high-ranking member of the university administration (see below).

⁸⁹ I took the information from the on-line catalogue of the Charles University Library (available at <http://ckis.cuni.cz>). I also checked some of the theses in the Library of the Department of Ethnology and found out that not all of them include supervisor names. I crosschecked the data available in the on-line catalogue with a list of theses defended between 1980 and 1988, compiled by Vladimír Chaloupka (Chaloupka 1991). Entries differ as to the dating (e.g. Pavel Klapetek's thesis is dated to 1960 by the on-line catalogue and to 1981 by Chaloupka's list, the thesis of Anna Kuželková is dated to 1983 by the former and to 1985 by the latter list), ten theses are missing from the online catalogue (theses by Nina Georgijeva, Kateřina Broučková, Irena Nývltová, Anastázia Prokopovičová, Vanja Ivanova Stojanová, Vlastimil Šafránek, Marie Pírková, Vladimír Chaloupka, Luboš Kafka and Barbara Šindelářová) and seventeen are missing from Chaloupka's list (theses by Tomáš Grulich, Marie Hartmannová, Irina Ivanova, Jana Macourková, Lubomír Procházka, Marie Špačková, Jitka Šafránková, Sylva Vokálová, Bohumila Fulínová, Judita Hrdá, Pavol Jurčik, Vesna Kolouchová, Dagmar Lachmanová, Hana Fišerová, Marie Kočandrlová, Světlana Juricová and Zdena Lenderová)

⁹⁰ P0022: 5, P0025: 1, P0029: 2, P0030: 2, P0032: 3.

Robek's "trafficking" with scarce vacancies could again create more durable bonds and Robek could in turn expect some loyalty from the chosen.

Not surprisingly, by the end of the 1980s, both institutions were populated mostly by alumni of the Ethnography Department who graduated after 1968. Even if Robek did not exert full control over the gates of the Faculty of Arts and of students admitted to study ethnography, he possibly had a better control over the gates of his disciplinary subfield in Prague. We can see that in the academic year 1987–1988, out of twelve lecturers, eight consisted of Robek and his colleagues who joined the Department in the 1970s and 1980s, the remaining four originally got to the Department via their previous CIAS affiliation.

3.4.1.3 The Communist Party and Ethnographers' Careers

When it comes to the underlying key according to which Robek allocated fresh graduates to his institutions, we may consider several possibilities. There existed a specific division of labour between lecturers (Charles University) and researchers (the Academy of Sciences). Robek might have weighed graduates' abilities as regards possible fulfilment of either role. What played an equal, if not more relevant role, was the amount of party capital, which, so to say, manifested in individual attitudes towards the establishment. In the two previous sections we have already seen the omnipresence of the Communist Party in the academic world. The salient criteria for assessment in the post-1968 purges of scholars were not scholarly merits, but previous political views and activities. Applicants to study at universities were subjected to a complex assessment by the applicant's grammar school, which also took into consideration applicants' political views and attitudes towards the socialist establishment (Jareš et al. 2012: 304–305). Let us now take a closer look on the role of party capital in ethnographers' careers.

In 1974 the Faculty of Arts approved a document which concerned the ideological profile of the Faculty. According to the document, the central role of the Faculty was education of future teachers for secondary and elementary schools and future cadres for public service, mostly for the ministries. In short, the Faculty played an important part in educating the future elite of the state and was expected to serve the socialist society at large. Research was not stressed as the Faculty's central purpose and those who would wish to pursue research careers should leave to the Academy of Sciences after finishing their degrees (Holý 2010: 13; Jareš et al. 2012: 139). This division was also strengthened by the fates of several people who were purged after 1968 and could not lecture for they were

viewed as ideologically compromised. These former employees were either assigned to research positions at the Faculty, were relegated to archives or were moved to the Academy of Sciences (Jareš et al. 2012: 70, 177; Petráň 2015: 474). Hence it is likely that active Communist Party members had better prospects in becoming lecturers as they were considered more trustworthy for carrying out educational tasks and more representative of the Communist establishment. By the end of the 1980s, we find the Ethnography Department populated by members of the Communist Party.⁹¹

As it has been stated in the introductory chapter, party capital could be acquired in several ways. First, it was the membership in the Communist Party. An individual could also become a member of one of the organizations connected to the rule of the Communist Party: The Union of Czechoslovak-Soviet Friendship, the Revolutionary Trade Union Movement or the Socialist Union of Youth. The premium was obviously put on the Communist Party membership, since, as a part of changes in the autonomy of the academic world after 1968, the new minister of education stipulated in 1969 that only the Communist Party members could become members of university convocations.⁹² This stipulation was also applicable to heads of departments and faculties, not mentioning other positions in the top university management such as deans, rectors and their deputies (Jareš et al. 2012: 52, 137). Concomitantly, the appointment of docents and professors, the two highest academic ranks, was supposed to proceed on the basis that the appointees met moral, political, pedagogic and scientific criteria. The applicants to the two ranks did not have to be Party members, but they were expected to manifest positive attitudes to the Party and optimally be members of other organizations (Jareš et al. 2012: 53, 92–93; Šima & Pabian 2013: 92).

Membership in organizations which were integrated in the rule of the Communist Party was not the only thing which was being considered. Equally important was how an individual was active in the organizations. Ethnographers were subjected to a biennial complex personal assessment which judged the commitment of a particular individual. These assessments, as their name implies considered several factors. Individuals were scrutinized according to how well they fulfilled their professional obligations, what their political views were, relations to the Communist Party establishment and membership in Communist organizations, their health conditions, family relations and character traits.

⁹¹ P0017: 4, P0021: 9.

⁹² University convocation (*vědecká rada*) is a body which approves long-term plans, study programmes and appointments of docents and professors.

Apart from these biennial assessments, individuals were subjected to other assessments before they were promoted (Jareš et al. 2012: 199–213).

If anyone wished to pursue a successful career in ethnography, he or she could improve his or her chances by becoming a member of some of the organizations which existed under the auspices of the Communist Party. To put it in an abstract way, it meant that the individual accumulated party capital which he or she converted into academic capital. Some positions or strategies of accumulation academic capital were proscribed to those who did not possess any party capital, while other strategies of accumulating academic capital were made easier as one improved one's chances of getting a stint abroad and having a possibility of getting into touch with relevant literature unavailable in Czechoslovakia at the time (cf. Jareš et al. 2012: 39, 225).

What is of no less interest is that contrary to the significance of party capital, Communist Party membership was not a necessary precondition for pursuing an academic career. I was able to glean several cases of Department graduates who were in the 1980s accepted to work at the Ethnography Institute and who were not members of the Communist Party. One of my interlocutors was forced to join the Party after finishing his doctorate, others were not pressed until after they finished their scientific candidature (that is obtaining the postnominal title CSc.). This uneven approach to Party membership signals that there were not some rigorous rules for joining the Party in relation to pursuing scientific careers. It rather seems that different criteria were evoked in individual cases and that there was involved some margin of manipulation of the criteria. I will return to this theme in Chapter 5.⁹³

⁹³ One of my interlocutors was admitted to the Ethnography Institute to study a yearlong doctorate, earning the prenominal title of doctor of philosophy (PhDr.). After that the interlocutor was forced to join the Party in order to be allowed to pursue a three-year postgraduate study crowned with the postnominal title CSc. (candidate of sciences, roughly equal to PhD.). The interlocutor refused and left the Ethnography Institute (P0030: 1). Another of my interlocutors was pressed to become a Party member only after having finished the postgraduate studies (P0034: 11).

The story of the first interlocutor suggests an interesting possibility as Robek could gain a lot by forcing the young ethnographer to become a Party member. One of the parents of the young ethnographer, whom Robek knew, had been deprived of Party membership after 1968 and Robek probably wanted to exploit the situation by offering the parent a decent career for the child, a career which the child could not have probably pursued at all given the fact that it came from a family of a previously purged Party member. Robek could achieve a symbolic victory by doing a favour to those whom history proved wrong and who fell into disgrace after 1968 (cf. Šimečka 1990: 133). However, Robek's plan misfired because the young ethnographer refused to join the ranks, left the Ethnography Institute and until the end of the 1980s had to work as a blue-collar worker. The interlocutor offers the possibility that the reason that blue-collar job in which the interlocutor ended up in was thanks to Robek's vindictiveness. It would not come as a surprise as Robek's generous gift was not accepted, and a gift unaccepted presents an insult to the gift-giver. On the contrary, there was not much for Robek to gain from others.

If we reconsider both admission procedures, we can better see the diminished degree of autonomy of the academic world in general and of the Prague subfield of ethnography and folklore studies in particular. It seems that intellectual capacities, or cultural capital of applicants and professionals presented only one of the things among others which mattered and which was considered. Moreover, both procedures were interlocked with mechanisms which were not under direct control of the academic world and which were, as Jareš and his colleagues repeatedly indicate, conducive to a rise of favouritism and nepotism reaching outside the field of ethnography and even outside the field of academia.

Before moving to other characteristics of Robek's reign we can see that Robek's personal policies had one important consequence not necessarily linked to the rule of the Communist Party. Robek staffed both his institutions with alumni of the Ethnography Department in Prague as these students were those over whom he could exert some control and whom he could mould to a certain degree. Though there existed some cases which disturbed these boundaries – such as Alexandra Navrátilová who had a chance to work at both branches of the Ethnography Institute (Hlaváček & Bortlová-Vondráková 2018: 152), the *Encyclopaedia of Vernacular Architecture* jointly written by the Prague-based ethnographer Josef Vařeka and the Moravian ethnographer Václav Frolec (Frolec & Vařeka 1983), or Ivo Frolec, a son of the latter, who graduated from the Ethnography Department in Prague – the circulation of alumni was rather restricted to Prague. This situation promoted scholarly endogamy in ethnography and further insulation of the three main subfields of ethnography – Czech, Moravian and Slovakian.

3.4.2 Struggles for Domination

3.4.2.1 The Command of Institutions

Not only did Robek exert control over the staff of both institutions, he commanded a host of institutional means and his whole reign is symbolized by a gradual acquisition of such means. Throughout the late socialist period, Robek was one of the stable members of the staff at the Faculty of Arts where he held several vice-dean posts (Jareš et al. 2012: 149–150). As head of the Ethnography Institute, Robek's powers reached outside Prague to Brno, where the other branch of the institute was located. Robek's deputy in Brno between 1971 and 1983 was Karel Fojtík and Alexandra Navrátilová between 1984 and 1988 (Thořová et al. 2005: 67, 76). By holding the two chairs, Robek was in control of educational means in Prague and research means in Prague and partly in Brno.

Robek also became chief editor of *Český lid* and president of its editorial board, officially in 1977, but he is thought to have been *de facto* journal's chief editor from 1972. Between 1972 and 1976, chief editor's duties were entrusted to Anna Pitterová (Woitsch 2013: 85).

In 1984, Robek revived *Národopisný věstník československý* and also became its chief editor. The *Bulletin* was issued once per year and it served as a prime medium for the Czechoslovak Ethnographic Society (*Národopisná společnost československá*), a professional organization which united ethnographers and folklorists from Czechoslovakia. The organization was headed by Helena Johnová and existed under the auspices of the Czechoslovak Academy of Sciences, which suggests, alongside Robek's involvement as the chief editor of the society's journal, who called the shots. Between 1984 and 1989, the journal published several essays, reviews and various reports, but was mostly used to publish proceedings from the state-wide ethnography conferences (Válka 2013: 114–115).

Robek also started a minor journal in 1974. It was mainly intended as an internal journal for the Ethnography Institute and it survived until 1991. It had rather a long title: *Zpravodaj koordinované sítě vědeckých informací pro etnografii a folkloristiku* (*The Newsletter of the Coordinated Network of Scientific Information for Ethnography and Folklore Studies*; further referred as to *Zpravodaj KSVI*). *Zpravodaj KSVI* did not seem to have any firm rules for publishing; in 1981 it was issued only once, in 1988 it was issued thirteen times. Some issues were intended as books, some as collections of mostly conference papers some of which included discussion contributions or minutes from subsequent discussions.⁹⁴

In addition to the journals mentioned above the Ethnography Institute had its own edition called *Národopisná knihnice*. It published monographs and edited volumes some of which were editions of conference papers some of which were not. It mostly catered for the needs of ethnographers from the Ethnography Institute. Robek was also involved in the edition called *Acta Universitatis Carolinae: Philosophica et Historica*, ran by the Faculty of Arts (and not under Robek's direct control). It was intended as a medium for scholars working at the Faculty of Arts. It published ethnographers' monographs and it also accommodated for ethnographers' semi-journal called *Studia Ethnographica*.

Apart from fulfilling their role of the prime means of communication within the professional community these media fulfilled two additional roles. Their second role was

⁹⁴ The journal is available on-line (http://zpravodaj.eu.cas.cz/uvod_podrobne.html).

symbolic as they represented the vigour of Robek's empire and testified to the productivity and performance of Prague ethnography. This role needs to be conceived in relation to the third role – the publishing outlets controlled by Robek published mainly works written by ethnographers working in the institutions under Robek's control or in institutions allied to Robek.⁹⁵ By reviving the annually published *Národopisný věstník československý* and by increasing the number of issues of the irregularly published *Zpravodaj KSVI*,⁹⁶ Robek created two new media which he had under his control and which offered publication opportunities to his network. By opening his journals only to some researchers, Robek could strengthen the bonds constituting his realm.

Robek also set his foot in other institutions and periodicals. He was a member of the editorial board of *Slovenský národopis*, but also became involved with innumerable regional museums, publishers and journals (Hlaváček 2017: 37). More importantly, Robek also established a close cooperation with the Czechoslovak Foreign Institute (*Československý ústav zahraniční*, abb. CFI). Jiří Hlaváček wrote that by establishing links with CFI, Robek secured himself extra funds for research as the institute had a different source of funding than the Ethnography Institute. Moreover, Robek gained important contacts as the institute's employees recruited from former diplomats and ambassadors as well as people from secret services. This also proved useful for the topic of ethnic processes as activities of the CFI were aimed at Czech and Slovak compatriots living abroad (Hlaváček 2017: 37).

⁹⁵ Out of two hundred and twenty-nine standard articles published in *Český lid* in the 1980s, only twenty-three were authored by researchers affiliated to institutions based in Moravia and only one included a Slovakian based author. Richard Jeřábek and Václav Frolec, two prominent Moravian ethnographers did never contribute to *Český lid* in the late socialist period, nor did prominent Slovakian ethnographers. Moravian ethnographers regularly published in *Národopisné aktuality*, a Moravian based journal and their writings sometimes appeared in *Slovenský národopis*, the major Slovak journal. This in a different way promoted the scholarly endogamy which has been mentioned earlier. Even if it would require further research, the situation was probably hardest for Moravian ethnographers as the copies of *Národopisné aktuality* were slim and the journal's typesetting offered relatively little space to its authors.

Among the contributors to *Český lid* we find ethnographers who worked for some regional ethnography institutions or scholars with different backgrounds who contributed on topics pursued by Prague ethnographers, such as the demographer Vladimír Srb, or Milena Hübschmannová and Eva Davidová, experts on Roma studies. It is likely that they were scholars who were part of Robek's network. Another thing which suggests that Robek used journals under his control to strengthen his networks is one issue of *Zpravodaj KSVI* which was designed as a collection of abstracts from a national student contest for the best diploma thesis in social sciences and humanities organized by the Faculty of Arts (9/1986). It suggests that Robek offered his medium to the Faculty as part of some exchange of favours.

⁹⁶ Contrary to seventeen issues of *Zpravodaj KSVI* printed in the 1970s, forty-six were printed in the 1980s.

3.4.2.2 State Planning

Probably the pivotal means of achieving domination in the field was the domination over state plans. Unfortunately, as far as I am aware, no one has proffered a comprehensive view on the state planning in Czechoslovak ethnography. Were the plans imposed as directives from above or did ethnographers have possibilities to interfere and contribute to the shape of the final version of the plan? What were the stakes for the planners and what were the stakes for ethnographers as well as for other participants involved in the formulations of plans and of their fulfilment? On what basis was the fulfilment of plans measured? Were the plans fulfilled or were they not? If not, what were the consequences? Even Doubravka Olšáková's recent and otherwise informative account of state plans in ethnography does not address these important questions and offers only partial answers to this questioning (Olšáková 2016). Here I draw on Olšáková's account as well as on an earlier work on socialist economies by János Kornai. Even if Kornai excludes budgetary institutions from his picture, such as universities or academic institutes (or the non-productive sectors), he maintains that the major features of the state planning are analogous to the situation of the state-owned firms (Kornai 1992: 114–130).

As regards state planning, plans were very likely not imposed as directives from the top and enacted by the responsible at the bottom, but their preparation involved a great deal of bargaining between various levels of Party hierarchy and those who were eventually responsible for carrying out the plans. As regards their major features, the plans probably set requirements and goals and provided funds for ethnography. This probably included the concrete topics to research on, the exact number of ethnographers working on the plan, finance to cover research and publishing costs, capacities of the printing works, and corresponding output defined by the time spent on research, numbers of conferences, articles and books. Mentions of the bargaining can be found in Olšáková's account or Daniel Luther's recollections (Luther 2006: 93; Olšáková 2016: 141).

The planning had one more important dimension. In the late socialist period, we find out that the planners authorized one ethnography institution which would supervise the enactment of a plan and serve as a coordinator of other ethnography institutions including university departments, academic institutes, museums and other cultural institutions. The coordinating institution found itself in a position to supervise large national institutions such as the Silesian Museum (*Slezské muzeum*), Moravian Museum (*Moravské muzeum*),

Wallachian Open Air Museum (*Valašské muzeum v přírodě*) Slovak National Museum (*Slovenské národné múzeum*) or Matica slovenská (Olšáková 2016: 146).

In the 1970s, the planners entrusted the Ethnography Institute of the Slovak Academy of Sciences (*Národopisný ústav Slovenské akademie vied*) and its head, Božena Filová, to supervise the state plans (Olšáková 2016: 137). During the 1970s, the Prague branch of the Ethnography Institute was rather marginalized and did not play any major role in the state plans in ethnography and folklore studies. The Ethnography Department did not take part at all (Olšáková 2016: 139). Robek was, however, able to turn the tables and beginning with the 1980s the Ethnography Institute in Prague became the coordinating institution of the seventh and eight five-year plans for ethnography designed for the periods of 1981–1986 and 1986–1991, if only after some debates and bargaining. According to Olšáková, the planners ordered the Ethnography Institute to shift their research work to the topic of ethnic processes and requested Robek to raise the numbers of ethnographers working on the topic (Olšáková 2016: 141). Although the planners stressed the importance of studying ethnic processes this did not cause a wholesale research reorientation and a concomitant reorganization. Robek was able to skilfully adjust the research fields previously pursued by Prague ethnographers (Czech National Revival, Slavic ethnography, working classes, socialist village and socialist society) so that they could subsist under the new research orientation throughout the 1980s (Olšáková 2016: 147). Hence some of the 1980s' ethnographers dealt with the role of village settlements in ethnic processes (Robek & Kadeřábková 1982) or with the role of working classes in ethnic processes (Robek & Moravcová 1982). Apart from these thematic crossovers, many articles did not say a word of ethnic processes at all.⁹⁷

It is more than likely that it was thanks to Robek's industrious activities that he was able to enlarge not only the numbers of ethnographers at the Ethnography Institute, but also the publication opportunities. The increased numbers of the *Zpravodaj KSVI* and the revived *Národopisný věstník československý* can be considered as fruits of Robek's accumulation of capital, or in other words, power. It is essential to stress that it was not easy to start a new journal under the conditions of state socialism. Contrary to the present times,

⁹⁷ So, when Markéta Křížová writes about Czechoslovak ethnography's sole focus on Czechoslovakia during the 1950s, a fact that she draws from the main research orientation of state plans at the time, her argument is not entirely valid. There was no necessary top-down connection between the plans on the one hand and actual research projects and works published by ethnographers on another, a fact that she admits a paragraph later (Křížová 2016: 312–313). Nahodil's manifold writings on non-European societies from the 1950s testify to this fact that ethnographers were not strictly bound by the topics set by the planning.

there existed several obstacles which hindered the establishment of new journals or the publishing of books. The Internet was still in its beginnings and the advantages of online publishing were not discovered yet. The official censorship could also play an important part, but it seems that the major limiting factor was the command economy which very likely limited the numbers of journals and books to be published. Anyway, if one wanted to raise the number of publication outlets, it was probably at the expense of other ethnography institutions and probably indirectly at the expense of other institutions as the total pool of resources to be allocated by the state was also limited by planning.⁹⁸

Becoming a coordinating institution of a state plan might have been the primary means of achieving domination within the field by securing funds not only necessary for the fulfilment of the plan. Increased funds might have allowed to raise the numbers of employees and hence the amount of work done, more writings published and hence a potential domination and increase in symbolic capital. Some accounts suggest that Robek was also able to raise the vacancies at the Ethnography Institute (Thořová et al. 2005: 66).

It is possible that Robek was able to enlarge publication opportunities as a consequence of his achieving the coordinating role. It seems that coordinating institutions of particular state plans were provided with more tangible and intangible benefits than all other institutions. It is very likely that the institution entrusted with coordinating the state plan also gained some control over the information about other participating institutions. Concerning state planning it is thence appropriate to speak of domination, that is achieving power at the expense of other competitors. That the stakes were high is testified by a quarrel between Robek and Bořena Filová, head of the Ethnography Institute of the Slovak Academy of Sciences (*Národopisný ústav Slovenské akademie vied*) at the time when her institute was the coordinating institution. Robek irately called into question of the eligibility of the Slovak institute to carry out state plans in ethnography and veiled his outburst into nationalist sentiments (Olřáková 2016: 137–138). While the animosities between the Czechs and the Slovaks might have been heartfelt, they were very likely enhanced by the fact that the role of the coordinator offered many other benefits.

⁹⁸ According to Antonín Kostlán (personal communication), beginning with the 1970s the funds available for the institutes in the Academy of Sciences were capped and the institutes had to struggle among each other to gain as biggest share as possible. Thus, a bigger share was always at the expense of others.

3.4.2.3 Robek as the “Chief”

The impression that a reader can get is that Robek exerted a direct control over a whole range of spheres. He held managerial posts, he controlled major journals, he was implicated in other institutions, he coordinated state plans, he supervised students and chose those who suited him the most for further careers in ethnography. If one listens carefully to narrators, their accounts further strengthen this impression. One of the interlocutors told me:

*... Robek wanted everything under control; and if there were any meetings at all, they did not last long. He usually came late or did not come at all. And if there was a so-called meeting, he made several points about running his errands [for the Department] and about what was to come. There was no discussion about that.*⁹⁹

Even though that the Ethnography Department consisted of Party members, it seems that Robek did not allow them to partake on administrative and managerial matters. It was likely the case that any possible participation on power could be achieved only by means of pursuing power struggles within the Communist Party but outside the narrow confines of the Ethnography Department or Ethnography Institute. Party membership was a necessary precondition and so was the appetite for playing games of power and climbing up the Party ladder. The sources do not indicate that most ethnographers were avid for playing such games. Moreover, this would inevitably lead to a possible conflict with Robek whose position was hard to contest. Robek had his deputies¹⁰⁰, but they did not figure at all in narrators' accounts as important people whose responsibility was to formulate policies or to decide. Some narrators mentioned that when Robek was by the end of the 1980s replaced by Bohuslav Šalanda as head of the Ethnography Department, Šalanda continued to visit Robek at the Institute and that it was Robek who actually continued to make decisions.¹⁰¹

⁹⁹ P0025: 5.

¹⁰⁰ In the annual lecture lists, two names figure as Robek's deputies at the department. The first was Jaroslav Šťastný who held the post until his death (1979–1982) and who was then succeeded by Bohuslav Šalanda (1982–1987). Vladimír Kristen was by some probably viewed as Robek's possible successor at the Ethnography Institute (P0032: 12).

¹⁰¹ P0029: 19, P0031: 12–13.

Robek also coordinated a close cooperation between the two institutions. Employees of the two institutes alongside students of ethnography joined in joint excursions. As a part of their degrees, students had to undergo shorter field trips usually led by ethnographers from the institute. Some ethnographers from the Ethnography Institute were also chosen to help students as external consultants to their diploma theses, a task to which they were very likely assigned by Robek.¹⁰² This collaboration had some limits as, perhaps for the politico-ideological reasons mentioned above, ethnographers did not lecture at the Department as it had been usual in the 1960s when Holý as an employee of the Ethnography Institute had lectured at the Ethnography Department as well as Stuchlík who had been based in Náprstek Museum. Speaking of the division of labour between the two institutions, one of the narrators recalled that Robek transferred Jan Pargač from the Ethnography Institute to the Ethnography Department. I elicited this information on the basis of a question whether it had been possible to switch between Ethnography Department and Ethnography Institute. The reply was that one had to “ask the chief” (*zeptat se šéfa*).¹⁰³

Also, ethnographers had to ask for permission even as regards almost trivial things. After Robek seized power, he discontinued the tradition of Christmas ethnography parties where alumni, students and professors regularly met (Hlaváček 2016: 117). However, from another’s narrator account, the parties were at some point in time revived:

I know that Oldřich Kašpar became intensively involved [in organization]. I am not sure, he might have organized the parties himself, but I am sure that Robek knew about that. Nobody would risk to organize anything like that without Robek knowing it. Not even people from CIAS, Josef Opatrný and professor Polišenský would dare to organize some secret shindig. And now some troubles without Robek knowing that! And he [Robek] was certainly invited.¹⁰⁴

To organize a party without Robek knowing it would have not been merely a faux-pas, a transgression of good manners and academic decorum as a senior colleague in a managerial position was not invited. What the narrator implied was that Robek had to know about everything that took place even if he did not personally turn up at the party.

¹⁰² P0014: 8–9, P0034: 18–19.

¹⁰³ P0022: 3. Pargač was probably transferred in the beginning of the 1980s. Pargač defended his thesis in 1973 and does not appear in lecture lists of the Ethnography Department until the academic year 1981–1982.

¹⁰⁴ P0029: 6.

Robek also had to resolve some troubles caused by students or colleagues. One of the cases worth mentioning is the case of Viktor Stoilov.¹⁰⁵ Stoilov was a student in mid-1980s. He wrote a critical appeal that reacted to an article unrelated to ethnography and folklore studies. Stoilov pinned the appeal to the notice board of the Faculty committee of the Socialist Union of Youth, but was spotted in that very act and reported. He was later interrogated by the State police in Robek's office at the Ethnography Department without Robek having been present. Stoilov, however, was allowed to finish his studies and successfully graduated. Photos from his research were later used and duly credited in Heroldová's article (Heroldová 1986: 222, 225, 232) and his thesis on the post-war immigrants in the village of Oloví was quoted (Nosková 1989b: 230). Stoilov also participated on an ethnography conference in 1986 proceedings of which were published later in 1988 in *Zpravodaj KSVI* (Stoilov 1988). As I was told by one narrator, Robek had a personal distaste for Stoilov, but this did not lead to any serious persecution even though Robek could have easily found a pretext for doing so.¹⁰⁶

3.4.2.4 Academic and Symbolic Mastery

Robek did not only pursue raw control as he also pursued academic capital via his scholarly activities. Robek began to earn his scholarly reputation in the 1960s. To mention only some of his writings, he and Otakar Nahodil published a book on folk superstitions (Nahodil & Robek 1959) and another one on the origin of religion (Nahodil & Robek 1961). Robek himself produced books on the history of ethnography (Robek 1964) and on social relations (Robek 1965). He continued to be academically active in the late socialist period, in which he wrote or co-wrote books on the Czech National Revival (Robek 1974a, 1977), ethnography of the working classes (Robek et al. 1981), on local traditions (Robek et al. 1987), on socialist village (Robek & Svobodová 1979) and innumerable articles touching almost every topic that was pursued by ethnographers in Prague at the time. His reputation reached outside the scholarly world as he also published a popular book on the history of the spa in the town of Poděbrady (Robek 1978a), and some time earlier even cowrote a book of bedtime stories for children (Vízdal & Robek 1968) and he also served as a consultant to the TV-series *Stepping into the Unknown (Krok do neznáma)* released in 1984

¹⁰⁵ I am grateful to Viktor Stoilov for sharing his story and for allowing me to mention it without anonymizing.

¹⁰⁶ P0034: 17–19.

and intended to smear Czechoslovak emigrants who left the country because of the Communist Party rule (Olšáková 2016: 142).

Robek's book on the history of Czech ethnography from 1976 (Robek 1976), a slightly edited version of his 1964 book can also be understood as an attempt to earn symbolic capital not merely by means of writing an erudite work. Since ethnography was primarily understood as a historical discipline, only those who mastered the history of their own discipline, could be appreciated as the discipline's true masters.¹⁰⁷ As Robek put it in the book, his account was by far (and apart from several minor writings) only one of two relevant accounts of the history of ethnography, the other had been written decades ago by Jiří Horák in 1932. Mentioning this exclusivity, Robek forced up the symbolic price of his own achievement (Robek 1964: 5, 1976: 5).

Some of Robek's symbolic capital stemmed from the fact that he was chief editor of *Český lid* was the most ancient from among the ethnography journals. The journal was established by the end of the 19th century by two prominent founders of Czech *národopis*, Čeněk Zíbrt (1864–1932) and Lubor Niederle (1865–1944) and its first issue appeared in 1892 (Woitsch 2013). We have already seen that Niederle's and Zíbrt's standing was not jeopardized by their having been prominent representatives of the bourgeois era of *národopis* and the journal's bourgeois roots could not be called into question. The symbolic capital of the journal stemmed mainly from three sources – its most ancient origins, it was founded by giants of the discipline and it was issued by the Academy of Sciences which was the main research organization of the state. Similarly, another journal under Robek's control, *Národopisný věstník československý*, possessed certain symbolic value too as it was considered a successor to journals established by Niederle and other founding fathers of *národopis* at the turn of the nineteenth century (Válka 2013). Though the poor appearance of the journal's incarnation which began to appear in 1984 could not by any means match that of its predecessors.

Robek was also the holder of the two most hallowed titles. He held the rank of *profesor* which was awarded by universities and he also held the rank of *doktor věd* (doctor of sciences) awarded by the Academy of Sciences. Even if these titles were generally awarded not solely on the basis of scholarly merits, they retained their symbolic aura derived from the scholarly world. Even if Robek fulfilled the political and moral criteria for

¹⁰⁷ This was also a symbolic strategy employed by young scholars who wanted to promote sociocultural anthropology from the early 1990s, Václav Soukup and Ivo Budil, who established themselves and became known as authors of books on history of sociocultural anthropology.

the career advancement as he displayed positive attitudes towards the Communist Party establishment, the titles in his case have also a basis in his scholarly merits.

By his students, Robek was not viewed as a stringent pedagogue. Even though that former students enjoyed other lecturers more, they remember Robek as a convivial lecturer who was often late to his lectures. An informant also told me that he could help students working in the archives as he had very good knowledge of archival materials.¹⁰⁸ One narrator also has the experience from lectures when Robek disagreed with an opinion of a student on an ideological basis. Robek corrected the student in a polemic manner from his own Marxist-Leninist point of view but did not punish the student in any other way.¹⁰⁹

3.4.2.5 Robek and the Limits of Intellectual Freedom

The picture of Robek who did not punish his students for their questioning of the official ideology or for their transgressions of the official order imposed by the Communist Party calls to inspect closely the status of Marxism-Leninism in ethnography and Robek's role. As it was asserted earlier, Marxism-Leninism was throughout the forty years of Czechoslovak Socialism the official doctrine of the Communist Party rule. The Party rule and the position of the official ideology can be aptly described by using Alexei Yurchak's notion of "authoritative discourse". The authoritative discourse was

"quilted" into a unified field of knowledge around three master signifiers – Lenin, the Party, and Communism. In fact, these three master signifiers were indivisible and mutually constitutive: the method for describing and improving reality was Marxism-Leninism (Lenin); the agent who used this scientific method to describe and improve reality was the Party; and the goal toward which the improvement was directed was Communism. (Yurchak 2005: 73)

The three master signifiers commanded esteem and it was impossible to publicly question the status of these signifiers, although as Yurchak persuasively shows, it did not imply that the populace widely embraced them in their private lives (cf. Kolář & Pullmann 2016). Marxism-Leninism at least officially gained the status of the one and only acceptable epistemic framework which necessarily influenced sciences and humanities. When it comes

¹⁰⁸ P0022: 3.

¹⁰⁹ P0033: 2.

to Robek, he tends to be portrayed by some as an ideological fanatic who by implication served as a guardian of the three master signifiers at least as regards ethnography.¹¹⁰ On the first glance, this portrayal seems to be precise and he appears to have played the role of ideological guardian well. His position entitled him to write prefaces for popularizing and specialist books published even outside his turf (Drössler 1980; Hubinger et al. 1985; Jazairiová 1987), editorials for *Český lid* or to give opening addresses during countless conferences and seminars. All of these are replete with libations to the official ideology and larded with flowery ideological phrases. In one of his editorials, Robek congratulated to the seventieth birthday of Gustáv Husák, at the time First Secretary of the Communist Party of Czechoslovakia and President of Czechoslovakia, in another he remembered an anniversary of the founding of the Communist Party of Czechoslovakia, and in yet another he mentioned the importance of the Great October Socialist Revolution for the development of ethnography and folklore studies (Antonin Robek 1981; Robek 1983a, 1987a).

Similar ideological libations can also be found in other texts of his colleagues (Brouček 1982: 8; Brouček & Vasiljev 1985a: 74, 78; Šalanda 1986: 24). And some other articles can be also perceived as offerings to the official doctrine by the virtue of being topically connected to some ideologically important event such as the Great October Socialist Revolution (Nosková 1987) or about the crisis of the Russian evolutionism which was surmounted by Marxist-Leninist ethnography (Kristen 1987). However, by far, writings which contained such libations or offerings to the official ideology do not seem to have been commonplace at the time and the majority of ethnographers' texts dispensed with such ideological offerings. It can be argued here, that many more writings which are free of such libations are actually based on Marxism-Leninism described in the previous chapter. Yet even in this case, there is a huge difference between more and less explicit uses of Marxism-Leninism. Apart from ethnographers' ideological libations, conscious and explicit use of Marxism-Leninism can be demonstrated in the work of Bohuslav Šalanda (Šalanda 1980b, 1984, 1986, 1989a) whose texts frequently refer to the classics of Marxist-Leninist ideology – Marx, Engels, Lenin and Nejedlý – and which are suffused with Marxist-Leninist jargon. Nonetheless, the absolute majority of writings makes do without any explicit references or jargon. An apt example is a book by Irena Štěpánová on clothing in Benešovsko region (Štěpánová 1987). Štěpánová's book opens with a description of

¹¹⁰ This is Skalník's view who calls Robek as a "communist nationalist fanatic" (Skalník 2005b: 14, 2018: 6) and it is also implied by Josef Kandert and David Scheffel who see in Robek a "chief ideologue" (Scheffel & Kandert 1994: 22).

ecologic conditions of the region and then shows how these conditions influenced local economy which in turn had an impact on the specific development of clothing. Her account makes do without any explicit references to Marxist-Leninist canon and the only trace of Marxism-Leninism which we can trace in her work is the materialist explanation of the sartorial development in the region and the specific conception of Czech history in the nineteenth century which we encountered in Chapter 2.

Moreover, not only do we find a lack of explicit Marxist-Leninist allegiance in the majority of texts, we also find some themes which seem to go against the dominant ideology, that is against the three master signifiers. In several articles on socialist villages and ethnic processes in the post-war Czechoslovakia, we find remarks critical of the collectivization policies. Collectivization policies were forcefully carried out by the Communist Party in the late 1940s and early 1950s. The policy of collective unified agricultural cooperatives, which emerged as a result of the collectivization, was one of the central pillars of Czechoslovak socialist economy until 1989 (Kornai 1992: 76–83). A good example of an attitude which questioned the effects of the collectivization is an article on marriage in a highland village by Nad'á Valášková (Valášková 1984). Before the collectivization that begun with the Communist Party's ascent to power, the private ownership of agricultural land had been perceived as desirable as it promised economic security and independence. As Valášková writes, this ceased to be the case after the forced collectivization that took place in the early Socialist Czechoslovakia. As a result of the collectivization, younger generations ceased to be interested in agriculture and sought for jobs outside of it. For similar reasons, women began to be less interested in choosing partners with jobs in agriculture (Valášková 1984: 41). A bit sharper attitude towards the repercussions of the collectivization of the rural areas can be found in the articles written by Antonín Jiráček (Jiráček 1982) or Iva Heroldová (Heroldová 1984). Jiráček even went as far as to mention voices critical of the collectivization in the USSR and Czechoslovakia (Jiráček 1982: 150–151). These articles contain references to an ambivalent reception of the collectivization among the people subjected to the collectivization policies. It is interesting that these articles written for *Český lid* voiced critical attitudes of farmers and were not accompanied by any clarification related to the false consciousness of the farmers affected by the collectivization or about their exposure to imperialist propaganda.¹¹¹

¹¹¹ The question, which I did not pursue, is whether such critical attitudes were possible only within the field of ethnography or whether they were possible within the confines of the public discourse.

Similarly, one article contained critical remarks of a factory management (Secká 1987: 204, 205, 207) and several others mentioned not always positive relations of the Czech majority to ethnic minorities (Nosková 1984b: 156; Secká 1987: 204; Valášková 1989: 141). Even though that some of these critical remarks were conveyed in the form of direct quotations from interviews or from village chronicles, they were not accompanied by any criticism on the part of ethnographers. Several texts (e.g. Nosková 1984a: 78–89; Valášková 1982: 159–160) also contained references to spiritual culture of the immigrants this time without being accompanied by a criticism of religion or superstitions, which had been Robek's speciality in the 1950s and 1960s. A picture of boy Scouts appeared in one article, even though Scouting was prohibited in socialist Czechoslovakia (Kristen 1986, fig. 37).

A rather curious situation obtained in relation to Western anthropology. In the previous chapter, we have seen that beginning with the 1950s, Western anthropology was considered as ideologically suspect and anyone who would openly refer to, quote from it or adopt ideas from it in an affirmative manner would expose himself or herself to a possible punishment. Even in the more relaxed 1960s, not all ethnographers would openly acknowledge inspiration from Western anthropology.¹¹² A case of Václav Soukup illustrates the intellectual climate perfectly. Soukup, who in the 1990s became one of the harbingers of social anthropology in the Czech Republic, had originally studied culturology at the Faculty of Arts where he had completed a dissertation thesis in 1981. In his thesis, Soukup had mentioned Anglo-American social and cultural anthropologists and unfortunately for him, Robek had been appointed as his examiner. During the thesis defence, Robek had pounded the thesis on the desk and yelled: “The bourgeois anthropology does not belong here!” (Půtová 2017: 147).

This story tallies well with the role of the Faculty of Arts as an institution for educating the future elites of reliable cadres and the incident gives us a good view on the ways of subjecting students to ideological discipline.¹¹³ Soukup eventually earned his degree and later published an article on intellectual currents within the “bourgeois anthropology”. Soukup in his article outlined each of the currents and accompanied the outline with a criticism from the Marxist-Leninist perspective (Soukup 1986). However,

¹¹² Jiří Woitsch mentions that Kramařík on his foreign visits to Sweden and West Germany in the 1960s excerpted parts from books by the American anthropologist Julian H. Steward, who, as Woitsch indicates, did not appear in Kramařík's articles or books (Woitsch 2016b: 18).

¹¹³ Another fact that might have played a role in the defence was that there had probably existed some rivalry between various departments and personal animosities that cut through the Faculty.

Barbora Půtová, who reported on this incident, was probably unaware that at the same time, Robek allowed “bourgeois anthropology” to appear in articles in *Český lid*. A case in point was František Vrhel’s article on cognitive anthropology (at the time also called ethnoscience) which used to be one of the branches of American anthropology in the second half of the twentieth century. Vrhel’s short text included references to W. H. Goodenough, F. G. Lounsbury, S. Tyler, W. Sturtervant or A. F. C. Wallace and also quoted Thomas Kuhn. These names were not accompanied by any Marxist-Leninist commentary as in Soukup’s case. Moreover, Vrhel was sharply critical of Marxist ethnography for it paid “daringly little attention to cognitive anthropology” (Vrhel 1985: 92). Given Soukup’s story above, it is almost a miracle that Vrhel’s article made it to print in the most important ethnography periodical of the time. Similarly, an article by Vladimír Kristen supported his argument on the development of evolutionism by a reference to Evans-Pritchard’s *Social Anthropology* (Kristen 1987: 135, 137). Last example concerns the candidate thesis of Václav Hubinger on the emergence of the state in Indonesia. The thesis listed among the sources works by Louis Althusser and Marshall Sahlins, authors related to the tradition of French Marxism. Although Robek seemed displeased with their presence in the text, from Hubinger’s recollections the defence was strenuous and eventually successful, but it does not seem that Robek’s reaction resembled the one during Soukup’s defence.¹¹⁴ This points out to a possibility that Robek behaved towards ideological transgressors differently according to whether they came from the ranks of ethnographers or not.¹¹⁵

We even come to a point where the status of the official ideology is reversed. One of my interlocutors suggested that writing an ideologically-laden article was a way of obtaining some extra benefits. It seems that ethnographers were not punished for an inadequate demonstration of allegiance to Marxism-Leninism but could gain some extra advantages by the demonstrating their allegiance even if, as it seems, they normally kept their distance from an explicit and open allegiance to Marxism-Leninism.¹¹⁶ This was probably a line which divided two different strategies. Those who sought for power were

¹¹⁴ I am grateful to Václav Hubinger for this information.

¹¹⁵ Another possible layer in Robek’s different approach was the different standing of Soukup and Hubinger. Soukup was at the time a regular student and it is likely that it was necessary to expose him to ideological disciplining. Václav Hubinger was above the level of regular students and he defended his candidate thesis which was considered as a research degree and was above the general university education so that the setting of the defence might have been more relaxed as regards ideological demands.

¹¹⁶ P0034: 19–20.

probably more inclined to mentioning Marxism-Leninism in a positive light, the rest did it only occasionally if they could obtain some benefits.

Of course, the criticism of the three master signifiers had its insuperable limits. Vrhel in his article did not address any academics and their work in particular, but offered only a brief and somewhat vague critique of Marxism-Leninism (Vrhel 1985: 92). Nor did Vrhel attempt to demonstrate the strengths of the cognitive approach and weaknesses of Marxism-Leninism by a thorough analysis of particular arguments. Cognitive anthropology was merely outlined in the article and Marxism-Leninism was not dismissed as such. Also, the readers can notice that articles which mentioned the collectivization or focused on ethnic processes in Czech borderlands rather effaced the role of the state and its apparatus from their accounts in a similar way that anthropologists had effaced the role of the colonial administration in colonies (cf. Asad 1973; Rosaldo 1986). For similar reasons, some writings could not even get into print. Ethnographers Helena Bočková and Jan Pargač witnessed a circumcision ceremony among the Turks in Bulgaria in 1980. They intended to write an account of the ceremony to *Český lid*, but Robek did not allow it as there were officially no Muslims and Turks in Bulgaria and he wanted to maintain good relations with Bulgarian officials (Hlaváček & Bortlová-Vondráková 2018: 35).

Anyway, even the cases given above, we are in a dire need to reconsider the claims which relate to the status of the all-pervasiveness of the dominance Marxist-Leninist ideology. In the previous chapter, we have already seen that the intellectual framework of Marxist-Leninist materialism was infringed by Bromley's theory of ethnos and its idealistic inclinations. The cases which have been listed in this chapter, shed further light on the unevenness in attitudes towards Marxism-Leninism.¹¹⁷ As Jiří Woitsch, I believe rightly, argued, it seems as if Robek in his editorial role also assumed the role to perform the necessary libations himself, liberating his colleagues from the necessity of doing the same in every single article that was written for *Český lid* or *Národopisný věstník československý* (Woitsch 2013: 86).¹¹⁸ Nonetheless, it is important to add that it was not even necessary for

¹¹⁷ It is interesting to note that ideological libations which we find in texts of Bohuslav Šalanda, Stanislav Brouček and Vladimír Kristen correlate with their role. They recruited from among younger generations of ethnographers who were probably also considered as reliable cadres who could one day replace Robek. Šalanda acted as Robek's deputy at the Ethnography Department, Brouček was a head of one of the sections at the Ethnography Institute and Kristen was by one of his contemporaries described as Robek's successor at the Ethnography Institute (P0032: 12). The relatively higher incidence of explicit Marxist-Leninist features in their works is probably a result of their roles.

¹¹⁸ *Zpravodaj KSVI* did not contain much ideological idiom since majority of its issues were primarily designated only for the employees of the Ethnography Institute.

Robek's colleagues to perform any libations in their books for which Robek did not even write a preface. It cannot be said that it was necessary for every Prague ethnographer to profess their ideological allegiance to Marxism-Leninism in their writings. This slackness in ideological discipline possibly had one essential consequence – it could earn Robek a good reputation among his colleagues. They could perceive him as a chief who provided them with a valuable leeway for ethnographic enquiry outside some strict ideological control.¹¹⁹

3.5 A Bourdieusian Lesson

The situation in Robek's turf calls for some comparative insights which, contrary to expectations, reveal to us that the power in the totalitarian state instead of having presented a monolithic force, manifested rather unequally. When we, for example, compare Robek to his contemporary Vítězslav Ržounek, head of the Department of Czech and Slovak Literature at the same Faculty in the same period (Jareš et al. 2012: 372), we can draw some important, although limited, conclusions. Both Robek and Ržounek were entrusted by higher circles to be in charge of departments at the Faculty in the post-1968 order, yet they pursued different ways of domination. While Robek got rid of unwanted colleagues in the early years of his reign, Ržounek continued to repress colleagues who survived the post-1968 Faculty purges. Compared to Robek, Ržounek's actions were almost unpredictable and his tools were more direct, crueller and excessively and openly ideological. Ržounek's actions created discord within his Department and this eventually led to his downfall in the second half of the 1980s (Holý 2010; Jareš et al. 2012: 225–230). In this light, Robek's managerial strategies appear smarter and softer and they relied on more indirect exercises of power by creating durable interpersonal bonds. Robek demonstrated far greater deal of ideological tolerance and this probably helped him to achieve domination on his academic turf and support from his colleagues.

¹¹⁹ Unfortunately, I have not been able to elicit much systematic information about the role of censorship and self-censorship which some of my interlocutors suggested. There are several reasons for why I paid a little attention to it. As the hackneyed story goes, authors during the times of socialism were compelled to squeeze ideological formulas and libations into their works as to appease the censors. Some might have done it out of persuasion, some as a pre-emptive step, others on the basis of censors' recommendation. However, as I have tried to show it in this chapter, the story does not tally with my experience with ethnographers' writings. Not all of their writings contained ideological idiom or claimed allegiance to the political system and its master signifiers. Quite surprisingly the ideological moorings were rather slack if we judge the situation by published writings.

Robek successfully pursued integrative strategies. On the one hand, he was able to fulfil desires of the post-1968 apparatchiks in power. He got the Ethnography Department and the Ethnography Institute ridden of inconvenient colleagues and marginalized those who did not have suitable cadre profiles. Yet at the same time Robek allowed many of them to continue and saved others who were threatened with existential difficulties. He extended his dominion by enlarging the publication opportunities and hence by proportionally reducing opportunities of Moravian and Slovakian ethnographers. He spiced his strivings with his own scholarly and symbolic pursuits. By the end of the 1980s, young ethnographers whom he chose and employed at the Department and the Institute represented a decisive majority.

In his *Logic of Practice*, Bourdieu drew a distinction between two modes of domination, between the elementary form of domination, which Bourdieu observed among the Kabyle people in Algeria, and the form of domination that can be found in modern societies. Of the former which corresponded to pre-modern societies he wrote:

A man possesses in order to give. But he also possesses by giving. A gift that is not returned can become a debt, a lasting obligation; and the only recognized power – recognition, personal loyalty or prestige – is the one that is obtained by giving. In such a universe, there are only two ways of getting and keeping a lasting hold over someone: debts and gifts, the overtly economic obligations imposed by the usurer, or the moral obligations and emotional attachments created and maintained by the generous gift... (Bourdieu 1990: 126)¹²⁰

Without the existence of institutionalized mechanisms that make it possible to exercise durable domination in modern societies:

the dominant agents cannot be content with letting the system that they dominate follow its own course in order to exercise durable domination; they have to work directly, daily, personally, to produce and reproduce conditions of domination which even then are never entirely certain. (Bourdieu 1990: 129)

¹²⁰ The workings of this system based on personal interactions within a highly rationalized and formalized system of domination of the state Socialism is splendidly depicted in the film *The Teacher* (2016) by Jan Hřebejk and Petr Jarchovský.

These parts that Bourdieu wrote on Kabyles apply pertinently to Robek. The power network which he constructed evinces all signs of elementary systems of domination. This is not to say that the academia of the late Czechoslovak society was not also based on objective, institutionalised mechanisms of domination based on formal rules and procedures and officially recognized forms of capital, especially the party capital. These objective mechanisms did not have to be recreated personally on a day-to-day basis (Bourdieu 1990: 130). Robek's sphere of possible action was limited by these mechanisms of the apparatus of the modern socialist state. These mechanisms he did not create himself and could not by himself change or abolish. Yet as a true Kabyle master he fully availed himself of the opportunities present to him by pursuing strategies that evince all signs of elementary modes of domination.

This is what allows us to connect various claims about Robek and his rule. The fact that the overall picture of Robek is ambiguous is only a result of his elementary strategies of domination. Robek's actions were structured by the possibility that the situation allowed him. When it was necessary or when a situation contained possible benefits, Robek could force a candidate to become a Party member. Where no benefits could be obtained, a candidate was not forced to become a member. He did not require his colleagues to be openly adherent to Marxism-Leninism, but he rewarded those who occasionally wrote something ideologically-laden. It was his very actions that created the research freedom for some, but destroyed it for others. Hence it would be a mistake to consider him either as a wrecker of the discipline or its saviour in the uneasy times of the late socialism. Robek was both. He gave to some and took from others.

In the retrospect it might seem almost as a miracle that the ethnographic empire that he was patiently building for almost two decades fell unexpectedly quickly, but here we have to remind ourselves that regardless of the durability of his personal networks, Robek was still at the mercy of institutionalized mechanisms of the state and the Party.¹²¹

Robek's empire began to crumble in the second half of the 1980s well before the Velvet Revolution. In 1986, Antonín Vaněk a new dean of the Faculty of Arts, announced faculty-wide personnel changes. Robek was first removed as a vice-dean and then was

¹²¹ Jiří Hlaváček suggests that Robek's downfall can be partly attributed to his previous success in accumulation of power. The increasing accumulation of capital of various sorts served as limiting of itself as it burdened its possessor with innumerable duties. Hlaváček observed that Robek's articles from the 1980s contain sentences that do not make much sense. Robek was probably too overwhelmed with keeping his own network of domination that he could not devote enough time to fulfil all of his obligations, including writing (Hlaváček 2017: 38–39).

replaced with Bohuslav Šalanda as head of the Ethnography Department (Petráň 2015: 612–613). Jiří Hlaváček says that Robek left because of personal quarrels with Antonín Vaněk which stretched back to the mid 1970s (Hlaváček 2017: 41). It is appropriate to repeat that it is very likely that Bohuslav Šalanda, who was appointed as Robek's successor at the Ethnography Department, closely cooperated with Robek after his departure, Robek lost a direct control over the Ethnography Department and over Faculty matters. Another loss came in 1988. The Brno branch of the Ethnography Institute was merged with other Academy institutes which researched on social sciences and humanities into a single Institute of Slavonic Studies based in Brno. While the Institute of Slavonic Studies remained a part of the Czechoslovak Academy of Sciences, its research was to be severed from that of the Ethnography Institute (Thořová et al. 2005: 77–78). Without delving deeper into the issue, it is possible that this reorganization was a result of struggles within the Communist Party which could, among other things, aim at divesting Robek of a substantial part of his empire. As it is evident not only from the history of socialism in Czechoslovakia, reorganization of various institutions, local authorities or organizations from above serves as a suitable means of power struggle. Thence it is obvious that the formal mechanisms of domination were able to easily exert more lasting influence than Robek himself was able to exert in his impressively built sphere of influence. Robek could fruitfully strive for domination as long as he could rely on the support within the Party.

At the Institute, there were ethnographers who were obliged to him in one way or another and Robek could continue with the maintenance of his sphere of influence. Unfortunately for him, the Velvet Revolution was too powerful an event and he could not prevent his downfall. Sometime after the Velvet Revolution erupted in November 1989, the Institute went on strike just when Robek was in West Berlin as a plenipotentiary of the Czechoslovak Foreign Institute. The employees of the Ethnography Institute also invited ethnographers who were purged after 1968 and together they deprived Robek of his key managerial post. It is likely that without a help from those colleagues, Robek would survive a few more years. But it is unlikely that he would have survived for long as he had been one of key people of the late socialist academic nomenklatura. The problem was that the enormous volume of party capital which Robek accumulated during his reign acquired a negative charge after 1989. Not surprisingly, after he was stripped of his last managerial post, Robek left with a feeling of injustice and hurt (Robek in Hlaváček & Bortlová-Vondřáková 2018: 280–282).

4. Being an Ethnographer: Some Aspects of Ethnography as a Scholarly Practice

Having inspected the power relations which shaped and influenced the field of ethnography and folklore studies and having inspected the degree of autonomy of the field, we are now better equipped to understand ethnographers' academic and scholarly practices, that is, practices which perpetuated ethnography as a discipline on a daily basis. While not all of these practices emerged as a direct consequence of the specific relations which I have described in the previous chapter, it is important to bear on mind that ethnographic scholarly practices were to a certain extent shaped by these relations.

Chapter 2 claimed that ethnography shared its intellectual roots with sociocultural anthropology as the opposition between idealist and materialist approaches was formative for both disciplines. Nonetheless, their similarities are not exhausted by the mere fact that both disciplines share the same intellectual roots and use similar intellectual tools. Ethnographers also undertook fieldwork. They spent some time in localities with people which they studied. They observed practices, interviewed interlocutors and filled questionnaires with them. Since ethnography was understood as a historical discipline, spending some time digging in the archives was also an important method of data collection. Research methods of data collection were usually determined by the requirements of the subject matter. Those who researched among ethnic groups usually went afield, those interested in more historical topics frequented archives. Regardless of the subject matter or temporal focus, ethnographers also depended on secondary sources written by ethnographers and scholars native to other fields such as history or linguistics. Ethnographers also took pictures, drew sketches and blueprints or collected and catalogized artefacts as ethnography was also keenly interested in material culture. They also organized exhibitions and partook on other popularizing activities. As regards their findings, ethnographers attended conferences where they presented and debated the findings with colleagues from within and outside ethnography. They also wrote articles which they sent to various ethnography and other journals where the articles came under the scrutiny of editors and reviewers before they were published. Ethnographers also wrote and published books and textbooks. Their colleagues read the books and wrote reviews which would appear alongside regular articles in specialist journals. As regards other minor genres,

ethnographers also wrote reports from various events including conferences or exhibitions, congratulated their colleagues to their birthdays or wrote obituaries to honour the deceased. So far, nothing dissimilar from anthropology's practice. While this enumeration of practices, which will be explored in more detail throughout this chapter, is necessarily incomplete, it points out to the fact that the bread and butter of ethnography professionals was generally not that dissimilar from the bread and butter of their colleagues in anthropology. Aside from similarities, this chapter and the two which follow it will also attempt to focus on some of the most important peculiarities of ethnography, on practices which were not always similar to practices that we could and can observe among anthropologists.

4.1 Another Meaning of “Ethnographic”

Chapter 1 devoted a few paragraphs to exploring the semantic multivalence of the word “ethnography”. Here I will elaborate on another layer of meaning, which I did not introduce earlier and which I reserved for this chapter. A comparison with anthropology will help to clarify the matter. When we speak of anthropology as a science, we usually mean anthropology defined as a set of methods and theories. Ethnography was also considered as a science and it also had its theory and methods which were designated as ethnographic. But in addition to this there was one extra set of phenomena designated as such. When ethnographers used the adjective ethnographic (*etnografický*), they did not only mean some scientific or research approach to some phenomena in the world, they also designated the phenomena to be studied as ethnographic.

This usually transpires from reading ethnographers' writings in which they spoke about ethnographically rich regions which for them had usually been various parts of South or Central Moravia or South or West Bohemia (Robek et al. 1987: 7) or some smaller regions like the region of the Giant Mountains (Robek 1985a: 4) and compared them to ethnographically poorer regions such as Central or Northern Bohemia even if the comparison remained rather implicit (cf. Pargač 1988: 16ff.). This means that these regions with their cultures and traditions possessed some intrinsic value as such, that they were interesting *per se* and an ethnographer could only more or less appropriate ethnographic phenomena which he or she encountered in these regions and that the ethnographic phenomena had existed prior to any research. Finally, it were not only regions, customs or

artefacts which could be designated as ethnographic, even various groups of people were referred as to ethnographic groups (Šatava 1981b).

This use, as far as I am aware, is absent in anthropology. Anthropologists indeed have regions or groups which determine their respective specializations. As an ethnographer could specialize in the agricultural tools of Haná region, an anthropologist might have been similarly interested in kinship in the Highlands of Papua New Guinea. As a folklorist might have wanted to collect folk tales among the peoples of Kysuce, an anthropologist might have pursued a specialization in healing practices in East Africa. These specializations cover larger and smaller regions and various dimensions of human existence such as economy, family and kinship, material culture, religion etc. However, it does not make much sense to speak about anthropologically rich and anthropologically poor regions. Contrary to anthropology, if a phenomenon was to be designated as worthy of ethnographers' interest, an ethnographer must have been aware of its ethnographic value in the first place. And this value had existed independently on any ethnographic research.

This somehow mirrors the differences in development of both disciplines in the twentieth century. Even though anthropology began as a study of the vanishing native societies which were deemed especially worthy of anthropological interest, as the writings of Tylor, Malinowski or Lévi-Strauss testify, anthropology successfully transformed into a universal science of human beings which it had claimed to be at least since Tylor and his postulate of the universal human mind. A similar movement towards recognizing modern ways as being worthy of study can be discerned in ethnography. Perfect examples are topics which emerged with ethnography's appearance in Czechoslovakia in the late 1940s such as ethnography of the working classes, ethnography of the socialist village or ethnography of the socialist way of life. In spite of these developments, ethnographers continued to be slightly prejudiced towards the modern ways of life up until the late socialism, traces of which can be recognized even today among ethnologists.¹²² If ethnographers studied modern ways of life, they usually did it on the basis of tracing temporal transformations of past traditions. They wanted to show which elements lost their original functions under the conditions of socialist life and became survivals and which of the elements could be transformed and remain functional¹²³ under the new conditions of socialism. In this sense,

¹²² This reminds me of an interview which turned to recent trends in studying kinship among the Roma peoples. My interlocutor wondered whether there was anything to study since the traditional Roma kinship system had disintegrated.

¹²³ Ethnographers of the late socialist period frequently spoke of customs, rituals, actions, habits, material objects, buildings or groups as of having some function (cf. Frolec 1984; Haišman et al. 1983; Krupková

the scope of ethnography alongside its purportedly engaged attitude was closer to works in the sociology of modernization (cf. Levine 1965).

Unfortunately for the reader, I will not venture further in this direction and apart from a few indispensable remarks I will leave the matter open for further enquiry. The important thing to stress here is that the notion of ethnographic value gave rise to specific kind of ethnographic texts. *Český lid* had a stable section called Materials Section (*materiály*). The section mainly published shorter contributions about interesting or newly discovered artefacts, manuscripts, songs or structures which contributing ethnographers thought to have been worthy of their colleagues' interest and of further investigation. Contributions to the section were shorter and were often accompanied by pictorial supplements and the section was not intended as a section for original contributions. Other major ethnography journals did not have an independent section of the same name, but articles of the same substance appeared there. *Zpravodaj KSVI*, one of the three journals controlled by Robek, was mainly oriented to publishing material contributions.

Another such kind of writings were books which we can call *anthologies* and which were compiled mostly by Department ethnographers. It demonstrates the very same attitude as materials articles. One of these anthologies listed Spanish and Portuguese printings available in the National Library (Kašpar 1984) and another was a collection of Czech ethnographic texts and travelogues from the eighteenth and nineteenth centuries (Kašpar 1983a). Also mentioned can be a selection of Mesoamerican folklore (Vrhel & Kašpar 1984), collections of contemporary folktales (Šalanda 1989b), popular theatre plays from the era of the Czech National Revival (Sochorová 1987a) or Czech legends and fairy stories (Dvořák 1984). These anthologies were usually introduced by the ethnographer-anthologist, but the introductions did not usually proffer some elaborated view on the collected material and the material was almost expected to speak for itself.

Even some regular articles which appeared in *Český lid* and other ethnography journals evinced the very same "materials" attitude. Some articles contained pictorial

1984; Nosková 1984a; Pargač 1988; Šalanda 1980b). Traditionally in anthropology and sociology, functionalism postulates "a purpose without a purposive actor" (Elster 1982: 454). Such an approach was typical of Radcliffe-Brown and Malinowski and can be traced in works of their successors especially in theories of religion and society (cf. Malinowski 1992; Radcliffe-Brown 1952) and, as Elster argues, this is also a hallmark of Marxist theories. Contrary to this restricted usage, ethnographers spoke of function quite loosely and their use of the term also encompassed any purposeful, pragmatic, deliberate or utilitarian activity. This use went hand in hand with an underdevelopment of the distinction between ethnographers' and natives' point of view. The distinction between the scientific and the native point of view in relation to the concept of function was nonetheless not entirely alien to ethnography, because it underpinned the Marxist-Leninist distinction between superstitious and scientific beliefs (see Chapter 2).

supplements or larger excerpts from texts and their purpose was illustrative rather than demonstrative and added but a little to the overall argument of the text (cf. Sochorová 1981; Štěpánová 1983). For example, as Iva Heroldová asserted, the only reason behind publishing larger excerpts from a popular play in one of her articles was that the play had not been published before (Heroldová 1982a: 205–208). Some other articles contained family photographs, snapshots, pictures of material culture or details of patterns (cf. Heroldová 1986; Nosková 1984a; Secká 1988b). Ethnography monographs written in the 1980s by Department ethnographers also contained pictorial appendices the purpose of which, again, seems to have been merely illustrative.

To summarize, an appropriate way of describing the specific mode of ethnography rested if not in a reversal of importance than at least in making equal two moments which we find in the famous Evans-Pritchard's dictum from *Social Anthropology*:

The essential point to remember is that the anthropologist is working within a body of theoretical knowledge and that he makes his observations to solve problems which derive from it. This emphasis on problems is, of course, a feature of any field of scholarship. Lord Acton told his history students to study problems and not periods. Collingwood told his archaeological students to study problems and not sites. We tell our anthropological students to study problems and not peoples. (Evans-Pritchard 1951: 87).

Ethnographic regions, customs and other phenomena in themselves were at least as important for ethnographers as the problems related to ethnography research and data which ethnographers collected. It is likely that this attitude of ethnographers towards their material might have added to the accusations of positivism, for these attitudes exhibit a great deal of fixation on primary sources which are almost taken as speaking for themselves. However, it should not be forgotten that this mode of ethnography work was always connected with the theoretical frameworks which animated the ethnographic material. Even the most descriptive works can be shown to have represented the material as related to a particular ethnic (or national) culture or tradition, or as having been related to a particular social (that is class) culture or tradition.

4.2 The Nature of Research

As it has been pointed out many times before (Hann in Hann et al. 2007: 10; Nešpor & Jakoubek 2004: 66–67; Scheffel & Kandert 1994: 15), one of the key differences between anthropology and ethnography was that the year-long participant observation in the fashion of Malinowski's ideal research had never taken roots in ethnography. One of the reasons was surely the lack of sustained funding for countless longer and continuous stays which were possible in anthropology and led to anthropology's expansion only thanks to munificent extra academic donors (cf. Goody 1995: 14–17; Stocking 1995: 404). Also, the relative closeness of the field might have played an important role as the main bulk of ethnography research was conducted within Czechoslovakia thus it was not necessary to organize long-time expeditions to foreign destinations, especially during the late socialist period. Lastly, to travel abroad required an official permit which was not easy to obtain for longer leaves, although it is worth mentioning that it was not wholly impossible.¹²⁴ Anyway, a fieldwork in the fashion of Malinowski would have required a very favourable constellation of political and economic forces during the years of the late socialism.

As it has been already stated, ethnographers drew their primary material mainly from two research activities. Either from their field trips, which they ordinarily called fieldwork (*terénní výzkum*), or from archival research. Both kinds of research played an important role in ethnography and reflected the specific research interests of ethnographers. As regards the history of ethnography and the topic of the Czech National Revival, ethnographers usually worked with written sources. Apart from scholarly works ethnographers made use of newspapers and magazines (Kašpar 1982; Šatava 1985; Štěpánová 1983) photographs, paintings and drawings (Moravcová 1986; Štěpánová 1985; Štěpánová & Märzová 1984), or chronicles and diaries (Robek 1974a, 1977). Among other sources of data we can find writings of the nineteenth-century revivalists and authors of realist prose (Moravcová 1986: 5; Robek 1964, 1974b, 1976; Štěpánová 1983: 204; Vařeka 1981: 142–143).

The remaining topics usually combined the study of archival materials with fieldwork. Fieldwork methods included participant observation (cf. Pargač 1988: 14; Scheufler & Šolc 1986; Valášková 1982: 153), the use of questionnaires (Šalanda 1989a:

¹²⁴ Miloslav Stingl, an employee of the Ethnography Institute up to 1972 could continue to travel and to write popularizing travelogues even later in the 1970s and 1980s (Hlaváček & Bortlová-Vondráková 2018: 210–211).

34), standardized and non-standardized interviews (Heroldová & Matějová 1987: 198). Ethnographers did not often disclose whether they utilized standardized or non-standardized interviews in their research, but it is apparent that interviews as such were widely used in the research on ethnic processes (Nosková 1984a, 1984b, 1989a; Šatava 1981a, 1986; Valášková 1982, 1987b), on socialist villages (Jiráček 1982; Pargač 1988; Valášková 1984) or on the working classes (Robek et al. 1981) and they were also utilized as means of collecting folklore material (Šalanda 1980b, 1980a, 1989a).

Since ethnographers were interested in long-term developments which usually covered several decades, they supplemented their material garnered during fieldwork with material collected from various archival and written sources as different as memoirs, letters and notebooks (Heroldová 1984; Kašpar 1986b), censuses and official documents (Heroldová 1988a; Nosková 1984b) or municipal and local chronicles (Jiráček 1982; Nosková 1984a). Some research also required to work with material culture in various appearances such as clothes and accessories (Krupková 1984; Štěpánová 1987), constructions (Kadeřábková 1985), household items or tools (Robek et al. 1981).

If we are to gain an adequate picture of the duration of research trips, we face a problem as ethnographers did not mention in their writings how much time they spend researching in the particular locality. Another trouble stems from the fact that when ethnographers visited some locality, they did not only spend time questioning their interlocutors or observing their ways, but their fieldwork also included spending some time in local or regional archives. If ethnographers mentioned some temporal duration in their texts they usually spoke of a wider period of several years during which they returned to the locality to conduct their research (cf. Haišman et al. 1983: 11; Nosková 1984b: 154; Valášková 1984: 44). This required me to ask my interlocutors directly. One of them conveyed that it was regular to visit the research locality three times a year for a usual duration of one week.¹²⁵ Another of my informants mentioned the same and added that during the holiday season, one visit could be twice that long.¹²⁶

When we speak of field trips, it is worthwhile to mention one peculiar thing. Some of my interlocutors recalled that there had been a feeling of territoriality similar to that related to ethnography journals. As each of the regions – Czech lands, Moravia and Slovakia had their own ethnography journals and institutions, Czech ethnographers were

¹²⁵ P0032: 4.

¹²⁶ P0034: 15.

“allowed” to conduct research only in the Czech lands, Moravian ethnographers in Moravia and Slovaks in Slovakia. The boundaries were not insuperable, but transgressing these research boundaries required some negotiating between the ethnography representatives of these regions.¹²⁷

Ethnographers began to learn the trade of their discipline during their university years. Students annually participated on field trips supervised by ethnographers from the Ethnography Institute a few of whom also served as external consultants of students’ diploma theses. Some of these field trips were intended as collective trips and involved not only students, but also professional ethnographers (cf. Haišman et al. 1983, notes No 1 and 2; Suchelová 1978).¹²⁸ As one of my informants recalled, ethnographers were conscious about pitfalls related to what they recorded during interviews and to a subsequent interpretation of statements made by the locals.¹²⁹ The same interlocutor recalled that the reputation of various ethnographic groups among ethnographers varied according to their willingness to be interviewed and to speak during interviews. Slovaks from Romania were known as an obliging group when it came to interviews, but Volhynian Czechs or other groups were not.¹³⁰ Research rules of thumb (cf. Hlaváček & Bortlová-Vondráková 2018: 133–134, 135–136) were very likely shared by word of mouth and some methodical knowledge even debated among ethnographers.

The presence of methodological carefulness and awareness somehow contrasts with the dearth of methodological treatises and with the lack of a sustained methodological discourse and with a lack of conferences on methodological topics. When it comes to methodological treatises, ethnographers used handbooks by J. M. Augusta from 1927¹³¹ and by Drahomíra Stránská from 1936 (Hlaváček & Bortlová-Vondráková 2018: 135). Some methodologically-oriented texts appeared in the late socialist period (Robek 1974b; Štěpánová & Märzová 1984), but writing on methodological topics was not much in fashion.¹³²

¹²⁷ P0032: 9.

¹²⁸ P0029: 3–4, P0014: 8–10.

¹²⁹ P0014: 10. Mirjam Moravcová stated in an interview that the Prague ethnography in the 1950s had been methodologically handicapped, but at the same time mentioned that there had existed some methods which ethnographers had stuck to (Hlaváček & Bortlová-Vondráková 2018: 134–136).

¹³⁰ P0014: 29.

¹³¹ I am grateful to Dana Bittnerová for this information.

¹³² It is a question whether the situation in anthropology was any different. According to James Urry, for a long time the only methodological handbook was the famous *Notes and Queries on Anthropology* published and continually updated between 1874 and 1951 (Stocking 1995: 439; Urry 1984: 55). Adam Kuper who had studied at Cambridge in the 1960s wrote that he and his classmates had received no methodological training

4.3 Hierarchies within Ethnography

Another immanent feature of ethnography were specific hierarchies. These hierarchies mutually overlapped and if they possessed something in common, it was the precedence and importance of seniority. One of the hierarchies which ethnography shared with any other discipline was that it was based on pedagogic and academic ranks which were granted by the Czechoslovak legislation and which were gradually achieved by individuals in the course of their careers.

These hierarchies have been a stable feature of Czech life as it has been a commonplace even outside the groves of academe for people to address each other by their titles appropriate to their ranks on formal and informal occasions. Usually it is the title of the highest rank which is used in an address. People also like to include their titles, both postnominal and prenominal, on business cards, doorplates or doorbells. At least as regards ethnography, titles were not included in cases when a name of the author appeared on the title page of a book or article or in contents of collections or journal issues. However, on copyright pages in books, authors' and reviewers' names appeared festooned with all the appropriate titles that ethnographers currently held.

These ranks reflect some temporal intervals as each of the titles requires some time before they can be earned and some ranks serve as prerequisites for others. It is presumable that a person holding the prenominal title of doctor of philosophy (PhDr.) might be either young or elderly, but a person holding the prenominal title of *profesor* (prof.) can be expected to be of higher age. It is interesting to note that before the late socialist period, the highest title of professor was regularly awarded to Faculty academics in their early forties, but beginning with the 1970s, it was regular to award the title to academics who were in their fifties (Petráň 2015: 478–479, 625), indicating a shift to seniority. It is apt to remind the reader of what was established in Chapter 3. Those who earned these titles prior to 1968 and were purged after 1968 could retain their titles, but according to stipulations effective

prior to their first fieldwork and recalled Jack Goody's words that "there was no real method, nothing that could be taught" (Kuper 1993: 60). H. Russell Bernard indicates that when he had written his *Research Methods in Anthropology* in 1988 the teaching of research methods had not been commonplace (Bernard 2006: vii). As Roy Ellen argued, there had been a difference between the British tradition of anthropology which had been more reticent to produce methodological directions and manuals and the more methodologically progressive American anthropology (Ellen 1984: 3). On the balance, it is fair to say that some methodology in anthropology had been developed within its theoretical discourse (cf. Malinowski 1960).

after 1968, candidates to the two highest academic ranks, *docent* and *profesor* had to meet other than professional criteria. But even after the purges and the newly set conditions for achieving two highest ranks, the rank system continued to carry with them an essential symbolic load.¹³³ While the titles in the first place symbolize an investment of time devoted to professional activities, accumulated professional experience as well as academic and research merits, these titles as such have a magical aura about them as their holders are often revered on the basis of the titles themselves.

The system of ranks stood for one of the important hierarchies in ethnography, but here I rather focus on three other hierarchies discernible within ethnography. These hierarchies are not visible at a first glance and require some systematic work with sources and insider knowledge. They are: Hierarchies based on the division of scholarly labour, hierarchies which involve the deceased ancestors and hierarchies derived from the discourse of maturation. These three kinds of hierarchies mutually overlapped and also overlapped with the hierarchy based on ranks. All of these hierarchies in some way or another included the principle of seniority and, besides functioning as hierarchies, they also reveal some substantial dimensions of ethnography practice.

4.3.1 The Division of Labour in Ethnography

Even if some commentators tend to present ethnography almost as a pseudoscience which almost in a fetishist manner revelled in ethnographic objects (Nešpor & Jakoubek 2004: 54), ethnography was actually more than a mere pastime taking delight in antiquarian collection, description and cataloguing of whatever ethnographers encountered during their research and perceived as worthy of their interest. We have seen that ethnography owed a lot to the divide between materialistic and idealistic theories and that these theories to a great extent informed ethnographers' approach to the material collected. Apart from these two theoretical edifices, which provided ethnography with the main theoretical framework, ethnography research had yet another epistemic dimension which also reveals us the first hierarchy peculiar to ethnography.

In order to be able to see this dimension of ethnography, we only need to return to Robek's articles of 1972 in which he manifested his dominant position. We remember that one of these articles Robek criticised the work of the Ethnography Institute in the 1960s

¹³³ One of my interlocutors reproached me for speaking of Robek instead of professor Robek.

and that it was mainly political concerns which underlay the manifesto. But if we pay close attention to his words, we can find employed some important notions:

There was no conception of teamwork, that is a conception of a truly scientific work. The institute basically focused on lesser forms of scientific enterprise, that is collection, catalogizing and classification of material, but the workplan itself was insufficiently designed and there were no attempts at generalizations and syntheses, that is, higher forms of scientific enterprise. (Robek 1972a: 232)

Regardless of whether this accusation holds for the work of the Ethnography Institute in the 1960s, it is obvious that collection of material was not an activity that was perceived by ethnographers as sufficient enough if ethnography was to claim the status of science. In Robek's view, ethnography needed syntheses and generalizations, or in other words, arriving at formulations of lawlike propositions and at generally valid statements inferred from the material collected regardless of whether one researched on the Ukrainians in Czechoslovakia or on the furniture in dwellings of the working classes.

What is of interest here is the spatial metaphor which differentiates between lesser forms (*nižší formy*) and higher forms (*vyšší formy*). While lesser forms imply less credit for those who practice them, the latter not only imply more credit, but are also perceived as something extremely valuable for the progress of ethnography. The hierarchy between lesser and higher forms actually very nicely manifests in *Český lid*. If we look into its main section – Articles Section (*studie*) – which was intended to publish standard articles written by Prague ethnographers, we can find several genres ordered in a hierarchy. These genres were not classified as special journal sections, but it is more than likely that ethnographers had them learned by heart and were aware of the differences between them. If we put aside Robek's editorials which opened the Articles Section of every issue, we can find three recurring genres in the very same section: *preliminary research essays*, *regular research essays* and *synthetic essays*.

Preliminary research essays were essays written by ethnographers about their fieldwork. These essays were shorter than *regular research essays* and ethnographers usually opened or concluded these essays with a statement that these essays had resulted from the first probings in their field and that a fuller account would follow in the future (Dubovický 1989: 204; Nosková 1989a: 229; Secká 1987: 203; Valášková 1987a: 86).

Sometimes they included a programme for any future study of a particular subject matter (Heroldová & Matějová 1987).

Regular research essays were longer and presented more comprehensive presentations of some research. These essays sometimes contained syntheses of various findings and generalizations that related to the immediate field of inquiry. Essays written about ethnic processes are a good example. As it was described in Chapter 2, several ethnographers wrote about two or more localities which they compared and tried to account for differences in rates of assimilation of various groups of newcomers to Czechoslovakia. They sought to explain rates of assimilation by a reference to the relative size of the groups or by the facts related to different cultural backgrounds of the groups in question. If an ethnographer worked with two or more localities or groups, he or she was more likely to make comparisons between them and infer some generalizing statements.

The last genre can be called *synthetic essays*. These essays explicitly aimed to produce syntheses and generalizations which were not bound by any particular locality, but they aimed to synthesize all material written up to the date by ethnographers and infer generalizations from it. Whereas the first two genres of texts, were produced by ethnographers regardless of position or age, the genre of synthetic essays appears to have been reserved only to those who stood high in the scholarly hierarchy, namely Mirjam Moravcová, Iva Heroldová, Jaroslava Kadeřábková and Josef Vařeka. Mirjam Moravcová wrote synthetic essays on the working classes, Iva Heroldová on ethnic processes, Jaroslava Kadeřábková on socialist society and Josef Vařeka on the Czech National Revival. Most of these authors also served as heads of the respective research sections within the Ethnography Institute. As far as I am aware, Stanislav Brouček, head of the last section did not write any synthetic essay for *Český lid* in the 1980s. The significance of synthetic essays is also marked by the position to which they were assigned in a journal issue, that is directly after the editorial.

The authors of synthetic essays share another thing in common. They at the same time recruited from among the eldest employees at the Ethnography Institute. Heroldová was born in 1926, Vařeka in 1927, Moravcová in 1931. Kadeřábková who was born in 1942 and Brouček who was born in 1947 were the youngest heads of the institute sections. Robek was born in the same year as Moravcová and his position is interesting here. While heads of the Institute's sections wrote synthetic essays on issues to which other ethnographers contributed with partial data and findings, Robek usually co-authored many of these synthetic essays, which presented him as the undisputable master of all the research

orientations (Moravcová & Robek 1981; Robek & Heroldová 1983; Robek & Kadeřábková 1982; Robek & Moravcová 1982; Robek & Vařeka 1988). It is interesting that Robek did not write synthetic essays as the sole author and he focused mainly on writing regular research essays on topics related to Bulgaria and Czech ethnographers related to it. On the contrary there are several articles which qualify as synthetic essays and were written without Robek as a co-author (Heroldová 1984, 1985; Vařeka 1981, 1985).

It is evident that not only the collection of material of various sorts was considered as indispensable for ethnography. Ethnographers endeavoured to capture some wider processes which were not apparent from partial fieldwork. They also strove to explain and provide answers to questions related to why something happens and how something happens. The importance of these higher forms of academic enquiry is highlighted by the fact that it was senior ethnographers who practiced these most essential forms of ethnographic enquiry.¹³⁴

4.3.2 The Cult of Ancestors and the Absence of the Reappraisal Discourse

We have seen that there existed a hierarchy within article genres and that only ethnographers who stood high in academic hierarchy produced the genre of synthesis and generalization in its purest form which was valued the most. As a matter of fact, hierarchies in ethnography can be extended as to include the realm of ancestors towards whom ethnographers adopted an attitude of reverence. This reverence can be conveniently demonstrated on an absence of phenomenon which I call the *reappraisal discourse* and which is a regular feature of anthropological discourse. We can pick the famous Scottish evolutionist James George Frazer as an example.

¹³⁴ The classification of genres is partly inspired by a similar one used by Daniel Sosna in his analysis of articles from an archaeology journal (Sosna 2013: 288–291). Interestingly enough, ethnographers' articles from the 1980s are amenable to a similar classification as articles of Czech archaeologists published between the mid-nineteenth and mid-twentieth centuries. What is of utmost interest is the fact that it was only senior archaeologists who were entitled to write synthetic essays: "Synthetic papers were written by senior archaeologists, who had to grow up into their position. [...] Therefore, synthetic papers appeared when a scholar had enough information to generalize and his experience and professional status reached the point when he could afford to synthesize." (Sosna 2013: 291). While my thesis deals with the reproduction of academic and scholarly practices between the 1970s and 1990s, it is worth to point out that some of these practices might have originated in the nineteenth and early twentieth centuries and survived under different political regimes. Unfortunately, this kind of investigation which would account for changes throughout the twentieth century is far beyond the scope of this thesis.

Frazer is today known for his voluminous classic *The Golden Bough* and as one among the founding fathers of anthropology. Frazer was appointed as the first ever professor of social anthropology by the University of Liverpool in 1908 (Stocking 1995: 172). He has been admired as a serious scholar in some circles (cf. Douglas 2002: 30–35; Weisinger 1961), but it is equally true that at least since the time of Malinowski, whose admiration for Frazer had never been unconditional (Stocking 1995: 234–235), there existed a strand of criticism directed against Frazer’s presumptive malign influence within and outside anthropology. Apart from Mary Douglas’s critical attitude (Douglas 2002: 34–35 [1966]), the well-known is perhaps Leach’s criticism satirically titled *Golden Bough or Gilded Twig?* Leach’s article, published alongside Herbert Weisinger’s complimentary article on Frazer, presented Frazer’s methods and theories to have been incompatible with the practice of anthropology (Leach 1961; Weisinger 1961). Yet at the same time other anthropologists who would perhaps not contest all of Leach’s criticisms tried to salvage some other important notions from Frazer’s work. Thus in the 1960s, anthropology witnessed a rise of neo-intellectualist currents (Horton 1968; Jarvie 1964; Spiro 1966) which gave a second breath to the original intellectualist conceptions of religion derived from Frazer’s and also Tylor’s works (see also Leach et al. 1966). Even later, we find other authors who tried to reappraise Frazer’s treatment of the rituals of life and death (Bloch & Parry 1982; Bourdieu 1977). Some even more recent reappraisal of Frazer’s intellectualist ideas, although they refer more to Tylor than to Frazer, is present in some of the recent incarnations of theories of cultural evolution (Mesoudi 2011). Not surprisingly, we can find similar reappraisals related to other ancient figures of Tylor or Durkheim who are of no less importance to anthropology than Frazer. And this is also the fate of some elder figures who have recently passed away to assume the position of ancestor spirits and whose legacy is to be reappraised in the future.

Ethnographers also paid tributes to their ancestor spirits, yet in a different way. In Chapter 2 we have seen that the ancient figures of the nineteenth and of the turn-of-the-century revivalists and *národopis* scholars were acknowledged by Marxist-Leninist ethnographers as ancestor spirits despite the largely bourgeois pedigree of their social origins and ideas. Lubomír Niederle is an apposite example for his position is comparable to that of Frazer. Niederle was appointed as *professor ordinarius* of prehistoric anthropology and ethnology at Charles University in 1904 where he also served one term as rector some years later (Havlíková; online). His early career is marked by his interest in the ideas of British evolutionism and he even got to know Tylor personally and invited him

to Prague (Robek 1964: 176). Niederle helped to organize the Czechoslovak Ethnography Exhibition in 1895 and several years earlier established the journal *Český lid* with his colleague Čeněk Zíbrt (Brouček 1979; Skalníková 1951a). Niederle later helped to establish important archaeology and *národopis* institutions and journals and held several important managerial posts (Havlíková; online). He was a prolific writer and wrote on Slavic and prehistoric archaeology, Czech history and topics related to anthropology and ethnology.

As founding fathers, both Frazer and Niederle occupy similar positions in the history of respective scholarly traditions, but Niederle was to be treated rather differently by his successors. While we can see that anthropological discourse critically reappraised Frazer's intellectual heritage, ethnographical discourse did not treat Niederle in the same way. There was no reappraisal of Niederle's or of any other ancestors' works and ideas by later ethnographers. The attitude towards the ancestors was mostly reverent and it usually manifested in various writings in which ethnographers enumerated names of famous ancestors and emphasized their intellectual merits without going into detail. These reverent attitudes were present and widespread in the 1950s (Kramařík 1951b; Nahodil 1951; Skalníková 1951a, 1951b, 1955) and can be found in the 1960s (Robek 1964). The same attitude was present in the late socialist period. Apart from regular articles and books (Brouček 1979, 1980; Robek 1972d, 1976; Skružný 1988) we find this attitude manifested mostly in the countless articles, forewords and editorials authored by Robek (Robek 1972d: 9, 1982a, 1984, 1985b; Robek et al. 1987: 7–11). It is important to say that regardless of the decade we encounter the same galaxy of prominent ethnographers of the past: Niederle, Zíbrt, Šafařík, Jakubec, Kovář, Polívka, Tille, Němcová, Erben, Havlíček Borovský, Bartoš, Tyršová, Nováková, Hostinský and others. None of these authors' works were critically and comprehensively reappraised by later generations of ethnographers.

Ancestors were subjected to criticism only occasionally, and if so, the grounds for criticism were either political and ideological or factual (Brouček 1979; Heroldová 1982a: 204; Robek 1964, 1972d, 1976). And if ethnographers dared to display some critical attitude towards the ancestors they at the same time reasserted ancestors' grandeur. Even when it came to achievements of the biggest ideological sinners, ancestors continued to be mentioned or listed as important scholars who had made pioneering contributions to the development of the discipline. Nowhere do we find, and that is a point worth stressing, some elaboration and discussion of concepts, theories, ideas or research methods. This is not to say that political or factual criticism is without relevance, but it says a little about intellectual things themselves. It is worthwhile to repeat what has been already shown in

Chapter 3 – even the last *národopis* generation, which was exposed to a harsh ideological criticism in the 1950s, was to assume its place among the revered ancestor figures. We cannot find a single ethnographer who dared subject any of the ancestors to a subtle criticism, showing both merits and deficiencies of the theoretical, methodological or conceptual framework employed by the particular ancestor and at the same time building some fresh and inspiring approach based on that ancestor’s heritage.

One of the reasons for reverence was definitely the fact that the ancestor figures collected a plenty of ethnographic material. However, Chapter 3 mentioned that it was possible to commend Western anthropologists for their collection of valuable empirical material (Holý 1963: 3–5; Tolstov 1953: 30–31). From the way that ethnographers spoke about their ancestor spirits, it is more likely that they praised not only the volume of material which the ancestors had laboriously collected, but at least two further things – it had been the ancestors who had created ethnography and bequeathed it to their successors and it had been the ancestors who had brought invaluable offerings to the grandeur of the Czech nation by means of their assiduous research.

4.3.3 The Discourse of Maturation

The hierarchy connected to the division of labour concerned the living, the other one concerned the relation of the living to the dead and the last one concerned the situation of the young ethnography apprentices. As regards the position of the youngest, it can be demonstrated on a discourse that spontaneously recurred during several interviews with interlocutors. I call the whole complex *the discourse of maturation*.

Ethnographers whom Robek chose to continue their ethnography careers after their graduation were usually sent to earn some practical experience before they could be accepted as internal aspirants at the Institute or lecturers at the Department. For Irena Štěpánová, this meant working for a regional museum (Hlaváček & Bortlová-Vondráková 2018: 230) and another ethnographer whom I interviewed was supposed to work as a secretary at the Institute and mentioned a few other colleagues who had to undergo similar stints. As the interlocutor told me, the Robek’s idea was that “one has to mature” or “one has to ripen” (*člověk musí vyžrát*) before he or she would be eligible to become a professional ethnographer.¹³⁵

¹³⁵ P0024: 1, P0022: 5.

Some ethnographers who had the experience of some extra-academic practice spoke of maturing as of something important. They asserted that Robek did not want fresh graduates and that he stressed that they had to mature first and earn some experience, that they had to serve their apprenticeship almost in the fashion of folk heroes who leave their native hearth for the school of hard knocks in order to become fully adult. Ethnographers who mentioned this idea seemed to have accepted and valued it as an example deserving of being observed even by graduates who intend to pursue scholarly careers nowadays.

The discourse of maturation appeared in recollections of another interlocutor of mine, but in a slightly different context. When I asked her whether she had ever thought about writing a book she told me that almost after a decade which she had spent in the Ethnography Institute Robek approached her and said that the time had ripened for her to write a book.¹³⁶ Not only does this attitude manifest in the hierarchies among ethnographers, but it also sheds light on the hierarchies related to different values ascribed to books and articles – contrary to articles, in order to be eligible to write a book, an ethnographer must have deserved it first by spending some time ripening into the position in which he or she would be experienced enough.

4.4 Erudition and Scholarly Fame

It would be a distortion to say that the position within the field was based solely on seniority or on some rigid hierarchies. In Chapter 3, I suggested that one could improve his or her position by accumulating party or clandestine capital. To accumulate capital of these forms could be the only way how to improve on one's position in terms of power. For to get in charge of a department or a faculty, to become a member of a university convocation and hence to be in a position to decide upon others, especially the party capital was indispensable. Even if individuals did not pursue managerial positions endowed with power, they could greatly enhance their academic prospects by pursuing these forms of capital as it allowed them to travel abroad where they could make contacts or purchase books which were scarce in Czechoslovakia. However, given ethnographers' craving for foreign literature which could provide them with some exclusive knowledge, it seems that the field of the late socialist ethnography allowed to pursue, or at least recognize as

¹³⁶ P0032: 10.

legitimate strategies which derived their symbolic capital from ways immanent to the scholarly environment.

Several of my informants of various age mentioned professor Karel Dvořák whom they attributed old-fashioned intellectual dispositions and wide learning.¹³⁷ Also, František Vrhel has been seen by many as a leading intellectual of immense learning and, as in the case of Dvořák, this judgement cuts across different generations including those who experienced the late socialism as well as those who began their university studies in the 1990s. This outlook also cuts across different specializations within the field. You can find people critical of anthropology, but they at the same time recognize Vrhel, who has partly presented himself if not as anthropologist then at least as a person with a wide knowledge thereof. When it came to Vrhel, one of my interlocutors spoke about impressions from Vrhel's lectures:

*... we didn't understand him, because he used lots of foreign words, thus we wanted to jot down everything, because we did not know which of the words to miss out and which not to. Some of my classmates memorized it, but I suspect them of not understanding all of it. However, I think, that until structuralism we all understood what was going on.*¹³⁸

In words of another interlocutor: "Vrhel's lectures were very incomprehensible. A shower of alien words. Evo-lutionism, difu-sionism. And students had no idea what was going on."¹³⁹ So, in spite of the fact that the field promoted extra-academic ways of improving on one's position, it does not mean that the display of erudition and eloquence as a sign of intellectual magnitude vanished. Besides, the esoteric character of Vrhel's lectures must have been alleviated by the fact that literature on anthropological theories on which he lectured was not generally available to students and that it also required some language proficiency to broach the anthropological discourse (see below).¹⁴⁰

¹³⁷ P0030: 2, P0014: 27, P0029: 22, P0034: 5, P0023: 16.

¹³⁸ P0014: 22. By "memorizing" the interlocutor probably points out to the fact that students had to pass oral exams at the end of the course and that memorizing points out to a strategy which favours remembering at the expense of understanding.

¹³⁹ P0017: 10–11. Similarly, another interlocutor remembered the incomprehensibility of Vrhel's lectures (P0035: 1).

¹⁴⁰ Of anthropological works translated to Czech only eight were available before 1989 (see Appendix 2).

The image of an intellectual was not necessarily reserved only for the elders. Dvořák was born in 1913, Vrhel in 1943 and a persona gifted with intellectual genius could be recognized even among younger students. One of my informants who studied ethnography in the 1980s told me his story about an examination to which he had subjected by professor Dvořák who had been astonished by the student's eloquence and learning (which, as the informant told me, had been a product of a shoddy preparation for the exam) and who immediately after the exam had discussed student's future prospects with his colleagues.¹⁴¹

Even though the strategies of pursuing intellectual fame by strictly academic means, could not have been much successful in improving on one's position within the field especially in terms of power, they presented a viable strategy which led to a specific kind of recognition among students and colleagues. This is especially evident in the case of Vrhel as well as in the case of Dvořák. It is apt to repeat, that the latter was one of the purged Party members after 1968 and who, in spite of having possessed negative party capital, was allowed by Robek to remain as a part-time lecturer at the Ethnography Department. Similarly, this appreciation applied in the case of Iva Heroldová, Mirjam Moravcová and Josef Vařeka, elder ethnographers who served as heads of Sections in the Ethnography Institute during the late socialist period (Hlaváček & Bortlová-Vondráková 2018: 153).¹⁴² Not all of the section heads were Party members (cf. Hlaváček & Bortlová-Vondráková 2018: 95) and in the case of these elderly figures, it seems that Robek capitalized on the fact that he appointed experts who at the same time possessed none to negative party capital.¹⁴³

4.5 Language Proficiency

When the generation of my parents think back to the years of Socialism, they usually recall the compulsory lessons of Russian language. Russian language was compulsory throughout all educational stages from primary to tertiary education. In spite of the status of Russian language other world languages were taught at grammar schools and universities during the period in question. This included languages such as German and Spanish with an ambiguous status as those languages were spoken in “friendly” countries which were in the

¹⁴¹ P0030: 2.

¹⁴² P0029: 3–4, P0032: 21.

¹⁴³ According to one of my interlocutors, Heroldová was suspended from the Communist Party after 1968 (P0034: 21).

sphere of Soviet influence (e.g. Cuba, GDR) as well in countries which were not (Spain, West Germany). But it also included French or English, languages more straightforwardly related to Western capitalist countries. Here I attempt to provide some account of ethnographers' language proficiencies¹⁴⁴ and on ethnography's relation to foreign languages during the period of late socialism.

Since ethnography and folklore studies was mainly interested in the history of Czechoslovakia, two languages were considered as especially important for its practice – Latin and German – as the important archival documents were written in these two languages and because some of the works of German scholars related to the regions in which ethnographers were also interested (Šisler 1984; Vařeka 1972). To get a better grasp of the language proficiency, I made use of a student's record book. According to a record book of one of my interlocutors who studied at the Ethnography Department in the 1980s, students had to attend three semesters of German, and two semesters of Russian, two of Latin and another two of unspecified "world language" which could probably be French, English or Spanish. It was a four-year degree and it is obvious that ethnography curriculum abounded in languages. However, when I inquired more about the nature of their language courses, it transpired that they were alike to what I was to experience decades later. They comprised of a few weekly lessons in a course of one semester. And compulsory language courses were to be found during the postgraduate studies.¹⁴⁵ The lessons were focused on grammar, vocabulary, reading and conversation and they presented a general language course rather than courses suited for extensive writing and other exigencies of academic work.¹⁴⁶

¹⁴⁴ One of the important things to consider when thinking about language competences is that acquiring a foreign language usually encompasses an acquisition of several interconnected skills which can be broadly categorized as speaking, listening, writing and reading. There are many different ways of improving these language competences and focusing on one particular skill does not necessarily entail a proportional improvement of other skills. For example, one could learn a foreign language so as to be able to read books in that language. However, when the same person wants to produce a text or speak in that language, one might find oneself at a loss. Obversely, reading in a foreign language might improve one's writing skills in that language.

¹⁴⁵ The postgraduate studies were known as research assistantship (*vědecká aspirantura*), they were awarded by a CSc. degree and they can be equalled to a PhD degree which began to be awarded in the 1990s. If a candidate was successful, he or she achieved the rank of candidate of sciences (*kandidát věd*) and earned the postnominal title CSc.

¹⁴⁶ P0024: 2, P0025: 10. The situation contrasts with what was customary in the 1960s. There was a method of teaching foreign languages to students of anthropology which was known as Masaryk's method and which Milan Stuchlík employed when he taught anthropology in Chile: "[Masaryk] had distributed his students books in all European languages and had expected the students to handle them." (Stuchlíková 1997: 230). According to Olga and Josef Kandertovi this method was also employed by one of their lecturers who distributed texts in various languages and required students to read them and present a summary of them.

Probably the most important use of foreign languages was related to maintaining professional contacts with colleagues from various socialist countries. Ethnographers attended conferences or spent some time for shorter research fellowships in these countries. This was especially the case of East Germany, Poland, Bulgaria and the Soviet Union (Robek 1980). And foreign scholars attended conferences organized by Czechoslovak ethnographers in turn. One such example is a large conference which took place in 1984 in Sobotín, which among Czechoslovak ethnographers hosted their colleagues from the GDR, Bulgaria, Poland and the Soviet Union (Haišman & Matějová 1985). Conferences with international attendance were also organized in by Moravian ethnographers (Frolec 1983, 1985). Moreover, students from the countries of the Eastern Bloc came to study ethnography in Czechoslovakia (Robek 1978b). For communication purposes during international conferences and stays, it is likely that Russian language played the role of *lingua franca* among the scholars from the Eastern Bloc. However, given the importance of German language for Czech ethnography, ethnographers could use it as a communication language too. Lastly, Antonín Robek also cultivated good relations with Bulgarian ethnographers and it is possible that Bulgarian language was also used as a means of communication and that some ethnographers were well versed in it (Bočková 1979; Robek 1982b).¹⁴⁷

As regards publishing, Czech and Slovak were the main languages of ethnography publications of the time. Rare exceptions of contributions in foreign languages written by scholars from Bulgaria, Poland or GDR can be found in *Zpravodaj KSVI* and in several volumes published in the edition known as *Národopisná knihnice*, but contributions of foreign authors for more important journals or publications were being translated to Czech (Bödiová 1983; Förster 1988; Garcia & Menocal 1989; Kasper 1987; Szyferová 1983).

This dominance of native languages in ethnography writing interestingly contrasts with the use of foreign languages in article summaries, which were never written in Czech. With the exception of Robek's editorials, every contribution to Articles Section in *Český lid* included a German summary.¹⁴⁸ Edited volumes published in *Národopisná knihnice* also included summaries in foreign languages. *Czechs Abroad* series of the edition included English summaries of the contributions, the *Ethnography of the Working Classes* series

¹⁴⁷ *Acta Universitatis Carolinae: Philosophica et Historica, Studia Ethnographica V* from 1980 as well as some other articles by Robek and his colleagues are a testimony of lively relations of Prague ethnographers and their Bulgarian colleagues.

¹⁴⁸ Some of these German summaries were titled *Zusammenfassung*, others *Resümée*.

included summaries in German and sometimes also in Russian. Books written by Department ethnographers usually included two summaries in foreign languages – one in Russian and second in another language, among which we find Spanish, German or English. A comparative glance at the situation in Moravia and Slovakia shows a similar situation. Summaries written for the Moravian journal *Národopisné aktuality* appeared in German, articles for *Slovenský národopis* were accompanied by one summary in Russian and another one in either English or German. Contributions to the volumes eight to ten of the *Folk Culture and the Present Day* edition which appeared in print in 1982, 1983 and 1985 contained summaries for each article in Russian, German and English and included contents and foreword translated to these three languages and French in addition to it.

Considering the usage of summaries, three things stand out in particular. Firstly, even if the Eastern Bloc was by default more Russian-centric, the uses of German far outshone the uses of Russian. This becomes even more apparent in cases of contributions of Czechoslovak ethnographers to the journal *Demos: Internationale Ethnographische und Folkloristische Informationen* published by *Zentralinstitut für Geschichte der Akademie der Wissenschaften der DDR*. The journal was in German, it was issued four times a year and had an international editorial board which consisted of professional ethnographers from across the Eastern Bloc. The contributions consisted of short reviews of books and articles.¹⁴⁹

The second thing is the contrast between Czech- and Slovak-centeredness of ethnographic discourse on one hand and the almost frantic uses of summaries in foreign languages on another. It seems as if ethnographers at the same time aimed to address a wider international audience and to convey that the local scholars produce works just as good as those produced on the international level. This interpretation probably gets more currency from the fact that some of the summaries were relatively long and extensive.

The third thing which stands out is that it was very likely not ethnographers who provided the summaries in foreign languages for their articles. If we stick to German summaries in *Český lid*, these summaries were only rarely written by authors themselves. They were mostly translated to German by Alfons Hubala, a lawyer by degree who worked

¹⁴⁹ While the apparent intention behind the journal was to promote international cooperation and dissemination of knowledge across the Eastern Bloc, contributions rather point to the insulated character of Czechoslovak ethnography at the time as Czechoslovak ethnographers reviewed only writings of their Czechoslovak colleagues.

as an external translator for the Institute (Redakce 1997).¹⁵⁰ An interlocutor of mine, who also contributed to *Demos*, told me ethnographers handed the original reviews in Czech.¹⁵¹ It implies that someone had to translate them to German. By the same token, it is likely that other foreign language summaries were provided for by translators too.

All these examples cast an interesting light on ethnographers' competences in foreign languages. At the same time, we find scholars who were more proficient in various languages. This situation enabled those who were more proficient in foreign languages to earn a better position. According to one of my informants, one of the younger ethnographers at the Institute was unable to finish his postgraduate studies in the set time, yet he was allowed to remain. One of the reasons was that this young ethnographer had been thanks to his family background proficient in English which made his presence suitable for Robek.¹⁵² This indicates that the overall level of language proficiency was low and that it was possible to improve one's position on the basis of a good language proficiency which was achieved outside university courses. It also seems that František Vrhel, Oldřich Kašpar and other scholars who got to ethnography via CIAS were far more proficient in foreign languages than usual. Vrhel was at the time also known as the translator of Jorge Louis Borges and he was originally trained as a linguist specialized in native dialects of South America (Vrhel 1976). Kašpar in his book worked with archival sources written in French, Spanish and Latin (Kašpar 1983b). Their colleague Leoš Šatava also displayed some fluency in English as he intensively worked with English written sources (Šatava 1989) and alongside Vrhel frequently contributed with reviews of English books to *Český lid*.

As regards languages which were indispensable for working with various ethnic groups, there existed two main possibilities. First, there were several ethnographers who were fluent in some foreign languages because of their family origins. Robek assigned these ethnographers to study groups of Slovaks or Ukrainians who were settled in Czechoslovakia since WWII.¹⁵³ One of my interlocutors whom Robek considered fluent in a foreign language which was suitable for Robek's plans (given the interlocutor's family origins) was probably accepted to study at the Ethnography Department, because of the assumed

¹⁵⁰ Every summary was ended by a note: "Překlad: Alfons Hubala" which says "Translation: Alfons Hubala". I encountered one case of a summary translated by the author of the article (Vrhel 1985), another one translated by someone else than either Hubala or the author (Šisler 1984) and one case which did not include any translator (Šisler 1989).

¹⁵¹ P0032: 17.

¹⁵² P0034: 10-11. The English was important because of the research on Czechs abroad which included the Czechs living in the USA.

¹⁵³ P0034: 8, 22.

fluency.¹⁵⁴ As regards more outlandish languages such as Vietnamese, ethnographers relied on external interpreters and translators (Heroldová & Matějová 1987: 201, note 13). As far as I am aware ethnographers did not conduct any research in Russia where they would capitalize on their fluency in Russian.¹⁵⁵ Russian language seems to have served mainly as a tool for communication or for reading ethnography works of Soviet provenance.

If we focus on the situation from the perspective of the many compulsory and elective language courses which the students were obliged to attend, it seems that the level of language proficiency was low. Even if ethnographers attended language courses it is unlikely that they became proficient in these languages so as to be able to produce texts in these languages unless they had had some previous grounding in some of the languages. However, at the same time, we discover a considerable number of ethnographers who were proficient in one or more foreign languages.

4.6 Ethnography as a Collective Enterprise

Robek's attempts to secure his ethnography empire with enough people fluent in various languages suggests an interesting view on ethnography as a collective enterprise – an enterprise to which all ethnographers regardless of their specialization and position in hierarchy or age could contribute with various skills and proficiencies. To get a better grasp of this specific milieu, it is appropriate to return for the last time to Robek's manifesto of 1972. In it we find mentioned another important notion – a notion of *fragmentation*. Robek used this notion as an accusation and levelled it against his predecessors from the Ethnography Institute: “It is obvious that there was no conception of teamwork in the Ethnography Institute... The professional activities of the institute were *fragmented*, since they were essentially focused on collection.” (Robek 1972a: 232, emphasis mine). Here, Robek made use of a hackneyed notion that had been in circulation long time before his ascent to power. In texts of Marxist-Leninists, we frequently find critical remarks on the idea of fragmentation or disunity in opinion (Kramařík 1953: 103; Skalníková 1951a: 1). The very same worries of disunity and fragmentation haunted ethnographers even during the more liberal 1960s (Anon 1965: 129; Tůmová 1964: 44).

¹⁵⁴ Unfortunately, my interlocutor was not fluent in the assumed language and when Robek found that out he went into a fit of rage. P0031: 4–5.

¹⁵⁵ One interlocutor told me about their interest in researching on ethnic processes in the Soviet Union. Robek dissuaded the interlocutor on the basis that Soviet ethnography had had enough experts to appreciate help from Czech ethnographers (P0029: 12).

If ethnography was to thrive, ethnographers should cooperate rather than pursue their individual interests as they had supposedly done in the 1960s (Robek 1972a: 232). To collaborate did not only entail that ethnographers should jointly participate on research or write articles or books together, both of which they indeed did. They should also cooperate by contributing with their various skills and proficiencies and different professional and educational backgrounds.¹⁵⁶ And most importantly of all, ethnography was presented as a cumulative enterprise in the fashion that everyone could contribute with their research even if it was only a mere collection. As we have previously seen, ethnographers contributed by collecting material and when they put together enough material they could also dare to infer general statements or law-like propositions, which was mostly the task for the elder among them. Both the material collected and the generalizations inferred added to the common stock of indisputable knowledge and both activities – collection and generalization – were praised as indispensable.

The collective character of work had one further dimension. Robek's statements which we find scattered throughout his editorials employ the language of discovering objective laws and understanding the objective reality which not only serve the needs of socialist society at large by helping to educate its citizens, but even serve prognostic ends (Robek 1983b: 65). By and large, the reality was one, its fundamentals existed independently on ethnographers' whims and it was only upon ethnographers' meticulousness to discover these fundamentals. In such a world there was no place for scientific dissent. And even if there were dissenting voices, as it was, for example, evident from Bromley's book where we find indicia pointing out to the fact that even the top Soviet ethnographers did not agree on all issues (cf. Bromlej 1980: 18, 95, 192), one of the main

¹⁵⁶ For example, there were ethnographers working at the Ethnography Institute who originally graduated from different departments of the Faculty of Arts or had professional experience from different disciplines. Some Ethnography Institute employees had background in Czech language and history (Stanislav Brouček), theory of culture (Zdeněk Uherek), history and archival studies (Stanislav Šisler) or Korean and Vietnamese studies (Ivo Vasiljev). Another good example is František Šita who originally studied pharmacology (Brouček & Jeřábek 2007: 219–220). One of my interlocutors suggested that the reason why Robek accepted Václav Hubinger to work at the Ethnography Institute was that Hubinger previously worked in the Encyclopaedic Institute of the Czechoslovak Academy of Sciences and had previous experience with compiling encyclopaedic dictionaries, an expertise suitable for the intent of compiling the Dictionary of Ethnic Processes (P0034: 23–24). Similarly, Stanislav Šisler was accepted to work at the Ethnography Institute to organize institute's collections (Motyčková et al. 1997: 167). However, both Hubinger and Šisler were allowed to write scholarly articles to *Český lid*.

This kind of cooperation also transcended the confines of the Ethnography Institute in an interdisciplinary manner. Conferences related to the ongoing work on the Encyclopaedic Dictionary involved historians, linguists or legal scholars (see *Zpravodaj KSVI* 3/1989 and 5/1989). The word “interdisciplinary” did not come with anthropology in the 1990s as some might be tempted to think, but it had been already known among ethnographers (Brouček et al. 1987: 7; Skalníková & Fojtík 1969: 256; Sulitka 1988: 228).

objectives of ethnography was to eventually decide who was right and who was wrong by testing, validating or refuting various claims by means of a careful comparison with objective reality.

The collective style of work becomes especially evident in the case of collectively written *synthetic monographs*. Synthetic monographs are not to be confused with what I earlier called *synthetic essays*. Contrary to synthetic essays, synthetic monographs are synthetic only by the virtue of synthesizing or aggregating knowledge united by a particular research topic. Nice examples are the synthetic monographs on the history of the working classes in Prague (Robek et al. 1981) and the book on the local lore of South Bohemia from an ethnographic perspective (Robek et al. 1987).¹⁵⁷

Synthetic monographs were authored by collectives of ethnographers each of whom usually wrote a chapter of different length to it. Nine ethnographers collaborated on the book on the history of the working classes in Prague and even twenty on the local lore of South Bohemia. Two further synthetic books on which ethnographers collaborated and which were finished in the 1980s were published in 1990. The first concerned the ethnography of West Bohemia and was co-authored by a team of twenty-eight scholars, mostly ethnographers (Bělohávek et al. 1990). The second focused on the Czech National Revival between 1770 and 1791. Thirty-seven authors collaborated on its creation (Petráň et al. 1990) and ethnographers represented only one third of them. For this reason, synthetic monographs were similar to edited volumes on particular topics, but contrary to edited volumes, synthetic monographs were more integrated. The chapters were assembled in a way that each contributor contributed with a chapter on a particular theme. For example, the book on South Bohemia contains one chapter about dwellings, one about agriculture, one about hunting and gathering, one about fishing, one about clothing, one about family etc. Instead of trying to provide different perspectives on one topic, the chapters were

¹⁵⁷ Local lore (*vlastivěda*) is a specific subject that covers knowledge from social and natural sciences that relate to a country as a whole or to its various regions. In Czechoslovakia and the Czech Republic, local lore has not had any university departments or academic institutes, but it has been taught as a subject at elementary schools and there have existed museums of local lore. The name itself – *vlastivěda* – is a compound of two words – *vlast* (homeland, motherland, fatherland) and *věda* (lore, knowledge, science). Hence it has stronger nationalist or patriotic connotations than the English name “local lore”. During the 1980s, various regional museums and archives published volumes on local lore of various micro and macro regions. Ethnographers sometimes contributed to these volumes and some of them served as members of editorial boards responsible for local lore volumes. Robek served at least twice as a member of such a board and other ethnographers sometimes also served as members or as editors (Brandl & Petráň 1981; Polák 1983; Robek et al. 1987).

designed to convey maximum information on a maximum possible subtopic united by some overarching topic.

It would seem that the soaring numbers of collaborators each of whom contributed a chapter or two would result in books of corresponding corpulence, but it was not so. The book on South Bohemia is of a common appearance and it covers only two hundred and sixty pages which on average makes thirteen pages for a contributor. Though it is true that compared to regular ethnography monographs written by Prague ethnographers, synthetic books were longer, of larger size, bound in cloth and covered with dust jacket and their overall design was fancier. The appearance of synthetic books possibly highlighted their importance in terms of hierarchy of publications in ethnography and placed them above ethnography monographs authored by single author.

Lastly, another publication that can be considered to fall into the ranks of synthetic monographs was *The Encyclopaedic Dictionary of Ethnic Processes* work on which begun in the second half of the 1980s and which was also considered as a work of prime theoretical importance. Not only was it designed to synthesize knowledge related to the topic of ethnic groups and processes in forms of hundreds of encyclopaedic entries (Brouček et al. 1987). Some of the entries were supposed to cover analytic terms and theoretical concepts which the authors intended to clarify as terminological clarification was also viewed as a theoretical work of higher importance (cf. Robek 1987b).¹⁵⁸ It was designed to contain some 2,500 entries and was scheduled for finishing in 1990 (Brouček et al. 1987: 7, 9). The task was entrusted to Stanislav Brouček then head of the Theory and History of Ethnography Section of the Ethnography Institute. However, the editing of the encyclopaedia was delayed and it would be never finished. Only preparatory materials were published.

¹⁵⁸ One is here reminded of Peter Winch's imagery inspired by Locke's *Essay Concerning Human Understanding*. It portrays under-labourers whose task is to clear ground a little and remove rubbish that prevents master-builders in advancement of knowledge (Winch 1990: 3–4). Not every ethnographer could hope to become a Zíbrt or Niederle, but every ethnographer could pull his or her weight by clarifying some terminological misunderstanding in order to clear the way for any future Zíbrt and Niederle. This is the picture that one envisions while reading introductory addresses and remarks of the issues of *Zpravodaj KSVI* related to the encyclopaedic dictionary (Brouček & Vasiljev 1988b; Robek 1987b). Conceptual clarification was perceived as important also by other ethnographers from Czechoslovakia (cf. Frolec 1982a: 11) and by Soviet ethnographers (cf. Bromlej 1980: 95).

5. Attitudes to Writing

5.1 A Quantitative Analysis

Ethnography as a discipline was without any doubt pervaded by the modern spirit of science. Ethnographers were driven by this spirit to contribute to the current level of knowledge. They underwent research and collected data which they recast into the form of articles and books. Ethnography was a writing culture, so to say. Original contributions published in books, collections or ethnography journals presented the primary means by which ethnographers communicated their findings regardless of whether description or argumentation prevailed in them. Of course, ethnographers also attended conferences and presented their papers there. But it seems that in terms of importance, conference papers played only a secondary role when compared to articles and books.

This chapter attempts to give a more comprehensive account of the late-socialist Prague ethnography as a discipline based on writing. I will try to capture writing habits of ethnographers which mainly relate to the quantity of written texts. The main questions of this chapter are: How much did ethnographers write? How many books and how many articles? How long were these? Can we spot some trends in writing? And if so, how can we possibly explain these trends? For the sake of these questions, the chapter employs a simple quantitative approach. More importantly, this chapter opens up a question which traces the relation between the volume and originality of ethnographers' writings, but here I provide only some background to possible answers. A more comprehensive picture is offered in Chapter 6.

The analysis concerns writings which were published only during the 1980s. This means that the analysis very likely includes writings written and accepted for publication in the preceding decade and excludes those which were finished in the 1980s, but which would appear in print in the following decade or which would never be published.¹⁵⁹ Since it is not easy to trace the fate of all unpublished works, the following analysis is necessarily imperfect as regards the writing habits of ethnographers. It is evident that the overall *written* output of ethnographers was larger than their overall *published* output. This is important to

¹⁵⁹ Among the works which exist only as unpublished manuscripts or as draft documents are Iva Heroldová's book about education in the times of the Czech National Revival (Heroldová 1980), a large work on the ethnography of the working classes or a work on the youth. I am grateful to Ludmila Kopalová, Dalibor Státník and Dana Bittnerová for the information about the existence of these unpublished works. On the contrary, two synthetic monographs which appeared in 1990, had been very likely written in the 1980s (Bělohávek et al. 1990; Petráň et al. 1990).

stress as my earlier article on the topic inappropriately depicted ethnography as a discipline which was almost deficient as regards ethnographers' written output (Balaš 2018). In this chapter, I would like to present a more detailed analysis which renders several of my earlier claims, as well as the earlier framing of the problem, obsolete.

5.1.1 Authors

As the volume of material is immense, this chapter does not involve written production of all ethnographers who during the 1980s worked in the two ethnography institutions. To make the analysis bearable I created two groups of ethnographers.¹⁶⁰ The first group consists of ten ethnographers of the Ethnography Department, the other of an equal number of ethnographers from the Ethnography Institute.

As regards the representativeness of the first group, the Ethnography Department reached the maximum number of employees (disregarding part-time lecturers and secretaries) in academic years 1980–81 and 1982–83 when there were sixteen employees. The decade's low was the year of 1988–89 when there were eleven employees. If we disregard the year 1986–87 for which I have no information available, the average number of employees per academic year was twelve and half in the 1980s.¹⁶¹

The reasoning informing the choice of the ten ethnographers who in this chapter represent the Ethnography Department is simple. These ten ethnographers became part of the department in Robek's era and they would continue to form the core of the department for a long time after 1989. Throughout the 1990s and 2000s, this group of scholars presented a decisive majority at the department and this cast them in a good position to influence the education of future generations of scholars by having the possibility to inculcate the standards of academic work onto them. This group consists of three female ethnographers: Jaroslava Krupková, Ludmila Sochorová and Irena Štěpánová; and seven male ethnographers: Ivan Dubovický¹⁶², Oldřich Kašpar, Jan Pargač, Bohuslav Šalanda, Leoš Šatava, Miloš Tomandl and František Vrhel. In 1985 the average age in this group was 35.6 years.

¹⁶⁰ These two groups of ethnographers and their texts are only relevant for the purposes of this chapter.

¹⁶¹ There are several sources of information backing this claim. The most important is represented by *Karolínky*. Another source is *Český lid* which included authors' affiliation. The last source which gave me some idea about the size of the two institutions were interviews.

¹⁶² Ivan Dubovický was in the 1980s a postgraduate student at the Ethnography Department and is not included in *Karolínky* from the decade. Nonetheless, he was prolific as author and hence is included in the analysis.

The second group is represented by ten ethnographers who in the 1980s worked at the Ethnography Institute. This latter group consists of ten ethnographers; six female: Iva Heroldová, Jaroslava Kadeřábková, Helena Nosková, Vlasta Matějová, Milena Secká and Nad'a Valášková; and four male: Stanislav Brouček, Antonín Jiráček, Vladimír Kristen and Stanislav Šisler. Apart from Heroldová and Matějová, members of this group also began their careers in ethnography in Robek's era.¹⁶³ As regards proportion, this group represents only some fifth of the employees of the Ethnography Institute during the decade.¹⁶⁴ In 1985 the average age of this group was 41.3 years. While the second group consists of older scholars than the first one, the average age of both groups suggests that these twenty ethnographers were in their prime.

The original idea behind the choice of the second group was to pick ethnographers who researched on one of the specializations pursued by the Ethnography Institute of which I decided to choose the topic of ethnic processes, for this specialization was probably the most important among Prague ethnographers in the 1980s. Since the Ethnography Institute had a specific section designed to study the topic, the obvious move would be to focus only on its members. The flipside to this option is that there were actually ethnographers who were not members of the section such as Stanislav Brouček, Vladimír Kristen or Nad'a Valášková, yet they contributed appreciably to the topic. The key according to which the final choice was made, was to pick ten ethnographers, each of whom had published at least one article on ethnic processes in *Český lid*, and then to extend the analysis as to cover the maximum of their published texts regardless of topic. Therefore, this group does not consist of all those who at the time wrote about ethnic processes as the number of Institute ethnographers who contributed with at least one article about ethnic processes to *Český lid* or other journals was larger. Moreover, many articles from the 1980s often combined two of the research specializations and a neat pigeonholing of ethnography articles and authors according to specialization is not quite possible. Besides, some of the articles published by the second group were co-authored by their colleagues who are not included in either group.

¹⁶³ Stanislav Šisler was originally a historian and had some prior experience as an assistant professor at the Faculty of Arts in Prague (Brouček & Jeřábek 2007: 219).

¹⁶⁴ I do not have the exact number of employees, but I counted some forty-two names by going through affiliations included in Articles and Materials Section published in *Český lid* in the 1980s, but these two sections do not include some other Institute ethnographers such as Petr Kolář or František Šita who did not contribute to these sections or to *Český lid* at all. Some information about the Ethnography Institute's employees was also elicited from interviews.

The fates of the ethnographers of this latter group after the 1989 were quite diverse compared to the former group. Some left ethnography for good, but some continued to be academically active in the Ethnology Institute or elsewhere. Whereas the members of the latter group could not exert much direct influence on younger generations of ethnologists, they could still influence the younger, if not as external consultants, then at least indirectly as their writings contributed to shape the face of ethnology from the 1990s onwards.¹⁶⁵

The person which is not deliberately included in either group is Antonín Robek as he left ethnography for good in the early 1990s and his influence was not formative on generations of students from the year 1990 onwards. Even though he continued to publish in the 1990s, he was in retirement and at the margins of the discipline, not holding any post. Moreover, Robek's role in the 1980s was more managerial and ceremonial (evident from the immense number of opening speeches which he delivered and from innumerable editorials and afterwords which he wrote for journals and books) than during the 1960s and 1970s when he had been building his reputation as a scholar (cf. Hlaváček 2017: 38–39; see also Chap. 3).

5.1.2 The Publishing Landscape

Now that we are acquainted with the authors, we should take a look on the publishing landscape, on all the means of publication which were available to ethnographers and which ethnographers made use of. The analysis will focus on disciplinary journals, edited volumes and books and will disregard other written texts such as popularizing articles or exhibition booklets or librettos.

¹⁶⁵ The subsequent analysis does not consider one important issue. Vanda Jiřiková stated in an interview that it had not been possible for some ethnographers to have their texts published in *Český lid* (and probably also in other journals) for political reasons in the 1970s and 1980s. She added that it had been especially the case of those who had been expelled from the Party after 1968. Nevertheless, the expelled had been allowed to publish in *Národopisné aktuality* and they had done so under pen names (Hlaváček & Bortlová-Vondráková 2018: 99). It cannot be ruled out that the following analysis, which attributes writings to ethnographers on the basis of openly declared authorship, includes writings that were actually authored by someone else. The question of authorship naturally casts an interesting light on the inability of the late-socialist power to deal with those purged from the Party after 1968.

However, as regards the choice of the texts for the analysis, I did not stumble upon any information which would put authorship of any particular text in question. The texts authored by concrete authors evince thematic uniformity and it is possible to discern individual writing styles. So, unless someone regularly published under someone else's name, I hold it unlikely that the texts chosen for this analysis were actually authored by someone else. Some texts also mention research trips on which some ethnographers participated and subsequently authored articles linked to that research. In sum, there exist more links between authors and their texts other than the attributed authorship.

5.1.2.1 Journals and Edited Volumes

Ethnographers' articles appeared in several major ethnography journals of the time – the Czech based *Český lid* (ČL),¹⁶⁶ the Moravian based *Národopisné aktuality* (NA) and the Slovakian based *Slovenský národopis* (SN). Each of these journals was designed to appear four times a year. The first was published by the Ethnography Institute and served mainly as a medium for Czech ethnographers. The second of these journals was published by the Institute of the Folk Arts in Strážnice and its contributors recruited especially from Moravian ethnographers. The last one was published by the Ethnography Institute of the Slovak Academy of Sciences in Bratislava (*Národopisný ústav Slovenskej akadémie vied*) and it was a medium to which especially Slovak ethnographers sent their contributions.

The analysis also covers three minor journals. The first was known as *Acta Universitatis Carolinae: Philosophica et Historica*, a journal ran by the Faculty of Arts. It appeared up to five times a year and it included several series dedicated to various disciplines cultivated at the Faculty. Ethnography had its own series called *Studia Ethnographica* (SE) which was a medium intended for Department ethnographers. As there were more thematic series than issues per year, the series were published irregularly and only one issue of *Studia Ethnographica* appeared in the 1980s (contrary to four in the 1970s). Another minor journal was *Národopisný věstník československý* (NVČ) which was a medium of the professional association of Czechoslovak ethnographers, but it predominantly published contributions of Prague ethnographers. It was designed to appear only once per year. The last of these was rather erratically appearing *Zpravodaj KSVI* (ZKSVI), a minor journal which mostly published conference proceedings from conferences organized by the Ethnography Institute and which also published contributions mostly of Czech ethnographers.¹⁶⁷ All these three minor journals were under Robek's control (see 3.4.2.1).

The analysis also includes contributions to an edition titled *Národopisná knižnice*. Established in 1970, the edition was under the control of the Ethnography Institute and

¹⁶⁶ The abbreviations in brackets refer to tables included later in this chapter.

¹⁶⁷ *Zpravodaj KSVI* was mainly intended as a periodical for the private purposes of the Ethnography Institute. Out of forty-six issues published in the 1980s, thirty-four contained a disclaimer informing that the issues were only intended for the private purposes of the Ethnography Institute. Three were intended for the private purposes of the Ethnography Institute and cooperating institutions, and nine did not contain any such disclaimer. Some of the non-private issues were even reviewed or announced in *Český lid* (Motyčková & Moravcová 1989; Suková 1985). Since some of the private issues included contributions by authors unaffiliated to the Ethnography Institute I hold it possible that even the private issues circulated outside of the Ethnography Institute.

published books and edited collections in several series each of which was focused on a particular specialization of Institute ethnographers. The edition followed its own numbering, but each of the series had its own numbering too. For example, the eleventh piece in the *Ethnography of the Working Classes* series was the thirty-fifth publication in the edition overall. The most important source in the analysis will be two series *Czechs Abroad*¹⁶⁸ and *Ethnic Processes* to which ethnographers from both groups often contributed.

Besides, included are several other edited volumes which were independent on the journals and editions mentioned above. The first one was about the Polish minority in Czechoslovakia, jointly published by the Ethnography Institute and the Regional Culture Centre in Ostrava (Robek et al. 1989), two volumes on the socialist village by Moravian ethnographers and a series titled *Folk Culture and the Present Day* also published by Moravian ethnographers.¹⁶⁹ Lastly, the analysis also includes chapters from one synthetic monograph on the local lore of South Bohemia (Robek et al. 1987). Contributions to these are included in the following tables under the column “other”. There appeared other synthetic monographs in the decade (e.g. Robek et al. 1981), but no member of the two groups contributed to them.

In the subsequent analysis, only those volumes and journals which contain contributions by the members of the two groups are included. It is convenient to add that not all synthetic monographs and not all volumes from the series here concerned contained contributions by the ethnographers included in the two groups. Also excluded are other journals such as Slovakian *Národopisné informácie*, edited volumes on local history to which ethnographers occasionally contributed, regional periodicals or periodicals related to disciplines other than ethnography.¹⁷⁰

The contributions which I have collected from the abovementioned sources will be classified according to the specific types of these contributions, that is: *standard articles*, *material articles* and *conference papers*. To avoid a possible misunderstanding,

¹⁶⁸ The *Czechs Abroad* series followed only its own numbering as none of the volumes in the series was attributed an overall number in the *Národopisná knižnice* edition. Only the first volume of the *Ethnic Processes* series was attributed an overall number in the edition, the remaining three volumes were not.

¹⁶⁹ The series was published between 1974 and 1985 and contained ten volumes. Only two volumes are relevant for this analysis for only these two volumes contained contributions by ethnographers of the two groups (Frolec 1982b, 1983).

¹⁷⁰ Also excluded are foreign journals as I have no information related to ethnographers' publication activities outside Czechoslovakia. The only foreign journal with which I am acquainted is *Demos*, but it published only reviews (see 4.5).

classification here employed does not represent *genres* mentioned in the discussion related to hierarchies (see 4.3.1). Contrary to genres, the classification according to types has a basis in journal sections which were explicitly classified by ethnographers themselves. In a few cases, contributions were not headed under an explicitly named rubric and in such cases, the classification has a basis in the preface which usually states what kind of articles are included. Excluded from the analysis are minor sections – jubilees, obituaries, reports and reviews.¹⁷¹ This analysis also disregards several discussion contributions, which represent only a tiny fraction of the overall written output and they will be discussed separately in Chapter 6.

5.1.2.2 Books

The division of books partly mirrors the division of articles. The first type is represented by *monographs*, original works written by ethnographers which aimed to contribute with new knowledge and which were in this sense on a par with standard articles. While their purpose was the same, the obvious difference was length. The second type of books is represented by *anthologies* which include indexes, encyclopaedias or anthologies of materials not written and compiled by the anthologists. The last type is represented by *textbooks* written for the purposes of educating of ethnography students. There is one issue related to the difference between textbooks and anthologies. The purpose of some anthologies (Šalanda 1989b; Vrhel & Kašpar 1984) was to serve as textbooks for students, but they fulfil all the criteria for being included in the category of anthologies. For this reason, the analysis treats them as anthologies.

Ethnographers from the Ethnography Department had their books mostly published in the series *Acta Universitatis Carolinae: Philosophica et Historica, Monographia* run by the Faculty of Arts. Ethnographers from the Ethnography Institute usually published in *Academia*, a publishing house which was under the control of the Czechoslovak Academy of Sciences, but no ethnographer from either group had his or her book published in it in the 1980s. Some books authored by ethnographers appeared within *Národopisná knihnice* and some even appeared in *Zpravodaj KSVI*. There were three more publishing houses relevant for the written production related to the two groups, but which were outside the

¹⁷¹ The reason for exclusion is that all these types of contributions which appeared in two sections called Reports and Reviews included only short contributions, which did not usually exceed one page, and cannot be considered as original contributions in the sense in which other articles can be.

world of Charles University and the Czechoslovak Academy of Sciences. These were *Státní pedagogické nakladatelství* (the State Pedagogic Publishers), which mostly published textbooks for students. The other two were *Odeon* and *Jihočeské nakladatelství* (South Bohemian Publishers).

5.2 Publication Activities Measured: Numbers of Texts

5.2.1 Standard Articles

Standard articles represent the original articles written by ethnographers. This type of articles appeared in journals in variously named sections. In *Český lid*, *Slovenský národopis*, *Národopisné aktuality*, or *Studia Ethnographica*, the section was called Studies (*studie* or *štúdie*).¹⁷² Elsewhere, for example in *Národopisný věstník československý*, the matching section was called Articles (*články*). The column *others* in Tables 5.1 and 5.2 includes chapters from the synthetic monograph on South Bohemia and from the *Czechs Abroad* series of *Národopisná knižnice*.

Compared to material articles (5.2.2) and conference papers (5.2.3), standard articles represent the longest type of articles which ethnographers wrote. And the longest articles of this type appeared in *Český lid*. The average length of the fifty-three articles from *Český lid* included in both Tables is seven pages. To get a more concrete idea of the length, seven pages of *Český lid* correspond to twenty-four “norm pages”, that is 1,800 characters including spaces.¹⁷³

From a brief comparison of Tables 5.1 and 5.2, it seems that ethnographers from the Ethnography Institute were more than doubly prolific if we judge their activity by the numbers of standard articles which they published. However, let us now for a moment move beyond the two groups in question and see overall numbers. *Český lid* in the 1980s published the total of 230 contributions in the Studies section. Forty of these were editorials which leaves us with 190 original contributions. Focusing now on the standard articles which were written solely by ethnographers from the Ethnography Institute and

¹⁷² In my previous article (Balaš 2018), I used the term *Essays Section* when speaking of the section in *Český lid*.

¹⁷³ The length of a single article was established by counting the amount of space covered by the main body of text, endnotes, which included sources, and demonstrative figures such as pictures or tables. I excluded pictures which were merely illustrative (see 4.1) and the summary. The model page is taken from Šatava's article (Šatava 1981a: 199) which scored 6,093 on characters with spaces. This model page equals to 3.39 norm pages. Since Šatava's article covers 7.1 pages in *Český lid*, it covers 24 norm pages ($7.1 \times 6,093 / 1,800$).

Ethnography Department during the 1980s, or written by one or more ethnographers from either institution in the same decade as joint authors, we are left with 128 contributions. This means that the remaining 62 contributions were written by authors unaffiliated to either institution. If we want to trace the different productivity of both institutions, we have to omit Robek's contributions, since he was affiliated to both. Here I will only exclude 9 articles written by Robek as a sole author. This leaves us with 119 contributions. Out of these, 24 contributions were written by authors affiliated to the Ethnography Department in the 1980s (all of these 24 contributions are authored only by one author). The remaining 95 were written or co-written by ethnographers who were affiliated to the Ethnography Institute in the 1980s. On the first glance, it looks as if the Ethnography Institute was far more prolific as its ethnographers contributed to 95 standard articles in total, compared to 24 articles written by the Ethnography Department. However, as it has been stated earlier, the Ethnography Institute was roughly five times larger than the Ethnography Department. If we imagine that the Ethnography Department had been five times larger and its employees had kept the same level of productivity, then the difference would not be so striking. This imaginary inflation would yield some 120 articles for the Ethnography Department which, compared to 95 standard articles by the Ethnography Institute, suggests, that as regards standard articles, ethnographers from the Ethnography Department were a bit more productive in terms of writing.

Returning to the two groups of ethnographers and their productivity represented in Tables 5.1 and 5.2, we can see that several individual ethnographers from the Ethnography Institute produced comparably more standard articles than any of their colleagues from the Ethnography Department. These differences in writing can be partly explained by the differences between both institutions. While ethnographers working at the Ethnography Institute had research as their prime activity, ethnographers working at the Ethnography Department were also expected to perform other demanding activities such as lecturing or examining students. Nonetheless, it becomes evident from a comparison that some ethnographers from the Ethnography Department were far more productive than their colleagues from the Ethnography Institute because in addition to their workload as university lecturers, they were able to publish equal if not larger numbers of standard articles. As we shall see throughout the analysis, this was not only the situation with standard articles.

TAB 5.1: Standard articles by Department ethnographers (jointly written included)

<i>member/journal</i>	<i>ČL</i>	<i>NVČ</i>	<i>SN</i>	<i>NA</i>	<i>SE</i>	<i>other</i>	Σ
Ivan Dubovický	1	0	0	0	0	2	3
Oldřich Kašpar	1	0	0	0	0	0	1
Jaroslava Krupková	1	0	0	0	0	0	1
Jan Pargač¹⁷⁴	1	0	0	0	0	0	1
Ludmila Sochorová	3	0	0	0	0	0	3
Leoš Šatava	3	1	0	0	0	1	5
Bohuslav Šalanda	3	0	0	0	1	0	4
Irena Štěpánová	2	0	0	0	1	0	3
Miloš Tomandl	1	0	0	0	0	0	1
František Vrhel	1	0	0	0	0	0	1
TOTAL	17	1	0	0	2	3	23

TAB 5.2: Standard articles by Institute Ethnographers (jointly written included)

<i>member/journal</i>	<i>ČL</i>	<i>NVČ</i>	<i>SN</i>	<i>NA</i>	<i>SE</i>	<i>other</i>	Σ
Stanislav Brouček	5	1	1	0	0	2	9
Iva Heroldová	10	1	1	0	0	1	13
Antonín Jiráček	1	0	0	0	0	0	1
Jaroslava Kadeřábková¹⁷⁵	3	0	0	0	0	1	4
Vladimír Kristen	3	0	0	0	0	1	4
Vlasta Matějová	3	0	0	0	0	1	4
Helena Nosková	5	0	0	0	0	0	5
Nad'a Valášková¹⁷⁶	5	0	0	0	0	1	6
Milena Secká	2	0	0	0	0	0	2
Stanislav Šisler	2	0	0	0	0	1	3
TOTAL¹⁷⁷	37	2	2	0	0	7	48

¹⁷⁴ Pargač co-wrote his only standard article in *Český lid* with Jaroslava Kadeřábková (Kadeřábková & Pargač 1982). At the time of writing it, Pargač was a member of the Ethnography Institute, but prior to its publishing, Pargač had been transferred to the Ethnography Department. As Pargač spent most time of the decade at the Ethnography Department, he is counted as a Department ethnographer.

¹⁷⁵ One of Kadeřábková's articles included in the *ČL* column is the one co-written with Pargač and included in TAB 5.1. One article by Kadeřábková included in the *other* column was co-written with Valášková and three other authors (Robek et al. 1987: 216–242).

¹⁷⁶ One article by Valášková included in the *other* column is the one co-written with Kadeřábková and three other authors.

¹⁷⁷ Although the sum of the column *ČL* is 39, two articles cowritten by Heroldová and Matějová appear twice (Haišman et al. 1983; Heroldová & Matějová 1987). Again, the sum does not fit in the column *other* as one publication, an article for a volume on local history, on which Kadeřábková and Nosková collaborated, appears twice.

5.2.2 Material Articles

The second type of articles is represented by contributions included in Materials Section of *Český lid* and by some issues of *Zpravodaj KSVI* which were intended as collections of material articles. I described the main underlying rationale of this type of article earlier (see 4.1). Compared to standard articles, material articles were shorter and, measuring the length of their textual part, it was not an exception that they were two pages long. Nonetheless, there are some exceptions even to the length (cf. Heroldová 1989). Material articles in *Český lid* sometimes included pictorial supplements. Several contributions to this section were entirely textual and they, for example, described basic characteristics of some ethnic groups (Brouček 1985a; Heroldová 1983). The main purpose of articles of such a type was to acquaint the readership with sources which the authors discovered during their research or to provide some elementary factual account. Only sometimes it seems that the section of *Český lid* reserved for materials contributions did not strictly follow the logic of these articles. The section also published research plans and concepts (Kadeřábková et al. 1981). Some rather resembled conference reports with extended summaries (Heroldová 1982b; Matějová 1982b). The material articles included in the column *other* are from *Czechs Abroad*.

**TAB 5.3: Material articles by Department Ethnographers
(jointly written included)**

<i>member/journal</i>	<i>ČL</i>	<i>ZKSVI</i>	<i>other</i>	Σ
Ivan Dubovický	0	0	0	0
Oldřich Kašpar	4	0	0	4
Jaroslava Krupková	0	1	0	1
Jan Pargač	1	0	0	1
Ludmila Sochorová	0	1	0	1
Bohuslav Šalanda	0	0	0	0
Leoš Šatava	1	3	0	4
Irena Štěpánová	3	0	0	3
Miloš Tomandl	0	0	0	0
František Vrhel	1	0	0	1
TOTAL	10	5	0	15

**TAB 5.4: Material articles by Institute ethnographers
(jointly written included)**

<i>member/journal</i>	<i>ČL</i>	<i>ZKSVI</i>	<i>other</i>	Σ
Stanislav Brouček	3	6	0	9
Iva Heroldová	6	3	1	10
Antonín Jiráček	0	0	0	0
Jaroslava Kadeřábková	2	0	0	2
Vladimír Kristen	2	0	0	2
Vlasta Matějová	3	0	0	3
Helena Nosková	1	0	0	1
Nad'a Valášková	1	0	0	1
Milena Secká	4	2	0	6
Stanislav Šisler	2	1	1	4
TOTAL	24	12	2	38

5.2.3 Conference Papers

Conferences presented one of the staples of academic life in ethnography. I counted some twenty-five conferences, colloquia, seminars or meetings in the 1980s organized or co-organized by the Ethnography Institute and six conferences organized by other institutions. It is more than likely that the number of conferences and similar events must have been significantly larger and it seems that the conference life was quite vivid and intense as ethnographers frequented conferences. Judging by the number of papers published in collections of conference papers from these thirty-one conferences, there were some frequent conference attendees. One ethnographer attended at least ten conferences in the decade as a speaker, another at least thirteen.

It is necessary to stress that in the 1980s it was certainly not the case that the papers delivered at various events would be usually developed into longer standard articles. Also, conference papers were published in their original shortness. These articles were far shorter than standard and material articles. Sometimes they included endnotes with sources cited, sometimes they did not. According to one of my informants, it was a commonplace for attendees to read their papers instead of presenting the paper off-the-cuff.¹⁷⁸

Conference papers were published in *Zpravodaj KSVI* and in some volumes of the *Národopisná knižnice* edition and were usually related to events organized or co-organized by the Prague branch of the Ethnography Institute or by the Ethnography Department. *Národopisný věstník československý* published papers from two state-wide ethnography conferences attended by ethnographers from across Czechoslovakia. This analysis also includes papers which were delivered by several Institute and Department ethnographers at conferences organized by Moravian ethnographers and subsequently published edited volumes.

The conference papers included in the column *other* are taken from *Czechs Abroad*, vols. 1 and 2, *Ethnic Processes*, vols. 2, 3 and 4 and the collection on the Polish minority in Czechoslovakia. Other conference papers of the ethnographers in question appeared in *The Socialization of Village and the Transformation of Folk Culture*, vol. 2 and *Folk Culture and the Present Day*, vols. 8 and 9.

¹⁷⁸ P0014: 13–14.

**TAB 5.5: Conference papers by Department ethnographers
(jointly written included)**

<i>member/journal</i>	<i>NVČ</i>	<i>KSVI</i>	<i>other</i>	Σ
Ivan Dubovický	0	0	1	1
Oldřich Kašpar	0	0	2	2
Jaroslava Krupková	0	1	0	1
Jan Pargač ¹⁷⁹	0	3	3	6
Ludmila Sochorová	0	1	0	1
Bohuslav Šalanda	0	3	0	3
Leoš Šatava	0	3	3	6
Irena Štěpánová	0	2	0	2
Miloš Tomandl	0	5	0	5
František Vrhel	0	1	0	1
TOTAL	0	19	9	28

**TAB 5.6: Conference papers by Institute ethnographers
(jointly written included)**

<i>member/journal</i>	<i>NVČ</i>	<i>KSVI</i>	<i>other</i>	Σ
Stanislav Brouček	1	4	3	8
Iva Heroldová	1	9	4	14
Antonín Jiráček	0	2	1	3
Jaroslava Kadeřábková ¹⁸⁰	0	5	2	7
Vladimír Kristen	3	6	4	13
Vlasta Matějová	0	5	2	7
Helena Nosková	0	8	3	11
Nad'a Valášková	0	4	1	5
Milena Secká	0	2	1	3
Stanislav Šisler	0	1	3	4
TOTAL	5	46	24	75

¹⁷⁹ The number in column *other* includes one jointly written with Kadeřábková (Pargač & Kadeřábková 1986).

¹⁸⁰ The number in column *other* includes the one jointly written with Pargač.

5.2.4 Books

Tables 5.7 and 5.8 contain numbers related to the books published by the two groups in the 1980s.¹⁸¹ These tables do not include synthetic monographs contributions to which are treated as standard articles, since synthetic monographs more resemble edited volumes.

Here the numbers and differences between the two groups are quite striking. The group of ethnographers from the Ethnography Institute which have I selected and which has turned out to be very prolific in terms of numbers of articles, lags behind ethnographers from the Ethnography Department in terms of numbers of books. Even if we disregard textbooks and anthologies which cannot be taken as original contributions, the ratio is one to four. It is even more striking if we consider that Department Ethnographers had responsibilities other than researching and writing.

If we search in on-line catalogues of various Czech libraries, it becomes obvious that book writing did not present a common activity in which ethnographers working at the Ethnography Institute indulged. Earlier, I have mentioned the fact that not all books which had been written made their way to eventual publishing and that this was the lot of Heroldová's book, which is only available as a draft document in the library of the Ethnology Institute. Even the other most prolific author from the ranks of Prague ethnographers, Mirjam Moravcová, published only one book as sole author in the decade (Moravcová 1986). On the other hand, ethnographers from the Ethnography Department proved to be far more productive when it came to books and far more productive when it came to workload in general.

If we want to explain the striking discrepancy between the two groups, then several factors have to be considered. In Chapter 3, we have seen that ethnographers who were members of the Communist Party had better chances to end up as lecturers at the university rather than researchers at the academy. It is possible that book writing was understood not as something naturally expected from professional ethnographers, but as a reward for ideological compliance. Moreover, if the Faculty of Arts was considered as an important institution of the state ideology, as Jareš suggests (Jareš et al. 2012: 139), publishing of books might have been important for the Faculty's role as a show window of the Marxist-Leninist ideology. Contrary to the situation at the department, book writing was not much in fashion among the ethnographers working at the Ethnography Institute.

¹⁸¹ The tables do not include Vrhel's and Kašpar's translations of literary fiction and the latter's stories for children.

**TAB 5.7: Books by Department ethnographers
(jointly written included)**

<i>member/type</i>	<i>monograph</i>	<i>textbook</i>	<i>anthology</i>	Σ
Ivan Dubovický	0	0	0	0
Oldřich Kašpar ¹⁸²	1	3	7	11
Jaroslava Krupková	0	0	0	0
Jan Pargač	1	0	0	1
Ludmila Sochorová	1	0	1	2
Bohuslav Šalanda	2	0	1	3
Leoš Šatava ¹⁸³	1	0	1	2
Irena Štěpánová	1	1	0	2
Miloš Tomandl	0	0	0	0
František Vrhel ¹⁸⁴	1	4	0	5
TOTAL¹⁸⁵	8	5	9	22

**TAB 5.8: Books by Institute ethnographers
(jointly written included)**

<i>member/type</i>	<i>monograph</i>	<i>textbook</i>	<i>anthology</i>	Σ
Stanislav Brouček	1	0	0	1
Iva Heroldová	0	0	0	0
Antonín Jiráček	0	0	0	0
Jaroslava Kadeřábková	0	0	0	0
Vladimír Kristen	0	0	0	0
Vlasta Matějová	0	0	0	0
Helena Nosková	0	0	0	0
Nad'a Valášková	0	0	0	0
Milena Secká	0	0	0	0
Stanislav Šisler	0	0	0	0
TOTAL	1	0	0	1

¹⁸² Oldřich Kašpar co-wrote one anthology of Mesoamerican folklore and three textbooks on ethnography of Americas with František Vrhel (Kašpar & Vrhel 1986, 1989; Vrhel & Kašpar 1984, 1985).

¹⁸³ Šatava's anthology appeared in two parts in *Zpravodaj KSVI* (6/1987 and 7/1987). An updated version appeared in one piece in hardback in 1994 and newly included (Šatava 1994).

¹⁸⁴ The columns *textbook* and *anthology* include the four books jointly written with Kašpar.

¹⁸⁵ Since the column *textbook* includes three books twice, the sum is only five (and not eight). Similarly, the sum in the column *anthology* is nine (and not ten), for one book is included twice in the column.

5.2.5 Repetitiveness

It seems that ethnographers had an outstanding written output in terms of numbers of texts.¹⁸⁶ However, if we are attempting to trace their attitudes to writing by measuring the volume of written production, we should include one important phenomenon in the overall picture. It is the phenomenon of repetitiveness which perhaps presents a stable part of life in any academic community. Scholars conduct their research and after some time working with their material they attend conferences where they deliver papers based on their research. A conference, workshop or seminar presentation is the first stage. A scholar delivers the paper to a community of colleagues and ideally receives some feedback. Then the scholar moves to the second stage. The originally delivered paper is extended in a fully-fledged article which is published in an edited volume, in a journal or as a chapter in a book, which becomes available for a wider audience. Sometimes, another stage follows when a scholar decides to publish a selection of already published articles in a collection and writes one or two more for this occasion.¹⁸⁷ We can trace such sequential chains of developing publications among ethnographers, anthropologists as well as in other disciplines. Several examples from ethnography deserve to be mentioned.

Obvious textual links connect Milena Secká's texts on Vietnamese *Gastarbeitern*, texts on Volhynian Czechs by Leoš Šatava or Jan Pargač's texts on villages in Central Bohemia. Secká presented her paper at a conference in 1986. The paper was published as a conference paper in *Zpravodaj KSVI* in 1988 (Secká 1988b). Ironically enough, it appeared a year earlier as a standard article in *Český lid* (Secká 1987). Both texts contain identical passages and differ only as the variant published in *Český lid* is only slightly longer and includes pictorial supplements. The same can be said of Pargač's conference paper published in 1982 and his later book on life in village communities. In the latter, Pargač uses whole paragraphs from the former without mentioning it (Pargač 1982, 1988). Very large degree of similarity can also be demonstrated on Šatava's writings on Volhynian Czechs which do not contain identical passages, but only slightly different passages which have obvious textual origins. One is a conference paper the other one a standard article and both were published in 1981 (Šatava 1981b, 1981a). A somewhat extreme case is

¹⁸⁶ To this we should also add that some academic degrees were conditioned by writing a thesis and that some of ethnographers' theses did not appear later as books. Nonetheless, theses are not included in this analysis.

¹⁸⁷ As far as I am aware, this publication strategy which has been commonplace in anthropology, was absent in ethnography. Good examples from anthropology are Turner's *The Forest of Symbols*, Geertz's *Interpretation of Cultures*, Douglas's *Implicit Meanings* or Asad's *Genealogies of Religion*. I was equally unable to find traces of the institution of published guest lecture series among ethnographers.

Heroldová's outstandingly long standard article in *Český lid* which was published without any changes two years later as a conference paper in *Zpravodaj KSVI* (Heroldová 1986, 1988b). The original conference where she delivered her paper had taken place in February 1986.

Regarding articles which later appeared as chapters, one of Šatava's papers later appeared as a chapter in his book on migration and the Czechs in the USA (Šatava 1985, 1989: 133–142). Similarly, Šalanda's book on folklore published in 1980 contains one chapter which is identical to an article in *Český lid* from the same year and two of the book's chapters are composed of large parts of Šalanda's earlier article on folklore (Šalanda 1978, 1980b, 1980a).

We also find examples of a reverse publication strategy. It can be demonstrated on several cases. On just two short pages of a material article published in *Zpravodaj KSVI*, Šatava very briefly summarizes his previous research on Volhynian Czechs (Šatava 1983). Information which he mentions in his new article do not differ from the two previously mentioned articles of his. Another case is represented by a materials article from *Český lid* authored by Stanislav Brouček. On just two pages, Brouček commemorates the ninety-year anniversary of the Czechoslavic Ethnography Exhibition. The text contains parts taken straight from Brouček's earlier book on the same topic (cf. Brouček 1979: 112–113, 1983: 107) and it seems that Brouček only summarizes some parts of his earlier book. The same strategy appears in a conference paper by Oldřich Kašpar. At the same conference where Heroldová delivered her two papers, Kašpar contributed with a paper on Thaddäus Haenke, an early modern Czech botanist who was nicknamed "the Czech Humboldt". Of a paper which is, if we disregard bibliography, slightly more than nine pages long, four pages are copied, only with minor omissions, straight from Kašpar's earlier monograph on Czech explorers and travellers (Kašpar 1983b: 85–89, 1986c: 166–170).

Facing such examples of repetitiveness, there are three things of differing importance requiring to be stressed. Firstly, the incidence of repetitiveness shows that the above quantitative analysis might be to a certain degree imprecise as the volume of original writings was lower due to the fact that some writings were reproduced and are hence included more than once. This was probably only the fate of a part of the written production – we find conference papers which their authors never developed into standard and longer articles as well as standard articles which were not based on previous conference papers; and the same findings apply to the relations between standard articles and book chapters. The second thing transcends the analysis itself and brings us closer to the gist of the issue

of writing. Although in some cases we find differences between original conference papers and articles which were based on these conference papers or between original articles and later book chapters, the differences are rather marginal or cosmetic. The later incarnations do not substantially differ from preceding versions and by no means is there a change in the overall argument. A sentence or a paragraph was occasionally added, the succession of paragraphs was changed, a paragraph inserted, some sentences reformulated, a word or two changed, but none of the changes substantially altered the resultant outcome. Thirdly, none of the texts which shares its origins with a previously or simultaneously published text, mentions the original or parallel version. It was definitely not customary that an author in an article mentioned that the article was based on a previous conference paper, or that some chapters of a book consisted of previously published articles.

This points out to the presence of a certain amount of undeclared recycling, which should be considered in the light of the fact that ethnographers normally quoted their previous works, but that they specifically did not mention the work which they recycled. So, the absence of reference to the parallel texts is even more conspicuous. In the contemporary times when authors are measured mostly by numbers of their original writings, any recycling is reminiscent of artificial and ethically unacceptable attempts to inflate one's publication merits. However, such an argument only partially fits the late socialist period. While it seems to me that writings had their symbolic weight as they represented authors' scholarly capability or, on a higher level, the scholarly potency of the scholar's home institution, I was unable to find any traces of the current culture of evaluation in the late socialist ethnography. Nonetheless, below, I offer an understanding of this specific recycling as well as the specific mode of writing.

5.4 Explaining Attitudes to Writing

There are several factors which should be included in an explanation as to what shaped ethnographers' attitudes to writing. Contrary to my original explanation,¹⁸⁸ the current

¹⁸⁸ The original explanation was based on Kornai's work on socialist economy and on its analogous application to the realm of socialist science. It considered an interplay between the scarcity of publication opportunities (resulting from socialist planning), the growing number of ethnographers willing to publish and the necessity to fulfil the plans (based on the set quota of written output). Such a scenario saw an increasing number of ethnographers and not proportionally increasing publication opportunities which led ethnographers to adopting a strategy of writing many shorter contributions. According to Antonín Kostlán (personal communication), state planning in science was only interested in ideologically laden production and it did not set some obligatory volume of output as did other sectors of the economy. Hence, the current model partly differs from the original explanation.

explanation takes into consideration several factors which possibly played a role in shaping ethnographers' attitudes to writing. The first is the fact that authors were remunerated for their articles for *Český lid* and possibly for other publications of theirs. One of my interlocutors mentioned that while their starting monthly salary was Kčs 1,500, one standard or material article or a review was rewarded with additional Kčs 300.¹⁸⁹ It is a question to what extent this was an efficient incentive, because we can identify several ethnographers from the Ethnography Department and the Ethnography Institute who in spite of the financial incentive had a rather low written output and seldom contributed to *Český lid*.

There was probably another incentive which partially overlapped with the financial incentive. As some sources testify, Robek commissioned his colleagues to research and publish on particular topics which went hand in hand with designing some issues of *Český lid* in the fashion of monothematic issues which reflected the pivotal specializations of Prague ethnographers (Hlaváček & Bortlová-Vondráková 2018: 142).¹⁹⁰ This incentive likely stems from Robek's strategy related to his long-term struggles for domination. Achieving dominance in the field of ethnography as well as displaying the vigour of his intellectual empire was, among other things, possible through publishing. It was not only necessary to command the publishing means, which Robek did. It was also important to have a stable inflow of writings to publish. One of my interlocutors even spoke of "feeding" *Český lid*.¹⁹¹ This recollection hints at the possibility that Robek's activities which involved publishing were two-dimensional: they were means of power struggles and they also promoted ethnography research. Under such conditions it is not hard to imagine Robek, the person directly responsible for the performance of his ethnographic empire, commanding, persuading or even cajoling his colleagues into writing and at the same time occasionally stressing that it would not mind if the contributor recycles some earlier work. Considering

¹⁸⁹ P0034: 23–24. Kčs refers to the Czechoslovak Koruna, the official currency of Czechoslovakia. For I learned this information by the end of my research, I was unable to corroborate the fact, the types of articles remunerated and the sum of money by asking more interlocutors or visit the archives. I was only able to ask about the remuneration one more interviewee who confirmed the remuneration for *Český lid* (P0032: 17). I have no information about possible financial rewards for authoring books.

This open-handed approach was not at odds with the chronic scarcity generated by state planning. As Kornai showed, socialist economies "earmarked" money. This means that money of every firm was allocated into several compartments according to their purpose (wages, investment, purchase of material etc.) and that money could not be converted between these compartments. For example, money which were not spent on investments could not be converted to wages or vice versa (Kornai 1992: 132–133).

¹⁹⁰ P0029: 2, P0031: 4–5, P0034: 8.

¹⁹¹ P0034: 22.

this, the recycling of texts mentioned above makes much more sense.¹⁹² Moreover, this mode of production fitted very neatly into the conception of ethnography as a collective enterprise. Every ethnographer could contribute to the enterprise and every contribution counted. Even short conference papers and material articles counted. However, this enterprise had one important limitation to which I now turn.

5.4.1 The Role of Scarcity

Chapter 3 introduced the role of state plans in the funding of ethnography and the struggles over the coordinating position. According to the recollections of the Slovakian ethnographer Daniel Luther, which I have mentioned earlier, the funds, which the planners were willing to allocate on funding ethnography research, were abundant and it was possible to reach a mutual agreement with the planners representing the state apparatus. This opinion almost implies a view that if ethnographers needed it, their representatives just had to request the planners to raise the funds to cover more positions or alternatively raise ethnographers' salaries, more research trips or more publication outlets. Even if the planners were reluctant, they might have eventually complied with ethnographers' wishes or find some compromise. In Chapter 3, we have also seen that Robek was successful in increasing the numbers of ethnographers working at the Ethnography Institute which probably had to go hand in hand with increased expenses for their fieldwork and research. Let us also recall that Robek more than doubled the numbers of *Zpravodaj KSVI* in the 1980s and revived *Národopisný věstník československý* in 1984.

¹⁹² This issue of recycling is relevant for one important reason. Since the 1990s, there erupted several plagiarizing scandals which were related to the post-socialist anthropology, ethnography and ethnology. One of those affairs touched Oldřich Kašpar from the Ethnography Department who allegedly plagiarized the work of his senior colleague, Josef Polišenský (see Opatrný 2003; on-line). Kašpar's writings are interesting for another reason, because the works which he published in the 1990s recycled large textual parts from his earlier works (cf. passages on various explorers in Kašpar 1983b, 1986c, 1997, 1999). The latter works published in 1997 and 1999 have two issues. One is that the recycling of the earlier works from the 1980s is either not mentioned (Kašpar 1999) or vaguely mentioned in the preface (Kašpar 1997). Interestingly enough, the book from 1999, which does not acknowledge earlier works at all, also includes passages from the 1997 book. The other issue is that while the two earlier works duly credit sources in the text, the two books from the 1990s include only bibliographies, making it seem that all the ideas presented in the book have origins only in Kašpar's work.

To conclude this digression, it seems to me that undeclared recycling of earlier texts in the late socialist period might have not been viewed as problematic by ethnographers. However, the same practice became increasingly problematical from the 1990s onwards with the appearance of audit culture, grant funding and stricter ethical codes. It is more than likely that authors in the 1990s did not perceive the issues which we perceive today because of their dispositions shaped in the 1970s and 1980s. Similar recycling is present in some of Vrhel's texts (cf. Vrhel 1994, 1995).

From this point of view, Robek's actions were successful and they fit perfectly into Luther's recollections. Funds were available and Robek was extremely successful as he was able to secure necessary funds which he funnelled into widening his research network by enlarging the pool of his subordinates and colleagues and into broadening the range of publication outlets designated for ethnographers' writings. Yet things begin to appear in a different light if we pay attention to little details which we encounter when going through ethnography writings or when listening to ethnographers' recollections.

In Chapter 3, I have also mentioned Jiří Hlaváček's assertion that Robek began a cooperation with the Czechoslovak Foreign Institute in the 1980s whereby he secured extra funds which he converted into enlarging publication opportunities. The only book by an ethnographer from the Ethnography Institute group included in the above analysis which made it into an eventual publication, was Brouček's book on the early history of the Czechoslovak Foreign Institute (Brouček 1985b). The book was jointly published by the Ethnography Institute and the Czechoslovak Foreign Institute. And so were the four volumes of the *Czechs Abroad* series which Robek started in the second half of the 1980 and which appeared in the *Národopisná knižnice* edition. Thanks to these funds and regarding the two groups of ethnographers, Robek was able to secure space for publishing thirteen conference papers, seven regular articles, two material articles and Brouček's book. Given the numbers in the tables in this chapter, this is not an immaterial number.¹⁹³ But why would Robek need to secure extra resources for his institutions, if, given Luther's claims, it was quite uncomplicated to negotiate more funding with the planners?¹⁹⁴

There are several more indices which insinuate that the situation with publishing opportunities was dire. The twelfth volume of the Ethnography of Working Classes published in *Národopisná knižnice* in 1987 was printed with a cover from the 1979 volume.

¹⁹³ Disregarding the two groups from the analysis, the four volumes, among other things, accommodated fifty-two conference papers, sixteen regular articles and two material articles.

¹⁹⁴ It is possible that this was also the case of the synthetic monograph on the local history of South Bohemia, the edited volume on the Polish minority and of two issues of *Zpravodaj KSVI*. The synthetic monograph was not published by the Ethnography Institute, but by South Bohemian Publishers (Robek et al. 1987). It hosted three contributions written by ethnographers of the two groups (out of seventeen contributions in total). The remaining contributions were written by other ethnographers mainly from the Ethnography Institute. The two issues of *Zpravodaj KSVI* (2/1985 and 3/1985) were published jointly by the Ethnography Institute and Správa Krkonošského národního parku (the Administration of the Krkonoše Mountains National Park). These two issues published six conference papers by ethnographers from the two groups (out of twenty-eight conference papers in total). Lastly, the edited volume on the Polish minority was co-published with two other institutions and it catered for two conference papers by members of the two groups (Robek et al. 1989).

This is a considerable number of publications and we could conjecture what would have happened if Robek had not been able to secure these extra funds independent on the budget bargained by Robek for his institutions.

The front sides differ only as regards the number in the *Ethnography of the Working Classes* series within *Národopisná knižnice* (No. 11 and 12) and as regards the number within the *Národopisná knižnice* edition as such (No. 35 and 36). Title, design and colours remain the same as well as the inscription on the back cover which says “Praha 1979”. The 1987 volume was distributed with a slip of paper containing a notice of apology: “The editors apologize that due to economic reasons the book had to be covered in a cover from an earlier issue.”

Similar troubles haunted *Národopisný věstník československý*, which appeared only once a year. The issues from 1984 and 1987 have the standard form of an ethnography journal, opening with articles and ending with reports. The remaining issues were designed mainly as conference proceedings, but include some reports too. The 1985 and 1986 issues published contributions from a nationwide ethnography conference which had taken place in 1984. The mere fact that the contributions had to be split into two issues indicates some publishing difficulties. The 1988 and 1989 issues published conference papers from the national ethnography conference that had originally taken place in 1987. The short introduction to the issue published in 1988 states that only a half of the papers could be published in the 1988 issue. The next issue which appeared a year later in 1989, did not publish the remaining half, but only a half of the remaining half. As the introductory statement signed by the editors on page three of the 1989 issue states:

We apologize to the participants who presented their papers at the conference as well as to all the members of NSČ [Národopisná společnost československá] that we do not publish all the papers as increased costs for publishing of this NVČ [Národopisný věstník československý] volume forced us to reduce the number of pages to a half at the last moment. The remaining papers will be published in their proper succession in the next volume of our bulletin.

Some issues of *Zpravodaj KSVI* were similarly delayed. Whereas some issues containing conference papers were published in the same year as the original conference, other issues were delayed and they sometimes appeared in print even two years after the conference had taken place. Since the papers were read at conferences and seminars and had to be prepared beforehand, it is unlikely that the delays were caused by waiting for all the participants to submit their papers. Problems with publishing probably explain the reverted order in which contributions of Secká and Heroldová appeared, that is first as

regular articles in *Český lid* and two years later as conference papers in *Zpravodaj KSVI*. Moreover, issues with publishing probably prevented some planned issues of *Zpravodaj KSVI* from making it to print at all.¹⁹⁵

Moving to books written by Department ethnographers, we find that several of these were substantially delayed. For example, Šatava's book on Czech repatriates in the USA was planned to appear in *Acta Universitatis Carolinae: Philosophica et Historica, Monographia* edition in 1985. However, the copyright page of Šatava's book indicates that the book was accepted for printing in 1987, but it was published in 1989 (Šatava 1989). The book by Pargač was also set to appear in the same edition in 1985, but it was published in 1988 (Pargač 1988). Similarly, Kašpar's book was designed to appear in 1980 series but was published in 1983 (Kašpar 1983b). It is likely that similar obstacles prevented some other books from being published and they have been available only as draft documents (Heroldová 1980).

This was not the fate of all the books and journals. Some other books were accepted for printing in one year and published in the following one. Luckier was the destiny of books by Šalanda which seem not to have faced any adverse conditions (Šalanda 1980a, 1989a). Also textbooks published in the National Pedagogic Publishers (Státní pedagogické nakladatelství) do not seem to have faced delays (Kašpar & Vrhel 1986, 1989; Vrhel & Kašpar 1985). This was also the situation of *Český lid* which was regularly printed and did not seem to face any problems with publishing and, without any doubt, this reliability further increased the journal's symbolic capital. Also, some issues of *Zpravodaj KSVI* were published in the same year in which the original conference or seminar took place.

In order to understand the vagaries and whims of publishing, it is appropriate to return once again to the topic of state planning, but at this moment we have to step outside the narrow confines of the planning related to ethnography and see the general situation of the Czechoslovak socialist economy. According to János Kornai, socialist economies suffered from the systematic failings which resulted from the way that the socialist planning worked. The planned economy, as Kornai persuasively shows, can be characterised as a

¹⁹⁵ For example, in 1981 only one issue, numbered 4, was published. In 1986, eight issues were published and were numbered from 2 to 9, indicating that the issue number 1 was not published. In 1987, seven issues numbered from 2 to 9 were published, but 1 and 8 were skipped. I am grateful to Ludmila Kopalová for the information about the missing issues.

The reason for this erratic appearance was due to the fact that issues of *Zpravodaj KSVI* were reproduced by ethnographers themselves on inadequate copiers and that it was hard to come by the paper which would fit the copiers (Thořová et al. 2005: 72). This is also a testimony of the inadequacy of the means of publishing available to ethnographers.

system which produces a chronic shortage of goods and labour. Moreover, the higher education was not a priority sector for large investments and the sector was underfunded (Kornai 1992: 171–180). All the indices that we encounter in relation to publishing in ethnography fit perfectly into the workings of the socialist planning. If we realize that ethnography was also influenced by the shortages and underinvestment, and it seems that this was a case to a profound degree, the situation with publishing in the discipline now appears under a different light. Underinvestment or shortage could manifest as shortages of paper, the lack (or insufficiency) of printing capacities (Thořová et al. 2005: 72), or by increasing prices (as the inscription on the slip suggests). Since ethnography was just one from among the host of other academic institutions each of which tried to gain as large slice of input as possible, it had to compete with other institutions for scarce resources. And this presented a serious obstacle for the opportunity to publish.¹⁹⁶

Besides these economic constraints, two further constraints have to be included in the picture. One of these is presented by the numbers of ethnographers. If Robek was successful in raising the numbers of ethnographers from the early 1970s onwards (Thořová et al. 2005: 66), then it looks like that the steadily increasing numbers of ethnographers were not accompanied by a proportional increase of publication space. This is especially evident if we consider volumes of *Český lid* which in times when Robek served as the journal's chief editor (1972–1989) covered exactly two hundred and fifty-two pages each.¹⁹⁷ Even if Robek was successful in increasing the published numbers of *Zpravodaj KSVI* and revived *Národopisný věstník československý*, which could cater for the need of publishing space, the layout of their pages could in no way match the amount of space provided by *Český lid*. These journals were sparser in terms of text and as we have seen, they were largely unreliable when it came to the regularity of their appearance. Hence, while the numbers of ethnographers increased, the proportional amount of space to publish, in a way, diminished.

The situation with monographs authored by single authors was very similar as the scarcity left its mark also on the shape of books. If we look at monographs included in Table

¹⁹⁶ According to Antonín Kostlán (personal communication), beginning with the 1970s, the money allocated to the Czechoslovak Academy of Sciences were capped. The only way how to earn more money was at the expense of other institutions within the Academy. This is possibly one of the reasons why Robek sought to secure extra funding outside of the Academy of Sciences.

¹⁹⁷ Years 1972, 1973, 1974 and 1976 presented an exception to this rule as the journal covered exactly two hundred and fifty-six pages in these four years. Beginning with 1990, the length of particular volumes began to differ. The numbers of pages per volume between 1990 and 1999 are as follows: 256, 320, 384, 352, 352, 364, 352, 352, 384, 388.

5.7, they were between 102 and 162 pages long (129 pages on average) and were printed on a paper slightly larger than A5, a page of which covered approximately 1.62 norm pages.¹⁹⁸ The only monograph by the other group was Brouček's book which covered 111 pages (including supplementary materials), but which was printed on much sparser layout using monospaced font and was by no means longer than any of the monographs produced by the first groups. Even books not included in the analysis, such as Mirjam Moravcová's book on the Czech national costume (Moravcová 1986) or a book on rafters by Václav Šolc and Vladimír Scheufler (Scheufler & Šolc 1986) were similarly long.

It seems that unless they were synthetic monographs, which were comparably longer than monographs authored by single authors, no medium to large-size monographs in Prague ethnography were published. Compared to historians in Prague who at the time worked at the Faculty of Arts and who produced considerably lengthier monographs (Haubelt 1986; Hroch 1986; Urban 1982), it seems as if the writing of the long monographs did not form a basis of establishing one's scholarly reputation and authority in ethnography. In other words, it did not form a basis of earning symbolic capital.¹⁹⁹ Maybe, it would be more precise to say that the writing of long monographs ceased to fulfil this role. If we search the history of ethnography, we find very long monographs (Václavík 1959) or jointly written monographs in which the written content of either author surpassed the length of any of the monographs produced by the two groups of ethnographers (Fojtík & Sirovátka 1961). This is not to say that ethnographers did not produce lengthy books at all. However, the lengthy books produced by ethnographers of the times fall in the rank of anthologies, that is books which do not represent some original or novel standpoint or analysis (cf. Frolec & Vařeka 1983). Nor is it to say that working on voluminous anthologies is less time consuming than writing long monographs. It is only to say that writing longer monographs

¹⁹⁸ The number of characters which covered one page is taken from Šalanda's book on oral tradition (Šalanda 1989a: 56). I first divided the number by 1,800 (characters per norm page) and second, I multiplied the resulting number by the number of pages that particular books have. In terms of norm pages, the shortest monograph covered 165 pages and the longest 262, the average being 180. This procedure is warranted by the fact that monographs authored by Department ethnographers were published in *Philosophica et Historica: Monographia* edition which used the same layout. Monographs published in this edition account for seven out of eight monographs in Tab 5.7, for this estimation excludes Vrhel's monograph which was published by a different publisher.

Chapters 1 to 7 of this thesis cover some 364 norm pages (including footnotes and tables).

¹⁹⁹ The comparison with history at the Faculty of Arts would of course deserve far more space and should also include the difference in the access to economic resources. It is possible that the field of Czechoslovak late-socialist history, or its Prague subfield, had more financial means than ethnography and that this factor made the appearance of larger monographs in history possible. However, this does not invalidate the concomitant absence of establishing one's scholarly authority through publishing landmark books in ethnography.

requires a cultivation of specific writing skills and that it seems that these skills were not particularly promoted in the late socialist ethnography.

To summarize, there were several pressures or constraints acting on ethnographers. The first was Robek's managerial strategy. He wanted to demonstrate the vigour of his empire and this he did by impelling and motivating his colleagues to publish. However, the limitations of the socialist economy curtailed the availability of publishing opportunities for Prague ethnographers. It seems that under the conditions of scarcity, two options were available: either give priority to publishing longer contributions which would lessen the overall number of contributions, or give priority to publishing a plenty of contributions with shorter contributions being no exception. I am inclined to view Robek's ethnography enterprise in the light of the second strategy, which might be called extensive (as opposed to intensive). It may account for the proliferation of the shorter genres of material articles and conference papers. It may also account for the appearance of some shorter standard articles and for the shortness of books in general. This extensive mode of publishing would also nicely fit into Robek's idea of ethnography as a collective enterprise to which everyone could contribute.

The economic constraints were mediated by Robek who could bargain with the planners, who scouted out untapped resources and who acted as a redistributor of space to publish among his colleagues and research allies. It would have been a surprise if ethnographers had not adapted their writing habits to the situation. Even if they were motivated to write more, they could not and they attuned their writing habits to the objective possibilities of the field, constrained by the general availability of publication opportunities. That ethnographers adapted to the situation by writing shorter articles and books has to be considered in the light of the constraints of the late socialist economy. Even if we can attribute some differences in written output to ethnographers' differing inclinations toward writing, as we may find graphomaniacs as well as slower writers, it is possible that the written output in the case of some was a consequence of the fact that ethnographers were aware that even if they wrote something longer, they knew it might not get eventually published or at least not in its entirety. Ethnographers themselves did not have to possess a detailed knowledge of the fundamental principles on the basis of which state planning operated, but it seems that awareness of the scarcity was present. To give an example, Ludmila Sochorová in her monograph on popular theatre stated that for technical reasons, the length of partial surveys of popular theatre plays had to be limited to only basic

characteristics (Sochorová 1987c: 24).²⁰⁰ It is also possible that some ethnographers were unmotivated to even try to write longer articles or books or any writings at all.

5.4.3 The Discourse of Maturation Reconsidered

The vagaries of the socialist economy cast a different light on hierarchies and especially on the discourse of maturation both of which I introduced a chapter earlier. There is no denying that the discourse represented (and still continues to represent) some deep-seated career ethos, possibly not exclusively in ethnography, but in the groves of Czechoslovak academe generally. The fame and merit come only with age by a patient accumulation of publications, by slowly building one's image as an expert in some ethnographic specialization and by collecting degrees step by step in their proper order from the first prenominal title PhD. to the most hallowed title of *profesor*. Getting older almost automatically equals a gradual achievement of academic mastery symbolized by the titles. Not only does this gradual mastery presuppose the idea that everything takes time and that by the time one learns something new one also gets older, but also the idea that when one is old enough, the age itself creates and consecrates the mastery. And to publish a first book is to reach an important milestone in one's career. Yet, given the economic conditions, there is something that betrays the honesty of the discourse of maturation. The fact that Robek did not possess sufficient funds to cater for all ethnographers to satisfy their writing desires whenever they felt like writing a book or article casts the discourse of maturation in a different light. Now, the discourse of maturation appears also as an *officialization strategy* (Bourdieu 1990: 108–109). Therefore, if Robek broached the idea of writing a book in the discussion with the young ethnographer from the Ethnography Institute and at the same time invoked the discourse of maturation (see 4.3.3), this might have been a result of a calculation by which he wanted to create a more durable subjection by offering the ethnographer a unique possibility of writing a book. The possibility could be experienced as unique in three ways – economically (given the scarcity), in relation to colleagues at the Ethnography Institute (“I was offered the opportunity to write a book”) and professionally (“the high-ranking ethnographer considers me to be mature enough to write a book”). It is not far-fetched to say that the material shortage was transmuted into an academic virtue. It is also imaginable that this strategy could be successfully employed in cases of non-Party

²⁰⁰ In terms of length, Sochorová's monograph does differ from monographs written by her colleagues from the Ethnography Department.

members who could be brought to closer orbit of the Party as a result of having felt an obligation to reciprocate the granted possibility to write a book. This probably also explains why ethnographers working at the Ethnography Department wrote more books than their colleagues from the Ethnography Institute. Since the Faculty of Arts of Charles University was considered as an ideologically important institution and since the Party membership was considered as important for upholding its ideological profile, the members of the Ethnography Department had probably a better position when it came to book writing. In their case, books can be understood as a kind of reward for their ideological complicity. This adds a fourth dimension of the uniqueness to the situation of our young ethnographers – “Even if I work at the Ethnography Institute where it is not common for ethnographers to write books, I was offered this rare opportunity.”

Similarly, when some ethnographers were compelled to undergo the period of practical training before they could be accepted as internal aspirants at the Ethnography Institute, this might have been a result of the fact that Robek could not offer places immediately to all the prospective Department alumni he had chosen, but only to some. The vacancies were very likely stipulated in the plans and it was possible that not everybody could follow with a professional career straight after graduation. Maybe not surprisingly, some interviewees recalled that they were not compelled to undergo similar training.²⁰¹

5.5 The Question of Quality

This chapter's main topic indirectly touches on the question of quality which echoes the question raised by Červinková mentioned by the end of Chapter 2. If ethnographers were constrained by the publication opportunities, what effect did it have on the quality of their writings? In spite of the extensive mode of writing which gave rise to shorter contributions and in spite of certain repetitiveness, which is also to be found in disciplines which themselves were not at the mercy of socialist planning, was not ethnography and folklore studies still an original field of inquiry? Regardless of imperfections of the quantitative analysis presented in this chapter, it did not chase away our fundamental thoughts. The most suggesting question to these persisting ruminations is: Of course, it was! The quality is in no way related to quantity. Anthropology abounds in short books of high originality that in terms of quantity match the length of publications written by ethnographers. If a short

²⁰¹ P0025: 1, P0029: 10–11, P0032: 2–3.

anthropological book can be highly original, why could not a short monograph on clothing or ethnic processes achieve an equal level of quality? Moreover, many of short anthropology books surpass much longer books in terms of quality. Sometimes, a pithy argument covering just fifty pages helps to solve a pressing intellectual mystery better than a voluminous, late Victorian tome. Just consider Gellner's *Nations and Nationalism* or Turner's *Ritual Process*, Mead's *Sex and Temperament*, Barth's *Cosmologies in the Making* or Lévi-Strauss's *Totemism*, Goody's *Domestication of the Savage Mind* or Harris's *Cows, Pigs, Wars and Witches*. And if you are still unconvinced, *The Gift* by Marcel Mauss is one of the most substantial and original pieces in anthropological discourse, yet it is at the same time one of the shortest. The question persists. The quality is not a matter of whether a book or article is more voluminous or less. In the next chapter, I will try to convince the reader that quantity and quality are mutually dependent, yet not as directly as may seem from our questioning of their relation. This, however, requires to inspect one more constraint that, besides the material scarcity, influenced the discourse of ethnography.

6. The Culture of Contention and Its Absence in Ethnography

This chapter proceeds similarly to the previous one as it focuses only on one peculiar dimension of ethnography, this time on ethnographers' attitudes to criticism. Here, a comparison with anthropology helps to highlight the peculiar nature of criticism in ethnography. I begin with a description of a concept which I call the *culture of contention*. The concept, I believe aptly, represents the situation inherent in the twentieth century sociocultural anthropology. Against the background of the culture of contention, I will move to consider the specific quality of criticism in the late socialist ethnography. This chapter strives to reveal practical differences which stemmed from different habitual constitutions of Czech ethnographers on the one hand and of anthropologists related to the field of Anglo-American anthropology.

6.1 Anthropology and the Culture of Contention

Let me begin with mentioning two debates which shaped the twentieth century discourse of anthropology. Neither of these debates seem to rank among the most notorious and both are limited to rather specialist issues in anthropology. The first of the debates began with Edmund Leach's Henry Myers Lecture of 1966 the text of which was published in the very same year (Leach 1966). In the lecture, Leach broached the topic of the supposed ignorance of physiological paternity among certain human groups. It is interesting how Leach opened his paper. He quoted from a paper by W. E. Roth, who in 1903 had possibly been the first to pose the problem, and then went on to criticise Frazer's later approach to the problem. In preparing ground for his own solution, Leach brushed aside some contemporaneous ideas held by anthropologists Clifford Geertz and Melford Spiro (Geertz 1966; Spiro 1966). Leach in the course of his paper engaged in a similar way with several other authors and their opinion on the subject. Steering between various intellectual shores, Leach attempted to find a satisfactory solution to the problem.

Two years later, a response to Leach written by Melford Spiro was published in *Man, New Series* (Spiro 1968a). Leach and Spiro's articles provoked several authors to further comment on the topic in the course of two subsequent years. Some of these comments only contributed with new insights, but some followed up with critical remarks

on previous comments (Burridge 1968; Dixon 1968; Douglas 1969; Kaberry 1968; Leach 1968b, 1968a; MacGaffey 1969; Needham 1969; Powell 1968; Schneider 1968; Schwimmer 1969; Spiro 1968b; Wilson 1969). In 1971, Susan Montague, a newcomer to the debate added her essay and Spiro's response to it appeared a year later (Montague 1971; Spiro 1972). Though the debate then lost its momentum, at least two more articles in *Man, New Series* reacted to it – one in 1975, the other in 1986 (Delaney 1986; Monberg 1975). The debate involved fifteen authors in an inter-Atlantic exchange and spanned over the period of twenty years.²⁰²

The other debate which I want to mention concerned animal symbolism. The debate was partially indebted to Lévi-Strauss and his ideas on the topic which he developed in his short book *Totemism* and its follow-up, *The Savage Mind* (Lévi-Strauss 1964 [1962], 1966 [1962]). In the first book, Lévi-Strauss reviewed and criticised previous attempts of mostly American and British anthropologists to provide a satisfactory account of animal symbolism. Lévi-Strauss's ideas later played some part in other authors' work on the subject (Leach 1964), but some later contributions to the topic seem to fall outside the scope of his influence (Douglas 2002 [1966]). Subsequent accounts offered critiques of Douglas's, Leach's and Lévi-Strauss's positions (Bulmer 1967; Halverson 1976; Tambiah 1969). These replies made Douglas acknowledge shortcomings of her own contribution (Douglas 1975, 2002). Also critical of her account, but from an ecological point of view, was Marvin Harris (Harris 1974), but as far as I am aware, no discussion between the two ensued. After some hiatus, the debate over animal symbolism gained new impetus thanks to a collection of essays edited by Roy Willis (Willis 1994 [1990]) and by the “spider and pangolin” debate (Douglas 1993; Fardon 1993; Heusch 1993; Lewis 1991, 1993a, 1993b).

These are just two examples of the countless anthropological debates which dealt with particular problems and cannot be ranked among the most notorious anthropological debates represented by the Mead-Freeman controversy, the Sahlins-Obeyesekere debate or the Yanomami controversy. All the debates in anthropology, however, share several things in common. They involve scholars with different scholarly backgrounds, people of different age from various countries,²⁰³ researchers of different intellectual orientations and

²⁰² The substantial part of the debate is covered in Leach's biography (Tambiah 2002: 269–289).

²⁰³ Even though *Man, New Series*, in which both debates appeared, has been a British-based journal, the contributors to the journal have recruited from various part of the world. Articles which appeared in the Articles Section and which were written solely by British and American authors constituted some two thirds of all the articles published in the 1980s, the remaining articles were written by authors from many parts of the world, including various European countries as well as Australia, Japan, Israel or India. Although one of

specialists in diverse regions. What is particularly telling is the age of contributors to these debates. If we remain with the Virgin birth controversy, Spiro and Leach were divided by a ten-year gap. Leach was born in 1910 and Spiro in 1920. As regards the remaining participants in the debate, Phyllis Kaberry was born in 1910, David M. Schneider in 1918, Harry A. Powell in 1919, Mary Douglas in 1921, Kenelm Burridge in 1922, Rodney Needham and Erik G. Schwimmer in 1923, Torben Monberg in 1929, Wyatt MacGaffey in 1932, Peter J. Wilson in 1933 and Robert M. W. Dixon in 1939, Carol Delaney in 1940. Susan Montague was the youngest discussant as she was born in 1942, making her twenty-two years younger than Spiro and thirty-two years younger than Leach. This suggests that in this particular debate as well as in many others, academic hierarchy did not play any substantial role as to who could contribute with a critical rejoinder, and none of the contributors exploited the age-distance as a weapon of disagreement.

Maybe a more telling example of the weakness of academic hierarchies and age in critical debates is the case of Peter Worsley, an anthropologist who won the Curl Bequest Prize in 1955 for his essay *The Kinship System of the Tallensi: A Revaluation* (Worsley 1956). By the time of its publication, Worsley was thirty-one years old. His prize-winning essay was based on a polemic with Meyer Fortes, eighteen years Worsley's senior and at the time holder of the prestigious William Wyse Professorship at the University of Cambridge.²⁰⁴

Similar examples testifying to the weakness of hierarchies in debates can be taken from other European countries and from scholars who occupied fields which were connected to the anthropological discourse and with social sciences and humanities generally. When Pierre Bourdieu published *An Outline of a Theory of Practice* in 1972, he was forty-two years old. In the book he adopted a critical stance towards a host of scholars including American and British anthropologists as well as towards elder figures of the two prominent French intellectuals, Claude Lévi-Strauss and Jean-Paul Sartre. Lévi-Strauss was at the time sixty-four and was already a holder of the prestigious chair at the Collège de France. Sartre was three years Lévi-Strauss's senior and by the time he was a world-acclaimed author and philosopher who had refused to accept Nobel Prize in Literature

the major criteria of admission to *Man, New Series* has been English language, there are examples of contributors whose contributions were not originally written in English. For example, this was the case of the early 1980s' debate on order in Melanesian religions begun by Ron Brunton (Brunton 1980a, 1980b; Gell 1980; Johnson 1981; Jorgensen 1981; Juillerat 1980; Morris 1982). Bernard Juillerat's reply was translated from French by Andrew Strathern, which indicates that there existed means of transgressing language weaknesses of participants to discussions.

²⁰⁴ The debate is reviewed in Sahlins's *Culture and Practical Reason* (Sahlins 1976).

several years earlier.²⁰⁵ Examples of critical debates can be found among scholars connected to German academia, which has been famed for its conservative and old-fashioned academic setting.²⁰⁶ In the late nineteenth and twentieth centuries, several controversies involving philosophy, history and social sciences broke out among German intellectuals (Frisby 1976: xv–xl). A good example is the positivism dispute (*Positivismusstreit*) which erupted in the 1960s and which touched the topic of the proper methodology in social sciences. The debate involved two senior philosophers Theodor Adorno and Karl Popper and their younger colleagues, Hans Albert, Harald Pilot and Jürgen Habermas. Habermas, for example, was born in 1929 and was half the age of both Adorno and Popper but this did not prevent him from keenly participating (Adorno et al. 1976).

Returning back to anthropology, debating, discussing and criticism have been something regular and widespread and there have existed special sections in academic journals reserved for such purposes. Take *Man, New Series* as an example. Its regular Correspondence section (from 1991 known as Comment section) designed for critical replies and commentaries hosted one-hundred and ninety contributions in total during the 1980s. Of course, not all were intended to be critical as some of them only touched interesting points which had appeared in previous articles or recently published books. Nevertheless, discussions were not limited to the Correspondence Section as disagreement also formed the basis of articles and books. For example, both Tambiah's and Bulmer's articles on animal symbolism were motivated by their disagreement with Mary Douglas whose ideas they aimed to subject to criticism and develop at the same time. Douglas's own book, *Purity and Danger* was itself designed as a polemical piece directed against earlier anthropological treatments of religion.²⁰⁷

The same attitude is also to be found in book reviews among which we can find many critical (as well as commending) ones.²⁰⁸ Reviews can be brief and merely summarize the book in question, but they can be used as powerful means for voicing dissenting views.

²⁰⁵ *Nobel Prize facts*; online

²⁰⁶ The conservative nature of German academia was suggested to me by several colleagues with whom I have discussed this thesis and also by one reviewer of my earlier article.

²⁰⁷ Various named sections intended for critical commentaries, rejoinders and replies have been a stable part of other anthropological journals as well. *American Anthropologist*, *American Ethnologist* or *Current Anthropology* can be adduced as good examples. Regular contributions to these journals are also often motivated by factual, interpretive or explanatory disagreement. A good example of a recent journal which is designed as a medium especially for critical discussion is *HAU: Journal of Ethnographic Theory*.

²⁰⁸ There was even a special irregular section for review articles in *Man, New Series*. It appeared five times during the 1980s. These review articles were usually longer and critically dealt with a book or several books on one topic.

As Robert Parkin observed, the usage of reviews for the purposes of criticism in social sciences can be traced at least to the beginning of the twentieth century. According to Parkin, Émile Durkheim and the group which condensed around his persona and *L'Année Sociologique* employed critical reviews as a means for establishing the group's own ideas (Parkin 1996: 37). What is not without interest is that at the time there existed communication channels between scholars of antagonistic intellectual bents and of different countries. Parkin mentions a correspondence between Robert Hertz and Sir James George Frazer (Parkin 1996: 198). Intellectual exchange across the Channel had been frequent at the time (Stocking 1995: 236) and academic hierarchies or seniority did not present any insuperable obstacles for critical exchange. For example, Hertz, who was a younger member of Durkheim's group, criticised scholars many years his senior, such as pater Schmidt (thirteen years his senior), L. T. Hobhouse (seventeen years) or E. S. Hartland (thirty-three years) (Parkin 1996; chapter 3).²⁰⁹

Debating and criticism in anthropology have not been confined to the print as there seems to have existed institutionalized means for non-written contention. According to Alfred Gell, whose description is worthy of being quoted in full:

The British-style (anthropology) seminar is a peculiar institution with rules of its own. A regular weekly (term-time) event, the 'ideal' seminar usually brings together some 20 or more participants, around a table, under the chairmanship of an experienced teacher and seminar leader. The chairman introduces, and generally gives moral support to, the speaker, while the audience undertake the role of critics, and may, indeed, ask extremely hostile-sounding questions. In a good seminar, there are usually three or four expert seminar practitioners, who can be relied on to give the speaker something of a grilling. The questioning goes on for an hour, allowing time for the more junior members of the seminar to intervene as well and acquire the interrogatory skills of their seniors. However, the seminar is not as unfriendly an occasion as it sometimes seems to visitors unused to its conventions. There is an

²⁰⁹ It is worth stressing that the members of the French sociological school reviewed not only works of their French colleagues, but also works of academics of other European nationalities, although it is unclear to me to what extent it was possible to move between academic chairs of different countries then. One could say that there is not much to gain from a critique of a British colleague and that in this case the space of possible stances does not really correspond to the space of available positions. However, it could be argued that ideas of foreign scholars could be influential in France and any polemics with these could serve as a "proxy war" between French scholars. It could also be means how to maximize symbolic capital, if not a position, within a supranational academic community.

implicit rule that really severe questioning is reserved for speakers who have shown, in the course of their papers, either that they possess the dialectical skill to handle even the most destructive questioning, or, on rare occasions, that they are so bumptious and thick-skulled that they are unlikely to comprehend the devastating nature of the questioning they receive. The mild, tentative, paper from an inexperienced speaker will not be dealt with harshly. Meanwhile, the skilled dialectician relishes the cut and thrust of debate, and exploits the opportunity afforded by hostile questioning to produce additional extemporized displays of wit, turning the questions back on the questioners and making fun of their positions. As the question period draws to a close, the skilled speaker elaborates the main points of the paper in a series of improvisations on themes suggested by the audience. Adrenalin flows copiously through the speaker's bloodstream by this time—now the hard questioning has been overcome—and unusual freedom of expression may be attained. The audience are enjoying themselves too. But the chairman must close the seminar once the time allotted for its duration is over, since, like Cinderella's ball, seminar bonhomie has a fixed temporal compass, which cannot exceed two hours, even by a second. At this point, the chairman thanks the speaker, conducts him to a place of refreshment, where adrenalin is tempered with alcohol, and happy, animated conversations ensue. (Gell 1999: 1–2)

As Gell commented on his essays published in his posthumously published *The Art of Anthropology*, to which the lengthy quote above presents a sort of introduction: “All the essays collected in this volume began life as texts intended to be delivered out loud to audiences, mostly at seminars.” (Gell 1999: 1) This attitude towards debating seems to be a disposition that anthropology students acquire during their university years. When Alfred Gell wrote the above notes on “seminar culture” of the British, he recalled how Meyer Fortes during Gell's years at Cambridge or how Raymond Firth at LSE had used to teach students and run their seminars. Gell was aware that his account of the specific habitus founded on social exchange of mutual criticism was a somewhat idealized description (Gell 1999: 2–5), but alongside the abovementioned examples of criticism, it can be used as a testimony of a distinctive scholarly environment which is very open to contention.²¹⁰ The

²¹⁰ A less idealized depiction of seminar culture is put to somewhat different uses in Bailey (Bailey 1983: 205–206) and traces of a feminist critique of seminar culture can also be found (Benthall 2007: 166).

debating and criticism, the mutual exchange of ideas flowing between scholars is what I call the *culture of contention*.

To avoid a misunderstanding, it is important to stress that not every anthropologist takes an active part in the critical discourse. Not every anthropologist produces critical reviews, or critically responds to the work of his or her colleagues, but every anthropologist can extend the arm to reach for the fruits growing on the branches of critical debates and build their work on these foundations to produce their own work. This is the most important dimension of the culture of contention. It is not to treat criticism and debates as an end in itself, but as a means to intellectual advancement of the discipline.

6.2 Criticism in Ethnography

Let us move to ethnography and see whether ethnographers displayed similar dispositions to criticism. I will focus on various media in which criticism appeared – on regular articles and books, on special sections reserved for discussions, on book reviews and on discussions at seminars and conferences.

6.2.1 Criticism in Articles

The most obvious criticism which regularly appeared in print is the one related to the ideological libations described in the second half of Chapter 3. This was a kind of criticism which was very frequent in Robek's editorials appearing in *Český lid* and it also occurred in other articles and books. It drew from a repository of hackneyed phrases that denounced the imperialist aspirations of the West or lambasted the inherent contradictions which the capitalist societies had been unable to solve, or alternatively the shortcomings of Western scholarly theories which had mainly served as an ideological assistant to expansionist, colonial and imperialist policies. This kind of criticism often went hand in hand with praising the classical figures of Marxist-Leninist ideology who had provided sciences with all the solutions already in the nineteenth and early twentieth centuries. We have seen earlier that in the 1950s, the fierce ideological criticism made by the young Marxist-Leninist ethnographers had usually targeted concrete Czechoslovak scholars in the 1950s and in their criticism, they had sided with similarly specific critique by Soviet ethnographers who had targeted American and British scholars. In the 1970s and 1980s, however,

ethnographers continued to display critical attitudes to capitalism and colonialism, but their criticism was rarely directed against particular individuals.

A similar kind of ideologically motivated criticism related to more concrete things, but things which had been concluded long ago and to people already deceased. A perfect example is Šalanda's essay on shepherd folklore (Šalanda 1986). In the beginning of the paper, Šalanda invoked the debate over the "meaning of Czech history", praised the intellectual position taken up by Zdeněk Nejedlý, and criticised the positions of T. G. Masaryk and Josef Pekař (Šalanda 1986: 24). However, Šalanda only briefly summarized the debate, repeated a critique well-known at the time, and was not motivated to reopen the issue again to provide some fresh thoughts or innovative ideas. While Šalanda's criticism targeted concrete figures, he only reasserted the ideological framework of Marxism-Leninism and Czech nationalism in more intellectual terms. Similar kind of ideologically motivated discourse occasionally appeared in relation to the founding figures of the nineteenth century ethnography. Brouček's criticism of members of the organizing committee of the Czechoslavic Ethnography Exhibition of 1895 (Brouček 1979), or Robek's criticism of political view of some past ethnographers (Robek 1964, 1976) might be said to fall into the same category of critique.

None of the critiques mentioned above was actually developed into an elaborated point of view. These critiques recurrently reasserted things which were in line with the dominant ideological or intellectual framework. Nonetheless, we also find kinds of criticism which can be understood as a genuine academic criticism based on disagreements related to factual, explanatory or theoretical issues, even if the criticism was sometimes based on Marxist-Leninist premises.

In her article on theatre plays, Heroldová was mildly critical of Čeněk Zíbrt, one of the founding figures of ethnography. The basis of her criticism was Zíbrt's previous incorrect assessment of authorship of a theatre play and of the year of its premiere (Heroldová 1982a: 204). We also find some examples of criticism addressed to the living. It can be found in Kašpar's book on early Czech travelogues where he disagreed with previous account explaining the motives of French colonial expansion (Kašpar 1983b: 40). In the same book we find Kašpar disagreeing with another author over the reception of the early modern overseas explorations (Kašpar 1983b: 57).²¹¹ Theoretical criticism manifests

²¹¹ The two authors with which Kašpar disagreed were Milada Červinková and Bohuslav Horák. Neither of them was an ethnographer.

in Vrhel's assessment of Lévi-Strauss's structuralism (Vrhel & Kašpar 1984: 13), and similarly, we find a critical (and at the same time commending) review by Vlasta Matějová and Tomáš Haišman which is concerned with several publications on the Roma minority (Matějová & Haišman 1986). Šalanda spoke out against Johan Huizinga for some unspecified reasons related to Huizinga's understanding of human condition (Šalanda 1980a: 52). Lastly, an outstanding example of criticism is Vrhel's short article published in *Český lid* in which he criticized Soviet Marxist-Leninist ethnography which had paid, according to Vrhel, minimal attention to American cognitive anthropology (Vrhel 1985). Unfortunately, Vrhel's sharp-edged criticism, was not aimed at any scholar in particular and did not take pains with a subtle elaboration of Marxism-Leninism and cognitive anthropology to demonstrate their relative weaknesses and strengths. In none of these cases did criticism serve as a starting point of formulating a rival and comprehensive viewpoint.

Apart from Heroldová's criticism based on some factual matters, the remaining criticisms were rather vague and too underdeveloped as they were restricted to a few statements and a reader hoping to find some elaborate criticism these articles might be eventually disappointed. Probably the most elaborate criticism which I have discovered can be found in the first part of the book on migration and Czech compatriots living in the USA by Leoš Šatava (Šatava 1989). The part opens with an elaborate discussion of a series of theories of migration and in it, Šatava reviews the theories, presents their strong and weak points and shows their possible applicability. Judged by the incidence of this kind of critical evaluation in the writings which I have dealt, it can be said that Šatava's approach was by far very rare in ethnography.

6.2.2 Discussion Sections

While articles and books contained but little disagreement there can be found several examples of contributions which were not intended as original works or material contributions, but as contributions to discussions. Discussion appeared in six issues of *Zpravodaj KSVI* of which one issue was devoted to the topic of the working classes (issue 5/1982), one to the topic of ethnic processes in the borderlands (issue 3/1986) and four were related to the publication of the *Encyclopaedic Dictionary of Ethnic Processes*, a collaborative and interdisciplinary enterprise on which ethnographers worked in the late 1980s (issues 9/1987, 2/1988, 3/1989 and 5/1989).

However, anyone searching these issues for a proof that ethnographers indulged in argument and debating will sooner or later find out that the debating contributions were brief and their spirit was rather similar to regular articles and books, that is collaborative and consensual. Moreover, some of the contributions related to the dictionary aimed to solve some technical and practical difficulties related to the final layout of the dictionary. The impression which a reader of these debates gets underscores the importance of consensus among ethnographers which nicely fits the idea of ethnography as a collective enterprise.

Probably the only signs of a critical spirit showed up in the revived Discussion Section which had not appeared in *Český lid* between 1965 and 1987. Even before 1965, the section had appeared rather sporadically and had not recurred on a regular basis.²¹² When the first contribution appeared in the revived section in 1988, it was accompanied by a note from the editors. The note conveyed that the opening of the section (making it seem that it had once been closed and in the meantime there had been no need to open it until recently as there had been no need to discuss anything) was instigated by the topic of ethnic processes and the intended encyclopaedic dictionary (Hubinger 1988: 43).

The Discussion Section which readers of *Český lid* began to encounter from 1988 was a direct extension of debates, contributions and minutes which had been previously published in *Zpravodaj KSVI*. The discussion also followed the article “The Dictionary of Ethnic Processes (An Outline of a Conception)” which appeared in *Český lid* and was co-authored by Stanislav Brouček, Václav Hubinger, Ludvík Skružný and Ivo Vasiljev (Brouček et al. 1987). Until the end of the 1980s, seven further contributions appeared in the section of which four were directly relevant for the dictionary (Brouček 1988; Brouček & Vasiljev 1988a; Hubinger 1988; Vasiljev 1988). The remaining three touched the topic of ethnic processes, but did not develop into any discussion (Haišman 1989; Sulitka 1988; Vasiljev 1989). Speaking in terms of average length counted in the previous chapter, all these articles were below the average length of seven pages of standard articles published in *Český lid*.

²¹² The section appeared three times during the 1960s. The first appearance was a text on an ancient Bohemian legend written by a folklorist Vladimír Karbusický. His article attempted to analyse the genre and provide dating. Karbusický also disagreed with previous and contemporary attempts to provide genre analysis and dating of the legend (Karbusický 1963). Two other articles appeared under the same section. It was the already mentioned article by Vanda Tůmová and a reaction to it by Stuchlík and Holý (Holý & Stuchlík 1964; Tůmová 1964).

The four contributions developed the terminology used in Bromley's book *Ethnos and Ethnography (Etnos a etnografia)*. Hubinger contested Bromley's original distinction between ethnos and ethnic group and argued that they meant exactly the same (Hubinger 1988: 43). Vasiljev welcomed Hubinger's attempt to reduce the number of terms, yet argued that Bromley's original distinction was justified (Vasiljev 1988: 169–170). Brouček, drawing on previous proposals of his colleagues, was critical of Vasiljev and proposed his own terminology (Brouček 1988). The last article co-written by all these three authors proposed a terminology and showed logical relations between the terms, but it did not contain any criticism (Brouček & Vasiljev 1988a).

The remaining three contributions were also contentious, but in different respects. Haišman's article can be understood as a critique of the failed state policies towards the integration of the Roma minority (Haišman 1989: 35). Sulitka's article can be taken as a review article of several Polish works towards which he adopted a critical stance (Sulitka 1988). Probably the most interesting is the last contribution by Vasiljev who contrary to the popular knowledge and contrary to the prevailing historical narrative argued that the Society of Jesus had had a positive influence on the development of Czech language during the early modern period. If Czechs venerated their martyr Havlíček Borovský who had written an epigram on the enemies of Czech culture in which he had placed the Society of Jesus on a par with "mildew and bookworms", Vasiljev's article contested this mainstream view (Vasiljev 1989).

However interesting and critical these articles and regardless of their possible relation to the *glasnost* and *perestroika* influenced policies of the state by the end of the 1980s, they are by far the only attempts to begin some discussion over important issues in ethnography.²¹³

²¹³ Quite another topic is the peer review in *Český lid*. The topic is related to the topic of criticism and would deserve far more space. In the 1960s, it was customary that contributions to Articles Section in *Český lid* mentioned names of two reviewers by the end of the article. This practice disappeared by the end of the 1960s as regular articles no longer appeared with reviewers' names. Notwithstanding, according to recollections of some ethnographers, peer review continued to exist throughout the 1970s and 1980s. One compared the peer review process to the current practice and reflected on the current stringency of reviewers. The same interlocutor recalled that the article originally submitted later appeared without one paragraph which was removed without prior notice (P0025: 2–3). Another interlocutor was only asked to change the title of the contribution (P0001: 12). When it comes to receiving feedback on writing, one interlocutor recollected that before submitting an article, the interlocutor had asked senior colleagues to comment on the article as well as which journal they would recommend (P0029: 11–12). Some information about the peer review in *Český lid* can be found in Woitsch's article (Woitsch 2013). I was unable to uncover any sources mentioning the peer review process in other journals.

6.2.3 Criticism in Reviews

Similarly, I was unable to discover any book review which would serve as a starting point of some controversy.²¹⁴ Prague ethnographers usually reviewed a host of different publications written by their Prague colleagues or from ethnographers from across Czechoslovakia. The reviews published in *Český lid* are also a testimony to the fact that ethnographers were aware of books published outside Czechoslovakia, including those published outside of the Eastern Bloc. The spirit of these reviews was informative. Their authors commonly offered summaries of books and edited volumes chapter by chapter, contribution by contribution, and very frequently, we find commending commentaries stressing the importance or the novelty of the work published. If disagreements appear, they are restricted to minor things and softened in a little while. The style of ethnographers' reviews can be aptly described as *information service*.²¹⁵

6.2.4 Face-to-Face Criticism

Going through articles and books published during the late socialist period, one is inevitably led to a conclusion that criticism in ethnography was a rare occurrence. The minutes published in *Zpravodaj KSVI* suggest that if any critique appeared, it was mild in essence and engendered a collaborative spirit. During several interviews with interlocutors I asked whether it had been commonplace to engage in a critical discussion with colleagues over their work and whether anything similar to anthropology seminars took place. As regards the situation at the Ethnography Department, one of my interlocutors told me:

“No, [there was no discussion] not only about articles but about anything at all. Because Robek wanted everything under control; and if there were any meetings at all, they did not last long. He usually came late or did not come at all. And if there was a so-called meeting, he made several points about running his errands [for the

²¹⁴ I picked forty reviews which were either written by ethnographers from the two Prague institutions or which concerned books written by ethnographers affiliated to these two institutions. What is interesting is the fact that it was normal for ethnographers to write reviews on books written by colleagues from the same institution.

²¹⁵ I owe this term to Dana Bittnerová. This also very nicely expresses the nature of reviews in *Demos* (see 4.5).

Department] *and about what was to come. There was no discussion about that. And to discuss each other's work – absolutely not.*"²¹⁶

Asking another interlocutor about the presence or absence of critical discussion, the interlocutor replied that at the time “authorities commanded much more respect than today”. However, the same interlocutor remembered that it had not been a situation of the younger who had been expected just “to sit and listen to” and that it had been possible to engage in discussions and, as the case might have been, to correct an opinion of a senior scholar.²¹⁷ Another of my interlocutors mentioned that informal discussions had taken place in pubs.²¹⁸ The last example comes from an interlocutor who compared his experience with ethnography to a later experience from a scholarly environment which he had obtained at a high-profile academic institution in Europe. The interlocutor replied that as a part of their fellowship, it had been an obligation to participate in two “strongly polemical” seminars and remembered that polemical discussions had been a part of academic bon ton at the institution. The interlocutor conveyed that they had not experienced anything even remotely similar in the 1980s’ ethnography.²¹⁹

To round out the picture, it is convenient to include the experience of ethnographers from other major ethnography institutions in Czechoslovakia. As Juraj Podoba, a professional ethnographer with firsthand experience with ethnography in Bratislava and Brno, who later became one of the first Czechoslovak scholars encountering practical anthropology at the first EASA conference in Coimbra in 1990, observed on his cultural shock:

I was especially astonished by how the Western academic community presented itself: their self-confidence, the critical discussions, ... the way young anthropologists took the stage and with sovereignty criticised the oldest, most respected figures in the field – something that in the academic environment in normalisation-era Czechoslovakia a young academic could only have done once. (Podoba in Hann et al. 2007: 29)

²¹⁶ P0025: 5.

²¹⁷ P0029: 5.

²¹⁸ P0035: 9.

²¹⁹ P0030: 8–9.

Taking written documents and interlocutors' recollections together we might get a more plausible view on one of the aspects of ethnographers' practices. Ethnographers were indeed accustomed to share their ideas with a wider scholarly audience. They attended seminars and conferences and discussed their work with their peers, senior and junior. The nature of their work required ethnographers' collaboration on research projects and joint writing. They read and cited their colleagues works. This without any doubt involved discussions and more likely than not some disagreement. At the same time, it is worthy to recall that some cases we have encountered so far point to the fact that some traits characteristic of the culture of contention were present. But then again, these traits seem to have been marginal to the common disciplinary practice. Ethnography was not utterly devoid of the culture of contention, but critical debates were rather scant, their spirit was mild and never escalated into prolonged or heated controversies, at least if we assess debating and criticism in ethnography from the point of view of practice regular among anthropologists. To this we can add the absence of the reappraisal discourse in ethnography which would otherwise include more polemic relations between the living and the dead. If a full-fledged culture of contention existed in ethnography, there are no traces of it in print and in recollections of ethnographers.²²⁰

6.3 Consequences of the Absence

But why is the culture of contention important? Are not discussions, debates, criticisms and controversies mere pastimes of scholars who don their intellectual pursuits in some elaborate academic ritual? The argument which I propose in this thesis is that the culture of contention with all its features presents the main engine of theoretical and methodological development in anthropology. The culture of contention based on an intermittent discussion over variegated issues of varying magnitudes creates something of a permanent revolution.

²²⁰ During my presentation at a seminar organized by the Centre for Theoretical Study, Zdeněk Konopásek mentioned that any criticism had equalled a personal attack in the sense that as a rule, any criticism had been understood as *ad hominem*. Even today, the word criticism (or critique) continues to bear negative connotations in the Czech public discourse as well as in the academic world and any criticism is generally viewed as a negative activity which is directed against individuals and not against ideas. Moreover, critics are often viewed as unacknowledged individuals who cannot produce anything original and the critique to which they subject others is seen as a result of their deep-seated enviousness. It is also a question for further inquiry, whether this criticism of criticism is not but a kind of *officialization strategy*.

While I am sympathetic to this suggestion, I was unable to uncover any systematic links related to scholars' views on criticism. This is especially the sort of phenomenon which would greatly benefit from participant observation. For reasons stated by the end of this chapter, I can only add that these attitudes to criticism probably emerged in the late socialist era.

Regardless of the type of text or genre, anthropologists, by means of writing their contributions, seem to perpetuate anthropological discourse by establishing links of agreement and disagreement with each other, their predecessors and also with non-anthropologists.²²¹

Here it is appropriate to reconnect the notion of culture of contention to the sociology of science of Pierre Bourdieu. The field of anthropology can be imagined as a *field of positions* occupied by various anthropologists. Anthropologists pursue strategies by which they strive to conserve or accumulate symbolic capital. In other words, academics pursue fame which accrues from pointing out to deficiencies of rival visions, from salvaging written-off theories or from formulating better versions of actual theories, from developing better methods or providing more sophisticated descriptions, interpretations and explanations of empirical material. These activities represent the taking of stances in the *field of possible stances*. We usually call the things which result from taking stances theories or schools, such as structuralism, functionalism, interpretive anthropology etc.

A particular theory is in one sense nothing but a petrified product of past struggles over mainly symbolic capital. And such a theory, can be utilized, variously adjusted or completely reformulated in the pursuit of further intellectual advancements fostered by the struggles over capital. Not surprisingly, anthropological theories have their own variants formulated by different authors, or by the same author in the course of his or her career. It is a process of continuous development and elaboration on the weak parts of the theory in its author's attempts to defend and save the theory in the face of criticism. Variations on one theory, which can be found in works of intellectually affiliated authors, then also lead to debates over what some particular label (structuralism, functionalism, postmodernism or else) stands for and who are its proper representatives. This means that labelling of some intellectual complexes, or conversely, rejection of particular labels, is itself a taking of stances.

To sustain the Bourdieusian viewpoint, these activities do not take place in a social vacuum and it would be a misunderstanding to treat resulting theories as products of divine geniuses of individual scholars. The refinements of theories in anthropology take place in

²²¹ A perfect representative of an issue which intellectually connected and divided scholars from various fields including philosophy, anthropology, sociology and economy was the debate on rationality. The origins of the debate can be traced to Peter Winch's book published in 1958 (Winch 1990 [1958]) which incited a series of reactions and counter reactions which appeared throughout more than two decades to follow (Hollis 1972; Hollis & Lukes 1982; Horton 1976; Tambiah 1990; Wilson 1970). Some information concerning the very origins of the debate can be found in Hall's biography of Ernest Gellner (Hall 2010, Chapter 4).

a field of fierce competition where individual anthropologists are exposed to objections and criticism, various sources of inspiration, different points of view or new empirical findings all of which are produced by their colleagues. As Bourdieu writes: “since the logic of competition which sets them against one another means, in the most radical cases, that producers’ best customers are also their fiercest rivals...”²²² (Bourdieu 1991c: 667–668). The anticipation of criticism also serves as a specific pull in writing an article, book or during a preparation of a presentation:

...the professional, who knows the weight of words, because he is at least familiar in practice with the arena where his speech will have to fight for breathing space, that is, the field of simultaneously possible stances in relation to which his own position will be defined negatively and differentially. It is his knowledge of this space of possibilities which enables him to ‘foresee objections’, that is, to anticipate the significance and value which, depending on the prevailing taxonomies, will be attached to a given stance, and to undermine in advance any inadmissible interpretations. (Bourdieu 1991d: 34)

According to Bourdieu, such competitive environment can thrive only under the conditions of high autonomy of the field:

In the sectors of the scientific field that have attained the highest degree of autonomy, the requirements for entry tend to become so elevated that producers have their rivals as their only possible consumers, and the only effective power is that given by scientific competence as recognized by one’s peers/competitors. (Bourdieu 1991a: 15)

It is this interaction between scholars which produces intellectual development in anthropology and perhaps also in many other scholarly fields. Matei Candea’s recent non-Bourdieuian account of the development of theories in anthropology, which is based on Andrew Abbott’s theory of fractals (Abbott 2001; Candea 2019), while offering an eminently complex account of how theory in anthropology develops in time, it tends to

²²² Maybe not surprisingly, the level of contention has been subjected to criticisms as it produces not always positive effects (Tompkins 1988).

downplay this world of constant struggle and therefore results in yet another intellectualist theory. What is missing from Candea's account are the conditions of production of such a discourse, although I believe, that he has intimately known this unsparing and competitive world.

While the struggle over symbolic capital can be perceived as commonplace in the case of anthropological discourse which has subsisted thanks to an unremitting influx of articles and books by anthropologists mostly affiliated to institutions in the UK, USA and France, the situation in the late-socialist Czechoslovak ethnography shows us that the field of science can be constituted in a different way, that is, in a way which does not allow struggles over symbolic capital to don the shape of intellectual competition. The situation in ethnography was very different from the one which has obtained in anthropology. As a result of the absence of the culture of contention, no theoretical approaches crystallized around various arguments. The presence of contradictory claims itself was not a sufficient condition of an emergence of controversy and never served as a basis of intellectual fission which would result into a formation of different intellectual camps. There were no ethnography idealists who would openly claim their allegiance to Bromley and no materialists who would try to defend the original Marxist-Leninist materialism. Nowhere in ethnography do we find a discussion that would revolve around the apparent tensions generated by the notion of survivals on the one hand and the notion of proper historical contextualization on another. These largely unperceived incompatibilities did not present any obstacles for ethnographers to continue in their work. By and large, it seems that the position takings in the field of possible stances in ethnography did not involve disagreement and criticism of colleagues. But why was that so?

6.3.1 The Unavailability of Theories?

The argument presented above goes against several accounts which I have mentioned in Chapters 1 and 2 and which explain theoretical inadequacy of ethnography and folklore studies in a rather straightforward way. We can usually find two interconnected claims. The first claim maintains that the theoretical and conceptual deficiency was a result of the sterility of the dominant Marxist-Leninist paradigm which was not imposed on the scholarly world by the merits of its intellectual persuasiveness, but enforced by the politics of the Communist Party. This claim is sometimes accompanied by another, which states that the socialist regimes were largely insulated from products of the capitalist West regardless of

whether they were beverages, clothes, music or the latest books of Western anthropologists. Not that these products were absolutely unavailable in the socialist Czechoslovakia, but their circulation was for various reasons restricted.²²³ This conviction seems to me to be only partially right. While it is without any doubt true that the latest intellectual products got into Czechoslovakia only in little numbers and that it was only after 1989 that Czech intellectuals were exposed to a sudden explosion of sources (cf. Paleček 2017: 9), this view is blind to the fact that Western intellectual products appeared in ethnography writings and that they were accompanied by some ideological commentary rather sporadically.

František Vrhel, for example, regularly reviewed the latest volumes of *American Anthropologist* in *Český lid*. From his critical article from 1985 we also know that he had access to Western anthropology on which he also lectured in the 1980s.²²⁴ In an introduction to one of the anthologies compiled by Oldřich Kašpar and Vrhel, both authors mentioned a party of definitely non-Marxist scholars including James George Frazer, Ernest Cassirer, Friedrich Max Müller, Claude Lévi-Strauss, Victor Turner, Clifford Geertz, Edward Sapir, Benjamin Lee Whorf, Franz Boas, Paul Radin, Edmund Leach, Émile Durkheim, Alan Dundes, Lee Drummond and Dan Sperber without any accompanying ideological commentary on their theories. The only critique present is based on an apparently materialist disagreement with Lévi-Strauss who according to both authors did not look for the “external determination of mythological structures” (Vrhel & Kašpar 1984: 5–33). Another good example of the permeability of the Iron Curtain is Šatava’s book on migration and Czech expatriates in the USA begins with an extensive discussion of theories of migration. In this part of Šatava’s book we encounter R. E. Park and L. Wirth of the so-called Chicago School of Sociology, Anthony D. Smith, Shmuel Noah Eisenstadt or two American sociologists Nathan Glazer and Daniel P. Moynihan, who by the time of publication of Šatava’s books were active in the American neo-conservative establishment

²²³ Between 1971, when *The Savage Mind* by Lévi-Strauss appeared in its Czech translation alongside a volume of selected anthropological papers edited by Josef Wolf, and 1993, when the Czech translations of *Nations and Nationalism* by Ernest Gellner and *Nisa* by Marjorie Shostak made their appearance, no anthropology book was translated to Czech language. See Appendix II.

The insulation of the Socialist Bloc from Western influences is described in works by Kornai or Yurchak (Kornai 1992; Yurchak 2005).

²²⁴ P0017: 11. Another of my interlocutors recalled an amusing story from Vrhel’s oral examinations when a student had been expected to enumerate representatives of some anthropological currents. The student had answered “There was Marvin... and also Harris.” According to the interlocutor, students of ethnography had acquired their knowledge about anthropology mostly from Vrhel’s lectures and from the small numbers of books which had been translated to Czech language by the time such as *Tristes Tropiques*, Tylor’s *Anthropology* or Morgan’s *Ancient Society* (P0014: 18).

(Gray 2007: 122). Šatava was also one of the authors who regularly reviewed English books for *Český lid*.²²⁵

That ethnographers in Czechoslovakia were generally aware of Western literature can be demonstrated on several other works. For example, the Moravian ethnographer Václav Frolec made use of some of Marcel Mauss's ideas related to gift giving, quoted Lévi-Strauss's introduction to Mauss and works by Ernst Cassirer and the French historian Jacques le Goff (Frolec 1984). Other Moravian ethnographers Vladimír Leščák and Oldřich Sirovátka made use of some formalist and structuralist concepts and even quoted Karbusický, one of the post-1968 ethnography émigrés (Leščák & Sirovátka 1982). If we focus on Prague ethnographers, Bohuslav Šalanda mentioned the Dutch historian Huizinga, quoted Polish translations of Raymond Firth and Mauss and Czech translations of Lévi-Strauss and J. G. Frazer (Šalanda 1980a: 52, 53, 113), Vladimír Kristen referenced Evans-Pritchard's *Social Anthropology* (Kristen 1987: 43).²²⁶ Quoting of these authors could be viewed as ideologically suspicious, but these authors' ideas normally appeared in ethnographers' texts and were unrelated to ideologically-driven critiques. Even Šalanda's disagreement with Huizinga and Vrhel's disagreement with Lévi-Strauss were spelled in intellectual terms. Not that their criticisms did not have any affinity to the political dimension of Marxism-Leninism, but the criticisms do not appear as having been motivated politically in the first place.

The first lesson which can be drawn is that regardless of how restricted was the circulation of the latest writings of authors from the West in Czechoslovakia, those works in limited numbers made their way to Czechoslovakia anyway and could be quoted, mentioned and referred to without any accompanying ideological commentary. It is possible that many more foreign works were actually read than the number of those which were eventually quoted. Some indications can be found in phrases used by ethnographers in their articles. There are articles which use phrases reminiscent of Durkheimian sociology (Pargač 1988: 97; Valášková 1982: 5) or American cultural relativism (Heroldová 1984:

²²⁵ There has circulated a fake review of J. R. R. Tolkien's *Lord of the Rings* which had supposedly originally appeared in the times of socialism in *Rudé právo* (lit. red law), the nationwide daily. The review plays on Cold-War binaries such as the West represented by the bad agrarian and landholding hobbits and the East represented by the good industrial and progressive Mordor. Contrary to this review which was probably written after 1989, it had been possible to write a complimentary review on Tolkien's work on folklore. What is also of interest is that Tolkien's lifelong literary project was likened to that of K. J. Erben, one of the leading revivalists of the nineteenth century (Klímová 1983). *The Hobbit* was published in a Czech translation in 1978.

²²⁶ Some of the Western books were available in Russian translations in the store named *Sovětská kniha* (lit. Soviet Book) and it was possible to order some books from abroad (P0001: 6, P0021: 5).

133; Nosková 1984a: 75; Secká 1988a: 162; Valášková 1982: 153). Some approaches seem to utilize theories of symbolic manipulation (Matějová 1982a; Moravcová 1986; Štěpánová 1987: 9), presuppose ecologic determination (Štěpánová 1987) and other try to propose explanatory schemes for different rates of assimilation of ethnic minorities (Heroldová 1985: 58; Nosková 1984a: 79). And to this we could add the various understandings of the nation included in the discussion of nationalism in ethnography in Chapter 2. It is very likely that ethnographers were influenced by some approaches and sources which they did not openly acknowledge and thus effaced the origins of their inspiration. It is also possible that some ethnographers learned about these approaches from their colleagues by word of mouth, but were not acquainted with any original writings. Besides, the presence of some ideas in the late socialist period might have been a heritage of the more open era of the 1960s.

Contrary to the allegations which tend to portray the late socialist ethnography as insulated from external intellectual influences and in spite of the insulation itself, it is obvious that local ethnographers were aware of theoretical developments outside of the Eastern Bloc. Ethnography's theoretical inadequacy, I believe, can be better explained by the absence of the culture of contention. Contrary to anthropology discourse in which participants have enough incentives and possibilities to work out their theories in detail, ethnographers did not have the prerogative of such an environment. The dearth of relevant social pressures led to the underdevelopment, or rather, the absence of the development of concepts, methods and theories. It is not to say that such a discourse produced unsatisfactory and deficient works. However, as Bourdieu's view suggests, scholars who are exposed to criticism in its various forms – that is in the form of opinion espoused by their colleagues or by other scholars at workshops, seminars or conferences, in the form of reviews of their books or on the form of disagreement printed on the pages of books and journals – have far more incentives to think their work through and work their writings out. Authors not exposed to such pressures have to rely more on themselves which is rather a limiting factor. It is not to say that more academic struggles automatically produce better theories, but it is sufficient to maintain that such an environment has a better conductivity to the production of highly-developed theories.²²⁷ It is at this point that we arrive at a fundamental distinction

²²⁷ This thesis does not account for other, not so beneficial effects of the culture of contention. More than a half century ago, Pitirim Sorokin observed that the “ever-operating forces of rivalry” may have a detrimental effect as they produce new Columbuses who discover things of which their sociological forefathers were well aware. The whole idea of repetitiveness, replication, rediscoveries, or selling “the old intellectual merchandise as the new” (Sorokin 1956: 19) is not discussed in this thesis apart from the instances of recycling mentioned

between anthropology discourse which emerged in and has been propelled by the world of unceasing competition and the discourse of ethnography which missed the competitive dimension.

6.3.1 Theories in Their Inchoate State and Underdeveloped Description

Due to the absence of the culture of contention, we can infer that all the theories (including concepts) we find in ethnography can be characterized as theories in their *inchoate state*. This situation encompassed five interconnected features which can be observed in the corpus of ethnographers' texts: (1) ethnographers did not always mention their theoretical inspiration, be it concrete works or authors; (2) these theories were not conscientiously developed and elaborated; (3) since the theories were not conscientiously elaborated, ethnography writings tended to blur the distinction between the theoretical ingredient and the empirical ingredient hiding the actual theoretical component in the empirical material, or, in other words, the theoretical component almost looked as a natural part of perceived reality; (4) this unawareness of the theoretical component made ethnographers complain about the theoretical insufficiency of their discipline and (5) apart from the short-lived attempts of ethnographers to refine the concept of nation and the like, no developed sub-discourse on theory in ethnography emerged.

In Chapter 2 I attempted to show that regardless of their dominance, the prevalent frameworks of Marxism-Leninism and analytic nationalism had been riven with contradictions which would have under different conditions served as a sufficient incentive to theoretical elaboration and development. We can imagine ethnographers upholding different positions in our fictitious debate between the camps of “survivalists” and “contextualists”. Similarly, another intellectual tension could lead to an establishment of different positions over the question of nations and ethnic groups. Are these distinct groups primarily defined by their culture or psychology or by the socio-economic base? However, it seems that ethnographers were not motivated by solving these contradictions, or at least motivated to delimit their explanatory range without at the same time putting the overall framework into question. Neither Marxism-Leninism nor analytic nationalism were developed by ethnographers into more complex and conceptually nuanced approaches.

in Chapter 5. The reason is that the temporal focus of this thesis is too narrow as to focus on these phenomena. Besides Sorokin's work, some other can be consulted on the topic of repetitiveness (Abbott 2001; Candea 2019; Gans 1992; Goody 1995: 94, 140).

Moreover, we find even more competing explanations, which from the outside point of view point out to a theoretical disarray. We can mention two examples. The first can be found in an article on jewellery by Jaroslava Krupková. Krupková evoked Marx and Engels's dictum of primary and secondary needs and said that while the human needs to beautify or adorn their bodies could not be considered to fall in the rank of primary needs, the tradition of wearing jewellery had appeared at an early stage of development of the humankind and became an essential component of culture (Krupková 1984: 92). Of course, Krupková did not forget to mention that we should approach the phenomenon of jewellery from a historicist standpoint (Krupková 1984: 93), but at the same time it seems that she stepped out of this requirement by presenting the wearing of jewellery as something universally human. The second can be found in Ludmila Sochorová's works on popular theatre. When she wrote about the play on the Biblical queen Esther, a story that had been appropriated by amateur playwrights of the nineteenth century, she mentioned that the central theme of the play had reflected the universal theme of taking a stand against evil. Sochorová argued that we can understand the play as contributing to the development of some universal humanism (Sochorová 1981: 169). Similarly, her book on the popular theatre, even if it works with the premises of national and social emancipation, is inclined to resort to explanations based on universals or on the notion of universally shared human culture (Sochorová 1987c). This is probably due to the fact that Sochorová viewed theatre plays as representing some universal human values, a view which she probably adopted from the theory of arts.

A third contestant was hence introduced – an explanation that referred to some universally shared human phenomena, firstly in the guise of some natural human inclinations, secondly in the guise of universal human values. The problem of evil or the need to adorn were unrelated to a corresponding base or alternatively to a particular national culture or psychology. What is obvious is that none of the ethnographers noted that such explanations were at odds with Marxism-Leninism or nationalism.

The inchoate state of theories within the late socialist ethnography and folklore studies looks like a theoretical Garden of Eden where all the theories happily coexisted side by side. There, materialism lay side by side with idealism as the lion and lamb. The Garden of theories in their inchoate state knew of no inner strife. In my earlier likening I stated that ethnography had not suffered from the absence theory as from inept uses of theory (Balaš 2018: 359). What I offer here is a more elaborated view on the theme: as long as theories remained unquestioned, there did not exist any incentives for employing them consciously

and conscientiously. And the possibility of questioning itself is conditioned by other than intellectual forces.

According to the argument of the previous chapter, ethnographers were constrained by the general publication opportunities. The lack of space considered alongside the absence of culture of contention sheds interesting light on the relation between quality and quantity broached in the previous chapter. As it was foreshadowed, this view is usually evoked in judgements related to individual works or individual authors. The view conveys that we cannot judge the quality of a single work on the basis of the quantity of the ink spilled on it. However, if we approach the relation not from the point of view of individual authors and their individual works, but approach it as the quality of a discourse related to its quantity instead, I believe that the same conclusion as in the case of individuals does not follow.

A discourse requires some necessary amount of space in order to thrive. Ethnography, ethnology and anthropology are largely dependent on descriptions which can be to a certain extent disconnected from their original theoretical context (which they do not always support) and reconnected to different theoretical contexts. Anthropology here serves as an appropriate example – there regularly appear works striving to reinterpret some original material which does not require the re-interpreters to carry the necessary fieldwork themselves, but it suffices to work with the material described by those whom they reinterpret (cf. Bubandt 2014: 7–12; Lévi-Strauss 1964; Tambiah 1968; Worsley 1956). In order to thrive, anthropological discourse requires to be constantly supplied with rich descriptive material so as to provide enough fodder to less empirical and more conceptual discussions. Monographs in anthropology are a good example since they also provide information on things which are not always directly relevant to the main preoccupation of the monograph. This “dangling” information can be taken up by later researchers pursuing utterly different objectives. The discourse of anthropology would not function properly without landmark publications dealing with theoretical issues as well as without a host of descriptive accounts.

If ethnographers were limited by the amount of available space (regardless of whether it was caused by paper shortages, superannuated printing techniques, unavailable printing capacities or increased costs of printing), they could not relatively freely indulge in discussions and in elaboration of their theories as either of these activities requires some necessary amount of space. When we, for example consider the two dominant specializations of Prague ethnographers of the late socialist period – the ethnography of the

working classes and the ethnography of ethnic processes – they were unrepresented when it came to monographs written by individual authors. The only monographs relevant for the topic of ethnic processes, the pivotal topic of the late socialist Czechoslovak ethnography, were Brouček's brief book on the history of the Czechoslovak Foreign Institute and Šatava's book on Czechs in the USA (Brouček 1985b; Šatava 1989). The rest of the discourse on ethnic processes comprised of standard and material articles, chapters in a synthetic monograph and conference papers. When it comes to books in general, the ethnography of the working classes was represented only by one synthetic monograph (Robek et al. 1981). This leads us to an almost paradoxical conclusion. Contrary to the popular opinion which tends to present description as ethnography's top-class quality, we might be led to a conviction that ethnography was actually lacking as a descriptive enterprise because ethnographers could not develop their descriptions even if they wanted to. This scarcity of empirical fodder further served as limiting a potential theoretical discourse.²²⁸

6.4 Explaining the Absence of the Culture of Contention

The only task which we need to undergo now is to attempt to find a satisfactory explanation for the absence of the culture of contention in ethnography. The question at a first glance looks like a tricky one as the goal is to explain an absence, or more precisely, a non-existence of some phenomenon. What follows is an attempt to explain why the culture of contention did not emerge in the late socialist ethnography. This explanation proceeds from a comparison of the field of the late socialist Czechoslovak ethnography and folklore studies with the field of Franglus anthropology. What is being compared here are not merely two intellectual, but two *academic* worlds.

There are at least three possible constraints which played part in shaping the disciplinary field of the late socialist ethnography and which played part in suppressing the emergence of the culture of contention. According to some scholars, the late socialist

²²⁸ There is one further constraint which possibly exerted its influence on shaping the writing habits of ethnographers. One of my interlocutors spoke of a book which the interlocutor was commissioned to write. When the manuscript was handed, but locked in a safe. The interlocutor was remunerated for the work (P0035: 5). This story tallies with the story of Josef Polišenský, the member of CIAS and the Ethnography Department in the late socialist period, whose manuscript on Czechs living abroad Robek hid away (Polišenský 2001: 268). These happenings could dissuade ethnographers who knew about these practices from writing longer pieces.

academia did not support academic virtues such as ambition, self-confidence and completely rejected the cultivation of intellectual ethos which not only concerned the role of scholars within academia but also their role as public intellectuals (Jareš et al. 2012: 106, 189). This constraint is not discussed in this thesis and I would like to describe two different and more structural constraints. Notwithstanding, it should be added that at least as concerns anthropology, there are countless examples pointing out to the high tolerance of mutual criticism which is possibly reproduced in classroom, conference and in-print interactions among anthropologists (cf. Gell 1999: 3–6; Tambiah 2002: x, 60).

The first of the structural constraints is related to the lessened degree of autonomy which was a result of the Communist Party encroaching upon the field of ethnography and folklore studies and which was also related to the state planning imposed from the above. The line of reasoning which follows these constraints has its basis in Chapter 3. The second set of structural constraints is related to forces immanent in the field and has a basis in Chapter 4.

6.4.1 An Explanation by the Diminished Degree of Autonomy

The first set of constraints is related to Bourdieu's variable which holds that larger the independence of the field, the more struggles for power over capital tend to confine themselves to strictly scientific grounds (Bourdieu 1991a: 15). This line of reasoning has its basis in the evaluation of the late-socialist situation: The extra-academic forms of capital, state planning and the Party encroached upon the field of ethnography and folklore studies and made ethnographers dependent on each other in ways which did not involve purely scientific or intellectual matters. We might envisage that if a scholar had dared to write an article in which she had critically assessed a work of her colleagues, she would have exposed herself to a riposte, which would not have been carried along scholarly lines (as a rejoinder aiming to challenge the critical assessment), but would have alternatively included more effective levers of the Party hierarchy. A critical article, for example, could have resulted in a negative assessment of the critic's child applying to study at a university. Or an aggrieved colleague could in turn decide not to support his colleague in some grave matters. To repeat – scholars were dependent on each other not only as ethnographers who desired to contribute to the intellectual progress of ethnography, but also as Party members to other Party members and as Party members to non-Party members. So far, this line of

inquiry makes sense and Bourdieu's sociological variable seems to offer a fair grasp of the situation (cf. Jareš et al. 2012: 189–191, 217).

An explanation along the same lines includes the role of state planning, which created another network of mutual dependency.²²⁹ In the Socialist Czechoslovakia, there existed an institutional plurality within ethnography: the coordinating institution of a plan supervised large national museums, research institutes and university departments. To complete the picture, there existed far more regional institutions, archives and museums scattered across the country where ethnographers also worked. It is presumable that such a plurality would guarantee some safe haven for ethnographers who would have dared to be critical of their colleagues affiliated to different institutions. However, this plurality was not only disturbed by the omnipresence of the Communist Party, but also by the fact that institutions participating on state plans were supervised by one supervising institution. A strictly academic criticism could become a pretext for a non-intellectual retaliation such as distributing less funds or curtailing publication opportunities. For similar reasons, it would have been unwise even of the coordinator to exploit their dominance and launch an intellectual critique of colleagues from dependent institutions as these colleagues could in turn gain the supervising role in the next five-year plan, and hence the original critic would have found himself in an inferior position. This could easily lead to a situation in which even those who were not in the supervisory role did not criticise those who were supervised as it was possible that the roles would be reversed in the future, resulting in a situation of “do not do unto others what you do not want others do unto you.”

6.4.2 An Explanation by Academic Autonomy

The above explanatory approach works with constraints typical of the late socialist state. However, the explanation from diminished autonomy ultimately falters because it does not consider intra-academic hierarchies as a constraint limiting the struggles in the field. The limits of the abovementioned explanation can be demonstrated on an example of recent Czech academia which, compared to its late-socialist predecessor, possesses a relatively high degree of autonomy, but which as a relatively small community gives rise to various internal dependencies. Karel Šima and Petr Pabian give an example of recent grant funding

²²⁹ To repeat, state planning presents a borderline case as it was imposed on the academic world from outside, but at the same time academics were in a position to influence the formation and enactment of plans by means of bargaining.

where scholars' proposals are judged by their peers from within their discipline, but across different institutions (Šima & Pabian 2013: 101). Another recent example of mutual dependencies is the achievement of academic ranks, especially of PhDs, *docents* and *profesors* which open to their holders wide-ranging opportunities to influence the academic world. If one aims to join these ranks, it requires one's work to be judged by senior peers from within the discipline who already occupy these ranks. In such an environment any criticism aimed at colleagues (especially senior), even if it is carried out on strictly expert grounds, might turn out to be an unwise strategy as those whose work is critically scrutinized, might soon be in a position to judge your work and cut your career short. Far wiser strategy might lie in writing laudatory reviews, avoidance of discussion, or in quoting the works of seniors as authorities on particular subjects.

The late socialist ethnography was an analogously small scholarly community and, as we have seen, was permeated by hierarchies too. Apart from the system of academic ranks, three other seniority-based hierarchies were at work (those based on the division of labour, cult of ancestors and discourse of maturation). An attempt at criticism of a higher-positioned colleague would offend the academic bon ton and this could very quickly turn against the initiator. It is more than likely that the hierarchies within ethnography, that is hierarchies based on the possession of capital immanent to the field, could further inhibit the emergence of the culture of contention.

The fact that Bourdieu was blind to this seemingly self-evident factor of the constraining role of the intra-academic hierarchies and of the dependencies which they produce is probably a result of the fact that French, American or British academic worlds with which he had more experience evinced a lesser degree of mutual dependency and a weaker magnitude of academic hierarchies. Such a constellation of field is described, I believe appropriately, by Sydel Silverman who claims that the development of American anthropology in the twentieth century was contingent on its "multicentric structure" which "could accommodate differences of theory and approach" (Silverman 2005: 283, 346–357). This multicentricity is also applicable to the British case and by the same token it is possible that the German *Positivismusstreit* of the 1960s could emerge only because of the fact that its protagonists occupied mutually independent posts – that is two German universities, one Max Planck institute and a university based in a different country. The German dispute points to the growing internationalization in social sciences which we can also find in the twentieth century anthropology. This internationalization did not entail only scholarly exchanges among authors occupying distinct national fields, especially the British,

American and French, but it also led to a more frequent movement of scholars between these national fields eventually contributing to the creation of a supranational one (cf. Kuper 1996: 180).

To sum it up, the key variable which allows the struggles for the capital of strictly scientific authority to be fought mostly by strictly scientific means is not only the degree of autonomy of the field, but also the looseness of internal hierarchies in the field. The looser the mutual bonds, the wider the breathing space which enables scholars to entertain and sustain critical spirit. Therefore, Bourdieu's variable is only applicable in conditions of "multicentric structure".²³⁰

This comparison should not lead us to a conclusion that more autonomous scholarly fields, which are typical for their multicentric structuring, are free of struggles that are fought with other means than those of strictly scientific authority. The literature offers many telling examples from the heyday of British anthropology and hints at enmities and animosities which obtained between the major figures of anthropology. We can read of personal distaste, strained relationships, severe intellectual disagreement and academic politics (Barth et al. 2005; Benthall 2007; Gell 1999: 4; Goody 1995: 68–86; Hall 2010: 117, 343–344; Kuper 1993: 59–60; Stocking 1995: 361–366; Tambiah 2002: 48–52). Some of these sources even testify to the fact that more than once, senior scholars invested their academic authority into suppressing or even silencing intellectual dissent. T. H. Eriksen in his biography of Fredrik Barth disclosed that one of the reasons why Barth did not win the Curl Essay Prize for an essay he had submitted in 1958 was that his essay challenged Evans-Pritchard's intellectual authority (Eriksen 2015: 40). Barth himself mentioned two similar stories in his earlier account of the history of British anthropology. The first story is about Meyer Fortes's rejection of Peter Lawrence's material gathered during a fieldwork in Papua New Guinea, because the material allegedly went against then dominant structural-functionalism. The second story mentions the posthumously published essays of Emrys Peters. According to Barth, the reason for posthumous publication was their incongruity with Evans-Pritchard's view on the Bedouins of Cyrenaica (Barth 2005: 35, 45–46; cf. Fortes 1984: ix).²³¹

²³⁰ I am sceptical to the possibility of providing an exact definition of "multicentric structure". To define it is a field characteristic by the relative looseness of hierarchies or the relative weakness of mutual dependencies is too vague. And to look for some ultimate empirical manifestation of multicentricity is to overlook the fact that one concrete variable can have different effectivity under two sets of partly differing variables.

²³¹ To show that intra-academic hierarchies are just not a speciality of anthropology, we can consider two further examples from sociology and philosophy. First, the example of Karl Popper who in the 1950s occupied

Nonetheless, it should not fall through the cracks that in spite of these cases, in spite of the presence of academic hierarchies in various fields of Western anthropology and in spite of their counter-influence on the development of the culture of contention, Western anthropology was, after all, conducive to intellectual debates, dissent and criticism, which contributed to the intellectual development of the discipline, especially when viewed in comparison with the late socialist ethnography and folklore studies.

This is the gist of my response to Hana Červinková and other representatives of the intellectual approach mentioned Chapters 1 and 2. The trouble is that these approaches do not help us much as regards the deemed explanation of differences between ethnography and anthropology. From the intellectualist point of view, there exist various discourses which can be judged by the elaborateness, durability or applicability of their theoretical apparatus, but which seem to propel themselves solely on the basis of their respective intellectual qualities. Top-quality discourses remain top-quality, mediocre remain mediocre and inferior are not even worthy of our interest. Only now we can respond to authors who are groping for a solution in the darkness of intellectualism. The perceived theoretical inadequacy of ethnography, which starkly contrasts with developments in Western anthropology, was not ultimately a product of intellectual deficiency of local scholars, but resulted from a specific scholarly world which was shaped by specific forces, that is forces which were largely beyond powers of individual scholars. These forces were economic constraints, the infringement of the autonomy of ethnography by the Communist Party and the strong mutual dependencies which existed in the academic world; and all three were characteristic of the late socialist situation.

Before I turn to the last chapter in which I attempt to sketch the postsocialist transformation of ethnography and folklore studies in the 1990s, I want to mention one essential question which is crucial for any future research, but which will remain

a chair at London University and who was turned down by both Oxford and Cambridge as a possible professor in spite of his international reputation. Bryan Magee says that at least in the case of Oxford, philosophers A. J. Ayer and Gilbert Ryle “did not want him on their territory” even though they regarded him as an outstanding philosopher (Magee 1999: 70–71). Similarly Durkheim was kept away from Sorbonne for a long time for his disagreement with several moral philosophers (Lukes 1972: 300–301). Both episodes demonstrate that both Popper in London and Durkheim in Bordeaux could pursue their careers as they could avoid an immediate subjection to their intellectual adversaries in the matters of career advancement. These examples demonstrate that if an academic debate is to thrive, there has to be some amount of breathing space which allows one to escape the situation that one shares home institution with, or is closely dependent on, his adversaries who might possess more academic power.

Coming back to anthropology, Fredrik Barth expressed an opinion that this domineering style went out with the retirement of big men – Evans-Pritchard, Fortes, Firth and Gluckman (Barth et al. 2005: 53), but since the history of younger generations of anthropologists has not been written yet, we might sooner or later discover that Barth’s opinion is applicable to the recent situation as well.

unanswered for it is beyond of the scope of this thesis: At what point in time did this system, nonconductive to intellectual debates, emerge? As we have seen, there is an opinion according to which the Czech academic world, since it has shared its tradition with more conservative and more hierarchy-driven German academic culture, has been equally less charitable to intellectual criticism. Some accounts point out that strict age-based hierarchies dominated Czech science in the late nineteenth and early twentieth centuries (cf. Sklenář 2013: 44; Sosna 2013). This suggests that the Czechoslovak late socialist ethnography inherited these hierarchies supposedly suppressing intellectual criticism. If we, however, peek in the past, we sooner or later find out that regardless of the strict, age-based hierarchies which existed in the academic world, there was also a prolific tradition of criticism which cut across hierarchies and differences of age and which was especially important for Czech social sciences and humanities of the late nineteenth and early twentieth centuries which presented a mixture of archaeology, biological anthropology, sociology, history and ethnology of the time. These debates transcended the confines of the academic world as they played some political role on the national scale. These concern the struggle between the so-called Museum School and University School, the Manuscript debates and the Debate over the Meaning of Czech History and subsequent debates on historical methodology, or the interpretation of Czech medieval legends (cf. Havelka 1995; Kalandra 2018a; Křest'an 2012; Sklenář 2013).²³² Similar debates took place in the first half of the twentieth century among Czech literary authors and artists (Ort 2016). All these debates were possessed by critical spirit. The situation possibly changed in the 1950s with the infringement of the academic world by the rule of the Communist Party. Yet some degree of intellectual discussion and criticism was a stable part of the 1960s' ethnography which was possibly a result of the political liberalization (Holý & Stuchlík 1964; Jeřábek 1964; Karbusický 1963; Kramařík 1961; Šach 1968; Tůmová 1964; Veverka 1969). It seems as if intellectual criticism in ethnography died out with Robek's ascent.

²³² Writing from the perspective of the recent Czech academia, Sklenář offers vivid details of the turn of the nineteenth century disputes among Czech scholars. As an explanation Sklenář holds that the unscrupulous level of criticism was a result of "an all-pervading spirit of amateurism" which did not differentiate between arguments *ad rem* and *ad personam*, as it was not only particular arguments which were at stake, but also personal honour. Sklenář opposes this scholarly setting to the recent situation which is typical of more professional conduct save for partial exceptions (Sklenář 2013: 44). I am inclined to view Sklenář's opinion as representing recent, rather contention-less Czech academia, which, if necessary, favours softer ways of expressing disagreement or avoiding it altogether.

7. Conclusion: Continuities and Discontinuities

In the introduction, I mentioned that the work on this thesis had originally begun as an attempt to make sense of the beginnings of Czech sociocultural anthropology after 1989. It is precisely the beginnings of Czech sociocultural anthropology with which this thesis concludes. In this concluding chapter I briefly summarize the findings. On the basis of these findings I further attempt to sketch some outlines of the post-socialist transformation of ethnography and ponder ethnography's heritage in relation to the nascent tradition of Czech sociocultural anthropology.

In the third issue of the 1989 volume of *Český lid*, published in September, Robek commemorated the bicentennial of the French Revolution (Robek 1989). The timing could not be more perfect: in November, the Velvet Revolution swept Robek away. The Communist Party of Czechoslovakia ceased to be the only ruling party and an era of democratic pluralism and free market economy followed. This entailed an abrupt end to the Party hierarchy grown through the society from local neighbourhoods up to the governing body of the state. Marxism-Leninism was abandoned as the official ideology, state planning was forsaken and the Secret Police lost its position as a coercive apparatus. These changes had an immense impact on the structure of the field of ethnography and folklore studies. As a result, it was not possible to pursue party and clandestine capital anymore and convert them into other forms of capital in order to improve on one's chances in the field. Universities and academic institutes achieved autonomy which they had not enjoyed at least for twenty preceding years. The end of Socialism also meant that citizens did not need party approval for travelling abroad. Thus, academics enjoyed an unprecedented freedom of travel which endowed them with an improved possibility to establish professional connections with colleagues abroad whom they could in turn invite to Czechoslovakia. Besides, scholars could more easily come by foreign journals and books and overtly seek intellectual inspiration outside the country.²³³

²³³ One of my interlocutors mentioned that at least up until the late 1990s, it was not so simple to come by foreign literature. One of the channels was opened by those who were awarded fellowships at foreign universities where they xeroxed books and let them circulate after they returned home (P0009: 10). In the same decade, foreign institutions like CEU or CEFRES established their libraries in Prague. These libraries were available even to those who were not themselves affiliated to these institutions (Skalník 2002b: 109; Fingerland 2013, online). Local universities also began to fill up their own libraries with foreign works. In this connection, Skalník mentions the library of the Faculty of Humanities (Skalník 2002b: 107). Lastly, foreign anthropology books were disseminated thanks to the Sabre Foundation (Plášek 1999: 74).

The above changes represent the most important discontinuities in Czech academia generally, including ethnography. Apart from these large-scale changes, we should pay heed to some no less important continuities, especially the personal continuity. The main factor behind the personal continuity was the “velvety” approach which was very soft towards the former members of the Communist Party. The revolutionary situation in Czechoslovakia was much more tolerant and the pressures from above to dismiss all the previous Party members from academia were not so strong as pressures in the former East Germany where the previous Communist Party members were ousted from academia (Hann 2009: 223; Skalník 2002a: vii). I am not in the position to speak on behalf of other disciplines, but as regards Czechoslovak ethnography generally and Prague ethnography in particular, they evinced a great deal of personal continuity. Both Stanislav Brouček and František Vrhel, two post-revolutionary heads of the Ethnology Institute and the Ethnology Department, recruited from the ranks of late socialist ethnographers. Vrhel replaced Šalanda, who remained at the department, and Brouček replaced Robek, who left the field for good. Some personal changes took place at the Ethnology Institute which was forced to dismiss many of its employees in the early 1990s as a result of retrenchment policies pursued by the management of the Academy of Sciences (Šima & Pabian 2013: 99).²³⁴ Some of the dismissed dispersed across various institutions (Bittnerová & Moravcová 2017: 175), remaining ethnologists were retired on a pension or left the discipline for good. The personnel at the Ethnology Department remained the same, although there is a strong indication that between 1989 and 1994 there existed pressures and counterpressures to carry out personnel changes.²³⁵

The strong personal continuity is a key to understanding the post-socialist transformation of ethnography into ethnology and sociocultural anthropology. Many of the scions of Robek’s era, his students and colleagues continued to be academically active after 1989. In this sense, the post 1989 situation resembled the Stalinization era which had followed the 1948 Coup d’état as well as the Normalization era which had come in the wake of the 1968 invasion. Ethnographers active in the late socialist period retained their dominance and this dominance had certain consequences – personal continuity allowed intellectual continuity as well as continuity in scholarly practice. And to these continuities we turn in brief now.

²³⁴ General information about retrenchment policies related to academic world in post-Socialist countries can be found in Györgi Péteri’s article (Péteri 1995).

²³⁵ P0001: 18–19; P0017: 5, 10; P0021: 18.

7.1 Intellectual Continuities and Discontinuities

As the 1990s' ethnology texts testify, former ethnographers were quick in throwing overboard Marxism-Leninism with its all-time classics – personalities, canon, hackneyed phrases and theoretical concepts. Some might consider this as an unfortunate occurrence as ethnologists jettisoned the ideological burden of Marxism-Leninism without attempting to salvage useful materialist notions. If there was something that ethnologists retained from the intellectual vision of socialist ethnography, it was its historical orientation, this time deprived of its specific teleology – ethnologists dispensed with the laws of history, the successive historical formations or the general materialist framing of the discipline. What remained was the idea of studying long-term processes and probably the notion of historic contextualization. What also, unsurprisingly, survived, was the doctrine of survivals alongside an evolutionary view on difference. Both the proper historic contextualization and the doctrine of survivals were divorced from the idea of formations defined primarily in terms of socio-economic conditions.

Analytic nationalism alongside some idealist conceptions discussed in Chapter 2 seemed to be strong as ever before and ethnologists continued to write on a family of topics related to the Czech National Revival. Thus we find eulogizing pieces on middle-class revivalists and past ethnologists (Brouček 1999; Štěpánová 1992, 1994, 2005, 2008; Štěpánová et al. 2001), on the Czechoslovak Ethnography Exhibition in 1895 (Brouček 1995; Brouček et al. 1996; Secká 1995; Štěpánová 1992, 1994) or on other topics related to the national emancipation of the Czech nation (Moravcová & Svobodová 1993; Štěpánová 1997). Needless to add that this time ethnologists wrote about the past without occasionally scolding their intellectual predecessors for their ideological failings. Those who are unaware of the intricacies of Marxism-Leninism and the late-socialist ethnography texts might consider these writings as a result of a turncoat strategy, for writing on bourgeois ethnography came back in vogue with the return of liberal capitalism to the country. Nevertheless, as we have already seen, nothing is further from the truth. It was regular to praise bourgeois ethnography before 1989 and this did not change hereafter.

The topic of ethnic processes remained a stable focus of orientation. For a few more years in the early 1990s, ethnologists continued in their conceptual discussion which revolved around Bromley's concepts and was related to the encyclopaedic dictionary, but the discussion soon died out (Brouček et al. 1991; Hubinger 1990, 1992; Uherek 1990). In

spite of the disappearance of the conceptual discussion, ethnological research on particular ethnic and national groups and processes remained steeped in Bromley's analytic nationalism deprived of its materialist ingredient, though some younger ethnologists working at the Ethnology Department offered new directions for the study of ethnic and national phenomena (Dubovický 1996a; Lozoviuk 1994, 1997). Not only can the continuity be traced on conceptual level, but also on the level of subject matter as ethnographers remained to be interested in ethnic groups in the Czech Republic and continued to research on Czech communities living abroad (Brouček 1993; Dubovický 1996b; Secká 1992, 1993; Šisler 1991; Uherek et al. 1997, 1999; Valášková 1992). Seven more volumes on Czechs abroad were added to the *Czechs Abroad* series of the *Národopisná knižnice* edition during the decade. The research on ethnic and national groups continued to have a strong moral appeal which was eagerly espoused by ethnologists. Here it is apt to quote from Oldřich Kašpar's preface to his book on Czech Jesuits: "even the members of a *small nation* are able to accomplish magnificent things." (Kašpar 1999: 7, italics original).²³⁶

This intellectual continuity was probably contingent upon the durability of Czech national feelings as a wider social phenomenon which had throughout the twentieth century united various strata of Czech society including Communist Party members, scholars or dissidents (cf. Šimečka 1990: 146), a fact also noted by Ladislav Holý in his seminal work on Czech nationalism. As Holý persuasively showed, even the socialist Czechoslovakia was based on national principles (Holý 1996: 7–8), and perhaps unsurprisingly, the first public demonstrations of 1988 and 1989 against the socialist regime had been carried out "in the name of the nation... against what was generally perceived as foreign oppression." (Holý 1996: 9). Perhaps, this explains the observation mentioned in the beginning of Section 2.3 that recent accusations of past ethnographers for their nationalism made by Czech scholars have been less frequent than accusations of Marxism-Leninism.

Idealism, which we have discovered in the heart of ethnography in Bromley's theory of ethnos, newly found some expression in a strong shift towards hermeneutic, symbolic, interpretive, structuralist and poststructuralist approaches (Krupková 1991; Skupnik 1995, 1997, 1998, 1999; Vrhel 1991, 1993, 1996). These intellectual approaches were not referred to before 1989, but they had been very probably known earlier as the sources listed in Vrhel's articles on postmodern ethnography and hermeneutics published in the early 1990s

²³⁶ For more information about the continuities in subject matter which stretch to more recent era, consult an article by Blanka Soukupová (Soukupová 2013).

demonstrate. Clifford Geertz became an intellectual role model for the younger generation of students and scholars affiliated to the Ethnology Department in the 1990s²³⁷ and a Czech translation of Geertz's *Interpretation of Cultures* appeared in 2000. While these idealist theories were largely novel in the Czech context, it cannot be said that they were without an idealist precedent.

7.2 Habitual Continuities and Discontinuities

Nonetheless, some degree of continuity cannot be merely traced on the level of the subject matter or theories. What ethnographers also carried into the new era were specific academic habitus which they acquired in the late socialist period. Thanks to the personal continuity in the field, ethnologists could continue to work in ways to which they had been accustomed to in the 1970s and 1980s. In this thesis I have focused on several dimensions of ethnographers' practices which resulted from ethnographers' scholarly habitus shaped to a profound degree by the possibilities of the field. In the three preceding chapters I explored several layers of what had constituted the everyday life in the late socialist ethnography: hierarchies, research ways, language competences, writing habits and attitudes related to discussing and criticism. I have tried to argue that ethnographers were limited by publishing possibilities in the first place. As a result, both description and argumentation were limited due to material constraints. At the same time, critical discussion among ethnographers was a rare occurrence. This was not only a result of the fact that in order to develop competing theories scholars required some necessary amount of publishing space, but also a result of the strong hierarchies immanent in ethnography and of extra-academic forces which hampered a critical discourse from emerging.

Such an analysis also sheds light upon the theoretical development in ethnography. Since ethnographers were not primarily motivated by the possibility of earning symbolic capital by means of formulating better theories or developing better concepts and methods, theoretical development in ethnography stalled and what we encounter in their writings are only theories in inchoate state which I described in Chapter 6. It was probably this underdevelopment of theory which had a further paradoxical effect: reading ethnographers' writings today, we can stumble over many theoretical concepts, yet it looks as if ethnographers themselves were unaware of these concepts *qua* theoretical concepts. Their

²³⁷ P0017: 7. In the words of one of my interlocutors who studied in the 1990s: "We all were inveterate Geertzians." (P0009: 21).

concepts blended with the empirical ingredient and were treated almost as a natural part of perceived reality. At the same time, the situation created a wide gap between what ethnographers considered to be empirical enquiry on the one hand and theoretical ruminations on another. This accounts for the unremitting complaints of the insufficiency of theoretical development in ethnography voiced by ethnographers themselves as well as for their various attempts to rejuvenate ethnography by injecting novel theoretical inspirations into it. In the 1990s, this time without being under any Marxist-Leninist duress, several ethnologists attempted to refresh the theoretically stalling discipline with new inspiration – Ivan Dubovický wrote an article which introduced the Chicago School of Sociology to Czech ethnologists, František Vrhel wrote something similar about postmodern and hermeneutic trends (Dubovický 1996a; Vrhel 1991, 1993) and Jaroslava Krupková offered a theoretically-minded work on the concept of tradition (Krupková 1991).²³⁸ Their contributions, however, did not show the way the empirical material could be connected to these theories. They convey the impression of the theory for its own sake and can be seen as supporting the existence of the peculiar native view on the theoretical/empirical divide created before 1989.

Let us now focus on the writing habits which possibly present the most complicated case. The major change seems to have occurred in the structure of the kinds of articles. While in the 1980s, as we have seen in Chapter 5, material articles and conference papers were very common, they somehow went out of fashion in the 1990s. The major reason was probably the end of *Zpravodaj KSVI*, discontinued in the early 1990s, which had up to then served as the main medium for the publication of material articles and conference papers.²³⁹ It is possible that this was a result of the lower number of conferences, seminars and workshops taking place in the 1990s, or alternatively a result of the fact that conferences took place, but the subsequent papers of which were not published. This would tally with the streamlining policies which may have limited the organization of such events.²⁴⁰

²³⁸ The 1990s' attempts of ethnologists to inject ethnography with a new inspiration resemble similar attempts from the 1960s (Polednová 1969; Skalníková & Fojtík 1969).

²³⁹ In spite of the decline in numbers, material articles continue to appear even today. For example, *Studia Ethnologica Pragensia*, the successor of *Studia Ethnographica*, continues to publish articles in the section. The section is called "Materiály" in Czech and is translated to English as "Reports" (see *Studia Ethnologica Pragensia 1/2017*). Conference reports in the same journal are headed in the section called "Zprávy", which is translated as "Notes".

²⁴⁰ The last issue of *Zpravodaj KSVI* appeared in 1991 and contained conference papers from a seminar from 1988. This was the only issue of the journal published in the 1990s.

This is not to say that the conference life died out or that conference papers ceased to be published. Some conference papers appeared in *Národopisný věstník*, *Studia Ethnographica* or *Národopisná knihovna*.

Table 7.1 shows the difference in output per decade as regards both groups. The table suggests that although written products had a different structure in the 1990s, that is a different ratio of various kinds of articles, ethnologists continued to keep a high output rate. This is especially evident in the Ethnography/Ethnology Department group which continued to maintain the high level of publication output. The only problem here is posed by the group from the Ethnography/Ethnology Institute. Whilst I am certain that the first group remained at the Ethnology Department throughout the 1990s, I was unable to trace the fates of all of the ethnologists included in the Ethnology Institute group.²⁴¹ Although the table includes all the writings which the members of the Ethnology Institute group published in the 1990s, the marked drop in production is definitely a result of the fact that some ethnologists of the latter group ceased to be academically active for various reasons in the 1990s.

TAB 7.1: Publication Output in the 1990s²⁴²

<i>decade</i>	<i>group</i>	<i>standard</i>	<i>material</i>	<i>conference</i>	<i>total articles</i>	<i>books</i>
1980s	Ethnography Department	23	15	26	64	24
	Ethnography Institute	48	37	69	154	1
1990s	Ethnology Department	51	2	9	62	17
	Ethnology Institute	34	1	8	43	2

For this reason, I offer another table which traces just the overall production of three ethnologists: Stanislav Brouček, Naďa Valášková and Milena Secká. All three were active

However, the number of conference papers published in the 1990s is outmatched by their numbers from the 1980s.

²⁴¹ According to an ethnology encyclopaedia (Brouček & Jeřábek 2007), Jaroslava Kadeřábková moved to the University of Economics in Prague after 1989. Iva Heroldová worked at the Ethnography Institute until her retirement in 1986, but she continued to publish for several years to come. Helena Nosková was employed at the institute until 1990 and her later scholarly career is connected to the Institute for Contemporary History of the Czech Academy of Sciences. Antonín Jiráček worked at the institute until 1993 after that he moved to work for the Czech Television. Stanislav Šisler continued to be employed at the institute until his death in 1996. The encyclopaedia does not provide any information of post-1989 fates of Vladimír Kristen and Vlasta Matějová.

²⁴² Tables 7.1 and 7.2 includes the same journals and editions (or their successors) which were included in the analysis in Chapter 5.

in the 1980s as in the 1990s. The first two worked at the Ethnology Institute, Milena Secká moved from the institute to Náprstek Museum in the early 1990s, but continued to be academically active.

If the numbers of the Table 7.2 are to be taken to represent the situation of ethnologists affiliated to the Ethnology Institute in Prague in the 1990s, then we can see a significant drop, but not among employees of the Ethnology Department whose level of written production, as the Table 7.1 shows, equalled that of the previous decade. The drop in the production of the Ethnology Institute could also be a result of the streamlining policies which restricted the scholarly activities within the Academy of Sciences, but which did not affect Charles University.

**TAB 7.2: Publication Output in the 1980–1990s:
Selected Ethnologists of the Ethnography/Ethnology Institute**

<i>decade</i>	<i>ethnologist</i>	<i>standard</i>	<i>material</i>	<i>conference</i>	<i>total articles</i>	<i>books</i>
1980s	Brouček	9	9	8	26	1
	Valášková	6	1	5	12	0
	Secká	2	6	3	11	0
1990s	Brouček	14	0	4	18 (-8)	0
	Valášková	7	0	1	8 (-4)	1
	Secká	6	0	0	6 (-5)	0

What in the context of writing seems to me to be of particular interest is the continuation of attitudes to book writing, especially in regard to monographs. I argued in Chapter 5 that the writing of longer monographs went out of style in ethnography in the late socialist period and that ethnographers did not resort to this practice as to the means of establishing their professional authority. It seems that this practice persisted throughout the 1990s, writing of longer monographs, or monographs generally, continued not to be essential for the discourse of ethnography as compared to article writing. For example, out of the seventeen books published by the employees of the Ethnology Department in the 1990s, only eight were monographs and the remainder composed of textbooks and anthologies. If we compare these monographs by their length, then except for two shorter monographs (Šalanda 1997; Štěpánová 1995), there was a marked continuity in the length

of monographs published in the 1980s and the 1990s (Kašpar 1990, 1992a, 1997, 1999; Krupková 1991; Šalanda 1990).

Concomitantly interesting is the structure of the kinds of books. Besides monographs and textbooks, ethnographers continued to compile anthologies and maintained the specific logic in which they presented their findings as facts which were literally able to speak for themselves. This was for example Šatava's encyclopaedia of national minorities in Europe (Šatava 1994),²⁴³ or an anthology by Oldřich Kašpar (Kašpar 1992b). As regards the use of illustrative figures mentioned in Chapter 4, even in this case, the logic continued to be the same.

As regards the culture of contention described in Chapter 6, it can be said that nothing substantially changed, although the field opened to an unprecedented even if still somewhat low level of criticism. If it cannot be said that criticism animated the discourse of ethnography in the 1980s, pretty much the same can be said of ethnology in the 1990s. The Discussion Section of *Český lid* which mostly revolved around the topic of ethnic processes and which hosted five more contributions in the early 1990s (Holý 1992; Hubinger 1990, 1992; Jiráček 1991; Uherek 1990), soon ceased to appear regularly and until the end of the decade it would host only three more papers (Kandert 1996; Langhammerová 1997; Mušinka 1999). We can find some sharp-edged and elaborated reviews, such as Ludvík Skružný's review of an ethnographic encyclopaedia of kitchen utensils written by his colleagues Vlastimil Vondruška, Tomáš Grulich and Violeta Kopřivová. The review accused the authors for classificatory obscurity and for causing confusion among the young students (Skružný 1990: 235), but it remained without a reply. Some critical evaluation of theoretical currents appeared in books (Krupková 1991).

Ethnologists' scholarly community was not animated by the culture of contention in the way in which anthropological communities have been. Contrary to American, French or British fields of anthropology, there did not appear enough incentives which would make Czech ethnologists engage in critical discussions related to their works. Some contention was present, but it did not lead to prolonged discussions. As a consequence, ethnologists had not much incentives which would in turn lead to elaborating, sharpening and honing of their intellectual tools. To this we can add the continuing absence of the reappraisal discourse mentioned in Chapter 4 or slow attempts in acquiring language competences

²⁴³ Šatava's encyclopaedia is an edited version of the same book which had appeared in two issues of *Zpravodaj KSVI* in the 1980s.

which would have possibly served as a means of quicker incorporation into a more international scholarly community.²⁴⁴

Generally speaking, these are some of the continuities which we can trace in the post-socialist transformation of ethnography. The situation can be aptly described by resorting to Bourdieu's concept of the *hysteresis effect*: whereas the field changes, habitus are driven by inertia and remain largely unchanged and attuned to the past possibilities of the field. It takes some time before habitus accommodate to the recent transformations of the field. In the light of what has just been said, there emerge at least two objections which may be raised against this concept in relation to the situation with which I have been dealing.

Firstly, while we may hold that the concept indeed reflects the post-revolutionary transition of ethnography, it is at the same time all too vague. Sticking to the concept and focusing on intellectual matters, for example, we might ask: Why did ethnologists quickly abandon Marxism-Leninism, but not nationalism? We would presume that Marxism-Leninism would not have been abandoned, but this does not mirror the actual situation. Of course, the answer as to why Marxism-Leninism was abandoned should be clear, but the concept alone is insufficient for finding the right answer, because it does not tell us why *some* features persisted and *others* did not. Secondly, and perhaps more importantly, the concept might eventually prove unsuitable for another reason. For we are dealing with strong personal continuity, we should remind ourselves that it is individual agents, who also structure the field of ethnography as a field of forces. If individuals who occupy the field do not change or are not replaced and continue to represent a majority, then the individuals themselves act as agents of inertia of the field. For these reasons, the hysteresis effect serves rather as a useful explanatory guide, because it presupposes a certain degree of conservatism in what people do under changed conditions. But the concept is nothing else.

²⁴⁴ Standard articles published in *Český lid* in the 1990s continued to be accompanied by German summaries, but some of the articles were published with English summaries instead, signalling a reorientation towards a more international audience. The six volumes of *Češi v cizině* published by the Ethnology Institute also included English summaries. The eleventh and the last contribution to the volume was the first of this series which was completely in English and it was probably a first ever such publication of the Ethnology Institute. The first ever English publication of the Ethnology Department is probably the *Ethnological Scripts* (Tomandl 2003). The volume contained a collection of articles translated to English, some of which were published earlier in Czech. These translated volumes do not mention any translator and it is highly unlikely that ethnologists provided the translation themselves.

It is upon any future research to contribute with further findings which would better illuminate the conditions which favoured continuity in some features, but discontinuity in others. Any such research should also pay heed to various transformations of the Czech academic world. For example, the strong academic hierarchies which we have encountered continued to play an important role from the 1990s onwards. Whilst the acquisition of the highest academic ranks ceased to be conditioned by some degree of conformity to the Communist Party rule, these ranks continued to play a pivotal role in the academic world as those who seek to join them are subjected to the judgement of peers already occupying these ranks. Moreover, ranks became crucial for the Czech higher education accreditation system or for the evaluation of funding proposals and research grants. In a relatively small academic community where these ranks are not bound to jobs, but are granted by the state legislation and universally acknowledged by institutions both public and private, they have a strong potential to create a network of mutual dependencies which can further curb intellectual criticism (Šima & Pabian 2013: 101). So even if the autonomy of the academic world is high, it is doubtful that scholarly struggles “tend to confine themselves to strictly scientific grounds.” (Bourdieu 1991a: 15). Any such research will have to include a far greater pool of scholars and it will have to deal in a more detailed fashion with some realities of the post-1989 academia and higher education generally – the disintegration of the state planning, the appearance of the free market and private publishing houses, the introduction of grant funding and the adoption of the audit and evaluation policies (cf. Šima & Pabian 2013). All these shall be considered on the background of habitual continuities, on the basis of the inherited mode of work. For the aforementioned reasons, conclusions related to the post-Socialist transformation of ethnography are hereby merely provisional.

7.3 A Clash of Two Worlds

The stress on the habitual dimensions of scholarly work does not only allow us to better understand the post-socialist transformation of ethnography into ethnology, it offers several crucial advantages for understanding the post-socialist attempts to establish an independent tradition of Czech anthropology. In a certain sense, all the previous comparisons with anthropology served merely as a tool for highlighting differences, for bringing out peculiarities of Czechoslovak ethnography, or at least its Prague specimen. Now, comparison becomes interesting for one more reason. It provides us with fresh insights into the hardships of the beginnings of the Czech tradition of sociocultural anthropology. But

how is that so? Are not ethnography and anthropology two distinct disciplines? As I will try to prove in this ultimate part, the fate of Czech anthropology has been inextricably bound to the heritage of Czech ethnography. The focus on habitual dimensions can reveal at least two points of contact between the two disciplines. Firstly, it was the failed attempt to establish anthropology within the Ethnology Department. Secondly, it was the professional trajectories of the first generations of anthropologists who began their professional careers in the 1990s as students in ethnology.

7.3.1 Savages from the West

In the 1990s, a diverse group of anthropology enthusiasts with various backgrounds took part on the attempts to establish Czech sociocultural anthropology. These individuals can be roughly divided in four groups. Some recruited from Czech ethnographers who were formerly employed at the Ethnography Institute or Ethnography Department or graduated from the latter. This group includes Václav Hubinger, Ivan Dubovický, Josef Kandert, Zdeněk Uherek, František Vrhel and Josef Wolf. Others recruited from émigrés who had been in the past linked to Czechoslovak ethnography both as students and professionals. Good representatives of the second group are Ladislav Holý, Andrew Lass and Petr Skalník.²⁴⁵ The third group consists of anthropologists of Czechoslovak origins who studied anthropology outside Czechoslovakia and who were unrelated to ethnography. Ernest Gellner, Leopold Pospíšil, Zdeněk Salzmán,²⁴⁶ David Z. Scheffel or Jitka Kotalová belong to this group. Lastly there were local scholars interested in anthropology and also unrelated to ethnography: Ivo Budil, Zdeněk Justoň, Zdeněk Pinc or Václav Soukup.²⁴⁷ The members of the last group were also unrelated to ethnography. While all of these individuals undeniably contributed to the beginnings of Czech anthropology, they had various stakes in the process and by no means comprised a united front.²⁴⁸

²⁴⁵ There is one more connection in the case of Peter Skalník whose parents, Olga Skalníková and Milan Skalník, were both professionally connected to ethnography.

²⁴⁶ Salzmán's professional interests, however, were related to ethnography and he even collaborated with Vladimír Scheufler, an Ethnography Institute member (Brouček & Jeřábek 2007: 191).

²⁴⁷ I was unable to gather more information on Milan Stanek and Paul Garvin who appeared at the scene in the 1990s and who in some recollections figured as agents of anthropology (P0017: 9; P0009: 9).

²⁴⁸ All these individuals are mentioned in Skalník's article in which he describes the post-1989 struggles over the establishment of anthropology (Skalník 2002b). In his article, Skalník tends to depict them as representatives of anthropology who by the virtue of their shared goal, that is, the establishment of anthropology, represented a united front. While this depiction is partly justified, I do not subscribe to Skalník's depiction.

Here I focus only on the attempts to establish anthropology within the Ethnology Department where two opposing “factions”, which included some of the individuals mentioned above, emerged. The first was represented by several anthropology enthusiasts whose aim was to modernize the Ethnology Department by “anthropologizing”²⁴⁹ it, the second was represented by those who held that the department should get closer to the tradition of European Ethnology. It was the latter faction which eventually succeeded in the struggle. I am fully aware that a satisfactory account of the failure would require a deeper account of the political struggles which rocked Charles University in the early 1990s. My task here is much more modest. I will try to proffer to the readers some insights into what contributed to the failure of the harbingers of anthropology at the Ethnology Department.

To provide a minimum of context first. In the early 1990s, Charles University was viewed by some as an institution which would deserve its own department of anthropology. Negotiations to secure an independent anthropology department at the university began immediately after the Velvet Revolution. As Petr Skalník, one of the most active promoters of anthropology, would later complain, these negotiations were eventually unsuccessful (Skalník 2002b). The move from the idea of an independent department to the idea of establishing anthropology at the Ethnology Department was probably a natural move after the failure of original negotiations since several champions of anthropology recruited from people previously affiliated to Czechoslovak ethnography. The idea of modernizing the Ethnology Department by “anthropologizing” must have suggested itself almost naturally to some of them (Hubinger 2005; Skalník 2002b: 104).

Turning to the concept of habitus, we can gain unexpected insights into why the champions of anthropology failed in their struggles. As one ethnologist from the Ethnography Department recalled the appearance of anthropologists on the local scene: Turning to the concept of habitus, we can gain unexpected insights into why the champions of anthropology failed in their struggles. As one ethnologist from the Ethnography Department recalled the appearance of anthropologists on the local scene: “... these gentlemen were really savage. They suddenly appeared and it was as if they were insultingly saying: ‘You are stupid, you have been doing it all wrong... And we are here to show you.’ ... ‘We are getting rid of the mildew and bringing salvation.’”²⁵⁰ The interviewee did not mention anyone in particular nor did the interviewee recall any concrete

²⁴⁹ I borrow the term from Skalník (Skalník 2002b: 104).

²⁵⁰ P0025: 9.

occasion, but from various sources we can garner examples of what possibly constituted this ‘savage’ behaviour.

Ladislav Holý represents the situation perfectly. At the time of his return to Czechoslovakia, he gave a lecture in Prague on his ongoing research on Czech nationalism. The lecture, as Josef Kandert would write later in Holý’s obituary, was not well accepted (Kandert 1997: 338). It was very likely Holý’s interpretive and constructivist approach to nationalism which affronted his audience, which was used to see the Czech nation almost as a given entity. From among the émigré anthropologists, Holý was not alone in his intellectual predilections which aimed at deconstructing the concepts of people, nation and tradition, that is something what ethnologists held as sacrosanct. Even more radical and critical tendencies at the time appeared in the writings of Andrew Lass (Lass 1989), less radical in the work of Ernest Gellner, whose book *Nations and Nationalism* was one of the early anthropology books translated into Czech language after 1989 (Gellner 1993). As we saw earlier, Gellner had started a polemic with Miroslav Hroch whose views were akin to those of Czech ethnographers.²⁵¹

It should not surprise us that while Holý’s and other anthropologists’ ideas caused commotion among Czech ethnologists, but for reasons stated in the previous chapter, it should not come as a surprise that these ideas did not spark any sustained debates between anthropologists and ethnologists over the concept of nation. To my knowledge, only one ethnologist contested Holý’s ideas in print back in the day. It was Václav Hubinger who reacted to Holý’s article on the Czech folk conception of the nation. The article appeared in *Český lid* and Hubinger’s reaction was followed by Holý’s response (Holý 1991, 1992; Hubinger 1992). By far, this was the only debate in which the contestants pitted against each other opposing theoretical backgrounds – analytic nationalism cultivated among Czech ethnologists and one of the constructivist approaches which were then in vogue among anthropologists.

So far, the issue can be taken as a matter of mere intellectual disagreement. Anyway, émigré anthropologists went beyond intellectual issues as they continually dropped critical remarks on other facets of the Czech scholarly community. In an interview published in 1990, Holý indulged in a little comparison which deserves to be quoted in full:

²⁵¹ Contrary to Lass and Holý whose criticisms were sharp to the point of callousness, Gellner’s criticism was much more careful in tone. In his review of Hroch, we can find formulations such as “Hroch, in the work cited, represents an interesting attempt” or “Hroch’s outstandingly well-documented argument deserves full examination.” (Gellner 1994: 182). This matches the impression that Gellner left on his Czech colleagues (Pospíšilová 2017: 205).

... publication activities represent an incessant dialogue or discussion and every publication is a contribution to the discussion. Every article contributes with some theoretical argument, it is a contribution to a constantly developing and never-ending theoretical discussion. Regardless of whether an article is about sexual relations, economic development, kinship, politics, it is a contribution to an incessant and never-ending theoretical discussion. And if a man does not have an idea of what the discussion is all about, he does not understand the significance of the article. If a researcher is interested in relations between city dwellers, he most likely reads articles on relations among the city dwellers, but none of the articles tells him anything beyond that. Data in the article convey information about relations between the city dwellers, but if the article is worth it, it conveys far more: a conception of the human being, a conception of social relations, a conception of culture etc...

I might be wrong and I am excessively critical because of my ignorance, but my impression is that the discussion which is normal in the West does not exist here. Here, an article is a goal in itself, because it is something that no one else has written yet. If it is for example an article about a jewel, the reader who is not interested in the jewel, does not have to feel compelled to read the article, because the article is about the jewel and nothing else... (Chorváthová 1990: 349–350)²⁵²

Here Holý commented on the absence of discussion among ethnologists and of a professional ethos based on it. This was not the only thing on which Holý left a critical comment. In his abovementioned reply to Hubinger, Holý remarked that his opponent misrepresented his position, stating that one of the causes of misunderstanding was that he (Holý) could not have “explicitly formulate [his view] in an article which is roughly half the size of standard articles in Western specialist journals” (Holý 1992: 265). This is an example of a critical attitude towards the local ethnological community and its practices, which Holý at the time shared his with other émigré anthropologists, and hand in hand with their criticism went other remarks on how the trade should be ideally done. We can add Petr Skalník’s remarks that local scholars should institute weekly discussing seminars

²⁵² When speaking about an article on the jewel, Holý probably had on mind an article by Jaroslava Krupková on coin jewels (Krupková 1984).

which were regular at Western universities at the time (Chorváthová 1991: 80). Or consider excerpts from an interview which Petr Skalník gave to *Slovenský národopis* in 1991:

I attended congresses of the International Union of Anthropological and Ethnological Sciences in India, Canada and last time in Zagreb. (Chorváthová 1991: 77)

For example, in Cape Town, I was responsible for inviting visiting professors from the USA, Great Britain, France and Poland... In the Netherlands, I was, for example, a member of the committee for a project... and every month we evaluated the progress of an ongoing research on women which simultaneously took place in Egypt, Upper Volta, Sri Lanka... (Chorváthová 1991: 80)

Another responsibility of mine is to visit my students during their fieldwork and, for example, I visited a student in Lesotho, thousand kilometres away from Cape Town. (Chorváthová 1991: 80)

I am off to a fieldwork in Papua-New Guinea... Then I want to... compare the issues of structural accommodation in Africa and here in Eastern Europe, because these are very similar issues... Africa is almost like Eastern Europe, because it has some nostalgic model of its own past. It is what partly inspires it and partly slows it down. Both Africa and Eastern Europe have one thing in common, that is, it is a question whether they will become highly developed countries or not. (Chorváthová 1991: 85)

These are only some of the world destinations which Skalník mentioned during the interview. If you compare Skalník's worldly travels to the realities of Czech academic life, then the difference is quite striking. Czech ethnographers were used to attend conferences mainly across Czechoslovakia and had limited possibilities to travel abroad even in the 1990s, because of limited funding possibilities and high costs.²⁵³ It would not have been a surprise if Czech scholars had perceived Skalník's interview as inappropriate showing off.

²⁵³ "The conference fee was not exorbitant, but thirty dollars for someone like you and me was a considerably high amount. [...] Prague was agreed on [as a conference venue] also because of the fact that it is more accessible to people from Eastern Europe." These are words from Václav Hubinger's report on the 1992 EASA conference in Prague which he helped to organize (Hubinger 1993: 336). High costs were also mentioned in his report on the 1994 EASA conference in Oslo (Hubinger 1995: 165).

Lastly, an affair erupted after *Národopisný věstník* (formerly known as *Národopisný věstník československý*) had published an article written by David Z. Scheffel's. In the article, Scheffel accused ethnologists of being insufficiently sensitive to ethical concerns (Scheffel 1992). The article is remarkable for the host of reactions which it provoked. In the context of our discussion, two replies by Czech ethnologists deserve special mentioning. One by Dušan Holý, the other by Dagmar Štěpánková. Holý's reaction titled "Against Skewing and Haughtiness" criticised Scheffel's arrogance in treating ethnologists and Štěpánková similarly mentioned Scheffel's disdainfulness (Holý 1993; Štěpánková 1993: 29).²⁵⁴

The émigré anthropologists did not hesitate to leave critical comments on scholarly practice of their Czech colleagues. In addition, by stressing what the ideal should be, the émigré anthropologists must have constantly annoyed their colleagues. Nonetheless, the main point which I want to make here is that it was not only the substance of criticism, which affronted their Czech colleagues, be it intellectual criticism or critique related to practical things. It was criticism as such which was perceived as something inadequate and subverting the established academic order. Any criticism was thus perceived as doubly problematic – on a substantive level and on the level of what was considered normal and appropriate for academics to do. Czech colleagues were not accustomed to such an unprecedented degree of criticism which for them was probably hard to digest.

It is without any doubt that there must have been much of colonial self-fashioning and showing off on the part of the émigré anthropologists, as it is evident from the excerpt in which Skalník compared the fate of Eastern Europe to that of Africa, or from Scheffel's article in which he framed the situation in terms of a relationship between modern Western anthropologists and their backward East European colleagues (Scheffel 1992: 4). However, the ways in which anthropologists behaved, should be perceived in the context of what was regular in the world of which they were part. The travels of Skalník were just a normal part of his responsibilities appropriate to his position (Hall 2010: 100).²⁵⁵

²⁵⁴ Scheffel was to play the role of *enfant terrible* in post-1989 Czech ethnology. His joint article with Josef Kandert (Scheffel & Kandert 1994) elicited similarly critical responses (Cf. Holubová et al. 2002). Another affair, which ultimately led to establishing a professional body of Czech anthropologists, began after Scheffel had accused Ivo Budil of plagiarizing his and Kandert's article (Budil 2003; Scheffel in Hann et al. 2007).

²⁵⁵ There are even examples of the specific conference folklore which developed among anthropologists and social scientists for whom travelling all around the world was part of their daily bread: "Two planes crashed over Tel-Aviv airport both of them containing Shmuel Noah Eisenstadt, one of the world's most avid conference attendees." John Hall mentions that this joke was sometimes said with Ernest Gellner as its main protagonist (Hall 2010: 100).

Anthropologists behaved in a way which was tailored to a world of incessant competition, where it was important to act in a self-assured way in order to secure and enjoy prestige. But their ways of everyday presentation, their scholarly *hexis* was foreign and repulsive to ethnologists, whose habitus were perfectly suited to the realities of Czech scholarly world where no fierce competition existed and where scholarly prestige stemmed from a different basis. Anthropologists must have perceived ethnologists as unprofessional dilettantes; for ethnologists, anthropologists must have been boastful, arrogant and pretentious.

What I attempt to argue is that it was not only intellectual differences which played such a crucial role in embittering the two groups. My contention is that habitual differences played a significant part in the relationship which developed between both ethnologists and anthropologists. Anthropologists naturally acted in ways unacceptable for ethnologists. When anthropologists made the differences explicit, it must have further exacerbated the mutual relations. This might have led to an estrangement which prevented anthropologists from winning sympathies among their Czech colleagues and which eventually aggravated anthropologists' chances from gaining a firm foothold in the school. Throughout the 1990s, sociocultural anthropology subsisted only in a shape of various curricula and lectures on various departments and faculties of Charles University.

7.3.2 The First Czech Anthropologists

This finally brings me to the second issue, the professional trajectories of post-1989 generations of Czech anthropologists who originally studied at the Ethnology Department. Local anthropology enthusiasts and anthropology émigrés were unsuccessful in gaining a firm foothold at the Ethnology Department which continued to be headed by František Vrhel and continued to be staffed with the same scholars who began their careers in the late socialist period. Even if some consider František Vrhel to have acted as a major obstacle in establishing anthropology at the Ethnology Department, they acknowledge that he at the same time supported anthropology's partial existence at the department (Skalník 2002b: 103–104).²⁵⁶ After all, he had been one of the few Czech scholars in touch with the developments in anthropology. During Vrhel's reign as head of the Ethnology Department,

²⁵⁶ Several interviewees of mine expressed a similar opinion concerning Vrhel. These statements indicate that Vrhel was successful as a political player as he was able to defend the Ethnology Department from an incursion of people whose vision was a complete transformation of the department into a department of anthropology.

students could enrol in several anthropology courses taught by himself and Ivan Dubovický who were later joined by Jaroslav Skupnik. Students could also attend lectures given by visiting professors, especially Leopold Pospíšil and Zdeněk Salzman or even collaborate on Milan Stanek's research (Pflegerová 1999). Former émigrés were usually conducive in providing opportunities for ethnology students to study anthropology at universities in the USA.²⁵⁷ However, even if students could learn about anthropology, and there were many who were eager to do so, their interactions with it were limited only to a narrow part of their curriculum which was otherwise dominated by the ethnologists.

Those ethnology students interested in anthropology represented an interesting paradox. They were keen to learn about anthropology and they began to devour anthropological knowledge. They revolted against the older generation of ethnologists, whom they variously accused for their Marxism-Leninism or positivism.²⁵⁸ A group of revolting students set up a first Czech anthropology journal and baptized it with the name *Cargo* and indulged in mutual discussions. However, they were educated in a scholarly environment which was dominated by ethnologists who could not train them in ways which would suit their aspirations of becoming professional anthropologists. If I am allowed a simplification – these young students identified with anthropological knowledge, but were educated as ethnographers (or ethnologists) in their practice. This is not to say that some of these students did not later successfully integrate in an international anthropological community. However, their ethnology education presented a baggage which was not particularly suited for a success in anthropology and their subsequent careers very likely involved a great deal of learning and unlearning.

To summarize, looking at habitual differences between the two traditions can help us to better understand two phenomena. The first is the clash of local ethnologists with Western-educated anthropologists who, perhaps unknowingly, contributed to the failure to win an independence for anthropology at the Ethnology Department. Second is the reproduction of specific scholarly ways inherited from the late socialist ethnography which acted as an obstacle in incorporating local scholars in a wider anthropological community. Even if local enthusiasts began to dote on anthropology, they were not taught the essential

²⁵⁷ P0004: 16; P0017: 12. Moreover, during the 1990s, Ivan Dubovický and Ivo Budil were grantees of the Fulbright scholarship (cf. Brouček & Jeřábek 2007). Salzman, Stanek and Pospíšil continued to be active at Charles University and were listed as part time lecturers in the Ethnology Department in the 1990s. Compared with the 1960s when ethnographers established contacts with the British school, the 1990s were mostly American-oriented.

²⁵⁸ P0017: 5–7; P0006: 8.

competences thanks to which their professional personalities would integrate to a wider anthropological community.

7.4 New Directions

This thesis presents an attempt to deal with some realities with the late socialist Czechoslovak ethnography and folklore studies, mostly its Prague incarnation, and its successors – Czech anthropology and ethnology. Any further research should deserve an inclusion of many more factors and many more sources as well as comparison with ethnography in other countries of the former Socialist Bloc, especially Poland, Hungary or East Germany. It is possible that some ethnography practices have their origins in the era before the late socialist period or even before the inception of Czechoslovak socialism. A long-term survey of the development of *národopis* and ethnography covering the whole twentieth century would be of equal importance in such a research.

If there is something to be underscored in particular in the end, it is the unreliable nature of labels. Various disciplinary labels with which I worked in this thesis have the advantage in offering us short referents for various disciplinary composites. Unfortunately, these labels tend to acquire only intellectual and institutional content in the contemporary discourse. The prevailing use of the labels thus obscures other, no less important dimensions of what constitutes scholarly disciplines. Scholarly disciplines do not only have intellectual content. Scholarly disciplines are not only set in some institutional background. Each scholarly discipline is a composite of various practices, but many of these go unnoticed by those who supposedly define the disciplines. It is for these reasons that I have avoided the talk of disciplinary identity. To say that being an anthropologist rests in the usage of anthropological theories (and not in ethnological), that it rests in working at an anthropology department (instead of an ethnology department) or in contributing to anthropology journals (and not to ethnology journals) or worse, in identifying oneself as an anthropologist (rather than ethnologist), is to present an incomplete and impoverished view on what shapes scholarly disciplines and their respective fields. Perhaps to the surprise of many, Czech anthropology and Czech ethnology share many things in common. They can trace a common ancestry to Czechoslovak ethnography and folklore studies. At the same time, if the reader recalls what has been established in Chapter 2, the intellectual differences between Czechoslovak ethnography and Western anthropology were not as sharp as many envisage today. In the relation to the use of various disciplinary epithets, I would like to

stress one of the methodological postulates which this thesis alleges to and which traces its origins at least back to Durkheim. The postulate conveys that a sociologist can in some ways know more about actors than actors know of themselves (Durkheim 1926: 18–19). This extra sociological knowledge is generated firstly by a systematic comparison of phenomena which have never been systematically treated by actors themselves and it is not hard to understand why. Hence, I hope that my thesis offers new directions, although it does so with tools which are in no way novel.

To those who would in spite of what has been just said insist that there exist some unbridgeable intellectual differences between anthropology and ethnography, I could object that this thesis does not substantially differ from scholarly preoccupations of Czechoslovak ethnographers. In the fashion identical to ethnographers, I single out one tradition and by presenting its historical development, I account for continuities and discontinuities within the tradition, and at the same time offer an analysis of why some features of this tradition survived, why some features transformed and why others became extinct. In a way not alien to ethnographers, I also dare to say that if the goal of the recent Czech anthropology (and ethnology too) is to reach the professional standards of its colleagues working abroad, it should pay more attention to competences related to writing and languages, it should open itself to intellectual criticism and strive to loosen academic hierarchies. The major difference is in the degree of subtlety of the tools used. Despite the fact that Bourdieu's sociology has imperfections and shortcomings on its own, it offers a far more complex tool than any of that which the late-socialist ethnography ever produced. Bourdieu's sociology was not brought to the light of day by a single intellectual genius. It emerged in highly competitive scholarly field and, in a sense, it is a product of collective practice. Even if local ethnography scholarship was also collective in a sense, it lacked the collective competitive dimension which curbed its intellectual improvement. Some anthropologists might find this conclusion odd, but to me it seems that the understanding of our own history truly offers us tools for dealing with our future. Or, to put it in the words of ethnographers: "From the time immemorial, the Czechs have been known for their love of history... The lesson that the history teaches is sometimes cruel, sometimes merciless, but nonetheless inescapable." (Robek & Svobodová 1979: 9).

Appendix I: Glossary

This Glossary includes all the terms which are mentioned throughout this thesis and it is not meant to be exhaustive. It provides convenient translations mostly of Czech and Slovak journals and institutions in English. This is to provide some elementary help to the understanding of Czech and Slovak names of these institutions and journals. In several cases I also included abbreviations. The reader will notice that whereas I use the term *národopis* throughout the thesis, I shun using the term when translating in English the names of some institutions. This is simply for the sake of convenience. Lastly, this glossary does not provide all the changing names under which particular institutions came to be known in various periods.

ETHNOGRAPHY INSTITUTIONS AND ORGANIZATIONS

Centre for Ibero-American Studies (CIAS)	Středisko Iberoamerických studií (SIAS)
Czechoslovak Ethnography Society	Národopisná společnost československá (NSČ)
Department of Ethnography and Folklore Studies (the Ethnography Department)	Katedra etnografie a folkloristiky (KEF)
Ethnography Institute of the Slovak Academy of Sciences Institute for Ethnography and Folklore Studies (the Ethnography Institute)	Národopisný ústav Slovenské akademie vied Ústav pro etnografii a folkloristiku (UEF)
Moravian Museum	Moravské muzeum
Náprstek Museum of African, Asian and American Cultures	Náprstkovo muzeum afrických, asijských a amerických kultur
National Institute of Folk Culture in Strážnice	Národní ústav lidové kultury (NÚLK)
Section for Ethnography	Kabinet pro národopis
Section for Folk Song	Kabinet pro lidovou píseň
Section for Integral Anthropology	Sekce integrální antropologie
Seminar for Ethnography and Ethnology	Seminář pro etnografii a etnologii
Silesian Museum	Slezské muzeum
Slovak National Museum	Slovenské národné múzeum
Wallachian Open Air Museum	Valašské muzeum v přírodě
Ethnography Society	Národopisná společnost (formerly Národopisná společnost československá)

OTHER INSTITUTIONS AND ORGNIZATIONS

Commission for the Establishment věd of the Czechoslovak Academy of Sciences	Komise pro ustavení Československé akademie
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Communist Party of Czechoslovakia
Czechoslovak Foreign Institute
Czechoslovak Society for Sociology
Department of Auxiliary Historical Sciences and Archive
studia
Institute of Health Education
National Pedagogic Publishers
Oriental Institute
Revolutionary Trade Union Movement
Socialist Union of Youth
State Planning Office
Union of the Czechoslovak-Soviet Friendship
(SČSP)

Komunistická strana Československa (KSČ)
Československý ústav zahraniční (ČSÚZ)
Československá sociologická společnost
Katedra pomocných věd historických a archivního
Osvětový ústav zdravotnický
Státní Pedagogické Nakladatelství (SPN)
Orientální ústav
Revoluční odborové hnutí (ROH)
Socialistický svaz mládeže (SSM)
Státní plánovací komise
Svaz československo-sovětského přátelství

JOURNALS

Czech People
Slovakian Ethnography
Ethnography News
Czechoslovak Ethnography Bulletin
Newsletter of the Coordinated Network of Scientific Information.
for Ethnography and Folklore Studies
Ethnography Revue
aktuality)
Ethnography Bulletin

Český lid (ČL)
Slovenský národopis (SN)
Národopisné aktuality (NA)
Národopisný věstník československý (NVČ)
Zpravodaj koordinované sítě vědeckých informací
pro etnografii a folkloristiku (Zpravodaj KSVI)
Národopisná revue (formerly Národopisné
aktuality)
Národopisný věstník (formerly Národopisný
věstník československý)

EDITIONS & SERIES

Ethnography Library
Czechs Abroad
Ethnic Processes
Ethnography of the National Revival
Ethnography of the Working Classes
Folk Culture and the Present Day

Národopisná knihovna
Češi v cizině
Etnické procesy
Etnografie národního obrození
Etnografie dělnictva
Lidová kultura a současnost

OTHERS

Czech National Revival
Czechoslavic Ethnography Exhibition

České národní obrození
Národopisná výstava československá

Appendix II: Translations

before 1990s (the year in brackets indicates the first translation)

- E. B. Tylor: *Anthropology* (1897)
- G. E. Smith: *Human History* (1938)
- L. H. Morgan: *Ancient Society* (1954)
- J. G. Frazer: *Golden Bough* (1960)
- C. Lévi-Strauss: *Tristes Tropiques* (1966)
- B. Malinowski: *A Scientific Theory of Culture* (1968)
- C. Lévi-Strauss: *Savage Mind* [*Pensée sauvage*] (1971)

In addition to these books, the year of 1971 witnessed a collection of texts titled *Cultural and Social Anthropology*. It was edited by Josef Wolf and translated to Czech by Michael Černoušek, Antonín Dušek, Marie Hejlová, Věra Jelínková, Vladimír Kadlec, Ivana Knytlová, Andrew Lass, J. Neumann, A. Suchánek, Ladislav Venyš, Zora Wolfová and Wolf himself. The collection included translated excerpts from the works of Tylor, Morgan, Frazer, Marett, Perry, Thurnwald, Boas, Benedict, Herskovits, Kroeber, Malinowski, Firth, Radcliffe-Brown, Evans-Pritchard, Fortes, Nadel, Murdock, Barnett, Linton, Redfield, Mead and Lévi-Strauss (Wolf 1971)

As I have learned from its inventories, many anthropology books and journals were also available in the library of the Náprstek Museum.

1990s

- E. Gellner: *Nations and Nationalism* (1993)
- C. Lévi-Strauss: *Histoire de Lynx* [*The Story of Lynx*] (1995)
- C. Lévi-Strauss: *La Voie des masques* [*The Way of the Masks*] (1996)
- R. Lawless: *The Concept of Culture* (1996)*
- E. Gellner: *Conditions of Liberty* (1997)
- A. van Gennep: *Les Rites de passage* [*The Rites of Passage*] (1997)
- L. Pospíšil: *Ethnology of Law* (1997)
- R. Willis: *World Mythology* (1997)
- R. F. Murphy: *Social and Cultural Anthropology* (1998)*
- L. Lévy-Bruhl: *La mentalité primitive* [*Primitive Mentality*] (1999)
- M. Augé: *Non-Lieux* [*Non-Places*] (1999)
- M. Mauss: *Essai sur le don* [*The Gift*] (1999)
- C. Lévi-Strauss: *Race et histoire* [*Race and History*] (1999)
- R. Benedict: *Patterns of Culture* (1999)
- E. Gellner: *Reason and Culture* (1999)

From the 1990s onwards, readers could read Czech translations of anthropological articles and essays which appeared in *Český lid*, *Cahiers du Cefres*, *Cargo*, other journals and various thematic volumes.

2000s

- C. Geertz: *The Interpretation of Cultures* (2000)
- G. Balandier: *Anthropologie politique* [*Political Anthropology*] (2000)
- J. Copans: *Introduction à l'ethnologie et à l'anthropologie* [*Introduction to anthropology and ethnology*] (2001)*
- L. Holý: *The Little Czech and The Great Czech Nation* (2001)
- C. Lévi-Strauss: *Le Totémisme aujourd'hui* [*Totemism*] (2001)
- R. F. Murphy: *Body Silent* (2001)
- E. Gellner: *Plow, Sword and Book* (2001)
- E. Gellner: *Nationalism* (2003)
- E. Gellner: *Language and Solitude* (2004)
- V. W. Turner: *Ritual Process* (2004)
- T. H. Eriksen: *Tyranny of the Moment* (2005)
- J. Goody: *The Development of the Family and Marriage in Europe* (2006)
- G. Bateson: *Mind and Nature* (2006)
- C. Lévi-Strauss: *Le Cru et le cuit* [*The Raw and the Cooked*] (2006)

- C. Lévi-Strauss: Du miel aux cendres [From Honey to Ashes] (2006)
- C. Lévi-Strauss: Anthropologie Structurale [Structural Anthropology] (2006)
- C. Lévi-Strauss: L'Origine des manières de table [The Origin of Table Manners] (2007)
- C. Lévi-Strauss: Anthropologie structurale deux [Structural Anthropology II] (2007)
- B. Malinowski: Sex and Repression in Savage Society (2007)
- T. H. Eriksen: Collection of essays on identity and multiculturalism (2007)
- T. H. Eriksen: Small Places – Large Issues (2008)*
- F. Bowie: Anthropology of Religion (2008)*
- C. Lévi-Strauss: L'Homme nu [The Naked Man] (2009)
- M. Halbwachs: Collective Memory [La Mémoire collective] (2009)

2010s

- T. H. Eriksen: Syndrome of the Big Bad Wolf (2010)
- M. Mead: Sex and Temperament (2010)
- C. Lévi-Strauss: Anthropology and problems of the Modern World (2012)
- T. H. Eriksen: Ethnicity and Nationalism (2012)
- R. Benedict: Chrysanthemum and Sword (2013)
- D. Graeber: Debt (2013)
- D. Graeber: Fragments of an Anarchist Anthropology (2013)
- A. Sutherland: Roma: Modern American Gypsies (2014)
- R. Charbonnier: Interviews with Lévi-Strauss [Entretiens avec Lévi-Strauss] (2014)
- M. Douglas: Purity and Danger (2014)
- T. H. Eriksen: Garbage (2015)
- V. Crapanzano: Tuhami (2016)
- A. Yurchak: Everything Was Forever until It Was No More (2018)

Asterisk indicates a textbook or an introduction.

The list contains 58 books total and does not include Slovak translations of anthropological works. The choice reflects rather orientation towards English, French and American anthropological traditions. Some of the books are not strictly anthropological, but were either written by prominent anthropologists or deal with anthropological issues. Three of the anthropologists are overrepresented in translations and together make one half of the whole list: Claude Lévi-Strauss (13 books), Thomas Hylland Eriksen (6), Ernest Gellner (6).

The list does not contain classical and other sociological works translated to Czech and authored Marx, Comte, Veblen, Durkheim, Weber, Berger, Luckman, Elias, Baumann, Goffman, Bourdieu, Latour or Luhmann. Other books dealing with theory in social sciences (Popper, von Hayek, von Mises, Winch) or more philosophically oriented works (e.g. by Merleau-Ponty, Barthes, Deleuze or Foucault), literary criticism (Frye, Said, Eagleton), religionists (Eliade, Girard, Pals, Segal), psychologists (Freud, Jung, Erikson, Piaget, Skinner) and books by sociologically and anthropologically oriented historians (e.g. Bloch, Huizinga, Gurevich, Duby, Burke, van Dülmen, Darnton) also appeared in Czech translations.

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