# Academia without Contention?
## The Legacy of Czechoslovak Ethnography and Folklore Studies in Czech Anthropology*

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**Abstract:** One issue of the post-socialist transformation of Czech higher education has been the many attempts to establish an independent discipline of sociocultural anthropology. As many observers noted, the establishment of a fully-fledged Czech anthropology after the collapse of communism in 1989 proved to be a rather difficult task. Many accounts offered various explanations for the uneasy state of emerging Czech anthropology, but none of them focused on the specific academic practices that anthropology inherited from its predecessor—Czechoslovak ethnography. While anthropological names, books, and theories entered wide circulation and have become a regular part of curricula since the 1990s, the specific way ethnography is practiced has remained unchanged. The article looks at the Department of Ethnography and Folklore Studies at Charles University, which, in the 1990s, became one of the departments where students were able to take courses in anthropology. While the students were given an introduction to anthropological knowledge, they were not led to adopt the specific set of scholarly attitudes that are intrinsic to sociocultural anthropology, the most important of which is a specific approach to academic debates. It could be concluded that this uneven distribution of academic expertise, this disunion between knowledge and non-knowledge, may have severely delayed the development of an autonomous tradition of Czech sociocultural anthropology.

**Keywords:** culture of contention, postsocialism, higher education, history of anthropology, history of ethnography and folklore studies

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## Introduction: anthropology and contention

Anyone who is at least a little bit acquainted with the Anglo-American field of anthropology cannot but notice the contentiousness inhering in it. A reader looking for proof of this need do nothing more than open any issue of any anthropology journal, choose an article at random, and read: 'Before presenting a final selection of Zande proverbs, I would like to make a few general observations. On the whole, anthropological treatments of proverbs has been unsatisfactory. The modern and professional anthropologists have for the most part ignored them in their publications…' [Evans-Pritchard 1964: 1]. Before presenting a comprehensive list of 53 Zande proverbs, Evans-Pritchard first devotes two pages of his article to criticism of his colleagues' views on proverbs [Ibid.: 1–2].

A good example of contention in anthropology is the debate over Mary Douglas's pioneering theory of pollution, taboo, and animal categories [Bulmer 1967: 21–22; Douglas 1975: 207–208; Tambiah 1969: 448–452]. Seeds of a more vigorous debate were sown in Edmund Leach's Henry Myers Lecture of 1966. A subsequent debate over the Virgin Birth stretched over five years and involved thirteen different scholars in an inter-Atlantic exchange.[1] These examples are not by any means an exhaustive account of the endless debates in anthropology in the 20th and 21st centuries and they perhaps do not even rank among the most important debates.

Not all articles in anthropological journals are written in a contentious style; nonetheless, debating is more the rule than the exception in the field of Anglo-American anthropology. What we find in debates is the criticism or approval of predecessors and contemporaries, various conjectures and refutations, proposals for alternative solutions, small insights from different fieldwork, acknowledgements of defeat or stubborn insistence on a propounded theory. Some of the debates concern minor and very specific issues; the validity of general theories is at stake in others. Last but not least, we witness clashes between senior anthropologists and their junior colleagues, the latter often being students of the former. Debates are moreover not limited to Anglo-American scholars, as anthropologists from other countries, especially from France,[2] have joined the intellectual exchange. As well as anthropologists from other countries, sociologists, philosophers, and biologists have also also visited the field of anthropology and keenly poured their contention into the debates.

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[1] See Tambiah [2002: 259–289] for a summary of the debate.

[2] The reference will be to Anglo-American anthropology, although it could be labelled as 'Franglus', using Katherine Verdery's label [Hann et al. 2007: 48]. Calling it Franglus would require that further similarities and differences between French and Anglo-American higher education be revealed. Regardless, contention also seems to be a staple of the French academic and intellectual fields from which Pierre Bourdieu drew material for his social theory (see below). As Bourdieu notes in reference to the field of intellectuals, a field that is closely related to the academic field, intellectuals' 'best customers are also their fiercest rivals' [Bourdieu 1991a: 668; cf. 1991b: 15].

This mode of scholarly involvement must seem so natural to its practitioners that it is almost impossible to imagine the existence of anthropology without debate. Perhaps the first anthropologist who turned his attention to contention as a vital part of a specific scholarly mode was Alfred Gell. Gell noted that there existed something that he called 'seminar culture', which, he suggested, is strongly manifested in the British tradition. For the argument presented in this paper, it is useful to quote Gell 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 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]

Gell's description does not mention discussions in journals or books but stresses a specific institution or custom. He took care to differentiate between the 'seminar culture' on one hand and writing 'for an imaginary reader' or 'reading papers in cold blood' on the other [Gell 1999: 2]. Here I would like to group the two types of academic exchange—oral and written discussions—together under the label of a 'culture of contention'.

A seasoned British anthropologist, Gell calls the seminar 'a social occasion', 'a game', 'an exchange', 'an ordeal', or an 'initiation' [Gell 1999: 2]. He does not use the word, but it is clear that the seminar is a ritual of sorts. If we use this term, the sociological implications become much clearer as the intellectual content recedes, allowing for a fuller sociological understanding of the rituals of contention. There are two important corollaries.

When students learn about the history of anthropology, sooner or later they learn about the labels of different approaches. It seems to me that this differentiation, regardless of how unprecise and misleading the labels can be, is a result of the contentiousness that is inherent in anthropological culture. It is apt to describe the differentiation using the terminologies of two contemporaries, Edmund Leach and Max Gluckman. As the former noted, ritual and myth maintain social controversy [Leach 2004: 13, 85]. In Gluckman's diction, ritual creates and maintains social cleavages, but also mitigates them [Gluckman 1962: 38–47]. Leach's and Gluckman's ideas actually describe exactly what went on between the two anthropologists. Discussions between the two drew some internal lines in anthropology; nonetheless, their contention served as a basis for the future theoretical advances of their students and future anthropologists [Kuper 1996: 158].

At the same time, we should be aware of an intellectualist trap and not consider anthropological debates purely as intellectual exchanges with no other than intellectual purposes or consequences.[3] If we approach our subject matter from the perspective of Pierre Bourdieu's sociology, debates represent clashes and struggles over dominance within the field of anthropology. Bourdieu's sociology allows us to get a better understanding of intellectual contention as inextricably linked to academic politics [Bourdieu 1991b].

Although Gell did not explicitly link his remarks on seminar culture with Bourdieu's concept of field, he did it by proxy by using Bourdieu's concept of habitus, the two being closely interconnected in Bourdieu's sociology [Bourdieu and Wacquant 1992: 127]. Gell invokes 'the notion of academic *habitus* founded on social exchange' [Gell 1999: 3; italics original].[4] Without any doubt, contention permeates the field of Anglo-American anthropology and its presence in books and articles and at conference venues and in classrooms shapes the field accordingly.

Gell was aware that instead of a full-blooded analysis he was offering sketches [Gell 1999: 2]. I would like to pick up his invitation at the point where he

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[3] What is labelled as an 'intellectualist trap' here does not neatly fit in any of the three intellectualist fallacies (or biases) of the sociology of Pierre Bourdieu [Bourdieu and Wacquant 1992: 39–40]. 'Intellectualist trap' relates to Bourdieu's idea that symbolic systems (anthropological theories) serve as instruments of both knowledge and domination [Bourdieu and Wacquant 1992: 12–15]. To call it a trap means that anthropologists themselves might not be fully aware of their theories (but even the products of their work, such as articles and books) working as instruments of domination [cf. Verdery 1991: 19].

[4] To avoid confusion, and unlike Gell, I will refer to academic habitus as secondary habitus [Bourdieu and Passeron 1990: 43].

left his theme and pursue its consequences on a comparative basis. My approach is similar to the comparisons that Tamás Hofer [2005] and Bea Vidacs [2005] made between Hungarian and US anthropologists, though neither of them referred to Gell or Bourdieu. In what follows I will focus on the academic practices of Czechoslovak ethnographers during the 1980s, mostly by analysing their scholarly articles. I will try to show that ethnographers' mode of scholarly involvement was different from what we know about Anglo-American anthropologists. I will argue that certain practices and the products of those practices were the outcome of the academic habitus shaped by the specific constellation of the field of ethnography at the time. From this analysis, I infer that this situation created particular difficulties for the establishment of Czech sociocultural anthropology and its prospective incorporation into a wider anthropological community from the 1990s onwards. Although I admit that this process may have been hampered for other reasons as well (such as financial matters or academic politics), in my analysis, I will focus solely on academic practices related to writing and contention and show how they shaped an academic field that differed considerably from the field of Anglo-American anthropology. Before I proceed with the analysis, it is important to introduce readers to several crucial terms and acquaint them with the situation in the field of ethnography during what is known as the normalisation era in the 1970s.

### *Národopis*, ethnography, folklore studies, ethnology: a jumble of terms

Since we want to understand the emergence and history of Czech sociocultural anthropology we need to understand that it was not established on a greenfield. Anthropology's beginnings are strongly linked to a previous discipline that was known by several different labels: národopis, ethnography and folklore studies and ethnology. I will explain these labels below.

*Národopis*, or literally 'nationgraphy' [Hann et al. 2007: 36], had existed since at least the end of the 19th century, when the future Czechoslovakia was still a part of the Habsburg Monarchy. *Národopis* focused on the study of populations, their traditions, material cultures, diets, architecture, clothing, arts, and customs. As Katherine Verdery writes, *národopis* was more local in its interests [Hann et al. 2007: 49]. Other accounts go as far as to stress its nationalist overtones [cf. Grill 2015; Holubová, Petráňová, and Woitsch 2002: 231–270; Scheffel and Kandert 1994]. Its focus of interest remained much the same after the Communist coup in 1948. A major difference was a shift in interest from rural populations to the working class and craftsmen [Grill 2015: 4]. The shift was aggressively promoted by several young *národopis* scholars who began to import ethnography into Czechoslovakia. 'Ethnography' was a label that came from the Soviet Union [Woitsch et al. 2016: 343] and it covered theoretical precepts, specific subject matter, and methods, and thus differed from anthropologists' use of this term [Ingold 2008]. The main Czech institutions devoted to ethnography were the Department

of Ethnography and Folklore Studies at the Faculty of Arts at Charles University (abb. KEF FF UK, hereinafter referred to as the Ethnography Department) and the Institute of Ethnography and Folklore Studies within the Czechoslovak Academy of Sciences (abb. UEF ČSAV, hereinafter referred to as the Ethnography Institute). Both were located in Prague. The former institution's main purpose was teaching, the latter's was research.

The term *národopis* was in fact never abandoned in Czechoslovakia. A sign of its resilience throughout the forty-year period of Czechoslovak communism is that *národopis* was the name of the flagship study programme offered by the Ethnography Department. The name of the study programme was also officially approved by the Ministry of Education.[5] Although they ceased to refer to themselves as to *národopisci* (*národopis* scholars), from the 1950s on ethnographers understood ethnography and folklore studies as two branches of *národopis* [Frolec and Holý 1964: 7].

The last appellative change so far came in the 1990s after the fall of communism. In attempts to break with the communist past, first the Ethnography Department and later the Ethnography Institute changed the 'ethnography and folklore studies' in their names to just 'ethnology'. The discipline's main journal, *Český lid* (The Czech People), was renamed *Český lid/The Czech Ethnological Journal*. Although the field nominally switched its allegiance in the direction of European ethnology, that allegiance was somehow split between streams of ethnology and trends in the area of sociocultural anthropology.

The various labels introduced above will be henceforth used according to their temporal context, but they will refer to a single, continuously developing discipline. As we can see, the seemingly complicated history of the discipline evinces a remarkable continuity. Political upheavals produced only few casualties among the staff. Unlike the arena of politics or the military, no *národopis* scholar was executed during the Sovietisation of *národopis* after the communist coup in 1948. Unlike the fate of Czechoslovak sociology, and despite the many hostile accusations levelled against *národopis* scholars, *národopis* was never prohibited as a bourgeois pseudoscience [Nešpor 2014: 299]. With the exception of Josef Voráček, who was dismissed in the early 1950s from his university post in Prague [Woitsch et al. 2016: 84], the professors of  what was deemed the 'bourgeois tradition of *národopis*' were allowed to teach until their forced retirement in 1956 [Woitsch et al. 2016: 87–88], and even after that they were allowed to publish articles and were never entirely erased from the history of the discipline. The normalisation of ethnography in the 1970s proceeded with somewhat similar results. When the discipline took its first post-socialist steps in the early 1990s and redirected itself towards European ethnology, again there was almost no change in staff, even though many of them had been Communist Party members. All the

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[5] See, for example, *Nařízení vlády ČSSR 89/1980 Sb.* and *Nařízení vlády ČSSR 33/1986 Sb. Národopis* is categorised under codes 71-05-8 and 71-10-8.

political milestones were almost alike in their consequences. This institutional and personal continuity set against a backdrop of political and nominal discontinuity allows us to approach this discipline as a distinct field [Bourdieu 1991b: 9–12]. In this article, I limit my interest to the Prague academic scene, which is represented by the field's two main institutions: the Ethnography Institute and the Ethnography Department.[6]

One last caveat is necessary for readers unacquainted with Czechoslovak ethnography. Although many of the scholars in this field conducted some sort of fieldwork among living populations, interviewed interlocutors, or performed questionnaire research, the discipline was, regardless of its name, considered to be primarily a historical discipline, primarily based on archival and antiquary research. Today, it is still possible to study and practice ethnology without doing any fieldwork. Not surprisingly, it has been considered appropriate for students to combine it with the study of history. Students take courses in Latin and German, two languages especially important for the demands of archival research in the region. The discipline's historical focus was even 'consecrated' by the Ministry of Education, where it was assigned to the category of historiography.[7]

## Normalisation ethnography

In order to get a better understanding of the specific scholarly habitus that Czechoslovak ethnography bequeathed to its successor disciplines, it is important to briefly acquaint readers with the situation in ethnography during what was known as the normalisation period [cf. Kolář and Pullmann 2016]. The normalisation period started in the years following the Warsaw Pact invasion of Czechoslovakia in August 1968 and ended in the late 1980s. A crucial figure in normalisation ethnography was Antonín Robek. Robek took over the reins of the field of ethnography with strong political backing in 1971 and became a powerful figure with a decisive influence in the field, as he became the head of both the Ethnography Department and the Ethnography Institute.[8]

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[6] The degree of isolationism or cooperation between different ethnography institutes in Czechoslovakia still remains to be determined. As well as the two Prague institutions, there was a Brno branch of the Ethnography Institute and an autonomous department of ethnography at Jan Evangelista Purkyně University in Brno (today Masaryk University). In Slovakia, there was an ethnography department at Comenius University in Bratislava and an ethnography institute at the Slovak Academy of Sciences. Yet another network of institutions existed among regional ethnography museums. There are several clues that indicate that the various spaces of ethnography presented relatively autonomous subfields.
[7] See Note 6.
[8] Concerning Robek's position I draw on work by Josef Petráň [2015] and by Jakub Jareš and his colleagues [Jareš et al. 2012]. Useful information can be found in a jointly written monograph published by the Ethnography Institute [Woitsch et al. 2016].

The beginning of Robek's era was heralded by the publication of the fourth issue of the 1972 volume of *Český lid*, which was very likely organised by Robek himself. In several articles promoting an ideological as well as an organisational vision (e.g. more cooperation within the discipline), a group of Czech ethnographers working at the Ethnography Institute at the time strongly condemned previous developments in Czech ethnography, which, they contended, represented reactionary, that is, antisocialist and anticommunist, tendencies in society. The authors proclaimed their strong allegiance to Soviet ethnography, the Marxist-Leninist doctrine, and socially engaged science[9] [Hynková 1972; Klímová-Rychnová 1972; Kramařík 1972; Robek 1972a, 1972b]. This 'thematic' issue can be seen as a counterpart to *Poučení z krizového vývoje* (Lessons from the Crisis), an official manifesto of the Czechoslovak Communist Party issued in 1970 that condemned the liberal course of 1960s politics in Czechoslovakia. That issue of *Český lid* should be primarily understood as a political pamphlet announcing a stricter Marxist-Leninist course in ethnography.

Moreover, with a few exceptions, Robek supervised the majority of dissertations in the Ethnography Department. Several academics from both the Ethnography Department and the Ethnography Institute whom I interviewed told me that it was Robek who approached them by the end of their last year and offered them the opportunity to pursue careers in ethnography.[10] Not surprisingly, since his appointment as head of the Ethnography Department in 1971, Robek began to gradually replace previous staff with fresh Department alumni. It is clear that Robek was fully in control of the people who eventually became doctoral researchers and later his colleagues at the Department and the Institute [cf. Jareš et al. 2012: 315–317; Woitsch et al. 2016: 328]. This is important to take into account because the analysis below will consist of reviewing articles of ten ethnographers who are all scions of Robek's era. By the end of the 1980s, they represented a decisive majority of the Ethnography Department's staff.[11] Unlike Robek, who had to leave his post in the Department in 1987, after the Faculty Dean announced

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[9] This is the exact opposite of what D. H. Price shows was the case in US anthropology since the late 1940s. 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 [Price 2004]. On the contrary, Czechoslovak ethnography since the 1950s was by default supposed to serve state interests in building a better society. Value neutrality in science was a sign of reactionary views. Ethnographers did not hesitate to refer to resolutions of Czechoslovak Communist Party conventions in their writings. Another question to answer is to what extent these proclamations were actually carried out.

[10] Interviews dated 23 May, 30 May, and 15 June 2017.

[11] This information is extracted from the lists of taught courses (Karolinky) at the Faculty of Arts of Charles University and from research on the dissertations defended at the Ethnography Department.

faculty-wide personnel changes [Petráň 2015: 612–613], his 'offspring' remained in the Ethnography Department, some until as late as 2014. This cohort of ethnographers is important for one reason: they were able to reproduce their scholarly habitus formed during the normalisation era by instilling standards of academic work into younger generations of students who began to study ethnology and anthropology in the early 1990s.

Robek also served as chief editor of two ethnography journals: *Český lid* (1977–1990) and *Národopisný věstník československý* (1984–1990) [Válka 2013: 114–115; Woitsch 2013: 85–86]. While the latter has been revived from its editorial slumber for the umpteenth time, the former, though discontinued only once, can pride itself on a very long history. *Český lid* was established by the end of the 19th century by two prominent founders of *národopis*, Čeněk Zíbrt (1864–1932) and Lubor Niederle (1865–1944). The first issue appeared in 1892. Although Zíbrt and Niederle belonged to the 'bourgeois' era of *národopis*, their legacy was not erased by Soviet-influenced ethnography. On the contrary, they were constantly referred to throughout the communist period and they seem to be the giants on whose shoulders the construction of ethnography rested as a whole. Although the publication of *Český lid* was discontinued in 1931, the journal become the most ancient periodical in the field after its resurrection in 1946. Since the 1950s, *Český lid* began to be published by the Ethnography Institute and thereby acquired further symbolic legitimacy as one of the leading scientific journals in the field. Because of its symbolic capital (having been founded by the 'giants' of the field, its long tradition, and being published by the Academy of Sciences), the articles published in *Český lid* are also endowed with the most symbolic capital. To have an article published in *Český lid*, rather than in any other Czech or Slovak ethnography journal, made or makes someone more of an ethnographer.[12]

The next part of this article will consist of an analysis of ethnographers' original writings in the form of scholarly articles written for *Český lid*. It can be assumed that the authors invested relatively more intellectual effort to write articles for *Český lid* than they would have for any other journal or for magazines and newspapers. In a sense, the articles thus represent the cream of ethnographic articles. I decided to analyse articles only from the journal's essays section, because that is the section that contained original writings based on various researches. The section opened each issue of *Český lid* and this position as the display window of every issue is another sign of its symbolic importance. The other sections, in the order of their place in the journal's contents, were Discussion, Materials, Reports (including obituaries and jubilees) and Reviews. The Discussion section did not appear between 1965 and 1987 and it was reintroduced as a recurring section in 1988. None of the academics in the analysis here contributed to this section, But most of them did contribute to the Materials section, writing about

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[12] In this article, I set aside the question of struggles over and the accumulation of various kinds and forms of capital.

an artefact, a song, some kind of structure, etc., which was described as a newly discovered or interesting piece for further inquiry, but the section does not seem to have been intended for original writings. While there were several reviews in the Reviews section that contained interesting insights, I decided not to include them since none of them is more than several paragraphs in length and none of them can be considered a review essay offering any deep argument or original contribution.

## Writing and ethnography

In the introductory section, I sketched some of the sociological consequences of contention in anthropology. But what about Czechoslovak ethnography? Was ethnography any different from anthropology? Did ethnographers indulge in debates? Did the contention expressed in ethnographers' articles generate internal lines of division within ethnography, deep-seated enmities, or unlikely alliances? These questions also touch on issues of writing in general. What did ethnographers write, and why and how did they write at all? What in general did they intend to achieve by writing their articles?

What comes to mind first when we speak of writing is how anthropologists or ethnographers created their textual representations of the other [Clifford and Marcus 1986; Fabian 1983; Lass 1989]. However, I am not interested in the poetics of Czechoslovak ethnography, but rather in something that can be called the extra-textual features of academic writing. Academic writing is a specific scholarly practice and involves many different norms, strategies, forces, or mechanisms. By analysing the written production of ethnography, we can get a better grasp of ethnographers' habitus and especially how ethnographers relate to contention. The analysis is based on seventeen articles written for the journal *Český lid* and published between 1980 and 1989 [Dubovický 1989; Kadeřábková and Pargač 1982; Kašpar 1982; Krupková 1984; Šalanda 1980, 1984, 1986; Šatava 1981, 1985, 1986; Sochorová 1981, 1983, 1987; Štěpánová 1983, 1985; Tomandl 1981; Vrhel 1985]. As mentioned above, the Ethnography Department's primary function was teaching, whilst the Ethnography Institute was a research institution. Contrary to the proclaimed division of labour between the department and the institute, ethnographers working at the Ethnography Department were also expected to conduct research and to publish. What we have here is the Humboldtian university ideal which stresses the indissolubility of the links connecting research with teaching [Šima and Pabian 2013: 62–63]: Who other than practicing ethnographers should educate future generations of ethnographers? Their writings allow us to examine not only the scholarly facets of habitus, but also its educational facets. The following analysis focuses on several interrelated themes: the volume of writing, contention, originality, and uses of theory. The analysis is enriched by insights into two extra-scriptural features of the culture of contention: the peer review process and inter-departmental seminars.

If we focus on the number of articles, the three most prolific writers are Ludmila Sochorová, Leoš Šatava, and Bohuslav Šalanda, each of whom wrote three essays. They are followed by Irena Štěpánová with two essays. From the remaining six members, each wrote one essay only (Jan Pargač is credited as an author alongside Jaroslava Kadeřábková from the Ethnography Institute). If we count the number of pages, then the most prolific writer was Ludmila Sochorová (25.5 pages total). She was followed by Leoš Šatava (22), Irena Štěpánová (20.5), Bohuslav Šalanda (18.5), Jan Pargač (11), Ladislava Krupková (10), Ivan Dubovický (6), Miloš Tomandl (4), František Vrhel (3) and Oldřich Kašpar (2). It is important to add that the number of pages in each case also includes footnotes, bibliography, and a summary in German. Several articles also include pictures, one has diagrams, and one large excerpts from texts, so that the number of pages of written text, i.e. the number of pages conveying some message, is smaller than what the above numbers seem to suggest.

The number of pages *per se* is, however, misleading because different journals use different typeface, size of font, width of margins, or paper size. To adjust the numbers, I created a provisional and very rough ratio based on a comparison of two pages full of text—one from *Český lid* [Šatava 1981: 199], the other one from an anthropological journal of comparable renown called *Man, New Series* [Bulmer 1967: 16]. The former scores 833 on words, 5257 on characters without spaces, and 6093 on characters with spaces, the latter 625 on words, 3043 on characters without spaces, and 3660 on characters with spaces. The resulting ratios are 1.4/1.7/1.7. A single page with nothing but text in *Český lid* contains 40% more words and 70% more characters than a single page in *Man, New Series*. This means that the pages in *Český lid* are denser in terms of text than the pages in *Man, New Series*.

After we adjust the original numbers of pages based on the assumption that they contain nothing but text in terms of the *Man, New Series* Bulmer ratio, we get the following numbers: Ludmila Sochorová (35.7/43.4), Leoš Šatava (30.8/37.4), Bohuslav Šalanda (25.9/31.5), Irena Štěpánová (28.7/34.9), Jan Pargač (15.4/18.7), Ladislava Krupková (14/17), Ivan Dubovický (8.4/10.2), Miloš Tomandl (5.6/6.8), František Vrhel (4.2/5.1), Oldřich Kašpar (2.8/3.4). The number before the slash indicates the 1.4 word ratio, the number after the slash indicates the 1.7 character ratios. If we reverse it, then Bulmer's article would cover 15.7/12.9 pages in *Český lid*—and is thus still a longer article than any of the above because the two longest articles are eleven-and-a-half pages long.

The ratio provides us with more feasible numbers, but even after the adjustment, we should bear in mind that the original articles did not just contain text. The extensive use of illustrations in some articles means that there is even less actual text. For example, Štěpánová's articles contain approximately eight and a half pages of illustrations out of twenty and a half in total. Nor should we forget that the augmented numbers above indicate the total number of pages written overall, so the number of pages needs to be divded by the number of essays. At best, this gives us some 12/14.5 pages per essay (as is the case with Ludmila Sochorová).

The above numbers show that the ethnographers working for the Ethnography Department did not write much. To underscore how little, I should point out that I also checked two other ethnography periodicals from the 1980s – *Národopisné aktuality* published by the Institute of Folk Culture in Strážnice and *Slovenský národopis* published by the Slovak Academy of Sciences.[13] In neither of these two periodicals was I able to find any essays written by the selected group [Jeřábek 1987; Zajonc, Mészárosová, and Kostovská 2013].[14]

Let us now focus on contention. The absence of a Discussion section throughout the normalisation era up until 1988 in *Český lid* sheds some light on the rather non-contentious character of Czech ethnography and folklore studies. For this reason, it is interesting to trace to what extent ethnographers indulged in argument and contention. The result is striking, as only one article was found that qualify as engaging in debate. In his very short article, František Vrhel reproaches contemporary Marxist ethnography for not paying enough attention to cognitive anthropology. Vrhel's brief piece, however, does not addressed any particular academic or his or her work, but is aimed at Marxist ethnography in general [Vrhel 1985a: 92]. Nor does Vrhel attempt to show the strengths of the cognitive approach with examples of any material. Cognitive anthropology is merely outlined in the article. It is crucial to note that Marxism is not dismissed as such.[15]

In the remaining sixteen studies, I was only able to trace a specific kind of contention, manifested at its strongest in Šalanda's papers. In one of his articles [Šalanda 1986], Šalanda refers to the dispute over 'the meaning of Czech history', a lengthy debate between Czech historians that involved names such as Josef Pekař, T. G. Masaryk, and Zdeněk Nejedlý. This debate reached its peak in the early 20th century and among the many questions it covered were the Czech National Revival and the role in it played by the Habsburg Monarchy and the Catholic Church [Křesťan 2012: 76–81]. At the time when Šalanda wrote the article, the debate was long over and taken as concluded, at least from the Marxist-Leninist point of view. Despite having written 'As we can see, struggles over the meaning of history become manifest even today in relation to economic and socio-political

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[13] To date I have been unable to check contributions to *Národopisný věstník československý* (Czechoslovak Ethnographic Journal) issued by *Národopisná společnost československá* (Czechoslovak Ethnography Society).

[14] I skimmed through the remaining volumes (1984–1989) of *Národopisné aktuality* (Ethnography News) as they are not included in the list compiled by Jeřábek. I was unable to ascertain whether there were any competing ethnography power cliques behind the various ethnography journals. The only contributions of any of the department members in these two that I was able to find were several articles by Robek in *Slovenský národopis* (Slovak Ethnography).

[15] Yet another matter would be to show that Vrhel worked with the premise of the incompatibility of cognitive science with Marxism, thereby covertly and esoterically dismissing the latter by espousing the former. It is beyond the scope of this article however to argue this point here.

processes'[16] [Šalanda 1986: 24], Šalanda did not mention any of his contemporaries as having revived the discussion, and it is likely that he was only parroting official Marxist-Leninist dogma. For this reason, the article cannot be taken as manifesting contention.

What is interesting is that none of the ethnographers argued with colleagues from the Department in their papers or with any other colleague inside or outside their field, or even with colleagues abroad. It might be contended that it is not really clear what benefits an expert on Baroque folklore would derive from debating with an expert on festive attire. Yet, as we can see in anthropology debates, anthropological writings usually consist of several different layers of argument and expertise in one field does not prevent one from entering a debate not directly related to that field of expertise. The opposite of criticism would be the expression of support for colleagues, their standpoints, and arguments, but I was unable to find any traces of this either. Not that ethnographers did not quote their colleagues at all. We can find several cross-references to Robek, to a few ethnographers from the Ethnography Institute, and even to contemporary ethnographers in other socialist countries. Nonetheless, the references do not show any signs of contention—colleagues' arguments are neither refuted nor praised.

We can approach the issue of contention from a slightly different angle. The authors do not seem to have been motivated by efforts to establish and secure themselves an original position in the field or stand out from their colleagues by developing some theoretical or conceptual innovation or adopting a different position from those of their colleagues. The prime source of motivation in their work seems to have been to obtain expertise in a particular topic [cf. Chorváthová 1990: 350; Hofer 2005: 353–354]. Irena Štěpánová focused on clothing, Leoš Šatava on ethnic groups, Ludmila Sochorová on folk theatre, and Bohuslav Šalanda on Baroque folklore. This contrasts with the practice of Anglo-American anthropologists, for whom adopting an original position is less likely to be motivated primarily by becoming an expert in a certain area—recognition is not won by increasing one's knowledge of facts as much as possible, but by establishing an original theoretical standpoint that differs from the standpoints of one's colleagues. Knowledge of facts is indeed important, but it is secondary.

**The question of atheoreticity**

The question of originality impinges on the uses of theory. Since theories present one of the central points of contention in anthropology, it might be revealing if we look at what use ethnographers made of theories. A common objection raised against ethnography concerns its putative atheoretical nature; its positivistic fo-

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[16] All quotations from Czech articles and interviews that appear in this article were translated into English by the author of this article.

cus on fact collection at the expense of theory building. A strong claim concerning the atheoretical character of ethnography was put forward by Andrew Lass, who noted that 'the discipline's practice is governed not only by a resistance to theory' and that ethnography 'has been the least open to the theoretical developments that have occurred in the social sciences and the humanities since the turn of the [19th] century' [Lass 1989: 10]. This argument has been reappearing ever since [Nešpor and Jakoubek 2004: 53–54; Scheffel and Kandert 1994: 20; Skalník 2002: 103, 2005: 14]. It is interesting to note that objections raised against the theoretical deficiencies of ethnography actually long predated Lass's article and were first phrased in native terms. This self-criticism recurred at least since the early 1950s in the writings of various ethnographers [Anon 1965: 129; Holý and Stuchlík 1964: 228; Kramařík 1968: 6; Nahodil 1951: 55; Robek 1972b: 232; Tůmová 1964: 45] and the issue seems also to have been significant for ethnographers.[17] It is, nonetheless, important to uncover several different layers of the meaning of atheoreticity.

What then do we mean by saying that ethnography was atheoretical? The term atheoretical might at first seem to be a contradiction in terms, as there cannot be an enquiry that does not work with any theory. Even the most disdained positivism in its various versions is a theory on its own. Fact collecting and inductive logic are theoretical concepts that are supposed to get us to the truth, which is yet another concept. You may be a good observer and use inductive logic even without having read a book by Hume or Carnap. This makes you at least an implicit theoretician. From this point of view, it cannot be said that ethnography was an atheoretical kind of inquiry.

Another reason to insist on the opposite is that normalisation ethnography claimed its allegiance, at least formally, to the principles of Marxism-Leninism, which is another specimen of theory. I will demonstrate the different degrees to which Marxism-Leninism was manifest in ethnographic writings at that time. Marxism-Leninism is moreover not the only theory that can be observed in those writings. For the sake of clarity, I will separately consider the official Marxist-Leninist doctrine as a theory on one hand and other theories on the other.

In most of the essays, Marxism-Leninism was present on the level of subject matter. Occasionally we find remarks on the emancipation of the working classes, an important Marxist-Leninist topic [Kadeřábková and Pargač 1982; Sochorová 1981, 1987; Štěpánová 1983; Tomandl 1981]. Another important topic was colonialism and slavery [Kašpar 1982]. In none of these articles do authors make conscious and explicit use of Marxist-Leninist theory. Only three articles reveal a stronger and more explicit Marxist commitment [Šalanda 1980, 1984, 1986], which is accompanied by relatively frequent use of theory-specific concepts such as structure and superstructure, bourgeois, class consciousness or ideology. Compared to his colleagues, Šalanda also uses Marxist-Leninist concepts in a more

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[17] It is likely that Lass, who studied ethnography and folklore studies at the turn of the 1960s in Prague, got this idea from his own colleagues.

conscious manner. Most likely, Marxism-Leninism was all too present, so that it would have been pointless to explicitly mention its basic premises in every single article. All the articles can be understood as providing empirical material verifying the Marxist-Leninist worldview. The same is observed in the essays written by Dubovický and Šatava, whose articles addressed the topic of national groups and ethnic minorities. These topics were related to a contemporary orientation within Marxism-Leninism, which was focused on the formation of national consciousness. This orientation was sustained by a specific five-year research plan [Woitsch et al. 2016: 147].

The specific way these writings had of handling theories other than Marxism or Marxist arguments is perfectly illustrated by an article on coin jewellery written by Jaroslava Krupková. Her article makes use of arguments that can be traced back to a variety of theoretical approaches. The article is interested in the classification of coin jewels, their origin and spatiotemporal diffusion, their function, and in their symbolism [Krupková 1984: 92–96]. She also makes several rationalistic 'If I were a horse' arguments [Evans-Pritchard 1965: 24] related to the magical usage of coin jewels [Krupková 1984: 94, 95]. But nowhere in her article can we find sources for her various conjectures. There is no mention of any representatives of evolutionism, diffusionism, or functionalism. At the same time, she argues from a sociological or historical context: 'It is necessary to approach the problem from a strictly historical standpoint, in order to establish the value of particular classificatory criteria. The assessment of the value and the function of a decorative item varies with the changing socio-historical conditions.' [Krupková 1984: 93] Two paragraphs later Krupková continues on an absolutely contradictory note: 'Nonetheless, the striving of human beings to satisfy the increasing needs of beautification and decoration…' [Krupková 1984: 93]. Again, no one is quoted in support of her conjectures. Moreover, there is an unresolved and probably unperceived conflict between universality and historicity, which at least in the case of Anglo-American anthropology serves as one of the driving forces behind adopting different positions and *ipso facto* theories. Krupková does not seem to have been motivated by any effort to resolve this contradiction. Moreover, from the way Krupková handles theories, we can surmise that questions regarding functions, origin, diffusion, symbolism, etc., were not approached as problems but as somehow natural features of her subject matter. Coin jewels must have an origin. Coin jewels definitely spread over time and space. Coin jewels have a function and a symbolism.

Probably the most conscious use of theories other than Marxism-Leninism can be found in Vrhel's only paper [Vrhel 1985a]. Vrhel, unlike his colleagues, displayed a confident and knowing approach to theory.[18] However, Vrhel's article, which deals with theories of language and cognition, revolves entirely around theory and could be called overly theoretical. Only in Šalanda's articles do we

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[18] It is of interest that Vrhel used a completely different quoting style.

find some kind of balance between facts and theories [Šalanda 1980, 1986], Marxist-Leninist and others alike.

From this overview, we can see that none of the articles was completely devoid of theory. While most of the articles can be called descriptive, it does not follow that they can also be deemed atheoretical. However, all the articles have three important things in common. First, ethnographers did not quote sources for their various conjectures. The case in point is again Krupková's essay, but the same practice can be found in the essays by Dubovický, Pargač, and Šatava. Another striking fact is that not even the works of the central figures of Marxism-Leninism were quoted. Second, nowhere do ethnographers forge strong links between theory and data on the basis of evidence. If there is some argument reminiscent of theory, it is not consistently pursued and it is not clear how the theory relates to the data on other than a very general level. The links between theory and data then become rather flimsy and it cannot be said that the data substantiate theories or that the theories explain the data in any meaningful way. Third, and most important, the way ethnographers wrote about their subject matter tended to render theory invisible.

If we are to account for atheoreticity, or rather, theoretical inadequacy, there are several possible explanations. The first that comes to mind is that the Cold War era severed links between the countries divided by the Iron Curtain. The iron divide prevented the circulation of ideas between countries in the two blocs. It was not easy to come by books written by Western colleagues who were more focused on theory. Moreover, any work that did not fit the Marxist-Leninist doctrine championed in the Eastern bloc could be dismissed as reactionary. The argument is sound but it does not explain why several local ethnographers read, reviewed [Šatava 1986b; Vrhel 1985b], and even quoted, as we saw in the case of Vrhel, American anthropology books and journals. The Iron Curtain argument may at best account for the small number of books that local scholars could get hold of.

Another explanation relates to the use of foreign languages. Apart from Latin or German, as languages particularly suited to research in ethnography, and apart from the compulsory Russian language, students could study other languages such as English or French. However, the impression I got from one interview concerning language skills for doctoral candidates was that the lessons were aimed at grammar and conversation rather than at reading and skills for academic work. Beginning with the first issue of *Český lid* in the 1980s, the German summaries for each of the articles were written by Alfons Hubala, a translator hired by the Ethnography Institute. This indicates that not even German language skills were promoted among ethnographers, who relied on an external translator instead. It is interesting to note that *Český lid* was a wholly Czech- and occasionally Slovak-language journal and that only Czechoslovak ethnographers contributed to it, the sole exception being four articles that were written by foreign scholars for the Materials section [Förster 1988; Garcia and Menocal 1989; Jovčeva 1980; Kasper 1987]. Translators are mentioned only in the article of Garcia and Menocal. It is unclear whether Jovčeva, Kasper, and Förster wrote

their original articles in Czech or in Bulgarian, German, or another language. The language argument has merit and can explain why it was harder for ethnographers to reflect on recent developments in non-Czechoslovak-language anthropology, ethnography, etc. But even if any Prague ethnographers knew German and Russian well, assuming that work written in German and Russian contained some discussion of theory, none of the ethnographers pursued themes related to theory. Also, although it is likely that Czech ethnographers maintained some contact with scholars from other countries and although several foreign scholars contributed to *Český lid*, there is no trace of any discussion of theory arising from any international exchange.

A third and perhaps more convincing explanation runs something like this: Since the theory of Marxism-Leninism was the official doctrine, it would not be reasonable to challenge it, however unintentionally, by forging stronger links between theory and empirical material. The official doctrine was upheld by political means and anyone who even unintentionally attempted to reveal its weaknesses by demonstrating its theoretical inadequacies on the basis of empirical evidence would have experienced unpleasant consequences. No matter how tempting this argument is, it falls short because, first, it does not account for instances of mild criticism of Marxism, like that in Vrhel's paper, and, second, it does not account for examples of the sloppy use of non-Marxist-Leninist theories. Weak links between theory and data are observed in the use of Marxist-Leninist and non-Marxist-Leninist theories alike.

The explanation I propose is that Czech ethnographers were used to handling theory in a way that was much different from Anglo-American theorising. Unlike the argument advocated by Lass or other natives, I do not think that ethnography suffered from atheoreticity as much as from the inept use of theory. Their specific approach to theory did not allow ethnographers to forge stronger links between material and theory, whether Marxist or non-Marxist, making it seem as if there was no theory present. Theory is a specific tool and there are as many ways of making use of it as there are ways of hammering a nail into a wall. There may be one way that is the most efficient one with respect to the particular design of the hammer, but that does not mean that the hammer cannot be utilised in different ways. There is nothing in the universe that could ultimately prevent anyone from using a different part of the hammer head than the face. But why not use the handle? Why not place the hammer in a display cabinet? What is true of hammers is true of theories. As a result, an ineptitude at handling theory prevented Czech ethnographers from using theory as a focal point of disagreement and contention. This argument transposes the issue of atheoreticity into a new key and gives the term atheoretical a better and a more precise meaning.

To support this thesis, it is instructive to look at ethnography articles published before the normalisation period was in full swing. Controversies were quite common at the time, but many of them concerned discussions over factual issues rather than heated debates over theoretical issues. At that time, it was possible to seek inspiration among non-Marxist theories. For example, it was pos-

sible to make extensive use of functionalist anthropologists [Holý 1963; Holý and Stuchlík 1968], openly look for inspiration in Western anthropology [Skalníková and Fojtík 1969], and quote the Austrian economist J. A. Schumpeter without the fear of being labelled as a bourgeois ideologue [Polednová 1969: 339–340]. This is an interesting point since at the beginning of the 1960s social anthropology still reeked of colonialism [Holý 1963: 3; cf. Nahodil 1951: 54]. But again, perhaps with the exception of Holý and Stuchlík's volume [1968], there is no trace of ethnographers investing much of their time into the discussion of theories.

Links between theory and empirical material were in most cases weak and it seems that the prime cause was the ethnographers' clumsiness at dealing with theory, at least when we compare ethnographic to anthropological uses of theory. Ethnographers did not possess the necessary skills to handle the hammer of their discipline. This may also account for the fact that, as Lass noted, ethnographers have never truly disputed the concepts essential to their discipline—for example, popular culture, nation, and tradition [Lass 1989]. Although there were native articles that touched on the point [Jeřábek 1964], the issue seems not to have occupied ethnographers' minds too much and it did not develop into a full-fledged controversy.

## Extra-scriptural facets of contention

There are two more things that highlight the specific character of normalisation ethnography. They relate to the writing and the culture of contention, but they are not evident in the writings; they had to be elicited in the interviews with interlocutors. These two features are especially important if our focus is the reproduction of scholarly habitus.

The first one brings us back to the seminar culture described by Gell. In several of the interviews I asked whether anything similar to seminars took place in the Ethnography Department or whether colleagues in the department discussed each other's work: '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 department] and about what was to come. There was no discussion about that. And to discuss each other's work—absolutely not.'[19] Touching the issue of Robek's leadership, the interlocutor acknowledged that if two ethnographers worked on the same research project, there was regular discussion on a personal, not a departmental basis.[20] In any case it seems that ethnographers in the Ethnography Department were not used to discussing

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[19]  Interview dated 15 June 2017.
[20]  Interview dated 15 June 2017.

their work. The non-existence of intra-departmental seminars was confirmed to me by two other members of the department.[21]

The second, related issue was the review process in *Český lid*. After describing the contemporary rigmarole and hardships I witnessed in my experiences with the peer-review process, I asked what peer review was like in *Český lid* and whether it was comparable to the contemporary peer-review process. One of the replies was: '…if the topic was accepted and also the overall impression [of the article], nobody intervened in the process anymore. Maybe with the exception of cases where grammatical or stylistic changes were necessary … However, the situation that you mention, that you receive your article full of notes, and that this is wrong and that should be added, that was not the case.'[22] Another interlocutor, also a member of the Ethnography Department, told me a similar story about a different article. The article was submitted and later published without any word from the editors.[23] Although there definitely was a peer-review process in *Český lid*, it seems to have been more of a formality, at least in several cases. The proper double-blind peer review that meets the current standards was not introduced in *Český lid* until 2006.[24]

The absence of inter-departmental seminars is further evidence of the patent non-existence of contention in journal articles and sheds further light on the fact that the scholarly practices of ethnographers were not, contrary to their Anglo-American counterparts, characterised by contentious social exchange. The peculiar quality of the peer-review process in the most renowned journal of ethnography also touches on this issue, since reviews play an important role in scholarly contention.

## A hard habitus to break

The history of ethnography and folklore studies during the normalisation era presents an interesting paradox. When Antonín Robek assumed his reign in the early 1970s, he reproached ethnographers for a lack of cooperation and their preference to pursue individual scholarly interests instead of submitting to the demands of

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[21] Interview dated 17 February 2017, addenda; and 23 May 2017, addenda.
[22] Interview dated 15 June 2017.
[23] Interview dated 23 May 2017.
[24] For this information, I am grateful to Jiří Woitsch, a long-time chief editor of *Český lid*. During our discussion, I learned that there was no archive of peer reviews and thus no materials with which scholars could study the peer-review process in *Český lid*. I was also told that the peer-review process was not standardised. Some contributions to the Essays and Materials sections were reviewed, others were not, the review process was not always anonymous, and in at least in several cases it was performed by members of the editorial board, not by external reviewers. Sometimes articles were accepted or even altered and published without the authors being notified.

ethnography. He also expressed an opinion that too much emphasis was put on collecting facts at the expense of 'syntheses and generalisations—higher forms of scientific work' [Robek 1972b: 232]. By the end of the 1980s, we see that as far as it concerned the Ethnography Department, Robek's policy achieved results quite opposite to its intentions. The lack of scholarly exchange, the general unwillingness to discuss each other's work, which went hand in hand with an increasing fragmentation of the field into individual research projects, was actually noted by several scholars, such as Ladislav Holý or Václav Hubinger, who took part in attempts to establish sociocultural anthropology in the Czech Republic in the Anglo-American fashion in the early 1990s [Chorváthová 1990: 350; Hubinger 1993: 337–338]. Again, a comparison with the 1960s may reveal interesting developments. When, for example, Holý and Stuchlík worked with their colleagues and students on their collection of writings about social stratification in Africa, several discussion seminars were held for the contributing authors.[25] The trend during the normalisation era was quite the opposite. (Czech) Ethnography's developed its non-contentious style and asocial character primarily during Robek's era as a consequence of his policies.

The durability and resilience of ethnography became apparent in the 1990s after Robek was dismissed as the head of the Ethnography Institute and after he retired from the field. If we trace the number of contributions to the Essays section in *Český lid* in the 1990s, we find that our group of ethnographers produced only nine articles in that decade, compared to seventeen in the 1980s. Moreover, the articles were written by only five out of the ten members of the staff—Vrhel (3), Štěpánová (3), Dubovický (2), Šalanda (1), Kašpar (1). Krupková, Sochorová, Pargač, Šatava and Tomandl did not publish any essays in *Český lid*. None of the Ethnography Department's personnel mentioned above contributed to the Discussion section either.[26]

It was noted above that the scholarly facets of ethnographers' habitus can be linked to educational facets. It cannot be expected from professors and lecturers who do not take writing and contention as the cornerstone of their academic practice to instil positive attitudes towards these activities in their students. This was one of the topics in some dozen interviews that I conducted with ethnology students who had studied in the Ethnography Department during the 1990s. In the students' accounts, most of the ethnology training involved taking notes during lectures and passing oral exams, for which the students had to invest time memorising knowledge from the lectures. The education was not writing-oriented or contention-oriented.

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[25] This was communicated to me by Josef Kandert, whose article about Zande social stratification is part of the collection.

[26] This sudden drop may be explained by the as yet unauthenticated fact that Robek assigned his colleagues to write articles and sent them out to conferences. When Robek retired from ethnography, his colleagues might have lost the strong incentive to write.

There were, of course, deviations from the dominant academic approach in the Ethnography Department. In the early 1990s, under the leadership of František Vrhel the department opened its gates to Western influences. Local enthusiasts for anthropology, who up to the end of the 1980s had occupied a marginal place in ethnography (e.g. Josef Kandert, Václav Hubinger) and professors from Western universities (e.g. Leopold Pospíšil, Zdeněk Salzmann, Paul Garvin, Milan Stanek) were given an opportunity to teach anthropology courses.

This opening up had some consequences for the local academics. Several members of the Ethnography Department absorbed a different academic style after study stays abroad, which would not have been possible on such a scale during the normalisation era. For example, Ivan Dubovický, who spent a year in the United States as a Fulbright scholar, wrote two unprecedentedly long essays [1996a, 1996b]. The same is true of Jaroslav Skupnik and Petr Lozoviuk, two students in the Ethnography Department and later associate professors there, whose approach to writing is markedly different in both style and volume compared to essays written by their colleagues [Lozoviuk 1994, 1997, 1998; Skupnik 1995, 1997, 1998, 1999]. Their different approach has apparently also found expression in their teaching style, which was mentioned in the interviews with students.

However, these 'maverick' scholars occupied the educational and political margins of the discipline. Hubinger left the Ethnography Department in 1994 after unsuccessful attempts to push through his draft for an anthropology curriculum.[27] The lists of lectures at Charles University show that visiting professors taught only electives and Skupnik and Lozoviuk never attained a strong educational position within the Ethnography Department in relation to senior colleagues. Here we arrive at a fundamental point. Those ethnographers in the Ethnography Department who began their careers during Robek's era still made up the majority of the department's staff until a few years ago. They were thus in a key position from which to reproduce their academic habitus. Whereas curricula changed quickly in the early 1990s and students could freshly pursue anthropological knowledge, their education was carried out mostly by academics whose habitus was unaccustomed to practices that promote a contentious style and extensive writing, important ingredients of the academic enterprise of Anglo-American anthropology. That is not to say that the ethnologists performed their task badly; they simply did their best. But their best did not fit the best of the new direction post-socialist academia wanted to move in. At the same time, it seems that members of the senior generation were not strongly incited to change their ways in the new situation.

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[27] Personal communication.

## Conclusion

This article can, unfortunately, offer only a piecemeal reconstruction and the conclusion that much work has yet to be done. With regards to attitudes towards writing, this analysis omitted the books that the ethnographers wrote and articles written for lesser journals. Any future analysis should also include conference attendance. Other mechanisms—for example, the peculiar academic hierarchy of degrees, where criticism of a senior colleague might be considered extremely immature and disrespectful—might have also played a role, but here they can only be hinted at. Above all, as a historical inquiry, this study cannot benefit from the participant observation that is available to working on the topic of university education [cf. Dvořáčková et al. 2014].

However, having analysed several interconnected features of ethnography writing, we are now in a better position to grasp the dynamics of the field of ethnography. Contrary to Anglo-American anthropology in which contention and writing influence the dynamics of the field, no culture of contention seems to have existed in normalisation ethnography. Ethnographers were unaccustomed to taking part in the culture of contention and its rituals and the entire dynamics of the field rested on different principles.

A decade ago, Chris Hann sparked a stimulating discussion after presenting a vision of a synthesis of the Central and Eastern European tradition of ethnography and *národopis* and Anglo-American anthropology [Hann et al. 2007]. In the discussion that ensued, several scholars replied that the desired synthesis was unachievable as the two traditions had been separated by a wide gulf. A recurrent theme was that the differences were mainly theoretical or intellectual: while *národopis* is the science of the colonised, local in orientation and historical in scope, anthropology is its very opposite—it is the science of the colonisers, it is cosmopolitan in scope and has strong ahistorical roots. I believe that I was able to show that these differences are secondary or even tertiary. The gulf is not that wide here. There is a causeway connecting the two sides, although it is regularly flooded at high tide.

If we want to find out where the gulf widens, we should look elsewhere. As Hann rightly observed in his article, '…some things change more slowly than others: more precisely, that norms, values, mentalities etc. have a force capable of defying the intended logic of legislative or economic changes' [Hann et al. 2007: 5; cf. Verdery 1991: 253–254]. Anthropology as knowledge was accepted quickly, but the same cannot be said of anthropology as a scholarly practice. This source of the resilience of some practices should be sought in the habitus, which, among other things, are 'systems of *durable* … dispositions' [Bourdieu 1977: 72, italics mine]. Had ethnography produced a different scholarly approach and had ethnographers been keener to share and discuss their theories and findings, a synthesis would be much closer to possible.

The discussion about multiple temporalities in post-socialism as well as other accounts of post-socialist developments in anthropology [Nešpor and Ja-

koubek 2004; Sárkány 2002] put too much unwarranted stress on the intellectual differences between anthropology and ethnography. I do not want to recast the old quarrels between idealism and materialism, but with the exception of some remarks [Bitušíková 2002: 143–144; Hann et al. 2007: 16] or standalone articles [Fillitz 2003; Hervik 2003; Hofer 2005; Vidacs 2005], anthropology, provided that we set aside the sacred cow of participant observation, is usually represented by its findings or by theoretical and conceptual advances and achievements. This approach takes into account only the fruits of the discipline and is not surprising: By their fruits you will recognise them. Unfortunately, this approach tends to disregard the fact that the tree also has branches, leaves, a trunk, and roots, and that it grows in specific surroundings. Only those few standalone accounts mentioned above, as far as I am aware, really paid attention to the facts of the tree other than its fruits.

It would be a grave misunderstanding to conclude that this debate relates only to petty fights in some socially and politically insignificant disciplines such as ethnology and anthropology. Research can provide us with the means to fight the most serious problems in Czech higher education. For this reason, I will conclude the article with a question. How much did late socialist education influence post-socialist developments in fields such as law, economics, journalism, political science, or veterinary medicine, which undeniably exert a far greater influence on contemporary Czech and other CEE societies?

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