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ANTHROPOLOGY'S
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IN CENTRAL AND
EASTERN EUROPE

A DEBATE

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Anthropology's Multiple Temporalities and its Future in Central and Eastern Europe

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(with comments from Milena Benovska, Aleksandar Bošković, Michał Buchowski, Don Kalb, Juraj Podoba, David Z. Scheffel, Petr Skalník, Michael Stewart, Zdeněk Uherek, Katherine Verdery and a reply from Chris Hann)

Abstract

Hann's essay takes a parochial academic anniversary in Britain as an occasion to reflect on ensuing changes of paradigm in social anthropology, notably the rejection of evolutionism and the neglect of history that accompanied the 'fieldwork revolution' led by Bronisław Malinowski. In the light of this discussion it is argued that the 'anthropology of postsocialism' of recent years should not content itself with ethnographic studies of transformation but would benefit from engaging more seriously with multiple layers of history as well as with adjacent social sciences. It is further argued that social and cultural anthropologists should form a common scholarly community with the 'national ethnographers', since these two styles of enquiry complement each other; but such integrated communities remain rare, in Britain no less than in Central and Eastern Europe. These propositions are discussed from a variety of standpoints by ten colleagues. Finally, Hann responds to their comments and criticisms and restates his position on the central intellectual and institutional issues.²

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² The lead essay by Chris Hann was commissioned by the Editors of *Sociologický časopis/Czech Sociological Review* to introduce a special issue of studies of postsocialism. After receiving his text the Editors decided to send it out to numerous colleagues for comment, and Hann was given an opportunity to respond to these responses. The special issue was published in Czech in February 2007 as Vol. 43, No. 1 of the journal. This Working Paper provides a full translation of the exchanges in that issue. Thanks to all the commentators for agreeing to this speedy dissemination of an English version, to Marek Skovajsa and his co-Editors for their authorization, and to Robin Cassling, who translated the comments of Juraj Podoba and Zdeněk Uherek and the statement by Ivo Budil (see Appendix). These translations and the organization and editing of the whole debate in *Sociologický časopis* were sponsored by the Institute of Sociology, Academy of Sciences of the Czech Republic (www.soc.cas.cz), and the International Visegrad Fund, Bratislava (www.visegradfund.org).

I

A century has passed since James Frazer received an offer in 1907 from the University of Liverpool to take up the world's first university chair in social anthropology (Ackerman 1987: 207-8). Frazer was the archetypal 'armchair anthropologist': an erudite classical scholar whose knowledge of obscure ethnographic details was encyclopedic. It stemmed almost entirely from books and the reports of travellers and missionaries. His anthropology, like that of the great majority of his contemporaries, was self-consciously evolutionist. Frazer's distinctive contribution, most famously in *The Golden Bough* (1890), lay in the field of belief and knowledge: he outlined a steady path of progression from magic to religion and then eventually to science. He would surely be astonished at the topics addressed by today's social anthropologists. As a scholar who took it for granted that the discipline was concerned with 'savages' and with the broad sweep of human history, he might be especially surprised that many anthropologists have become specialists in the analysis of social change as it unfolds before our eyes in the contemporary world.

Studies of the postsocialist transformation of Eastern Europe of the kind presented in this special issue (of the *Czech Sociological Review*) provide a good illustration of how far anthropologists have come: their work is increasingly read by other social scientists in search of answers to puzzles left unresolved by the standard models of 'transition' in large disciplines such as economics, political science and sociology. It is not my task to discuss the papers in this special issue: they speak for themselves, and in any case some of the key themes are highlighted by the Editors in their introduction. Rather, I would like to use the open-ended invitation that has been extended to me to offer some general reflections on social anthropology's varying engagement with questions of history and evolution. I hope that these reflections will help to place the contributions collected here in a wider intellectual context. I shall argue that there will always be a need for fine-grained ethnographic observation; the need is especially great in times of major social change, when the insights of ethnographers may be especially valuable in complementing, re-shaping and occasionally correcting the paradigms used in other disciplines. So let me make it absolutely plain at the start: ethnographic studies of the recent transformations in the former socialist countries are indispensable. This is the major priority of my department at the Max Planck Institute for Social Anthropology in Halle.

But I also want to argue that good ethnography forms only one part of social anthropology. As the second decade of 'transition' draws to a close it is time that the anthropologists working in this region begin to take up temporalities other than the postsocialist present. At the very least this should mean paying careful attention to how socialist-era history has impacted on the most recent developments. It could also mean something more ambitious, e.g. assessing the significance of socialism in the long-term history of Eurasia. However history is brought in, this expansion of the temporal framework will raise crucial theoretical issues that have not yet been adequately faced by the ethnographers of transformation.

These thoughts lead me in the concluding section to pontificate about the relationship between social anthropology and certain neighbouring disciplines. The relationship to sociology is evidently strong in the Czech case (or else this special issue would never have come about). But I am more concerned with the relationship to the field of *etnologia*, *národopis*, *folklor* etc. Of course it is not my job to advise colleagues in other places how to design their institutes, appoint their staff and organize their seminars. Nonetheless I shall try to explain why, despite the anguish this stance causes to some of my closest friends in the region, I think that it is generally a mistake to attempt to

create a separate discipline called social anthropology, as a rival and competitor to the established intellectual communities. A genuinely comparative and cosmopolitan anthropology department would be able to integrate colleagues working on contemporary transformations with those specialized in other periods of history, and the integration should be mutually beneficial.

II

The scholar who did most to displace the paradigm of James Frazer (which he discovered when still a student in Cracow) was Bronisław Malinowski (Jarvie 1964, Gellner 1995). Ernest Gellner has argued ingeniously that the shift towards synchronic analysis based on the observations of the fieldworker can best be explained with reference not only to Malinowski's rigorous training in the empirical philosophy of Ernest Mach but also in terms of his strong cultural identity as a Pole, i.e. a member of a nation which had not been treated kindly by the march of history (Gellner 1988). Of course Malinowski was not the first researcher to carry out fieldwork. Recent scholarship has drawn attention to deficits in his contributions and punctured some of his self-serving claims (Young 2004). As for the theoretical foundations of Malinowski's 'functionalism', since his death in 1942 they have hardly found any supporters. Despite these and other shortcomings, the work carried out in the Trobriand Islands between 1915 and 1918, painstakingly written up during the following two decades, set new standards of "close-up" analysis and inaugurated a "golden age" for the British school of social anthropology in the last decades of the British Empire (Kuper 1983).

This work was synchronic, in the sense that the published accounts were based solely on fieldwork carried out in a specific place at a specific time. In actual fact, as Michael Young's biography makes clear, Malinowski moved around quite a lot. He spent altogether about six months in the specific community of Omarakana on the island of Kiriwina, but he did not spell out the chronological details in his publications. He gave away precious little information concerning the administration of the colony, exceptionally well run by Assistant Resident Magistrate Raynor Bellamy, a medical doctor and graduate of Cambridge. Malinowski's Trobriand monographs were "out of time" (Thomas 1989) in the sense that he made no effort to place the micro-analysis in wider temporal frameworks. He condemned anthropologists' penchant for evolutionist speculation and 'conjectural history', but he did not replace it with any rigorous new temporality, e.g. by engaging with the consequences of colonialism, including Christian missions. Rather, the ideal undergirding the Malinowskian revolution was that, through the direct methods of field research, anthropologists could gain access to a timeless other, to an unsullied 'noble savage' or *Naturmensch*.

To his credit, Malinowski eventually realized the inadequacy of such accounts. In an Appendix to the last and richest of his Trobriand monographs (1935) he is critical of his failure to document the impact of the Europeans who preceded him in the region. By this time he was explicitly encouraging his students to study processes of 'social change'. This was a key element in his strategy to transform social anthropology from the study of exotic *Naturvölker* into a discipline that might provide useful advice to colonial administrators, and perhaps even for plantation managers, and thereby attract funding as an applied social science. This can be viewed as a second revolution. Malinowski was certainly the key figure in opening up a remarkable phase of expansion for British social anthropology in the last decades of the Empire (Goody 1995).

Members of this school differed in the attention they paid to history. While some emphasized ongoing processes of transformation (e.g. Schapera 1947), others addressed the issue of change

only when undertaking a restudy of their original field site (e.g. Firth 1959). In general, though they soon began to replace Malinowski's 'functionalism' with more complex bodies of theory, the social anthropologists did not engage seriously with long-term history. Edmund Leach (1954) was famously critical of Edward Evans-Pritchard's (1940) 'equilibrium' analysis of the political system of the Nuer of the southern Sudan. His own account of the political systems of Highland Burma emphasized a dynamic oscillation between different models of polity. However, according to a later critic this 'pendulum' still lacked an adequate grounding in the long-term political economy of the region (Nugent 1982). Leach rejected this criticism and insisted that he had undertaken exhaustive archival research on top of his fieldwork. In most cases, however, the fieldworker never set foot in the archives. Fredrik Barth's celebrated work on Pathan political leadership (1959) could hardly be dismissed as a static or synchronic study, and theoretically it led him to develop his influential ideas concerning 'transactionalism'; but it failed to set the dynamic rivalries it documents in a context longer than the recollections of the informants (Meeker 1980).

More satisfactory forms of engagement with history have developed continuously since the demise of the European colonial empires. Unsurprisingly the rapprochement was pioneered in regions such as South Asia, where anthropologists could no longer afford to remain ignorant of the text-based scholarship of the Indologists (Cohn 1987). Many anthropologists working in Europe found themselves drawing intensively on the work of colleagues in history, though few went into the archives themselves (Davis 1977). At the same time numerous historians began to apply anthropological concepts in the course of micro-level enquiries constituting the new field of "historical anthropology" (e.g. Medick 1988). In recent years the entire field has been strongly influenced by debates over 'Orientalism' and new bodies of theory such as 'postcolonialism'. Widely admired works in the anthropological literature of recent years include Jean and John Comaroff on Southern Africa (1991) and Nicholas Thomas (1994) on the Pacific.

In summary it can be noted that the shift from armchair evolutionism to fieldwork-based synchronicism was not as dramatic as Malinowski liked to claim, not even in Britain. The new methods carried the danger that ethnographers would abstract from dissonant external elements and locate their object of study outside of real historical time. However, and this too was attributable to Malinowski's lead and success in fundraising, by the late colonial period most ethnographers began to build diachronic elements into their study. The extent to which they engage with history continues to vary greatly, as do theoretical paradigms. But what I wish to emphasize is that very few contemporary fieldworkers in social or cultural anthropology attempt to explain their data with reference to any form of evolutionist theory. Thus, while Bronisław Malinowski is generally viewed as the key founder of the modern British school, James Frazer epitomises the anthropology that was displaced; Frazer is no more than an obscure ancestor, of no contemporary relevance.

III

In the field of postsocialist studies there is not as yet any body of theory comparable to postcolonialism. Some anthropologists working in postsocialist societies – including several contributors to this collection – have drawn on postcolonial theory.³ But the main unifying feature of the work gathered here is that it is based on the primary hallmark of the modern discipline i.e. fieldwork. In line with general trends, some have carried out 'multi-sited ethnography' and the sites

³ See Verdery 2002 for a stimulating assessment of the possibilities. I have commented recently on tendencies that I consider undesirable (Hann 2005a).

include NGOs and government offices as well as the archetypal rural *Gemeinschaft* favored by earlier generations of fieldworkers. But the details of location are surely secondary. What matters is that the anthropologist is able, on the basis of close-up observations of social action, to complement other disciplines' accounts of what is actually unfolding in turbulent times. Fieldwork gives privileged access to social meanings and thereby to realize the goal of Weberian sociology. It enables the anthropologist to pinpoint the relative strength of various norms and values, and also the informal social networks which are often crucial to the strategies of almost all groups, 'losers' as well as 'winners'. In short, light is shed on invisible, tacit forms of knowledge, on beliefs and practices which can never be captured in the statistics of economics or even the most sensitive surveys of sociologists. All this is surely a crucial contribution. On the other hand, critics might argue, an astute investigative journalist might be able to do just as good a job. What, then, are the features of an anthropological approach, which distinguish it on the one hand from qualitatively oriented sociology and on the other from the *feuilletons*?

For me, the key to the answer lies in theoretical questions, which are best approached as questions of temporality. Let me explain this in two steps, first with regard to the past in the present, as this can be investigated by the fieldworker; and second, with regard to questions requiring more serious historical engagement and perhaps even an evolutionary perspective.

Like other species, humans live in time. We are alive in the present but, to a much greater extent than any other animal, we have expectations and aspirations concerning the future. These are influenced above all by the experiences we have had in the past, i.e. by our memories. The field of memory has itself been a major focus of inter-disciplinary scholarship in the postsocialist countries, above all because of the repression that distorted memory at both collective and more personal levels in the socialist era. Anthropologists have made major contributions to this literature (Watson 1994; Pine, Kaneff and Haukanes 2004). Commemoration of past events is constantly shaping our understanding of the present. While collective memory can also be explored through studies of school textbooks and public rituals, other levels require more sensitive probing of subjectivities that only intimate access through fieldwork can make possible. At the same time the narratives of informants must be placed alongside other sources, and they will not always find confirmation in the archives.

The general thrust of a good deal of the anthropological work on postsocialism is that some things change much 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. This message comes through very strongly in the work of the Halle Focus Group on "Property Relations", which between 2000 and 2005 undertook a series of investigations of decollectivization (Hann 2005b). Others have shown the importance of continuities in cultural understandings of the person in the postsocialist factory (Dunn 2004). Even studies of highly ephemeral phenomena of the transition period, such as the soup-kitchens studied by Melissa Caldwell in Moscow, which ceased to exist shortly after her monograph was published (Caldwell 2004), are valuable in part for the light that they shed on cultural persistence. In general, anthropologists have pointed out a dissonance between the blueprints favored by policymakers, internal as well as external, and the intractable realities of postsocialist communities. To give an example for the Czech case, the late Ladislav Holý (1996) posited a contradiction between the goals of aggressive neoliberalism (as it was not yet called) and the deeply entrenched egalitarian solidarity of the great bulk of the Czech population.

Now, as every Czech reader knows better than I do, such arguments cannot be pushed too far. Were he alive today, Holý would be astonished by the changes that have taken place in a very short time. Anthropologists must also come to terms with rupture. Humans sometimes display the ability to come to terms with radical changes in every walk of life. They may assert transformation as the very essence of their new identity, as Joel Robbins has argued recently on the basis of his work among Melanesian Pentecostals (2003). In the postsocialist case, too, anthropologists have been led by what Robbins calls the “continuity thinking” of the discipline to focus on elements which suggest a grafting on of the new to older cultural components; but Robbins argues that we must also take seriously what people assert when they tell us they wish reject everything to do with their past identities.

This warning is salutary: anthropology must be more than a ‘science of continuity’. We know that Pentecostalism has made inroads in many postsocialist countries (Wanner 2003) and the point surely applies to other groups as well. Yet it is also true that many outcomes, at all levels, have been shaped by the kind of intangible continuities which Holý sought to analyze. Some aspects of the way in which born again Christians disseminate their gospel may be strongly shaped by networking practices that developed under socialism, even though the actors may be unaware of such similarities. The only way to test such possibilities is to make systematic enquiries about past arrangements. The possibility of change must be raised constantly, and continuity may often be the exception rather than the rule.

Obviously this research challenge is radically different from the goals of Malinowski as he wrote up his Trobriand monographs. There is no conceivable way of taking the ‘transition’ societies outside of time; the problem is how to formulate any analysis of lasting validity when the object of study is, by definition, in a condition that is the very opposite of equilibrium. We must learn from earlier debates and resist the temptation to assume that a new equilibrium position is within reach, perhaps just around the corner. The flux of postsocialist societies is merely a heightened form of the continuous process of change to be found in all forms of society; from this perspective the study of postsocialism highlights the need for more sophisticated models and methods for studying social change everywhere (Ellen 1994).

As a result of such work a good fieldworker, fluent in the local language, may eventually be able to speak with some authority about differential rates of change, even without any systematic historical work. On the other hand, claims about, say, the persistence of egalitarian values, recorded in the anthropologist’s notebook *circa* 2000, will appear more plausible if they are born out by supporting data in the life-history narratives the anthropologist has collected. Of course the history narratives recorded in 2000 may differ radically from those that might have been told in 1989. The next step, to assist in interpreting the ‘remembered histories’ of informants, is to undertake some archival work, often digging deep into issues that might appear parochial but nonetheless shed light on more general processes of adaptation. The vagaries of record-taking in the socialist years might in turn make it desirable to consult other higher-level archives, or to supplement work in one region with further enquiries in another with a view to attaining a more balanced overall account. In short, many key problems of ‘transition/transformation’ lead ineluctably into historical research into the socialist period, both in the form of oral histories and systematic archival work.

I note in passing that some of the more satisfying accounts of postsocialist societies have come from scholars whose personal links to the subjects of their research date back to the socialist years (Verdery 2003, Creed 1998 Kideckel 1993, Lampland 1995). I do not mean to argue that only

ageing scholars of my generation are qualified to address postsocialist societies with the appropriate temporal depth! It is open to researchers of all ages to take oral history seriously and to engage explicitly with the abundant historical materials now available.

IV

So far I have argued that anthropologists investigating postsocialist transformation should not content themselves with a 'presentist' perspective. While the deep assumptions of the discipline tend to presuppose continuity, the challenge facing us is to specify differential rates of change. This has implications for theory (e.g. concerning the utility of the concept of culture to indicate long-term rigidities in values and meaning-systems) but also for practice, as governments reform political, economic and legal institutions, and previous certainties, e.g. in the field of jobs and social security, are shattered.

Is it possible that postsocialism specialists might do more than make a nod to history? Might they even help to rescue the concept of evolution from the bogeyman status it presently enjoys in the eyes of most social and cultural anthropologists? James Frazer specialized in religion and was not known for his contributions to economic, political or legal anthropology. Yet like distinguished contemporaries in Germany (e.g. Eduard Hahn) he believed that religious beliefs could play a key role in economic and technical evolution. There is a fascinating lecture in *Psyche's Task* (1909) in which Frazer addresses the theme of property. His basic argument is that numerous apparently irrational practices and 'superstitions' have unintended consequences in terms of establishing property rights. Frazer's theory amounts to a whimsical liberal teleology: human institutions have evolved progressively as a result of cumulative experiments by 'savages'. However, later fieldworkers provided supportive illustrations of his basic functionalist point, e.g. Raymond Firth's demonstration of the economic logic which lay behind the power of a Tikopia chief to impose a ban (*taboo*) on the harvesting of coconuts at particular times (1939).

Now, while many scholars have viewed socialist ideology as superstition, I think few would wish to interpret the property changes introduced by socialist powerholders in the twentieth century as a further stage in benign evolutionary processes of variation and competition. The consequences of these beliefs were economically disastrous as well as emotionally and socially destructive. The costs in terms of lives lost as a result of rural revolutions in the USSR and China were enormous. The later consolidation of collective farms, state farms and people's communes may have secured relatively high living standards for the rural population and more security than generally found in non-socialist countries at comparable levels of economic development (Hann et al 2003). But this cannot blind us to the traumas of the socialist property revolution, nor to the high costs of unscrambling socialist property regimes after 1990.

What, then, can we learn from anthropological work on rural decollectivization? The most fundamental point to emerge from our studies is the importance of history. For example, the degree and style of implementing collectivization varied considerably even within Central and Eastern Europe. This helps us to understand why the entrepreneurial propensity of Hungarian villagers, many of whom had adapted successfully to the economic reforms of 'market socialism' after 1968, was significantly greater than in neighbouring countries, where more orthodox socialist models had been imposed and little room left for individual initiative. If we broaden the comparative framework we can recognize a contrast between Central and Eastern Europe as a whole and the former Soviet world. In the former, the ideas and practices of private property were more or less

well established before the imposition of socialist rule. In Russia, however, the penetration of market mechanisms and private property was more limited (Kingston-Mann 1999); this helps to explain why so few Russian villagers have taken advantage of the possibility of becoming independent *fermery*. China, with its long history of petty capitalism, is in many respects closer to Central Europe than to Russia. Such historical contrasts across the Eurasian landmass can help us to understand the very different paths followed in the era of ‘decollectivization’ – and also to recognize that boundaries we have long naturalized, including the ‘continental’ divide between Europe and Asia, are the highly contingent product of relatively recent developments (Hann 2006).

In short, by starting with ‘remembered history’ in the field, linking this to archival analysis, and joining forces with historians such as Esther Kingston-Mann, anthropologists can contribute much more to an understanding of postsocialist societies than would be possible if they confined their attention to the documentation of behavior in the present. The institution of property is a good example of the need to consider theories of long term change and the relevance of evolutionary theory. Moreover the evidence gathered in situations of complex transformation can shed light on some of the fundamental assumptions of contemporary evolutionist work. For example our researchers have shown that considerations of envy as well as ideals of fairness and practices of reciprocity and mutual aid are all abundantly evident in rural Russia (Heady and Miller 2006). To study this interplay as it unfolds through the strategies of ‘flesh-and-blood’ actors is more demanding than the conduct of experimental games with one’s students, or the replication of such games in the field. Those currently engaged in theorizing the evolutionary significance of cooperation, reciprocity and altruism could learn much from engaging with the actual, historically evolved complexities that form the principal subject matter of the anthropologist.

V

The discipline whose contours I have outlined so briefly in the preceding sections is thus a peculiar hybrid, forever moving between ‘snapshot’ ethnography and world history, occasionally still dabbling in debates about the evolution of the species, while at the same time researching highly localized details of custom with the enthusiasm of amateur *národopisci*. Fieldwork was the chief characteristic of twentieth century social anthropology, and its virtues have been abundantly demonstrated in the contribution that anthropologists have made to studies of postsocialist transformation. But I have argued above that even studies of self-evidently ephemeral moments in world history will be most instructive when they engage at some level with issues of continuity and discontinuity and therefore make some use of historical methods; all subjects of the fieldworker in the present, including of course the ‘presentist’ Roma on whom Skupnik reports in this issue, have their unique human brains, containing memories that shape both their present behaviour and future expectations.

If social anthropology is a hybrid or chameleon discipline, where does it belong in the wider academic division of labour? Modern universities typically have a clear line of demarcation between faculties of social science and faculties of history, while questions pertaining to human brains are dealt with by yet other specialists. The problem is posed with peculiar force in those former socialist countries of Central and Eastern Europe, in which no discipline closely resembling social anthropology was recognized. Rather, the field was dominated by ‘national ethnography’ (Hofer 1968), sometimes known as ethnology, and subsuming subjects such as folklore and material culture. The general focus was on one’s own nation; although much of this scholarship

was concerned with local and regional variations, the ‘native ethnographers’ were also, sometimes consciously, caught up in their respective national movements. Their dominant temporal mode was determined by this political context: the ethnographers’ mission was, above all, to document the culture of the peasants, in the conviction that among them one could find, in unsullied preindustrial settings, the essential traits of the nation. The subject was located in a Faculty of History and, alongside history, training also emphasized philology.

The broad contrasts between *Volkskunde*, nation-centred anthropology, in Eastern Europe, and *Völkerkunde*, comparative enquiries carried out by anthropologists from those Western European states that established overseas empires, have long been recognized (Stocking 1982). However, relatively little attention has been paid to the remarkable persistence of the nation-centred tradition in the era of Marxist-Leninist socialism, in which one might have expected that the ostensible ideology would have prescribed quite different approaches (Hann, Sárkány and Skalník 2005). Some changes and innovations occurred in socialist-era *národopis*, e.g. some ethnographers began to analyze the rural population as a highly stratified group or class; but many preferred to shy away from contemporary studies (even after it became possible politically to conduct research in the socialist countryside) and to continue working with the dominant temporality of their pre-socialist predecessors (Kuti 2005). The discipline was typically located in a Faculty of History, where it had been traditionally, but even for the distant past the engagement with concrete processes of social change was often very limited. The bias was completely different from the synchronic bias which I have noted above as characteristic of twentieth century social anthropology, but from the perspective I wish to defend it was no less impoverishing.

It would have been very surprising if those dissatisfied with the old nation-centered paradigm and attracted by western currents in cultural or social anthropology had not sought to exploit the opportunities of postsocialist reconstruction to establish a new discipline on the local intellectual landscape. Of course it was unrealistic to imagine that the old ethnographers would simply vacate the stage, and besides their national mission was now once again highly relevant. Therefore the natural inclination was to seek new alliances, e.g. with sociology, cultural studies and media studies, and to “struggle” (Skalník 2002) to introduce social anthropology as a new subject on the curriculum. The institutional consequences have been extremely diverse, e.g. within Hungary, which is the country I know best (Hann, forthcoming). After a decade and a half of often chaotic experimentation there is still little sign of stable structures emerging and anthropology’s future is uncertain.

Some of my good friends in countries such as the Czech Republic question my implicit premise of a unified ‘anthropological field’. I am sympathetic to those trying hard to raise the profile of social anthropology as a discipline in its own right (especially when their biography reveals them to have suffered unjustly under socialism); but I cannot support them. Why this lack of solidarity with those who belong to my own professional tribe and wish to expand its territory? As a card-carrying social anthropologist whose first fieldwork projects were based in Hungary and Poland, it now seems to me retrospectively that I was both arrogant and naïve in supposing that I could break new ground by carrying out long-term fieldwork in the Malinowskian mode in eastern European villages. I assumed (and later taught students in Cambridge) that the ‘anthropology of Europe’ began in the late 1940s, when Evans-Pritchard allowed Julian Pitt Rivers and Paul Stirling to study rural communities in Andalusia and Anatolia respectively. The works of these scholars (Pitt-Rivers 1954 and Stirling 1965) and comparisons with other parts of the world were certainly important for

my investigations of ‘the transformation of peasantry’. But in retrospect I feel I should have invested much more time than I did in evaluating the results of the contributions of the ‘national ethnographers’. My study was by no means at the synchronic extreme: it reached back into the first decades of socialism and even earlier, but in a very limited way. Recent work on the same region of the Great Hungarian Plain by the ethnographer Antal Juhász (1997) has opened my eyes to the richness of the historical data that I might have brought to bear on my project.

I used to justify my focus on the present with the argument that the Hungarian *néprajzosok* were not politically free to document those aspects of social transformation that interested me most, e.g. concerning the functioning of cooperatives and ‘democratic centralism’ in the political sphere. This defence is specious in the free conditions prevailing today but even in the 1970s it was rather disingenuous. At any rate in the case of Hungary, many ‘national ethnographers’ were by this time themselves actively researching social change (admittedly not through extended individual fieldwork in the manner of Malinowski) following the imposition of collectivization (Bodrogi 1978). Mihály Sárkány was one of the key participants in this work, and he has recently headed a major restudy of the same village in Northern Hungary, in order to assess the impact of postsocialist changes. We have found it fascinating to compare and contrast our data from different parts of the countryside (Hann and Sárkány 2005). At the same time, Sárkány teaches courses in Budapest on Africa, where he has fieldwork experience, on economic anthropology and on theory and method in social anthropology. He is, admittedly, an exceptional figure among Hungarian *néprajzosok*. Yet he has spent his entire career promoting wider social anthropological agendas *within* the institutional framework of *néprajz*. So long as this is possible, in other words so long as multiple spatial and temporal frameworks can be adopted within the established discipline, I can see no justification for importing a ‘new’ subject from the West.⁴

As far as I can judge from the literature which comes my way, there is increasing common ground between the social anthropologists and the *néprajzosok* or *néprajzok*, in terms of the subjects they address and the methods they employ. Certainly the social anthropologists offer more than synchronic snapshots, while the national ethnographers are no longer so fixated on their ‘traditional peasantry’. Greater temporal promiscuity in both camps might be mutually beneficial. But to the extent that the evolved professional identities should persist as distinct intellectual communities, I suggest that the ethnographers and socio-cultural anthropologists should form their own clusters within a single department or institute. Those working on questions of contemporary transformation should not be afraid to present their results to seminars dominated by an ‘old guard’. Synergies would be further enhanced if the latter could also be persuaded to attend talks by those conducting research abroad, and if foreign researchers were invited to present the results of their studies on one’s own people. I do not underestimate the difficulties involved. I recognize that some of those investigating transformation will continue to look to sociologists and political scientists for their theoretical inspiration and consider an investment in history and philology to be time wasted. On the other hand I can understand that some representatives of national ethnography fear that their discipline will simply disappear if they do not resist what they perceive to be the juggernaut of *cultural anthropology*. But in the type of department I have in mind sub-groups with a *Volkskunde* orientation would continue to operate, alongside other sub-groups. This is not fantasy: in a number

⁴ I am aware that such ‘duplication’ has been accomplished with some success in certain Scandinavian countries. In a sense the duplication has been present since the nineteenth century in the German-speaking countries, where there is to the present day little mixing of *Ethnologie* (*Völkerkunde*) and the *Volkskunde* stream (nowadays labeled *europäische Ethnologie*, *empirische Kulturwissenschaft* etc.).

of places, e.g. in Poland, old established departments have changed their names by adding ‘and cultural anthropology’, and I believe real progress is being made toward the sort of consortium I have tried to indicate here.

Let me conclude by going one step further. I am arguing that the expertise of the ‘native ethnographer’ on his/her home society, rather than inhibiting the generalizing comparative perspective of more ‘cosmopolitan’ styles of anthropology, can provide a base or bedrock for scholars with those further objectives. A cosmopolitan department of anthropology can benefit from having such strong local roots. From this point of view it is the British student of social anthropology who is in the less fortunate position, when compared with students in the Czech Republic or Hungary. For example, the study of folklore hardly exists as an established academic field in Britain, and the only way to study the preindustrial rural population is to enroll for courses in social and economic history. In short, British anthropology students have little or no opportunity to engage with their own traditions. In my view they would benefit from a plurality of spatial and temporal perspectives, and this should include at least some minimal level of engagement with the social evolution of their own country. This would help to promote a vision of anthropology as a mature synthesis of *Volkskunde* and *Völkerkunde*, neither the celebration of our own people nor an obsession with ‘the other’, but rather a social science distinguishing itself from sociology (not to mention cultural studies, media studies etc.) not only on the basis of its methods but also its uniquely broad temporal and spatial range.

Anthropology and Related Disciplines: the view from Bulgaria

*Milena Benovska*⁵

The social changes that have taken place in the years of postsocialist transformation have been a dramatic challenge to the undramatic life of an academic scholar, particularly when he/she has been a part of it. As in other countries, it has stimulated a resolute reorientation of Bulgarian ethnologists/ethnographers and folklorists from the hermetic and hermeneutic study of own ‘traditions’ towards the analysis of postsocialist everyday life (Ivanova 1998; Elchinova 2007). This reorientation has not been all-encompassing but it has been lending face to efforts at innovative research. My professional development since 1976 in three different academic institutions has given me a relatively comprehensive view of academic life in Bulgaria, allowing the possibility of offering a parallel to Chris Hann’s deliberations. I shall offer a viewpoint to his article confined only to questions connected with my professional competence. Quoted at the beginning of each of the three deliberations are key theses of the article to which my commentary has been addressed.

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1. “*Ethnographic studies of the recent transformations in the former socialist countries are indispensable*”.

In support of this statement I can add a great number of arguments from the point of view of an insider. Insofar, however, as this thesis is not seriously challenged, I shall focus on the question of the study of socialism and its heritage.

2. “*Assessing the significance of socialism in the long-term history of Eurasia*” and considering the suitable methodological tools for the achievement of this goal, it is noteworthy that Chris Hann, one of the pioneers of the anthropology of socialism during that period itself (Hann 1980; Hann 1985), now puts forward its study in connection with and depending on the anthropological investigation of postsocialism. My own interest in socialism came into being in the wake of my work on a book dedicated to the postsocialist transformation (Benovska-Sabkova 2001). Viewed from a specific Bulgarian perspective, the anthropological study of socialism could, however, have an importance of its own, prompted by the agenda of society today. Bulgarian society has been deeply split in the assessment of that age. Large groups of ‘losers’ of the ‘neo-liberal transition’, are in an irreconcilable conflict with another group of people, which is no less numerous, that have endured suffering or loss under socialism or who appraise their postsocialist present as successful. The painstaking and unbiased scrutiny of the socialist past, armed with specific anthropological methods, could give grounds for a better understanding of the epoch, and, hence – for a convergence to a common view about it.

Obviously, the investigation of socialism at the end of the second decade after the collapse of the Berlin Wall could not apply the same methods used by anthropologists while studying the system ‘live’. In his article, Chris Hann suggests that *the memory* of socialism be studied, with all possible precautions as to the deformations to which it has been subjected. The following are the basic methods suggested: fieldwork in the form of autobiographical narratives combined with the study of archival sources. This combination of methods has been applied in Bulgaria since the 1990s, not only in connection with the ethnological study of the legacy of socialism (Benovska-Sabkova 1995 Petrov 1998; Dobрева 1997), but also in the wider process of revising methods in other social sciences and in the humanities: history, sociology, cultural studies (see for example Koleva 2000). The interest in these methodological instruments has been associated with the impact of ‘historical anthropology’, which infiltrated Bulgaria (as well as Serbia/former Yugoslavia), mostly via Austria, initially by way of authors like Michael Mitterauer, Karl Kaser and later Gert Dressel. Worth mentioning as an illustration is the eloquent formula *Between the Archives and the Field*, which has served as the title of the published results of a Serbian-Austrian project in the field of historical anthropology (Jovanovic, Kaser and Naumovic 1999). A special role has been played by the Bulgarian journal *Balkanistic Forum*, which during the past 15 years has been popularising the works of these Austrian authors and the writings of their fellows from different Balkan countries.

As Chris Hann mentions, information drawn from autobiographies often differs from that acquired in archive studies. My own experience has borne this out. This, however, is not a setback for the method. What has to be anticipated from the comparison of the archive data with those, collected from autobiographies is complementarity rather than supplementarity. If the two are compared as two different types of narratives, the scholar is bound to note the different intentions

of their authors. The state archives are a state institution and it is one of their functions to exercise power through the knowledge preserved in them. This, however, makes it possible to mutually enrich and verify memory expressed in oral narratives and the knowledge hidden in the dusty archive depositories. It is not insignificant, however, what type of archive sources are being studied. Sources that inform on the actions of the central authority under socialism are no doubt necessary. They are, however, of priority interest to historians, and, at the same time, are insufficient when we take an interest in the everyday life of the people. Personal archives, those of enterprises or agricultural cooperatives (where informants have worked), or a non-standard source like ‘citizens’ complaints’ (hard to access and in a poor state in Bulgaria), all present unsuspected viewpoints on the events of ‘lived history’. Written autobiographies and memoirs (published and unpublished) reflecting the age of socialism are no doubt also valuable sources, notwithstanding the fact that Bulgaria is not among the countries in which the writing of autobiographies is a mass or established tradition. Thereby it becomes possible to come closer to a certain definition of the study of the everyday culture as “history from below” (Maus 1946: 351-358).

This is not, actually, ‘a nod to history’, because history in its classical form usually has little interest in the petty concerns of ‘little people’. What has been learned from the written sources adds further details to the knowledge invested in the autobiographical narratives as well as contributing to redressing the indubitable subjectivity of the latter. The better understanding of processes under socialism also contributes to the better orientation in postsocialist complexities. In the final count, this intellectual effort takes us closer to penetrating into the intricate intertwinings and coexistence of drastic social changes and long-term continuity.

3. *“I think that it is generally a mistake to attempt to create a separate discipline called social anthropology [in Central and Eastern Europe – M.B.], as a rival and competitor to the established intellectual communities. A genuinely comparative and cosmopolitan anthropology department would be able to integrate colleagues working on contemporary transformations with those specialized in other periods of history, and the integration should be mutually beneficial. [...] I can see no justification for importing a ‘new’ subject from the West. [...] I am arguing that the expertise of the ‘native ethnographer on his/her home society, rather than inhibiting the generalizing perspective of more ‘cosmopolitan’ styles, can provide a base or bedrock for scholars with those further objectives. A cosmopolitan department of anthropology can benefit from having such strong local roots”.*

Unlike in the Czech Republic, Hungary and Poland (Hann, Sárkány and Skalník 2005), no tradition developed in Bulgaria of investigating ‘exotic cultures’ in the period prior to the establishment of socialism. Probably precisely for this reason, the quoted hypothesis of Chris Hann comes considerably closer to the existing picture in Bulgaria. By the way, the difference between the above-mentioned countries and Russia has also to be pointed out. The pre-revolutionary paradigm of studying ‘exotic’ societies (including regions, far away from the multinational Russian Empire) was inherited and continued by Soviet academic practice. That is why the almost automatic change in the names of ethnographic institutes and departments soon after the disintegration of the Soviet Union in the early 1990s and their renaming in institutes of “Anthropology and Ethnology” was justified and corresponded to academic practice.

Going back to the Bulgaria of today, I must say that some of my colleagues are convinced of the usefulness of the introduction of anthropology as *an independent* discipline without fearing any possible accusations of wanting to imitate Western models. Furthermore, these colleagues are frankly attached to the paradigm of anthropology. This has existed for several years now in two Bulgarian universities: an MA programme in “Anthropology” (St Kliment Ohridski University of Sofia); and an independent “Anthropology” Department (New Bulgarian University). And yet, the supposed compromise (or hybrid) of Professor Hann’s between different intellectual traditions exists for various reasons. As it has also been surmised in the article, for the time being the available capacity of these new structures is insufficient to outline the disciplinary boundaries between social and cultural anthropology. Moreover, this is hardly necessary for the time being. In short, the overwhelming majority of scholars have been brought up in the traditions of ethnocentric *ethnography, folklore studies, or ethnology*, and their wish, alone, for reorientation (when it is there) is not always sufficient. These academic institutions do not have at their disposal the resources required for the development of ‘*a cosmopolitan*’ kind of anthropology. The independent (i.e. Bulgarian) financing of fieldwork in more or less distant countries is insufficient.

The academic context, however, is even more contradictory. Noteworthy are also two other factors outside the analysis of Professor Hann. First, the specialty “Anthropology” (in the New Bulgarian University) attracts a greater number of students than the established “Ethnology” at the St Kliment Ohridski University of Sofia. Probably “Anthropology” also attracts students because it is conceived as a possibility for academic qualification new to Bulgaria. Second, the role of the ambitious young people should also be pointed out, who are coming back to their country with degrees in social and/or cultural anthropology received from universities in the USA, Great Britain or other countries. They are at the beginning of their careers and for the time being they cannot exert a decisive influence on the development of the discipline. They are working with the awareness of a mission: the reaffirmation of anthropology as an independent discipline. It is hard to forecast in how far their efforts will be successful in the future. Their motives, however, are far from the coercion of ‘intellectual imperialism’ (abstract or real) that would enforce academic models ‘foreign to the country’.

At present the coexistence of different views on the development of anthropology and related disciplines is a fact in Bulgaria. It is not hard to surmise, however, that this coexistence is connected with tensions and resembles the lot of spouses trapped in a ruined marriage, because they do not have enough money to divorce. It is hard to forecast the future and I must therefore leave open the conclusion of these humble notes. I am, however, confident that over the short term the development of the discipline(s) in Bulgaria will continue, provided one bids farewell to the self-exotisation and the aesthetisation of one’s own culture, and if what is adopted from present-day social anthropology is above all an adherence to socially responsible research.

Between Ethnology and Anthropology: some former Yugoslav perspectives

*Aleksandar Bošković*⁶

Introduction

It is impossible to overstate the importance of Chris Hann's contributions for the study and understanding of former 'Eastern European' anthropologies/ethnologies. I use these terms with a great deal of apprehension – on the one hand, 'Eastern Europe' (or, less geographically oriented, 'postsocialism') have for a number of years been terms loaded with values and implied hierarchies. ('East' understood as inferior to 'West'; 'postsocialist' countries understood as not quite 'there,' in the 'civilized world' yet...) On the other, in these countries (just like in others, like South Africa between 1949 and 1990), the distinction between ethnology and anthropology is a very important one – depicting not only different methodologies, but also important political, social, ideological and cultural implications. Most of all, in the former communist-ruled countries, 'ethnology' was perceived primarily as a 'national science' – a science that was supposed to dig deep into the 'soul of the people' and contribute to understanding of 'us' as so superior to 'them'.

In this brief paper, I will argue that this region actually presents an intriguing opportunity for a specific 'anthropology of anthropology', taking lead from some of Hann's arguments, but also using examples from Croatia, Serbia and Slovenia.

The Problem of Perspective

I fully agree with Hann when he points to the dangers of using "a 'presentist' perspective." This is fully in accord with a hierarchical worldview present in so much of the scholarly research on 'post-communist' societies. As there are no more 'primitives' today (in a sense, we are all indigenous now), the best way to study these 'others' is to assume that they live in a never changing world – similar to the evolutionist perspective of anthropologists at the end of the 19th century. This perspective creates problems in mutual understanding and communication that culminate in 'Western' anthropologists' refusal to consult their 'Eastern' counterparts while conducting fieldwork in countries like Bulgaria or Romania, with the pretext (a very serious accusation, actually) that 'they' collaborate with the state institutions and are as such 'untrustworthy.' It also seems a bit out of date, given Stuchlík's (1976) questioning of anthropological assumptions related to knowledge and understanding.

More interestingly, it could also lead to debates, like the one between Buchowski (2004) and Hann (2005a). I felt strange when reading this exchange, as I could relate to the arguments of both scholars, although I do feel that (whether one agrees with him or not) Hann is probably not the best scholar to be singled out for the "patterns of anthropological production" criticized by Buchowski. On the other hand, over the years, I could also witness how any attempt to introduce critical methodology in a particular (in my case, Serbian) ethnological/anthropological research, or to emphasize the importance of basics such as conducting fieldwork, could lead to accusation that I was attempting to introduce "a colonizing [Western] discourse."

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Comparative Approach

When Hann writes that

“little attention has been paid to the remarkable persistence of the nation-centred tradition in the era of Marxist-Leninist socialism, in which one might have expected that the ostensible ideology would have prescribed quite different approaches (Hann, Sárkány, Skalník 2005). Some (...) ethnographers began to analyze the rural population as a highly stratified group or class; but many preferred to shy away from contemporary studies (even after it became possible politically to conduct research in the socialist countryside) and to continue working with the dominant temporality of their pre-socialist predecessors (Kuti 2005),”

noting that this led to a bias in research, I should also note that this bias was and is difficult to overcome, given that scholars from outside ‘Eastern Europe’ show remarkable lack of proficiency in native languages. This could lead to a specific backlash, exemplified in the idea that “it takes one to know one” – as I noted elsewhere (Bošković 2005a), with regard to Serbian ethnologists extreme dislike of the work by Van der Port (1998, 1999) – with whom they refuse to argue!⁷ So, the response to the perceived arrogance of the ‘outsiders’ is stubborn insistence of the ‘natives’ that they alone can comment and understand their ‘own’ cultures.

But the ‘national’ versus ‘comparative’ attitude is also of considerable importance when regarding methodologies. As noted above, the newly established disciplines of (usually cultural) anthropology in former ‘Eastern Europe’ notoriously lack any *comparative* perspective (Bošković 2005b) – crucial for the development of social and cultural anthropology elsewhere (Holý 1987). While I agree that ethnology and anthropology are *closely related*, this makes it quite difficult for me to accept that they are *the same*. I will never understand how people could simply wake up one morning (usually, in 1990), and decide that they have become anthropologists – without ever studying any form of anthropology, and in many cases in former Yugoslavia, blissfully unaware of any theoretical developments since early 1960s.

This does not mean that anthropologists should not develop close links with ‘local’ ethnographers – the fact of the matter is that fieldwork of exceptional quality was conducted in many communist countries between 1940s and 1990 – but cooperation should be based on complementarity and mutual understanding, not on some strange ‘blurring of genres.’

Historicizing Anthropology

Finally, I am in full agreement with Hann’s view that too little attention has been paid to the issues of ‘temporality.’ On the other hand, ethnology departments within the former Yugoslavia were established usually within Faculties of Philosophy. I will just briefly note here some examples related to Slovenia and Croatia.

Although the teaching of *ethnology with ethnography* began at the University of Ljubljana (Slovenia) in 1919, the Department was established only in 1940, with Niko Županič (1876-1961) as the first Professor and Chair.⁸ Because of the Second World War, the first academic year was never completed.

⁷ I have to note that I regard his work as really excellent, so it is a pity that it did not generate any reactions in Serbia.

⁸ Županič also taught in Belgrade and Zagreb, so the developments of ethnology in Slovenia cannot be really appreciated without a somewhat broader (‘Yugoslav’) perspective. This goes for Croatian and Serbian traditions as well – but they also need to be put in the context of wider developments in Central Europe in the second half of the 19th and the beginning of the 20th century.

In Croatia, Aleksandar Gahs (1891-1962) started teaching *general and religious ethnology* at the Faculty of Theology in 1923, as part of the Program in Comparative Science of Religions. He taught this subject until 1960, and established himself as an expert in Siberian studies at the time. At the University of Zagreb's Faculty of Philosophy, the Chair and Program in Ethnology was established in 1924, but it had no significant developments until 1927. Then it really 'took off,' thanks to the work and energy of Milovan Gavazzi (1895-1992). Gavazzi, who was trained in Slavic ethnology by Lubomir Niederle, but also very much influenced by the evolutionism of Fritz Gräbner and culturalism of Alfred Kroeber, established the course that included not only studying 'folk customs', but also history of ethnology, Slavic traditions, and non-European cultures, but also new methods, such as ethnographic film. However, the 'proper' University Department was founded within the Faculty of Philosophy only in 1960.

After the liberation and re-unification of the country, Yugoslavia was re-built, and this process slowly trickled into the social sciences and humanities. New (communist) authorities felt necessary to emphasize the study of 'the people' (*das Volk*), and a good example is establishment of the Institute for Folk Art in Zagreb in 1948. Among the early interests of the researchers at this Institute was ethnomusicology, but primarily gathering and documentation of the "Croatian traditional culture" (Rihtman-Auguštin, Muraj 1998). When it comes to institutional developments within the University, it is interesting to note a quite original position taken by Gavazzi, who immediately after the Second World War tried to establish something that we could today call 'cultural studies.' However, he was unsuccessful in this (and perhaps too far ahead of his time), pushing instead for the study of 'traditional customs.' One of his students, Rihtman-Auguštin (1926-2002), noted that Gavazzi was extremely conservative in his lecturing and supervision: even though he was very well informed about new theoretical and methodological developments, he simply chose to ignore them (2001).

In 1936, Gavazzi was joined by Branimir Bratanić (1910-1986), who was interested in history and theory of ethnology, as well as in agricultural societies of the Old World. Bratanić was also very much interested in studying and creating maps, and later was one of the co-founders of the journal *Ethnologia Europea*. The two of them will dominate all aspects of Croatian ethnology until the early 1980s.

The main focus of the program in Slovenia was 'ethnogenesis' and ancient history of Slavic peoples. Other subjects (like non-European cultures) were taught by Gavazzi (as a Visiting Professor), and physical anthropology by Škerlj. In 1955, Vilko Novak (1909-2003) arrived at the Department, and it was under his influence that the studies of ethnology in Ljubljana gradually began to move from folklore towards 'anthropological sciences.'

However, the discipline also began to develop in different places at the University of Ljubljana. Božo Škerlj (1904-1961) was teaching social anthropology at the Faculty of Philosophy's Department of Sociology from the early 1950s. Another self-taught anthropologist, Stane Južnič, began teaching social and cultural anthropology at the Faculty of Social Sciences a decade later, and one of his first students, Vesna Godina, started working in a Postgraduate Program in Anthropology in 1983 (Godina 2002: 15). Among other things, Južnič was a proponent of the 'four-field approach' in social and cultural anthropology, making this program quite unique even beyond the former Yugoslavia. The different terminology (ethnology/ethnography, social anthropology, cultural anthropology) clearly indicated different territories, so that there should be no competition for (scarce) resources.

Concluding Remarks

It is impossible to predict directions of future developments in the region. I support Hann's call to establishment of cosmopolitan perspectives as the key for future developments, but my experience of research and (more recently) work does not provide many grounds for optimism. When it comes to the countries which formed Yugoslavia, competition for resources, uncertainties about the countries' future developments, the uncertainties of upcoming younger scholars (as the already established ones will not give up their academic positions), and internal infighting all go to make for interesting observation.

Some Lessons from the Importance of History in the History of Central European Ethnology

*Michał Buchowski*⁹

Chris Hann, as he so often does, provides insights into several interesting issues related to anthropology and postsocialism (although he seems increasingly reluctant to use the term). He is famous for his commitment both to fieldwork that has been done and should be done in the region as well as an interest in local anthropology. The article is mostly about the latter and in my reading, its main idea can be rendered as follows: History is an important, although in the annals of the discipline often neglected, part of anthropological studies; in Central Europe there is a long thread of historically inclined research that can, after refinement, be utilized in future studies. I cannot agree more.

Let us start with history then. Chris Hann writes that our ethnographic findings should be placed in a wider historical context. Sometimes the anthropologist should 'even' visit archives in order to find data complementary to their fieldwork materials. Also, 'personal memory' can be used as a sort of historical source. British anthropology was not particularly good in this respect, but still, one can find several scholars who incorporated history within anthropological research or, in other words, who were somehow historically minded. Also, Malinowski, the inventor of synchronic studies, at the very end of his career became aware that change has to be integrated by anthropologists. Step by step (e.g., Schapera, Firth, Leach, Cohn, and Davis), progress was made in this respect and more recently we can find studies saturated with historical accounts, such as those written by Jean and John Comaroff (US scholars) and Nicholas Thomas. The story in itself is interesting, but a bit surprisingly there is not a single mention of Marshall Sahlins, the pioneer of historically informed anthropology. I do not actually have in mind his earlier works from the 1950s and 1960s written in the neo-evolutionist and Marxist paradigm, but a series of books on Polynesia that started with *Historical Metaphors and Mythical Realities* (1981), continued with *Islands of History* (1985), followed by *How 'Natives' Think? About Captain Cook, For Example* (1995) and crowned with his latest book entitled *Apologies to Thucydides* (2004). I personally consider these works landmarks in historically minded anthropology, but, of course, Chris Hann can have a

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different opinion and in a short outline he is by no means obliged to mention all attempts at bringing historical perspectives into anthropological sight.

However, I do not bring up Sahlins in order to show erudition. I realize that Sahlins' approach might also be seen as cultural determinism and proving that people not only have cultures and histories, but are also slaves to them. Nevertheless, his theory can help us to solve some puzzles Chris Hann finds in postsocialist studies, namely the question of continuity in change (or change in continuity) and ruptures in historical development. All the masterpieces authored by Sahlins and cited above show the intricate relations between structure of history and human agency, that when viewed from this perspective allow a reasonable interpretation of social and cultural changes that occur in different contexts. Captain Cook's visit to the Hawaii in 1779 turned out to be a 'mytho-practical' event with long-reaching consequences. Since he perfectly fitted the image and the timetable of the yearly visit of the Hawaiian god Lono, he was welcomed as such. This unprecedented reenactment of the myth incited an avalanche of transformations that led to the structural repositioning of different groups of social actors in Hawaiian society. This 'structure of conjuncture' caused tremendous alteration of the system in which both continuity and change can be explained in their cultural context and portrayed as they had unfolded in history. There is no room to give a full account of Sahlins' argument, but I think that he is not only a modern precursor of historically minded studies in Anglo-Saxon anthropology, but his ideas can also be employed in our interpretations of postsocialist changes in various regions.

This leads us to the specificity of the postsocialist transformation. 'The flux of postsocialist societies', writes Hann, 'is merely a heightened form of the continuous process of change that can be found in all forms of society.' I have the same opinion, and I would like merely to add that by referring to the concept of 'postsocialist transition' we have in mind a specific set of changes that had been spurred by the 'Fall of Nations' in 1989 and carried under the banners of market economy and democratization. Culturally particular, complex, spatially and socially diversified, historically contingent – nonetheless alterations that are an integral part of the lives of 'flesh and blood' people in 'Eurasia', to employ terms so favored by Chris Hann. Change inherently entails an historical factor, and anthropologists should mobilize all available methods to include them in our considerations, like working in the archives, evoking memories of the people involved, and interrogating the nostalgia some exhibit for the (socialist) past. This kind of history-sensitive anthropology helps us to see how structure and history are intertwined in a historical process; how 'objective', imposed from above (if 'the above' and 'the bottom' can be distinguished at all) arrangements are actually digested and received by various groups of social actors; how this 'structure of conjuncture' reshapes social relations and hierarchies.

Therefore, I also share the view that 'there is no conceivable way of taking the 'transition' societies outside of time'. However, for at least three related reasons I am reluctant to engage in the discussion on 'differential rates of change'. '[S]ome things change more slowly than others', writes Hann, 'more precisely... norms, values, mentalities etc., have a force capable of defying the intended logic of legislative or economic changes'. First, this kind of stance assumes that change is teleological and it is only a matter of time before, for example, 'egalitarian values' will adjust to the pace of economic changes and neo-liberal ideals. Since economic reforms and political re-organizations are inflicted by global forces (international capital and invincible western democracies) in concert with local political authorities that subscribe to these forces, changes in the infrastructure are already well advanced and the superstructure, despite the inertia of cultural

values, will have, sooner or later, to catch up. Although Karl Marx might have thought so, I am less convinced. This kind of Marxist historical materialism leaves no room for the actual dialectical dynamics of history that materialize at the interface of structural factors (not merely a sum, but a product of global forces) and actors' interests. In other words, this issue should not be posed in terms of 'a dissonance between the blueprints favored by policymakers... and the intractable realities of postsocialist communities'. My second reason for taking issue with the idea of 'differential rates of change', itself a derivative of the first one, is that this stand inevitably presupposes an umpire who 'referees' paces of change in various walks of life. My question would be – who is entitled to undertake this role? International agencies gauging the advancement of the very reforms they have prescribed? I hope anthropologists will not join this self-referential circle. Third, this kind of reasoning easily slips into a domain of 'mentalities' ('socialist bad work habits', 'Russian soul', 'learned helplessness', etc.). In this model, culture and mentalities, terms never precisely specified by those who used them in their argumentation, are cited as obstacles to transformation – change would happen if only peoples' minds and routines could be altered. These arguments are very often skillfully manipulated by the advocates of a 'new deal' in their strategy of blaming the people (victims) for all failures on the road to eternal prosperity and happiness (cf. Kideckel 2001; 2002; Buchowski 2006).

Let us move to another important point of my argument. I understand Chris Hann's unease about the fact that history was disregarded for so long in British social anthropology. However, for Central European ethnologists, history has always been the air they breathed, a *nihili novi sub sole*. He rightly detects that "we, Central Europeans" were sensitive to history, and in university structures across the region, virtually all anthropological departments are located in Faculties of Historical Studies (not faculties of history!). I can only confirm, by referring to the Polish tradition, that history was an inherent part of ethnographic studies, both in the study curricula and research agenda. History was a natural part of ethnology both for scholars in the interwar period (e.g., Stefan Czarnowski, Stanisław Poniatowski, Jan Stanisław Bystróż, Kazimierz Moszyński) and in the postwar socialist period (e.g., Kazimierz Dobrowolski, Józef Burszta [1985]).

Let me offer some illustration in order to substantiate the above statement. Czarnowski (1956) considered history a type of verbal, schematic or symbolic representation of events, things, people and their acts. This representation then integrates the various experiences accumulated by generations of human groups and expresses their main values. He also wrote that the past weighs on the present, and all forms of life have their roots in the past. The past is never static, and the present always reshapes the past, selecting and assimilating appropriate ingredients. Dobrowolski (1967) developed a theory of historical setting (*podłoże historyczne*). It entails the wholeness of cultural artifacts inherited from previous generations. Historical setting consists of biological, geographical and cultural backgrounds. An important part of this setting is an historical consciousness that is a correlate of social structure and culture. It is composed by the memory of the past, the content of historical knowledge appropriate to a given social class, and an assessment of one's own past. Recollection of the past is transmitted in an oral form from generation to generation, whereas historians purposefully strive to build historical narration. Does it not all sound familiar? I see many similarities between these two Polish scholars' concepts and Hann's postulates.

The power of historically-oriented ethnography explains why Bronisław Malinowski did not have the highest currency in the Polish ethnological community. This fact surprised British

anthropologists when they began to have more extensive contact with Polish scholars in the first half of the 1980s, as they clearly expected Polish researchers were somehow genetically endowed to be anti-diachronic functionalists, but nothing could have been further from the truth. However, for western scholars it was inconceivable that they could learn anything about doing anthropology from ‘backward’ socialist academics. It seems that this image continues to thrive. Even Chris Hann, as I already mentioned an ardent promoter of ‘postsocialist’ scholars and scholarship, barely mentions them in his references cited in the article discussed.

Moreover, ‘easterners’ were perceived as practicing only ‘national ethnography’, and this opinion about Eastern European ethnologists continues to prevail today. An excerpt from Adam Kuper is indicative: “Scholars in Eastern European countries tended to share a traditional, nationalist preoccupation with peasant traditions, and their work had little theoretical content or comparative range” (1996: 192). Elsewhere, I have tried to show (Buchowski 2000; 2004; 2005) that this was definitely not the case with Polish ethnology. For example, Lévi-Strauss’s books (*Tristes Tropiques*, *Anthropologie Structurale*, *Le Totémisme Aujourd’hui* and *La Pensée Sauvage*) were translated into Polish by 1970. These and other translations left their imprint on the way ethnology was conceptualized and practiced by local scholars. At the same time, many extra-European studies in Central Asia, Africa and Latin America were carried out and books on these topics also published. These efforts can hardly be classified as ‘nationalist’. The other related, although equally skewed, representation is that Central European ethnologists were solely studying peasant communities (see both Kuper cited above and Hann 2007). In addition to the aforementioned studies on extra-European societies, several works on workers’ culture in industrial centers and on small-towns communities, as well as the so-called mass or popular culture, were carried out. And these remarks do not apply to Poland only, but are equally valid to at least some of the former communist countries. Paradoxically, from the perspective of some native ethnologists, it is actually those Western anthropologists who did their fieldwork in Central and Eastern Europe who appear to be fixated on peasants (cf. Prica 2004), and not the other way round. Apparently, fixation is in the eye of the beholder.

The idea of a synergy of *Volkskundler* and *Völkerkundler* perfectly fits my image of anthropology and is standard practice in countries such as Poland, Russia and Slovenia. It is a tradition that has lasted for decades. It turned out that it was not that difficult to reconcile ‘anthropologists’ and ‘ethnologists’ in the same departments since in many cases they were reading the same books, many among them ‘western’, and addressed similar issues, even if they remained attached their distinct intellectual traditions. The relatively small size of the anthropological/ethnological communities in the countries concerned was also a factor that helped this course of action to take place. Internal divisions into anthropologists (‘us’) and ethnologists (‘them’), although sometimes healthy for intellectual discussions, do not make real sense when reified in an administrative structure whose importance is then systematically overemphasized. Bearing in mind that ethnology departments existed for several decades across the region (‘mine’ will be ninety years old soon), statements like ‘We are the oldest social anthropological department in Eastern Europe, established in the 1980s’ sounds ridiculous. Such statements are meant to create an illusionary hierarchy, but do not translate to the state of the art on the ground. Luckily, such attempts at making exclusions are perceived by those familiar with the issue as ludicrous.

No doubt Chris Hann’s appeal to reinforce both ethnography and history in ethno-anthropological work is a recommendation that all of us should take seriously. In this endeavour, local ethnological

traditions cannot be ignored and, indeed, should be reinterpreted, developed and fruitfully employed in our efforts to advance anthropological studies everywhere. These are the lessons from the Central European ethnological tradition that should not be forgotten.

The Path to the Post-Colony: why folklore won't save us

*Don Kalb*¹⁰

Chris Hann's reflections on anthropology in CEE deserve a frank response. Chris argues that it was misleading to think, as he once did, that the CEE region needed independent institutions for social anthropology like 'the West'. He gives two reasons why that was a mistake. First, anthropology as *Völkerkunde* (as against *Volkskunde*) is an export from the dominant and ex-colonialist West. Such academic imperialism is not only discredited these days, it is also perverse in that it will set in motion institutional fights with established fields in CEE that will leave little room for a newcomer discipline to flourish. Secondly, the anthropology of postsocialism (as much of anthropology in general, he seems to imply) has been suffering from an important shortcoming: a lack of systematic attention to longer-run histories. For these reasons he now advocates for anthropologists in CEE an alliance with local institutions of folklore. This could provide social anthropology with the necessary local alliances while it would simultaneously help to liberate its repressed historical perspective. The end result would be better than most curricula in the West, he suggests. Anthropology students in the UK or the US still learn very little about their own local histories, making it hard for them to develop balanced yardsticks for comparison.

I studied anthropology with Anton Blok (*The Mafia of a Sicilian Village* and other anthropology and history work) in Nijmegen, the Netherlands, in the late seventies/early eighties. We thought we studied 'Historical Anthropology' and the 'Anthropology of Europe' (as well as other anthropologies), and read E. P. Thompson and Barrington Moore as well as Eric Wolf. I did fieldwork 'at home' for my dissertation on regimes of industrial modernity (Kalb 1997) and I worked with both oral history and archival materials to cover a time period of a short century, 1850-1950. I was also involved with debates on anthropology, history, and method (Kalb, Tak 2005), and I am a founding editor of a journal dedicated to the conjunction of anthropology and history (*Focaal – European Journal of Anthropology*). Clearly, how could I be unsympathetic to the general import of Chris's main points?

These days I also meet erudite East European folklorists who tell me about French structuralism and Italian micro history, do excellent research, both historical and contemporary, and don't resemble the proverbial antiquarian or nationalist freaks sometimes associated with folklore at all. Alliances with such people in my eyes are the most natural thing to do. Indeed, most

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anthropologies on the European continent and in Scandinavia have actually evolved within such coalitions with folklore, ethnology etc.

However, when we look more closely at Chris Hann's argument, his conclusion that social anthropologists in CEE should expressly seek a coalition with folklore becomes far less evident. He starts with the fieldwork-dominance within modern anthropology. Fieldwork in the Malinowskian mode allowed for rigorous empirical research, but it also served to sever the earlier close link in the discipline with the evolutionary history of mankind, exemplified in Chris's reference to Frazer. In his view, this has subsequently led to two serious weaknesses in current anthropology, including the anthropology of postsocialism: 1) a heavy emphasis on contemporary change at the cost of understanding longer continuities; 2) little awareness of how micro-studies fit in with wider regional histories and cosmopolitan theory.

Now, clearly, an alliance with folklore may help to redress the first shortcoming (though alliances with historical departments or with historical sociology would do so too) but will not be of much help in repairing the second one. I know few informed people who would argue that folklore studies are meant to advance the goal of a cosmopolitan and comparative science of humankind, whatever qualities they might otherwise attribute to it.¹¹ Folklore, obviously, does not help us much to (re)embed anthropology in a cosmopolitan science of human history. This crucially important issue for the future of anthropology therefore disappears through the backdoor shortly after its introduction in the text – though I know Chris is serious about it.

Why start with Frazer and not with Eric Wolf or Marshall Sahlins if you want to emphasize the importance of articulating local and global histories for anthropology? Where Frazer poses a problem, Wolf and Sahlins – perhaps the two most important anthropologists of the post-war generation – offer still highly relevant advice. Incidentally, a key anthropologist of (post)-socialism, Katherine Verdery, named her former Chair in Michigan after Wolf. Which is to underline that attempts to move beyond the temporal limits of fieldwork-based anthropology, also in postsocialism studies, are decidedly less rare or new in anthropology than Chris induces.

Indeed, such historicizing studies had become a strong and interdisciplinary movement by the late seventies with close links with 'the new social history' in Anglo-Saxon countries (not just in the US, see for example the cooperation between Goody and Thompson, or MacFarlane's work, in the UK; in a sense also Gellner), the *Annales* in France, microhistory in Italy, and everyday life approaches in West Germany.¹² My recent book *Critical Junctions* (Kalb, Tak 2005) revisits the paradigmatic shifts and struggles that underlay this movement. For our present debate it is relevant to note that the anthropology-and-history field served as one of the prime arenas for a clash of paradigms that led, on the one hand, to 'the new cultural history' (see Hunt 1989), an

¹¹ Interestingly, toward the end of his story Chris suddenly adds the human brain to our anthropological research interests (significantly, in connection with memory). I presume that this unexpected move must make up for the lack of leverage that his desired alliance with folklore brings when it comes to redeeming the big questions of history and 'evolution' for anthropology. To me this is a disastrous move but I won't go into it since Chris, too, spends less than five words on it

¹² The Max-Planck-Institut für Geschichte in Göttingen and the Maison de Sciences de l'Homme in Paris devoted regular roundtables to the conjunction of anthropology and history. Several US departments specialized in it, as did some European ones. Journals such as *Comparative Studies in Society and History*, *Critique of Anthropology*, the *Journal of Historical Sociology*, and recently *History and Anthropology*, *Historische Anthropologie*, *Anthropological Theory* and *Focaal* have been closely associated with it. In the mid-eighties anybody discussing developments in US anthropology, even those who would rather defend Geertzian hermeneutics, had to relate explicitly to the anthropology-and-history-issue, witness Sherry Ortner's overview of *Theory in Anthropology since the Sixties* (1994). Even George Marcus in his *Anthropology as Cultural Critique*, while more excited about the possibilities of postmodern cultural essayism and multi-sited research, spent almost half his pages discussing the strengths of historical political economy (Marcus, Fischer 1986), as anthropology-and-history in the US had become generally known by the later eighties.

anthropological history with renewed Geertzian or Douglassian emphasis on the static interpretation of symbols and the evocation of cultural grammars as universes of meaning (Robert Darnton is a good example), and on the other to a more self-conscious historical anthropology that tended to combine a Gluckmannian focus on social practice (See Handelman 2005) with a Wolfian interest in articulating the hidden histories of ‘people without history’ in their agential relationships with overarching regimes of power (for Wolf’s methods see also Schneider, Rapp 1995).

I would suggest that anthropology, including the anthropology of postsocialism, is still confronted with these basic and clashing science programs today. I would also say that alliances with folklore will in practice often seduce anthropology to the cultural idealist side. This is so because folklore is not notoriously strong or interested in the social analysis of power, practice, and process, and tends to isolate its subjects in ‘cultural quarantine’ as old style snap-shot anthropology used to do. This shows especially in how it deals with the idea and the occurrence of ‘survivals’.

For anthropology-and-history to emerge, what was needed was a critique on the basic nineteenth century bi-polarities in Western historical visions. The first critique hit at the synchronic interpretation of cultural systems of meaning, mentalities, and cosmologies that sprang from the romantic reaction against the French revolution and occupation of Europe (“culture versus reason; tradition versus progress”) and came via Herder, Dilthey and Weber, to Boas, Mead and Geertz in American anthropology; and via de Saussure to Levi Strauss and other language-based social theories in France.

The second strand of critique took aim at evolutionist and, later, modernization approaches (twins in ‘scientific’ western superiority feelings). These approaches had become a crucial counterpoint and subtext against which, and with which, twentieth century anthropology had defined itself.

I will focus on this second strand of critique since the first does not need repeating (see Kuper 1999 for a long analysis, or Kalb 2005 for a recent elaboration). The criticism of modernization/evolutionist models went far beyond the rejection of the conjectural and speculative approach of nineteenth century evolutionism that Chris mentions in relation to Frazer. In particular it aimed at two crucial properties of the evolution/modernization paradigm: teleology or unifinalism/unilinearism, and the idea of evolution or modernization as a spontaneous process of increasing social complexity through differentiation and integration (the biological analogy). These issues came ultimately together in the question of power, and indeed both paradigms historically served (and still serve) to mystify and naturalize issues of power and inequality in the world.

Against teleology and the presumed spontaneity of social process in the direction of modernity, historical anthropology looked at the historical and geographic unevenness of its spread, and saw that unevenness as a function of the differential starting points, power resources, orientations, and interests of its core power-wielders, in interaction with the resources, orientations, and interests of subaltern or intermediate groups. It thus brought history, conflict and struggle into the center of the equation and gave it an agonistic and contradictory character instead of a smooth one. Modernity then, in actually situated experience, did never arrive pure or “en bloc” (see Tilly 1984). It was riddled with ‘un-simultaneities’ as it was exported through power, persuasion and exploitation from its social and geographical cores into the subaltern classes and the less developed areas overseas and in the hinterlands.

This led, among others, to a new view of ‘survivals’, and this is what interests us here. Instead as ‘old fashioned habits of the mind and the heart’, they were now seen in two new ways: 1) as

actively sustained by their functionality within the situated material life arrangements of subaltern groups (as functionalist anthropology was specialized in showing) and 2) at the same time as issues and vehicles of actual and active contention between classes and between modernizing states and subjects. In this vein, E.P. Thompson retuned for instance the Polanyian concept of moral economy from a straightforward historical fact into something that workers and artisans actively invented and recreated as they struggled with the rules of the market economy that encroached on their communities. It was through struggle that they discovered and remembered, as it were, their own normative visions and imagined traditions of moral economy. Eric Wolf and Jim Scott did something similar in their work on peasants and peasant protest. The anomalies of modernization were now no longer explained as spiritual residues of earlier times – survivals; backward looking customs and superstitions, expressed for example in luddism – that would inevitably wither away with time, but as contentious issues in the articulation of hidden and dominant histories. Thompson's "customs in common", thus, were the cultural moments of material life arrangements and actual struggle rather than icons of a time lost. They were friction points of uneven and combined development. In this way, these authors also helped to lay the groundwork for what later came to be called post-colonial and subaltern studies. Friction, contradiction, divergence, subalternity, post-coloniality and geographies of power had taken the place of equilibrium, convergence, consensus, and the assumption of homogeneous space coupled to linear time.

While this may be a much too brief and partial discussion of the methodological and theoretical issues at stake, it should be clear that much of folklore studies remains grounded in the old bipolarity of an authentic peasant culture versus modernization. It continues to expand its collection of 'survivals' of the old ways in an effort to dispute the full hegemony of the new. Needless to say, this is a powerless position against the modernizers, leaving the field of interest guided purposive action entirely to them.

There are some problems in the current anthropology of postsocialism that help to draw it closer to this idealist position of folklore. I will discuss these problems by way of looking at Chris Hann's own recent report on Tázlár (Hann, Sárkány 2003). My suggestion is that at critical moments of interpretation Hann and Sárkány sometimes overlook the lessons of historical anthropology and revert to a folkloristic understanding of survivals. Chris's embrace of folklore, thus, does not come as a surprise.

Let me start with emphasizing some of the strengths of this work on the socialist and postsocialist *Agrarfrage*. The strengths lay, to my mind, in a broad vision of the mixed arrangements between collective and private practices/properties in most socialist countries since the sixties, which enabled a partial modernization of once underdeveloped and peripheral agricultures and brought standards of living, including an incipient consumerism, which had not been enjoyed by as many rural people before, nor after.

The emphasis is on *partial* modernization: in a contradictory 'symbiosis' with large-scale and mechanized collective farms a basis developed for what we could perhaps call socialist peasantries. Households drew from collectively/state held assets to develop intensive household farming on small privately owned or used plots. They turned to collective organizations for procuring cheap inputs and marketing some of the outputs. This is un-simultaneity in action: agricultural collectivization and large, relatively capital intensive, outlays under socialist regimes helped to nurture, paradoxically, a peasant mode of life that was richer and better developed than among the dependent peasantries in pre-war Central and Eastern Europe.

I have two consecutive problems of interpretation that highlight the issue of folklore within the anthropology of postsocialism. Here comes the first one, and it is situated in the socialist phase of Hann's and Sárkány's "great transformation". Tázlár's collectivization, writes Hann, had been even more flexible than in most Hungarian areas. Tázlár's peasants had been allowed to keep control over much of the original plots. In the seventies, however, a new collective farm management started to encroach on some of their plots and this led to a sequence of disputes between socialist leaders and village people. Now, my question is, why does Chris describe the peasants' reluctance to give up their holdings as inspired by the "*sentimental* strength of the old ties of property..." which was "*still* an important base of community norms and values..."? (*op. cit.*: 124) Chris, apparently, sees the motivations behind peasant protest as if they are something like survivals in the folkloristic mode: at various points in the text, as well as in the introduction to the book, he sees such preferences as the product of memories of an earlier peasant moral economy, memories to which people are sentimentally attached. Historically correct or not, this is clearly a folkloristic explanation and not one that closely studies the cultural dynamics of popular conflict.

My second critique concerns a similar issue of survivals in the subsequent postsocialist phase. The strengths, again, of the work lay in the empirical demonstration of how the legal fictions of both socialism and liberalism (pure collectivization versus pure privatization of the land) do not cohere with the needs and desires of peasants. And indeed it demonstrates the profoundly perverse effects on agricultural productivity and rural well-being of the fundamentalism of private property and unregulated markets. The problem concerns the way Chris explains this perversity of neo-liberalism. Like the collectivization conflict in the seventies, he suggests it works through the misfit with social norms stemming, somehow, from an earlier moral economy. Community social norms in postsocialist villages, he emphasizes, exhibit complex mixes of private access to land and strong social norms on the rights and duties of collective reproduction. Liberalism promotes the former but destroys the latter.

Again, why do we need the social norms and the memories of an earlier moral economy here? Why is the misfit with something like 'cultural preferences' necessary? To me, private access to land and cultural conservatism seem rather general prerequisites for peasant reproduction worldwide. And indeed, what is so specific about the demise of peasant agriculture under the neo-liberalism of the nineties and early 2000's in Central and Eastern Europe? A voluminous literature illustrates that peasants everywhere in the world since the implementation of neo-liberal reforms – in Latin America, Africa, and Asia – are being pushed off the land and are leaving agriculture as a primary pursuit *en masse*. Postsocialist privatization is certainly not the only story here. Indeed it is a chapter of a much bigger book that is surprisingly ignored in the Max Planck book (see for example Marc Edelman 1999, 2002; Deborah Bryceson et al. 2000; Mike Davis 2006). In this sense, the exclusive focus on property forms and socialization/privatization of property in this work of anthropology, takes the appearance of postsocialist transition for its essence, and overlooks the fact that private property transition was part and parcel of a larger and much more significant package that included trade liberalizations and the prevention or phasing out of price supports, cheap credits, and marketing boards as well, not to speak of other rural development and welfare policies.

The heyday of the peasant everywhere on the globe was the seventies, when all sorts of public and collective arrangements in credits and marketing, and, under socialist regimes, partial collectivizations, allowed small cultivators a certain wealth and stability under national

developmentalist regimes. After IMF-imposed structural adjustment or shock-therapies, or just simple step by step liberalizations by national states unable to sustain positive national budgets in the global rat race, that period has come to a definite close everywhere. There are numerous regional trajectories here, sure, and they are important for systematic comparative research, but the general story shouldn't be lost. Indeed, there is a powerful global logic behind it. Peasants face enormous obstacles in their daily reproduction unless they are willing and able to become part of global agribusiness and the global supply chains or transform their cultivating activities into just one of their sources of income.

Of all of these global developments we hear regrettably little in the prime anthropological collection of postsocialist agriculture. There are dispersed remarks, for example on the competitive pressures on wine production, on adverse market conditions in several products, on the EU "Common Agricultural Policies" which finally allow some sort of stable rural development to take place in the new EU countries – but are themselves one of the main culprits in WTO negotiations while being hollowed out year after year. But there is no comparison with developments in other world-regions, there is little sense of comparable problems in wine-farming elsewhere, there is no clue on which supply chains are coming in and setting what sort of pressures and limits for peasant life, there is even insufficient awareness of the precariousness of the CAP. But this is what we need if we want to understand current developments and the problems for peasant reproduction. There is an excellent research-based literature on most of these global processes, even within anthropology itself (for example see a.o. Harvey 2005; Kalb 2002, 2005, Kalb et al. 2004), but, tellingly, there is only a reference to the journalist Jeremy Seabrook – as if social science has nothing to say on the big things.

The same is true for what Hann and Sárkány call the new cultural dominance of the city over the countryside. The city, of course, is hardly Budapest, Prague, or Vienna, as it was in 1930. It is the global imagery of consumption, distributed by transnational media messages and the stories from traveling and migrating kin or friends. Wherever we look in agriculture, from Mexico to China, young people are reluctant to take over small-scale production from their parents. Agriculture is becoming just one of multiple sources of family income that include remittances; and it is often only sustained by the elderly. Younger people in most settings are lured to metropolitan wealth and consumption and prepare themselves for migratory lives rather than staying local and digging the soil. For some of them the village will remain a social policy and a form of belonging to which they will ultimately return, to others it will become a memory or a family link. Again, here is a general and global process at work that could have been interrogated for its specific dimensions in postsocialist countrysides, but we do not hear much about it.

There are quintessentially anthropological issues involved. While neo-liberal globalization puts relentless pressure on small-scale cultivators, they as well as their children, albeit in different ways, are at the same time enchanted by the neo-liberal promises of consumption, movement and excitement. Ambivalences abound, and it is the volatile everyday politics that springs from such ambiguities between and among parents and children that become expressed in the current swings of religion, nationalism, small-holder activism, self-defence leagues or anti-globalist mobilizations in the countryside. And they do so differently in different regions, depending on their insertion in the global marketization process, and on their local histories and moral economies. They also depend on national states and the ability of national elites to carve out new instruments for agricultural development and respond to demands from below.

Should the fledgling institutions for anthropology in Central and Eastern Europe seek alliances with folklore, as Chris Hann argues? Folklore promises greater attention for local histories – a good thing. But it encourages even less attention to the insertion in and interaction with global histories than postsocialist anthropology itself is inclined to do. This will lead to a failure to address the core questions of local and global social change. I have implied that the existing anthropological emphasis on contemporary social change, *pace* Hann, is all but sufficiently cosmopolitan. In the end this may undermine the public legitimacy of the anthropological pursuit. Anthropology in Central and Eastern Europe, as well as postsocialist anthropology in the West, needs alliances that allow it to live up to its promise as a peculiarly cosmopolitan human science. If folklore helps, why not collaborate? If cultural studies promises a good alliance, such as in Sofia or Lviv, please accept the offer and see where you can get. The combine with sociology, such as at Central European University or in Cluj, is well suited to help re-develop a global and cosmopolitan perspective, certainly if comparative historical sociology is being taught. The bottom line is that anthropology in the region should try to contribute to academic perspectives on situated social change that help to lift the region out of its obsession with its putatively singular postsocialist predicament and out of its singular orientation on a putative West. In that sense, indeed, it should help regional intellectuals and researchers to reach for the post-colonial. That will be a measure of its success.

Social Anthropology in East-Central Europe: intellectual challenge or anachronism?

*Juraj Podoba*¹³

The first time I was confronted with the essential disparities between Anglo-Saxon or Western social (and cultural) anthropology and East-Central European ethnography (*etnografia, národopis, néprajz*) was at the first biennial conference of EASA in Coimbra in 1990. I was there as a young and, befittingly, naive Eastern European ethnographer, along with a group of colleagues, just shortly after the fall of the Iron Curtain. I could not get over my surprise, and the nature of that first encounter was a kind of cultural shock. I unexpectedly found myself face to face with an advanced, modern social science that, while it dealt with a similar subject area (though thematically much broader) as East-Central European ethnography and folklore studies and was also based on ethnographic research, did so in a substantially different way. For me, with my education and background, cultural anthropology at that time represented something in the area of Tylor, Frazer and especially Morgan, the students of Franz Boas, maybe even Bronisław Malinowski, and Claude Lévi-Strauss, who, next to Morgan, was the author I had probably read most, partly because Czech and Polish translations of his work existed, and partly because, owing to the domestic structuralist tradition, in Slovakia represented mainly by Piotr Bogatyriov and his students, Lévi-Strauss was actually the only Western ethnologist that the (Czecho)Slovak community of ethnographers had been more deeply familiar with since the 1960s, at least in the sense that young ethnographers were expected to be capable of discussing his works and structuralism in general, and in doing so display their grasp of this kind of academic literature. What I encountered in Coimbra, however, was

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something quite different from the definition of anthropology derived by Czechoslovak ethnographers from translations of the classic works of its *'founding fathers'* and Wolf's compendium (Wolf 1971), which represented the alpha and omega of their knowledge about this seemingly related social-scientific field.¹⁴ There was thus much at the meeting that I did not understand (and not just owing to the level of my English skills at the time), and I was especially astonished by how the Western academic community presented itself: their self-confidence, the critical discussions, the overwhelming array of topics dealt with in the individual workshops, the presentations at the plenary sessions, often in connection with the appearance of some legend in the field, whom to that time I had only read of, 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. If I remember well, the participants from Eastern Europe at this event were evidently more preoccupied with the aggressive presentations made by Western European feminists at the closing plenary session than they were by the academic programme of the plenary sessions, workshops, and round-table discussions. And that also added to the cultural shock.

However, I experienced the biggest cultural shock later when I spent a longer period of time at the universities in Zürich and Cambridge. There I really became aware not just of how large the gap is between ethnography (*národopis*) on the one hand and ethnology/anthropology on the other, but also of the gulf between the social sciences in Western liberal-democratic social settings and in the regions that had been under the control of communist regimes since the end of the Second World War (with the fascist prelude of the 1930-40s); essentially, the difference between the situation in Western European and in post-communist academic institutions.¹⁵ It was a confrontation between the archaic, pre-scientific, descriptive field of ethnography, with no theory or methodology of its own (if here by scientific theory we mean not ethnic theory, or the occasional, demonstrative, but intellectually quite toothless, launches into structuralism, or sporadic inspiration drawn from theory in the field of the arts), and a modern, theoretically and methodologically elaborated social science that endeavours to reflect on a broad and diverse array of fundamental issues in the sphere of social and cultural development, and to do so in literally a global comparative context.

However, this cultural shock also included some not very positive or pleasant experiences. When in the spring of 1992 I spent a semester lecturing at the University in Zürich to students in the Department of Ethnology, I was quite naively convinced that in the immediate aftermath of the break-up of the totalitarian bloc Western European intellectuals and academics in the fields of the humanities and the social sciences, and even just young educated people in Western Europe, would be very interested in learning about what had been going on behind the Iron Curtain for the past forty to seventy years, what kind of society had emerged out of the social experiment that, under the influence of Western ideological concepts, was forcibly introduced into the semi-feudal agrarian and early industrial Eastern European societies. The very opposite was true. And if anyone showed an interest, it was usually political scientists and geographers, and some historians. The

¹⁴ From the post-war period to date, one of the predominating opinions has been that this is essentially the same field, that ethnography, *Volkskunde*, European ethnology, anthropology are temporal or regional variations of the same discipline.

¹⁵ In some Central and Eastern European regions, like Slovakia, for example, the professionalisation of ethnography occurred under these regimes. In the period prior to that there had only been amateur collectors, researchers from central cultural and academic institutions, representing ideologically rival versions of a 'national science', and foreign researchers (see Podoba 2005, 2006).

most ignorant of this region among scholars to date are found in the anthropological community. Consequently, for me it was a pleasant surprise when during my visiting fellowship at Cambridge University – at the Faculty of Social and Political Sciences, and not among the anthropologists – I discovered the existence of a small group of British anthropologists, mostly students of Ernest Gellner, focusing on the issue of the ‘anthropology of communism/post-communism’. Similarly, however understandable it may be, it was nonetheless not a pleasant experience to realise how little interest Western colleagues showed in the results of the work of several generations of East-Central European ethnographers and folklorists. The second and even more frustrating encounter was with the ideological tenacity of many left-wing academics, including those at the very top, similar in character to a kind of mental block, and their consequent inability to approach the critical study of social reality in the contemporary world dispassionately and, insofar as possible, objectively, solely on the basis of scientific methodology.

This contrast of two very different intellectual traditions led me to reflect on the field’s outlook within the new circumstances of an open society at the end of the 20th century, when even in the late and unequally modernised regions of East-Central Europe, of which Slovakia is a part, folk culture has become only a part of our cultural heritage, its forms and manifestations now institutionalised, and ethnographic ‘salvage’ research has become a permanent anachronism. The ethnographic tradition established by Chotek and Mjartan had been thematically and intellectually exhausted since the 1960-70s; in this situation a focus on social history or social anthropology appeared to be the most logical solution (for more on this topic, see Podoba 1991). However, there was a good deal of ethnographic information and data available, some of it of very good quality, and innovative and inspiring analytical work had been done by some individuals that the ethnographic mainstream consistently ignored or sidelined, or in some cases even ostracised (on this topic, see Podoba 1994, 1998, 2002, 2005, 2006; for the broader context of this issue, see Leščák 1988), which was capable of serving as good-quality, stimulating, and valuable sources of information for anyone in the social sciences studying Central, Southeast, and Eastern Europe, including, of course, Anglo-Saxon social anthropologists. When thinking about this at that time, even I was convinced, like Chris Hann is today, that it makes no sense to establish and institutionalise a new field when, say, at Comenius University in Bratislava and the Slovak Academy of Sciences there are already specialised ethnographic departments that have a long history; which over the course of the first half of the 1990s changed their names from *národopis* or ethnography to ethnology (and during this decade the university department added cultural anthropology to its name). I, too, felt that the solution lay with the gradual, and certainly not simple, easy and rapid modernisation of East-Central European ethnography, which from the 1970s had been slowly changing anyway, and by the start of the 1990s was certainly not the same field as the one defined by its doyens and first generation of students, then mainly women; and that it is necessary not just to modernise it theoretically and methodologically, but also to thematically enrich it in connection with the above-mentioned dual focus on historical or social-scientific study.

However, current reality is such that even though both these alternatives are represented in Slovakia, the Czech Republic, and East-Central Europe to a minority degree in work from the past two decades by what are now ethnologists, the majority of post-communist post-ethnographers have turned their attention in a different direction. The modernisation of ethnography has become a focal interest of just a relatively small number of Slovak post-ethnographers, and even the majority of this minority has mainly been gravitating towards the context of historical disciplines. It seems

that the main post-revolution contribution of Central European ethnography/ethnology is its effective assistance in constructing or reinforcing social history (see Podoba 2003b). The so-called ‘anthropologising of ethnology’, which is sometimes discussed in academic circles, and less so also on the pages of academic journals (e.g. Kiliánová 2002), is more fiction than fact. There is no room in the scope of this paper to analyse this more than decade-long process, but I have published a number of texts on this problem elsewhere (see Podoba 2003a, 2005, *forthcoming*).

The nature of the difference between the two fields, or groups of fields, and even the reality of development in the academic sphere in Central Europe with the emergence of the open society seems to escape Chris Hann (2007). His reflections focus on the issue of research ‘at home’ and ‘abroad’ and on combining or synthesising research – in the sense of the Malinowski tradition of British social anthropology – with the more historically oriented approaches of East-Central European ethnographers. In my opinion this is a very narrow perspective, and perhaps that is why the author fails to grasp the essence of the fundamental differences between anthropology/ethnology and ethnography (*národopis*). The text may be thought provoking for students of anthropology educated strictly within the bounds of the British school of social anthropology. But for an ethnographer educated in East-Central European departments of ethnography and folklore studies, many of his assertions are platitudes.

For me, as a student of Václav Frolec, who was educated in an environment where romantic, ethnocentric, post-Chotkian ethnography, in its Moravian (post-Václavík) rendering, was combined with the non-romantic so-called ‘ethnography of the present’, which, by the time I was a student in the late 1970s and early 1980s, represented a contemporary attempt at modernising the field and expanding its thematic scope on the basis of an interdisciplinary approach, reflections on contemporary socio-cultural processes and everyday culture are naturally placed within the larger historical context. East-Central European ethnologists regard oral history as a standard method. Given that East-Central European ethnographers study – roughly and simplistically put – culture rather than society, ethnographic analysis on the basis of continuous and discontinuous processes uses more than just the method of oral history, but also studies recent and historically more remote social and cultural phenomena and artefacts. The study of material and social culture certainly allows this. The study of *longue durée* processes is in my view a standard and very effective tool for obtaining knowledge and an understanding of the contemporary world, but it is certainly not essential. The selected method of research is in the end the individual choice of each researcher, made in relation to the goals he/she defines in the research. Much of what Hann advances in his text I most naturally employed even in my dissertation, defended in the Department of Ethnography at the Faculty of Philosophy of J.E. Purkyně (now Masaryk) University in Brno in 1981, as did the other students in the field of ethnography at the university. To his arguments I would add that, although knowledge of the late socialist period is essential for recognising the causes and character of transformation processes and understanding post-communist societies, it is also necessary to return to the pre-communist, early industrial age. That facilitates an understanding of the deeper context of socio-cultural development. The transition societies are characterised by a remarkable mixture of liberal democracy, ‘wild’ Eastern European capitalism, mental and socio-cultural stereotypes from the period of real socialism, and many socio-cultural phenomena from the period of semi-feudal agrarian society. In some rural areas, pre-socialist phenomena have been re-surfacing since the early 1990s, and knowledge of recent and even early ethnographic material helps anthropologists to obtain a better understanding of the present.

So in this regard one has to agree with Hann's arguments, even though – as I noted above – they are too commonplace to evoke (in East-Central Europe) more serious attention. But how this argument weighs in against the intention, or today already the reality, of establishing anthropological departments in academic institutions in East-Central Europe is something that escapes me. Hann's arguments perhaps rightly caution against adopting a too one-sided and thus epistemologically and methodologically narrow interpretation of the field; but they are not reason enough to oppose the existence of the field itself.¹⁶ I take no issue with his argument that the social sciences studying transformation processes should be oriented towards drawing on the knowledge of 'native' ethnography and from research on recent and more historically remote socio-cultural phenomena; I have been endeavouring to do just that since the early 1980s, when I began conducting my first independent ethnographic research on the Moravian and Slovakian countryside in the late socialist period, and it is in this vein also that I guide my doctoral students. Even the most recent books published by Slovak ethnologists take this approach (e.g. Danglová 2006). However, this in no way makes Hann's resistance to the institutionalisation of social anthropology in academia in East-Central Europe correct.

There are numerous reasons for the institutionalisation of socio-cultural anthropology, and they are based on the very specific behavioural strategies adopted by East-Central European ethnographers – or at least the majority of them – since the political change in 1989.¹⁷ One such reason is that most ethnographers are simply not pursuing efforts to modernise the field or Hann's proposed synthesis. On the contrary, since the fall of the totalitarian regime, most of them have rejected the so-called ethnography of the present, which could be modernised if necessary by drawing on the theory and methodology of contemporary social sciences, including anthropological methodology (on this topic see Bitušiková 2002), and only a small proportion of them study contemporary transformation processes.¹⁸ Another reason is the methodologically unclear or vague definition of 'ethnology' and the methodical and methodological chaos and disorder that dominates this field. Within the scope of this paper it is not possible for me to devote more space to this fundamental problem in contemporary East-Central European post-ethnography, but the problem itself is in my opinion a basic argument in favour of institutionalising social anthropology in East-Central Europe: in order to educate students of anthropology, from the very first year of undergraduate study, in an unambiguously, clearly and comprehensibly defined methodical and methodological framework. The use of quality ethnographic work from the preceding decades is no obstacle to doing this and can only contribute to the advancement of social anthropology in this region of Europe.

Chris Hann's text has one basic shortcoming. The author, an economic and political anthropologist, is trying to grasp the discussed problem not through the lens of political anthropology but from an ideological perspective. But the problem is that this issue is much more

¹⁶ I do not feel myself sufficiently well grounded in British anthropology to debate this with a professor from the University of Kent, but it seems to me that not all British anthropologists dogmatically insist on the ahistorical approaches of British functionalism. I am convinced that many of them also endeavour to obtain the kind of understanding of the historical background to the phenomena they are studying that Chris Hann is promoting in the study of post-communism.

¹⁷ For more on these strategies and their implications for the development and current character of the field, see Podoba (2003a, *forthcoming*).

¹⁸ For more on how the tradition of Central European ethnography established by Chotek and Mjartan broke up into a diverse mixture of various interest-driven specialisations, sub-disciplines, and individual strategies, some even outside the defined framework of the individual branches of the social sciences and the humanities, all under the label of 'ethnology', see Podoba (2003a, *forthcoming*).

about the inertia of institutions and the viability of social networks, about prestige and very specific power and existential interests, than it is an epistemological and methodological dispute. And it is also a matter of the unwillingness and inability of East-Central European post-ethnographers to abandon their set ways by, for example, developing a methodology in this field that moves more in the direction of ethnology/social anthropology.¹⁹ Hann's vision of cooperation or, rather, the institutional unification of these two very close and at the same time vastly different ethnographic fields is more utopian than it is realistic.²⁰ Had he published these views ten to fifteen years ago, I would certainly have agreed heartily with him. My personal experience of the past seventeen years, however, has led me to the opinion that the most appropriate alternative to the modern scientific study of society and culture on the basis of qualitative methods, and especially ethnographic field research, is through the institutional grounding of social anthropology in East-Central European academic institutions. It will then be a matter of common sense on the part of domestic anthropologists not to ignore good older and current ethnographic work and incorporate it into their study programmes in anthropology, and to adopt the knowledge, findings, and stimuli acquired in the study of recent and historically more remote cultural forms and socio-cultural processes for use in the future anthropological study of post-communist societies.

The Past and the Future of Anthropology in East-Central Europe: comments on Chris Hann's vision

*David Z. Scheffel*²¹

Professor Hann sees the future of anthropology as a relevant and stimulating discipline hinging on three key requirements: the necessity to maintain ethnographic fieldwork; the need for a serious engagement with history; and the desirability of respecting local intellectual traditions. I see little sense in commenting on the first two points. Here Hann largely re-asserts a position embraced by most western anthropologists, and I suspect that neither his insistence on good ethnography nor his arguments in defence of a thorough historical analysis is likely to raise any eyebrows among our colleagues in postsocialist Europe either. On the other hand, Hann's third point about the interaction between local and imported intellectual traditions addresses several contested issues that I would like to comment on.

It is refreshing to hear a senior western academic offer a laudatory assessment of the intellectual traditions of any *East* European field of study, and anthropology is certainly no exception. Hann is right to call attention to the ignorance of most British and North American anthropologists about indigenous contributions to the discipline, and he is right to point out the possible advantages of a comprehensive study of humanity anchored in a 'mature synthesis' of folklore (*Volkskunde*) and

¹⁹ The generally widespread attitude towards the methodological advancement of the field and the critical evaluation of the strengths and weaknesses of ethnography from the period of real socialism, and towards a general reflection on the field, is exemplified well, for example, in Beňušková and Ratica (2002), especially on p. 399.

²⁰ The fact that in East-Central Europe there really exist such academic departments is an example of the exception that confirms the rule. It is no accident that we find them in Slovenia and Poland, which within the socialist bloc always occupied a unique position. For the most part, however, it is just a matter of a change in logo (the name of the field and the academic department), or an effort to indicate methodological changes through decorative detail.

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ethnology (*Völkerkunde*) as it might be foreshadowed in some universities of postsocialist Europe (Hann 2007: 27). Hann's self-conscious dismissal of the social anthropological model as an example worthy of emulation throughout east-central Europe is certainly unexpected from a leading social anthropologist, and Hann clearly knows that his position is going to ruffle more than a few feathers among the indigenous 'westernists'.

Unfortunately, Hann's assessment is hampered by a lack of attention to the very historical analysis, which he so admirably advocates throughout his essay. As a serious student of east-central European anthropology, he should know that when it comes to the hoped for synthesis of (native) folklore and (cosmopolitan) ethnology its intellectual history is replete with illustrations of parallelism rather than symbiotic co-existence. Some readers of this journal will be aware of the fierce competition between these two (sub-)disciplines as a formative force behind Czech anthropology. On the one hand, we see a subject akin to contemporary 'native studies' emerge in the latter part of the 19th century and usually designated as *národopis*. Soon thereafter, a sister discipline called general or comparative ethnography appeared – not as a complement but as a determined rival. The competition between the two subjects has been a distinguishing feature of Czech anthropology for more than a century, and as such it has little to do with socialism or postsocialism (Scheffel, Kandert 1994). The contemporary desire of some of our colleagues in east-central Europe to import social anthropology is merely a modern reaffirmation of a much older and unresolved problem.

Can the tension between (parochial) native studies and (cosmopolitan) ethnology disappear under the aegis of a 'mature synthesis' as proposed by Hann? There are indications of cooperation here and there. *Český lid*, the unabashedly nativist flagship journal of Czech ethnography founded in 1891, has recently been hyphenated into *Cesky lid – Ethnological journal*, and some university departments in some countries seem to have at least attempted to bridge the chasm between the two sub-disciplines (Scheffel 1999). But for every example of cooperation there are other examples of polarization and outright secessions. In the Czech Republic word has it that the kind of synthesis proposed by Hann is an idealistic vision that has little chance of realization.

For my part, while I agree that *Volkskunde/národopis/native studies* has much to offer to the student of local culture and society, my limited experience with the practitioners of this craft makes me rather doubtful about their ability – or perhaps willingness? – to go beyond their own experiences and tolerate, let alone seek, explanations that transcend the local setting. I learned this lesson several times as a result of critical assessments I had made of Czech *národopis* (Scheffel 1992; Scheffel, Kandert 1994). The sub-text of native reactions has always underlined the illegitimacy of an 'other' casting a critical eye on 'our' practices and beliefs (Langer 2002; Woitsch 2002). This is, of course, a stance common to the practitioners of most brands of native studies (Black studies, women's studies, Aboriginal studies, etc.), and it is precisely in this point that we see the divergence from (cosmopolitan) ethnology or anthropology particularly well.

Having recently emerged from a long era of exceptionally comprehensive parochialism, many of our colleagues in postsocialist Europe understandably yearn for new models of social science that will connect them to developments in western scholarship. The universalism and cosmopolitanism of social anthropology make it a logical choice worthy of emulation. But one should not naively expect that a mere change of terminology will suffice. As Professor Hann points out, the culture of socialism continues to influence the postsocialist period, a condition aptly summed up by the German neologism "*Mauer im Kopf*". It is such mental barriers that stand in the way of genuine

scientific progress as their impact is far more pronounced than that of the label by which we choose to designate a discipline.

Let me illustrate this with an example from my own exposure to postsocialist Czech anthropology. Recently, I came across an edited volume of essays dedicated to the history of teaching anthropology in several European countries (Dracklé, Edgar, Schippers 2003). One of the contributions caught my eye as it dealt with the intellectual history of Czech ethnology and was written by a leading advocate of its transformation into western-oriented anthropology, Professor Ivo Budil (Budil 2003). As I read my way to the core of the article, I began to sense that I had encountered certain phrases and even entire sentences before. Eventually I realized that I was reading fully preserved sections of an article I had written jointly with a Czech colleague some ten years earlier (Scheffel, Kandert 1994). We find here entire sections of our article, including English translations of direct quotations from Czech ethnographic literature. Here and there a word has been changed or the sentence order modified, but any impartial reader would have to recognize that a significant part of this text is only a slightly altered version of our earlier publication. The fact that the author is not an academic novice but an experienced and prolific scholar and head of a department which has done far more to emulate the western anthropological model than any other Czech institution (Budil 2003: 100) compels me to believe that we are not dealing here with a deliberate violation of academic honesty. Indeed, several Czech colleagues consulted on the issue of scholarly rules and regulations confirmed that local tolerance of practices akin to plagiarism is considerably higher than what I am accustomed to in Canada, and that such basic a principle of scientific activity as the definition of the boundary between one's own and someone else's work lacks general agreement.

What relevance does this anecdote have for the future of anthropology in east-central Europe? The point I am trying to make is that we should take seriously the prescriptive connotation of the word *discipline*. Precisely because it is an academic discipline, anthropology depends on the maintenance and enforcement of certain standards, including academic honesty. It is my impression that this disciplinary dimension of scholarship is taken less seriously in the Czech Republic than Canada – to use two countries which I am well acquainted with – and I suggest two reasons for this. The first one has to do with the long Czech tradition of bending the results of science, in the widest possible sense, in order to support certain political ends. Unlike the residents of countries blessed with a high degree of political stability, the Czechs have a tendency to view scholarship as a utilitarian activity governed by pragmatic considerations. The idea of pursuing 'truth' – as it is inscribed into the mottos of many western universities – is likely to evoke ironic laughter rather than admiration. The second reason derives from the famous Czech mastery of the art of outwitting higher authority. This powerful trait of the national character, immortalized in Jaroslav Hašek's 'good soldier Švejk', provides ready justification for even the most extreme form of relativism that easily deconstructs any type of order imposed from 'above'. Although each of these traits clearly predates the imposition of communism (Scheffel, Kandert 1994), the totalitarian order generated particularly favourable conditions for their survival and expansion far beyond their initial scope. It is of little wonder, then, that we encounter both well beyond the end of state socialism.

Returning to Chris Hann's call for the recognition of the contribution made by indigenous ethnographers, yes, let's recognize it. But what does this contribution consist of beyond snippets of localized descriptions suited for an ethnographic atlas? Of all the native students of Czech and Slovak folk culture, only Bogatyrev – notably a Russian émigré – has managed to produce

something of lasting international importance. To sum up my argument, I suggest that our colleagues in the postsocialist countries should worry less about the name they attach to the subject they practice and more about the standards that transform it into a credible academic *discipline*. This entails not just the setting up of professional organizations but also the drafting of codes of conduct, standards to be employed in the evaluation of manuscripts for publication, and criteria for the granting of higher degrees and promotion. Professional misconduct, including academic dishonesty, should be defined and censored. Without such rigour our colleagues run the danger of constructing a Potemkian façade rather than pursuing credible scholarship.

My Preference Lies with Social Anthropology

*Petr Skalník*²²

In his home country United Kingdom and later in post-wall Germany, Chris Hann has been a very successful social anthropologist, pursuing a splendid career of a leading western specialist on “socialism” and “postsocialism” (Hann 1993, 2002, 2006). As a ‘card-carrying’ social anthropologist, trained among others by Jack Goody at Cambridge, he carried out two long-term field researches, in Hungary and Poland respectively (Hann 1980, 1985). During the research periods and while writing up he apparently did not find the works of Hungarian peopleographers and Polish peopleogists (their disciplines are called *néprajz* and *ludoznawstwo* respectively) very inspiring. After the ‘Wende’, in 1999, when he landed in Halle as one of the directors of the newly established Max Planck Institute for Social Anthropology he realised that he knew “nearly nothing” about the “*Ethnographie*” in the former German Democratic Republic. He wanted to know more about it and find out whether its theories could be employed within social anthropology. For the sake of comparative approach, also characteristic for social anthropology, he organised in 2003 in Halle a conference which discussed the developments and theory trends within local analogies of social anthropology during the era of communist rule in not only the GDR but also Czechoslovakia, Hungary and Poland. The volume which came out after the conference (Hann, Sárkány, Skalník 2005) is to my mind a testimony to the interesting, sometimes fascinating, though basically fruitless, vicissitudes within disciplines characterised during most of the studied period by isolation, ignorance about world trends in social anthropology, cultural anthropology and ethnology as they were practiced especially in major capitalist countries (USA, UK, France, Germany, Netherlands, Scandinavia). Frankly speaking social anthropology (and for that matter cultural anthropology and ethnology) did not miss anything substantial by knowing nearly nothing about ‘socialist era anthropology’.

When reading Chris Hann’s text I could not help recalling my own experience. When I started my academic career early in the 1960s my initial impression was similar: I did not find nationography (*národopis* in Czech and Slovak) of interest for its intellectual sterility, isolationism, inability to study the present in which I lived, and therefore turned to social anthropology and

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African study as a viable alternative for a career. I admit that I was lucky because my mother opened my eyes to both the impotence of *národopis* and an inviting promise of social anthropology. Ladislav Holý and Milan Stuchlík were my first mentors in social anthropology. I never regretted my decision to become social anthropologist even though I had to suffer a lot for it.

When I returned home in 1990 after years in exile which among other gave me opportunity to experience the blessings and trappings of long-term fieldwork, I did not want professionally anything else than to put my talents and abilities to the task of introducing social anthropology to my country of origin. My effort was rewarded by several age-groups of students who continue to foster the young plant of social anthropology in what is now the Czech Republic. However, as they are, I am also quite well aware of the fact that this is no walk through a rose garden. The *národopis* establishment, after suppressing ethnography during the communist era, does under the usurped name of ethnology everything possible not to let social anthropology exist as a fully recognized academic discipline along with others (cf. Skalník 2002). Therefore till this very day there are no university departments of social anthropology in the Czech Republic²³, no doctoral programmes in social anthropology, no docents or professors of social anthropology. It is not much better in other post-communist countries. At best the epithet ‘cultural anthropology’ was added to the names of the existing departments of ethnography/ethnology in Bratislava, Poznań, Wrocław, Ljubljana and some other places. In Prague, the Faculty of Humanities at Charles University created a new department, that of “General Anthropology”. In the Western Bohemian University at Plzeň the formerly existing Department of cultural and social anthropology was recently renamed to Department of Anthropology, ostensibly because biological anthropology is taught there as well. In the Faculty of Natural Sciences in Brno the Department of Anthropology, originally offering courses and degrees in biological anthropology has recently added some portion of social and cultural anthropology into its curricula.

In this light it is quite strange to read Chris Hann ‘pontificating’ about the redundancy of social anthropology in former communist bloc countries. I wonder whether Malinowski in his time had to face “established intellectual communities” telling him that social anthropology was a mistake. Was the Malinowskian revolution really such a mistake and would it have been better to incorporate folklore studies, folklife studies, and ethnology as they were practiced before Malinowski by Frazer, Seligman and many others during the pre-Malinowskian generations with the revolutionary principles of social anthropology? I daresay that if that had indeed happened then, there would have been no social anthropology and therefore also no point for Chris Hann to do research in Hungary and Poland, not to mention promoting social anthropology in Halle and from there throughout post-communist Eurasia. Instead he would have happily languished in his native Wales describing the vanishing or vanished customs of his great folk. But I do not want to ridicule Chris Hann’s suggestions. I simply want to point out their utopianism and absurdity. The young generation of students and those relatively few PhDs trained and working in social anthropology in today’s Europe to the east and southeast of Halle knows pretty well that the grand compromise as proposed by Chris Hann is not a priority, it is as undesirable as the return from chemistry to alchemy.

At the same time I agree with Hann that social anthropologists should know about and study the works of peopleographers and their likes, be aware of history and carry out research diachronically

²³ Social Anthropology under that very name is taught on Bachelor and Master level in the Department of Social Sciences of the University of Pardubice, and on Bachelor level in the Department of Sociology of the Masaryk University at Brno.

(e.g. community re-studies, see Skalník 2005b). I also understand his problems when looking back at his own arrogance and *naïveté* while carrying out his social anthropological research in communist-ruled Europe. Similar problems were faced by his British colleagues who worked in Africa and elsewhere outside Europe and belatedly realised that they were (seen as) part and parcel of colonialism and imperialism. But as Michał Buchowski showed quite persuasively recently (Buchowski 2004), some kind of arrogance continues even today when quite few ‘native’ social anthropologists live and work in the post-communist countries but they continue to be either ignored or not taken as equals by their western European or American colleagues. Thus in social anthropology we have a two-speed Europe already, enhanced by much smaller funds spent for research in its eastern part in contrast to its western part.

In order to overcome the gaps we should rather unite forces for strengthening and perfecting social anthropology in both parts of Europe but especially in the east of the continent. No softening of standards will help us. The appeal of EASA is exactly in attracting to social anthropology of those who were formed as scholars outside or beside of it. Therefore “Perspectives from home” on post-communist realities will be offered soon in a collection of essays written by social and cultural anthropologists from the ‘other’ Europe (Kürti, Skalník *forthcoming*). I would, for example, suggest that local social anthropologists and researchers from neighbouring disciplines examine critically those works of social anthropologists who like Hann, Stirling, Pitt-Rivers or Verdery came from foreign countries in order to offer insight into societies alien to them. This is what Europeanists within EASA might do with profit in the coming years.

To build “a genuinely comparative and cosmopolitan anthropology department”, as Chris Hann suggested, might be a nice dream but it is unrealistic because the power holders within nationography in the Czech Republic and its analogues elsewhere in post-communist Europe would not allow it. Recent discussion about social and cultural anthropology published on the pages of the nationographic (‘ethnological’) journal *Český lid* (see volumes 91-93 for 2004-6 of this journal) to my mind fully corroborated this. One of the participants in this discussion wrote that social anthropology was “unheard of” in “our and neighbouring universities” until it was “smuggled in” in the lectures of Holý and Stuchlík when they taught as external lecturers in the Department of Ethnography and Folklore at Charles University of Prague (Vařeka 2005: 182). The same author discounts completely any difference of method between sociocultural anthropology and *národopis*/ethnology. Those who became persuaded if not card carrying social anthropologists and work in post-communist Europe know well that social anthropology with its revolutionary theory and method causes havoc in the ranges of the traditional nation or peopleography: in the mirror of social anthropology these nationalist disciplines and their practitioners discover that they are naked like that proverbial king who believed that he had new clothes. If Chris Hann really means what he writes he should first start at home: merge *Volkskunde* (or European Ethnology) with *Völkerkunde* (or *Ethnologie*) departments in Germany, Switzerland and Austria, or for that matter social anthropology departments with English, Welsh or Scottish folklife studies research and teaching institutions in Britain. I wonder what the result would be: revolution or counterrevolution?

Reply to Chris Hann

Michael Stewart²⁴

Chris Hann's forthright and splendidly clear argument for a multiple reorientation of the European ethnological and anthropological tradition is, to say the least, timely. Without donning the robes of Cassandra, it is fair to say that teaching and research in anthropology in Europe is at something of a turning point – at least looked at in the *longue durée*. It is not clear to me that the discipline in its current form is going to be able to provide answers to the most interesting questions in social life and in the human sciences that will emerge over the next thirty to fifty years: mostly because these almost all now require inter- and multi-disciplinary collaboration. Chris's suggestion is in effect to move about more in time and this will of course require working with others from different scholarly backgrounds. However since, for the time being, disciplines remain the only sites within which to cultivate the scholarly skills necessary to pursue research, Chris's suggestions as to immediate measures and strategies certainly offer one path to the future for many of the discipline's current practitioners.

In particular it seems to me that while most departments of anthropology are at the moment more or less happily producing ever greater numbers of local studies, better or worse examples of 'ethnographic research' that contribute to the local or area field of knowledge, it is unlikely that this approach will continue to attract the kind of bright, ambitious students a discipline needs in order to reproduce itself. There are only so many theses that can be written about Romany populations of eastern Europe within a descriptive, ethnographic approach, for instance. At the same time, so many of the exciting developments in social theory and analysis that impinge directly upon the interests of anthropologists are taking place outside the field of anthropology and sooner or later, if it has not begun already, the drain brain will begin. In this respect I find it surprising that Chris is so dismissive of the work from biological anthropology (or rather by biological anthropologists but right at the heart of social anthropological concerns) on altruism and its social underpinnings. The research initiated by Robert Boyd and P.J. Richerson (e.g. 2005) and taken up by J. Henrich (Henrich et al. 2004), and now many others, has laid down a fundamental challenge to a whole series of preconceptions both within anthropology and, as importantly, without. And it has been carried out in a way that enables others, outside the discipline, to read and comprehend its significance for their own work.

This work is perhaps unfamiliar to some of the readers of this journal. In brief Boyd and Richerson have adopted a series of economic games which they first of all used on university student populations in different countries in order to see if altruistic behaviour could be detected in a systematic manner cross-culturally. More recently ethnographers have been coopted to carry out 'ethnographically appropriate' versions of these games through which they claim they can test (in conditions of anonymity) the willingness of humans (in different parts of the world) to make altruistic sacrifices for others, that is to renounce maximisation of their wealth and other benefits – without any obvious benefit accruing to themselves. Social anthropologists alongside some other social scientists had been banging on for years insisting that altruistic behaviour could be found in

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all human populations but no one had demonstrated this in a systematic and persuasive manner. Thanks to Boyd, Richerson and their collaborators it is now common to read acknowledgements to this effect in a broad literature. But their work has also thrown up fascinating questions that could only arise because of its quantitative approach. Altruistic sharing of resources seems to be more common in some societies than in others – why this should be so can best be answered by sociologists and anthropologists in precisely the kind of comparative work Chris would like to encourage. Finally, and most controversially (this was Boyd's initial interest in the field) one possible implication of this work is that what biologists call 'group selection' might operate in human populations. Whatever the merits of this claim it might be seen as a little churlish for social anthropologists to complain that the use of formal games to isolate and investigate one aspect of social life cross culturally is to miss the richness of altruistic behaviour as live ... and most basically this kind of disciplinarian border making is precisely what the rest of Chris's article in effect argues against. Indeed, so common is this kind of scepticism in our community that I have come to think that anthropologists should fit mental alarms such that every time they feel like pouring cold water on work from a neighbouring discipline the alarm reminds them to pause and reconsider!

More positively, I am struck in particular by the influence on Chris's thinking of Jack Goody's approach to anthropology. The extraordinary corpus of comparative and historical work Goody has produced over the past thirty years has as yet no parallel in the discipline and it would certainly be a wonderful challenge to try and institutionalise an approach to social analysis based on this model. And here the intellectual coherence of Chris's approach becomes clear. Parts of Goody's work are precisely the kind of studies that could emerge in expanded departments of ethnology – where a solid and deep grounding in the history and ethnology of a country could be linked to a comparative *longue durée* analysis. I wonder if the Max Planck Institute for Social Anthropology might not be the ideal place to encourage in a more formal fashion the creation of such a team of researchers? In a similar vein, I find it serendipitous that Chris's hailing of the Frazerian legacy should have appeared more or less simultaneously with the splendid collection on kingship edited by Declan Quigley (2005) which (through the work of Luc de Heusch, in particular) pays most effective and persuasive tribute to the enduring value of Frazer's analyses of divine kingship.

While I agree wholeheartedly with the goals Chris articulates for a unified field of ethnology and social anthropology I remain less than convinced by the pragmatics for achieving this. It is all very well and good asserting that he will not support the creation of anthropology departments in de facto opposition or at least competition with ethnology departments, insisting that the two teams work alongside each other. But in practice Chris has only one example of such a functioning unit and indeed names only one person who has successfully bridged the two traditions. Now, of course there are others we can think of: Vintila Mihailescu, currently head of the National Museum of the Romanian Peasant in Bucharest is a shining example. But as he himself would be the first to admit he often feels that he is being torn asunder limb by limb as he tries to straddle the worlds and research interests of those who hold to the label ethnology and those who have adopted the title of 'anthropologist.' In almost all actually existing universities in eastern Europe the competition for resources, students and limited research funding is simply too intense to expect a spirit of altruistic cooperation to break out! Even less so because of the way university appointments are so often still politicised. If the minister of education makes appointments to lectureships and has to approve or

not new courses in every university in the country there is inevitably a degree of politicisation that would shock those from countries with educational administration devolved to the university level.

This is not to say that I am unsympathetic to Chris's desires. Indeed I well remember taking younger Hungarian anthropology students to task some eight or ten years ago for the bilious way in which a debate about the relationship of anthropology to ethnology was conducted amongst their seniors. In academic papers and elsewhere (in Hungarian so few outsiders were aware of this going on) scholars from the two camps denounced each others as hangers-on of fascism and the like. My students agreed that the tone of these debates had rapidly become unacceptable and totally unconstructive but they resisted fiercely my assertion that they should have stuck it out in the department of ethnology at the Eötvös Lorand University in Budapest. What I had not realised but my students made all too clear was that when it came to choosing subjects for their master's theses, for instance, their more traditional teachers were pushing them forcefully and then insisting that they go off and study traditional crafts or story telling among the peasants. And this, they told me in outrage, in 1991 when firstly there were next to no practitioners of traditional crafts (so they were in effect asking informants to reconstruct past practices) and secondly there were far more interesting things to study in the villages to which they had been sent.

In other words, our outsiders' wishes for a harmonious and fertile union of the two disciplines no doubt express the ideal solution to a tricky problem. But it is only rarely that it is going to be practical. Instead the job of outsiders, it seems to me, is not to take programmatic and absolute stances but to back 'winners' as and when they emerge and to collaborate with whomsoever is doing interesting and innovative research – without prejudice as to where their intellectual home is. I am sure that this is how Chris runs his section of the Max Planck Institute for Social Anthropology and the same policy applies in the Marie Curie SocAnth anthropology programme which I coordinate.

Czech *národopisci* and socio-cultural anthropologists in a changing environment

Zdeněk Uherek²⁵

When reading Ernest Gellner's 'Anthropology and Europe' (Gellner 1992) or the introductory parts of Chris Hann's *The Skeleton at the Feast. Contribution to East European Anthropology* (Hann 1995), one is struck by the descriptions of the evolutionary shifts in the conception of East and West in Europe, and how these shifts were connected to the economic aims and power aspirations of the European elite. It is clear from these works that in modern Europe a considerable number of economically, religiously, or politically articulated divides emerged and subsided, or in cases persist to date, that affected European thought and created barriers that were difficult to surmount, but at times created a space in which rich synthetic work thrived. Both these works also provide an idea of how the terms 'East' and 'West' have been manipulated over time. The East-West polarity

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is occasionally reminiscent of the approach to another set of concepts, that of the savage and the civilised, nature and culture, emotion and reason, with all that is wild, natural, and savage being ascribed to the East, while the rational, tame, and civilised remain the domain of the West.

Kuhn (1962) regards scientific theories as shared thought schemes that emerge consensually out of the scientific community's agreement about what is relevant within certain recognised paradigms, and in this regard it is logical that, for the anthropological community, a key dividing line scientists agree on is one that they experienced empirically and that had an effect not just on the object of their research but also on the scientists themselves. While we could identify all sorts of divides, the one we have in mind here is the Iron Curtain, the divide between the former state socialist countries and Western Europe.²⁶

This divide was redefined after 1989 as a divide between the post-communist transition states and Western Europe and gave rise to two large blocks of states that had not existed before communism fell. What had existed were certain states in which the governments spoke similarly of building up a socialist or communist system, but the position of each individual state-socialist or communist country towards the West and towards one another differed substantially. This 'block' comprised the satellite states of the Soviet Union, the Warsaw Pact countries (which were in quite different positions, with Romania and Poland being quite unlike Czechoslovakia), and then there were states like Yugoslavia and Albania. The policy of individual Western European countries towards the state-socialist countries and towards one another was far from uniform. In the mid-1990s Claire Wallace aptly described the position of the Central European states and the transition countries in the Baltic region and the Balkans as a buffer zone. With regard to Western Europe, she noted correctly that the opening of borders with the former communist bloc served to unite Europe like never before (Wallace, Chmouliar, Sidorenko 1996). Her observation of conceptual disunity before 1989 is illustrated in a story of something that happened to a friend of mine on a trip to the UK sometime around 1980. He was asked where he was from, and he answered: "Guess. I'm from one of the socialist states." "Aha!", they beamed knowingly, "France!"

Sharply defined borders and a sense of unity go hand in hand. The notion of a block of countries with a common past that are now together going through the process of transition is sometimes even adopted by East Europeans, who could hardly have experienced any such kind of united block either before or after 1989. Before 1989 the communist states were poised as a threat to one another, they occupied each other and enacted economic blockades, and after 1989 the political elites again found themselves in unequal positions during the negotiations to join NATO and later the European Union. The Czechs see the border between the East and the West a certain way, the Croatians another, the Ukrainians another, and the Russians yet another.

Communism lasted for a relatively short period, in most Central European countries for roughly forty years. It is now seventeen years since it ended in Europe. But it appears that what is referred to as the post-communist or transition period may in duration ultimately rival the communist period. Therefore, it makes sense to consider Hann's idea of focusing on other temporalities than the postsocialist present (cf. Hann 2007). On the one hand, it is necessary, as Hann states, to place the current period within the broader historical context and review it from an historical perspective, and on the other, I believe, it is necessary to seek much more in the present than just its transformation from the socialist past.

²⁶ This text is based on data analysed as part of a research project of the Institute of Ethnology of the Academy of Sciences of the Czech Republic, no. Z90580513.

When we speak about post-communism or the social transformation, what we are referring to is the very period that we are living in, which is specific primarily in that it follows another period that profoundly affected our lives, and, as implied in the term 'transformation', owing to the distinctiveness or epic character of the communist period that societies are transforming out of, it casts a shadow over the current period. But I believe that for characterising the current period this term is now misleading. It corresponds more to an external perspective, for example, the view from Western Europe, whose populations communicated with the states on the other side of the Iron Curtain relatively little up until 1989. From the everyday perspective of Eastern Europeans today, however, the situation looks different. The generation of Eastern Europeans under the age of 25 is one that had very little experience with the communist past. An empirical study conducted by Jiří Černý, Markéta Sedláčková, and Milan Tuček indicates that in 2001 and 2003 people in the Czech Republic regarded 1989 as an important historical turning point, but they regarded development thereafter as constant and linear. They weighed the positive and negative aspects before and after 1989, and their opinions on the transformation (that is, the period after 1989) differed substantially by education and profession. Often they did not even express expectations of any major changes to come from the Czech Republic's accession to the EU, as they felt that such changes had already occurred (Černý, Sedláčková, Tuček 2004).

In the Czech Republic, the transformation period followed the 'normalisation' period in Czechoslovakia (a reference that now already sounds comical), during which, after massive communist purges, society, in the words of the writer Šimečka, stagnated and was paralysed (Šimečka 1990). Although, as Katherine Verdery, for example, has noted, life 'behind the Iron Curtain' may have held an air of mystery and seemed fascinating to Western researchers, especially until they made their first visit (Verdery 1996: 8), it also evoked a sense of danger, darkness, and strange conspiracies, which from without made it seem adventurous and dynamic. But from the everyday perspective of an individual living in socialist society, the atmosphere was characterised by boredom, provincialism, lethargy, and a lack of information and contacts. Official cultural production was on the whole sub-standard, and the more educated strata of the population even then lived off cultural imports, just as they do today, the only difference being that the state tried to control the supply of cultural imports and, given that the socialist consumer was cut off from the producer, the imports acquired different meanings. In the words of Leszek Dzięgiel, it was a state-controlled shortage (Dzięgiel 1998). I would thus venture to argue that in many respects the communist period was not so culturally distinct that all subsequent epochs should be regarded as a process of transforming from socialist culture, making up for and overcoming it, or that any distinctive, characteristic features of that epoch should be sought. The continuous references to overcoming communism are more likely indicative of the low level of creativity among authors in Eastern Europe today, who are incapable of detecting contemporary meanings in current events. They seem to have shed their role as the social diagnosticians of the present, which under the label of post-communism is just a reflection of the past.

I agree with Chris Hann that there is little in social anthropological literature about the communist period that we are continuously making reference to, that no coherent theory on this period has been formulated, and that the theoretic literature on post-colonialism is certainly much richer. There is no straightforward answer to be found in anthropological literature to the question of whether socialism or communism can be understood as a specific social system, of whether it was not just a form of dictatorship, which, based on convictions about a more just social order,

could be created anywhere – in Siberia or the Caribbean, in a society with a feudal system or in an advanced European capitalist country – its survival relying not just on a totalitarian order and on hope (soon abandoned) in a better future, but also on a redistribution system from which a relatively large number of people are able to profit. The question of what socialism or communism actually was and how to classify it was discussed in Hann's edited volume, *Socialism: Ideals, Ideologies, and Local Practice* (Hann 1993) in the early 1990s, but since then the discussion has progressed little.

There are a number of reasons for the lack of authoritative studies of socialism and communism. The control over information meant that it was difficult for distinguished researchers to study the system *in situ*, from within and during its existence, but also the data that emerged within the socialist states were distorted. The theory on which socialism was based had little in common with real behaviour, the positions occupied by individual groups did not correspond with the role they played in society, economic data was at odds with the personal experiences of ordinary citizens, journalistic reports presented only an incomplete picture of events, and election results had little to do with the real political views of the population, and so on. In such circumstances, everyday life must instead be reconstructed primarily out of narratives, as Hann prefers. An important aid and expert on these aspects of life is the ethnographer – *národopisec*, a figure that over the course of many years is engaged in the collection of information and the study of his/her local society and its customs. However, the kind of ethnographers – *národopisci* Chris Hann refers to are, at least in the Czech Republic, few in number; we mostly find them in regional and outdoor museums or in archives, and they can in no way vie with the kind of social anthropologists and ethnologists that huddle in institutions, writing texts and theories in an environment of flourishing international cooperation. The latter have long prevailed over *národopisci*, with whom they have never really had to contend for dominance in the humanities and the cultural sciences, because the *národopisci* that Chris Hann has in mind were already relegated to the sidelines of Czech academia during the communist period.

Ethnography, as a discipline grounded in detailed regional field knowledge of rural culture, enjoyed its heyday in Czech society between the 1890s and the 1930s. During that time it developed simultaneously as an academic discipline and as a relatively large amateur community of enthusiastic collectors. The academic elites that laid the intellectual foundations of the field were often, especially at the turn of the century, both ethnographers and qualified specialists in some other academic field that already had an established methodology and strong academic foundations. They included top figures in the field of history and cultural history (Čeněk Zíbrt, 1864-1932), archaeology (Lubor Niederle, 1865-1944), literary theory (Jiří Polívka 1858-1933), musicology (Otakar Hostinský 1847-1910), and other scientific fields. Many of them had studied at universities abroad, and they drew inspiration for their ethnographic work from the kind of the German academic environment that had produced Franz Boas or the kind of imperial Austrian background out of which Bronisław Malinowski emerged.

Czech ethnology at the turn of the century was mainly a re-worked version of an academic model imported from Germany. In its imported form it was easily institutionalised in the Czech lands, as it was wherever the prevailing concept of the nation was ethno-cultural. It appealed to top scholars in the social sciences, people with a strong cultural education, as they were the ones best equipped to search for and detect parallels between high culture and folk culture and synthesise them, and to unearth patterns of human creativity using tools previously applied exclusively to the theoretical

study of high, elite culture. Only a qualified architect could make a qualified study of folk architecture, a professional musicologist folk music, and a literary theorist folklore and oral culture. In order to seek parallels between high and low culture and apply the language of science to the culture of rural society it is first necessary to have good knowledge and an expert grasp of high culture and cultural theory.

Ethnography emerged out of the application of individual specialisations of high culture to a detailed knowledge of folk culture, and its focus was distinct from that of colonial anthropology. Its founders were not influenced by nationalism; rather, it was they who shaped nationalism and constructed a particular idea of the nation. In the sphere of culture, they demolished professional barriers by assigning equal value to high and low culture. While this involved a kind of cultural relativism, it was not a question of making relative value judgements on the culture of individual ethnic groups but on the culture of the individual social strata that were meant to form a single nation.

The difference in the approach to the familiar and the remote among colonial social anthropologists and ethnographers is the source of the distinct focus of these two fields. While colonial anthropology attempted to gain an understanding of at least the basic features of different cultures, ethnographers tried to assign a new position in the spectrum of cultural values to one part of the national culture, the culture of the rural strata, which they regarded as their own culture. They studied the speech of the popular strata not so that they could understand and communicate with the villagers, the way anthropologists did, but so that they could examine their oral expression. Ethnographers had no need to study the concepts of kinship and family structure, as social anthropologists did, because they were already familiar with the kinship and family structures of their topic, as, after all, many of them came from rural society themselves, and what they were interested in instead was showing the kinds of values and norms that applied in rural families. Ethnographers were not interested in the question of the transmission of social knowledge, the way social anthropologists were, because the children of villagers and their own children attended the same schools. Rural religion was interesting to ethnographers for the remnants of pre-Christian customs and superstitions it contained long after such features had vanished from the cities, but that is all, because the ethnographers were as much Christians as the villagers, and there was nothing remarkable about church attendance. The structure of power in the village, which in other ethnic communities is a topic that absorbs the attention of colonial social anthropologists, held little appeal for Czech rural ethnographers, as the villages followed the same laws and norms that applied throughout the state. Ethnographers were interested in folk art, festivals and holidays, folk song and dance, as these are the elements that reveal the distinctiveness of the villagers, their creative skills, their aesthetic sense, and other values for which the urban strata tend to prize the creators of high culture, which had previously been separated from the village culture by an insurmountable barrier.

The ethnographer's spectrum of interests was thus distinct from the interests of social and cultural anthropologists, even though they all shared the same intellectual background, in figures like Bronisław Malinowski and Franz Boas, and also a number of other researchers with an ethnographic focus; for example, in Czech ethnography, there were Lubor Niederle and later Karel

Chotek (1881-1967), scholars whose joint studies are still fascinating works.²⁷

The point where social and cultural anthropology begin to thematically and methodologically resemble ethnography is logically the point where anthropologists begin to practice at home anthropology or where the anthropologist moves beyond an elementary level of understanding other cultures and begins to address questions of values and meanings. Conversely, ethnographers begin asking questions that are similar to those posed by social anthropologists as soon as they begin to study their communities with greater historical context, as in historical anthropology. For example, information about the structure and functions of the peasant family in the 18th-19th centuries is important today, while to 19th-century researchers these questions seemed less interesting.

In my view, then, an encounter between social and cultural anthropologists and ethnographers, as Chris Hann mentioned, would be interesting and productive, and I certainly don't think that it would be an encounter of diametrically remote groups. However, the problem is that, as noted above, ethnographers, at least in the Czech Republic, have become few in number and are more likely to be found working in regional organisations than in academic institutions. The decline in the number of ethnographers under the communist regime occurred as a result of the fact that early village ethnographers tended not to be pro-communist but were rather traditionalists, nationalists, sometimes pan-Slavists, supporters of the agrarian parties, and sometimes admirers of T.G. Masaryk. After 1948 they consequently became the target of criticism, from both state and party organs, and from the emerging generation of ethnographers and folklorists, which, adhering to the spirit of contemporary ideas about the role of the social sciences, began largely to focus on other topics, and if they did study villages, then it was in terms of changes connected with the adoption of socialist values and norms. Scholarly work on the village environment did emerge, but it was not as common. Interesting, for example, were the studies by Jiřina Svobodová on the changing relationship of the village population to the land in connection with collectivisation (1973), or *Komárov*, a study by Zdeněk Salzman and Vladimír Scheufler about a Czech agricultural community (1974).

While ethnographers proceeded to take up new themes under communism, they still had difficulty addressing the issue of methodology. They often tried, with varying success, to apply the methods used in traditional village research to topics of the working class, industrial society, and urban culture. They rarely succeeded in applying Marxism to ethnographic studies, and so they used evolutionist perspectives instead, which are relatively similar to Marxism in their historical reflection and the way they both work with stages of development of society. They often just gave up on attempting theoretical work, and instead focused on sub-topics and on descriptive, empirical work.

While the generation of ethnographers to emerge in the 1950s was taught by the first generation of rural ethnographers, who still managed to pass on the breadth and foundations of their theoretical knowledge, the social isolation and lack of contact with international trends in science during the communist period often took their toll on subsequent generations of ethnographers. The direction of research they pursued no longer had much in common with the original ideas of the

²⁷ Here I have in mind Chotek's and Niederle's joint study on the region of Moravian Slovensko (Chotek et al. 1918) and a series of studies by Chotek on the region of Moravian Horácko. The titles of some of Chotek's independent works sound very contemporary, for example, his *Ethnické rozdíly v anthropologii dítěte se zřetelem k Slovensku* (*The Ethnic Differences in the Anthropology of the Child – the Case of Slovakia*) (1922), even though they do not exactly contain what a contemporary social anthropologist would expect.

ethnographic movement. Their science had a retrospective focus and only rarely drew on the synchronic methods that had replaced evolutionism in the West.²⁸ The state of Czech ethnography, and especially its isolation from world trends, was criticised in the early 1990s by the anthropologist Ladislav Holý (Chorvátová 1991), and several authors have attempted to analyse the situation of Czech ethnography under communism. Shortly after 1989, Vanda Jiříková and Zdeněk Mišurec (1991) prepared a brief study that took stock of the situation, and perhaps the best-known critical work on this topic was written by David Scheffel and Josef Kandert (1994). Lydie Petráňová (2000), focusing on the period 1953–1963, and Olga Skalníková (2005) and Petr Skalník (2005a) have also written some stimulating texts on this topic. But no more complex treatment of the question has yet been attempted.²⁹

Given that Czech ethnography was influenced by positivistic and historical trends up until 1989, it is no surprise that immediately after 1989 an attempt was made to quickly change research methods in this field. The first generation of ethnographers to emerge after 1989, who were already calling themselves social or cultural anthropologists, often tried to adopt an interpretive, ahistorical, culturally relativistic and anti-evolutionistic approach. They were extremely critical of ethnographers and they frequently linked the nationalistic ethnography of traditional rural culture to the ethnographers of the communist era. However, this reaction only partially brought Czech social anthropology closer to world events. While British and more so French anthropology were beginning to increasingly take history into account and trying to synthesise synchronic and diachronic methods, the new, post-communist generation of anthropologists was just beginning to oppose historicism and evolutionism. While internationally the trend was towards trying to attain a balance between the culturally relativistic approach and the concepts of universal values and evolutionary concepts, in the 1990s a generation of staunch cultural relativists emerged in the Czech Republic. While research elsewhere in the world was trying to find a compromise between description and interpretation, in the Czech Republic we began to come across interpretation without rules and above all without description.³⁰

This first generation of anthropologists to emerge after 1989 focused its criticism on ethnography – *národopisci*. This was very convenient, as ethnographers had no place from which to launch their defence and no means of doing so. A ‘mob’ of irate social anthropologists enthusiastically exorcised a non-existent opponent, celebrated their victory, and were able to blame the damaging influence of ethnographers as responsible for their failures. This was a secure and almost touchingly childish game. This situation only began to change at the end of the 1990s, particularly in connection with the rise of a new generation that was systematically in touch with what was going on in anthropology outside the country. After spending six months or a year abroad some of these students returned to the Czech Republic and began to take an interest in Czech ethnography or ethnology and began writing their seminar papers or dissertations on topics in the field of Czech

²⁸ In this respect ethnographers focusing on everyday culture and folklorists studying popular literary, musical, dance and theatrical forms of expression were in a somewhat different situation. Ethnographers used historical retrospectives, while in folklore analyses a structuralist approach was also present throughout the communist period.

²⁹ A potentially good starting point for further analysis is a meticulously prepared study by Lydie Petráňová and František Bahenský, *The Institutional Foundations of Czech Ethnography in the Years of Building Up Socialism and an Outline of the Main Periodicals* (2002), in which there appears a surprisingly high proportion of publications prepared by Czech authors during the socialist period on the basis of study outside European territory.

³⁰ This was not, however, the case at every institution. At the Institute of Ethnology of the Academy of Sciences of the Czech Republic, for example, research on foreign and Roma communities commissioned by state and international institutions stimulated researchers to use a combination of quantitative and qualitative research and detailed description methods (Uherek 2002; 2004).

ethnography. This was logical, as the local history and foundations of this field were a topic of interest to their colleagues abroad and one that Czech students were in a position to contribute to describing and interpreting (though most lost their enthusiasm for this historical retrospective over time).

Czech anthropology is probably becoming increasingly in step with the trends in social and cultural anthropology abroad. However, that does nothing to alter the fact that *národopisci* are rare birds, and in Czech academia will probably continue to be so. The *národopisci* are gone and they won't be back; all that there will be is good or bad anthropologists in their fields. *Národopisci* will neither harm nor help them. However, as at home anthropology flourishes there is bound to be a revival of various approaches used by the soon-to-be extinct *národopisci* and this will probably confirm that anthropological and ethnographic approaches were never as remote as may have been thought. Solid anthropological fieldwork today produces the kind of data that *národopisci* once used to collection.

'Franglus' Anthropology and East European Ethnography: the prospects for synthesis

*Katherine Verdery*³¹

Chris Hann raises a number of useful points in his article about social anthropology in Central and Eastern Europe; I find myself in agreement with many of them. In my comments, I will expand upon his remarks about the relations between social anthropology and the national ethnographic traditions of East European countries. Although I share his hopes for a convergence of the two, I argue that developments to date suggest this is doubtful.

As is well known, there was no equivalent to social anthropology before or during the socialist period in Central and Eastern Europe (henceforth, CEE); there was only what Hann (following Hofer [1968]) calls "national ethnography." After 1989, in each of these countries various scholars acted to bring in a new discipline – Franco-Anglo-US anthropology (hereafter 'Franglus,' which I prefer to the troublesome term 'western') – that, they hoped, would displace the older ethnographic tradition. Hann states outright his lack of sympathy with this effort and promotes, rather, "a mature synthesis of *Volkskunde* and *Völkerkunde*" as the most appropriate anthropology for CEE. He envisions a "cosmopolitan department of anthropology" containing "sub-groups with a *Volkskunde* orientation" operating alongside other sub-groups. In principle, this is a good idea,³² but given the realities of the postsocialist environment, I think it very unlikely that this anthropology will be institutionalized soon. Let me explain why, in hopes of promoting further discussion of this fascinating issue.

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³² I also share his self-criticism of his arrogant disdain for native ethnography in our own early field experiences in Hungary, Poland, and Romania. At the time, I saw Romanian ethnographers' work as antiquarian, and I believed my use of 'theory' – in my case, world-systems theory – marked my own work as superior. I have since become more humble, especially in my collaborative project with two Romanian ethnographers.

My thoughts on the matter were provoked by Michał Buchowski's paper *Hierarchies of Knowledge in Central-Eastern European Anthropology* (2004). In it Buchowski criticizes western scholars who have worked in CEE for not citing the work of indigenous scholars, engaging with them intellectually, or including them in edited volumes. Why, he asks, was/is there not greater overlap of the scholarly discourses of, and fruitful dialogue between, CEE-style ethnography and Franglus anthropology? Buchowski's way of posing the question helps to show why the matter is more complicated than Hann indicates.

We might begin by reviewing the arguments of Tamás Hofer (1968) and Joel Halpern and Eugene Hammel (1969), who distinguish the place of anthropology in an *empire*-building project (Franglus) from that in a *nation*-building project (CEE). The colonial context enabled Franglus anthropologists to study 'others' and encouraged knowledge that was *comparative* (from different colonies) and *theoretical* (creating models that might apply in other colonial contexts). Franglus anthropology emerged well *after* the consolidation of west European national states; it could play little role in nation-building. By contrast, the national ethnography that developed in Eastern Europe was quite different. It took shape *together with* the creation of national states across the region, as 19th-century movements of national liberation threw off Ottoman, Russian, Prussian, and Habsburg overlords. New elites built up national cultures by looking to the 'folk' to reveal the nation's original character. For this purpose, neither *comparison* nor *theory-creation* was useful; it required *close description* of local traditions, instead.

To overstate the situation: Franglus anthropology was the creation of overseas *colonizers*, CEE ethnography that of Europe's *colonized*, with expectable differences of emphasis. It is scarcely surprising that there was not much overlap of these discourses and that dialogue was difficult, as Buchowski rightly claims. Indeed, it could hardly have been otherwise. Therefore, I find Hann a bit naïve in proposing that we should conjoin both kinds in single departments. To do so would require much work to suture together two traditions with such opposed origins, philosophies, and epistemologies.

Nor has the post-1989 context eliminated that difference, for the frame of the postsocialist period is neo-colonial. Does this mean that Franglus anthropology is once again a tool of empire? Is it true, as scholars like Buchowski claim, that "western anthropologists" are now imposing their discipline upon CEE? This is a complex question, into which Peter Skalník's edited book *The Struggles for Sociocultural Anthropology in Central and Eastern Europe* (2002) offers unexpected insights. These contributors (all from CEE) tell us that Franglus anthropologists are not trying to *impose* their anthropology on CEE: rather, the impetus comes from CEE scholars trying to *import* it. According to these papers, the postsocialist era offers an opportunity for would-be anthropologists in CEE to achieve upward mobility and to gain access to western benefits such as grants, trips abroad, etc., by building up western-style anthropology as a symbol of 'democratization.' There have been two main forms of doing so, the book argues. In one, some indigenous ethnographers and folklorists began calling themselves "anthropologists" or, at least, ethnologists; they did a bit of reading and then created programs in anthropology (or, sometimes, ethnology) that did not differ much from what they were doing before. In the second, local scholars from various disciplines who had already been reading 'Franglus' anthropology, or who returned home with Ph.D.'s from 'Franglus' universities, tried to set up their own anthropology programs, labeling those older ethnographers 'false anthropologists.' Unsurprisingly, battles over 'authenticity' ensued, to establish whose anthropology was 'correct.' In the fierce competition to

set up anthropology programs, competitors seek to draw in Franglus anthropologists for help with syllabi, books, short courses, fellowships, invitations to visit, etc., each by claiming to be the only 'real' anthropologist. Traditional ethnographers, however, have resisted this move: nearly all the papers in Skalník's book complain how difficult it is to institutionalize Franglus anthropology and create jobs in it, against the opposition of already-entrenched native ethnographers. Skalník's report of his own futile efforts to build Franglus anthropology in the Czech and Slovak republics is particularly revealing.

I myself was asked to participate in building anthropology at the state university in the Romanian city of Cluj, and I attended several meetings there in the Sociology Department, which had initiated the new program. (At one meeting, someone commented – only partly in jest – that the actual *content* of the program really didn't matter; the point was for *their* program, instead of the proposed programs in the universities of Timisoara, Iasi, and Bucharest, to capture the symbol 'anthropology' and to gain academic allies in 'the West.')

There were three main contenders for creating anthropology in Cluj: an out-of-favor old-regime sociologist looking for a new source of status; the head of the Ethnography Institute, who had long experience of excellent work in that tradition but not much exposure to Franglus anthropology; and the new head of the Sociology Department, who appointed a largely self-taught young woman with extensive reading in contemporary Franglus anthropology. The university rector backed her and encouraged her further training, but during her studies abroad, the head of the Ethnography Institute took over her position. As things developed, I was struck by how specific the outcome was to a chance combination of local alliances, timing, personal talents, and risk-taking. In other Romanian cities the anthropology being built would be different, based in different constellations of disciplinary and personal relations.

Another experience illuminates the issues further. In 2001 I was among a group of scholars invited to the Central European University in Budapest, where the rector was planning to create an anthropology department and wanted some guidance about what it should look like. The group included one or two aspiring anthropologists from each CEE country and Russia, as well as a handful of anthropology professors from the U.S. and U.K., making a total of about 20 participants. In a particularly revealing moment, people introduced themselves and described the backgrounds from which they had come to anthropology: very few had backgrounds similar to anyone else's. One came from philosophy, another from history, another from ethnography, from folklore, from philology, from geography, from sociology, even from chemistry; a few had some formal training in Franglus anthropology, others had read in it on their own; some had read in a variety of disciplines and thought anthropology was a good way to bring them together. The question of 'authenticity' shadowed every discussion; our advice to the rector was rather unfocused; and the CEU ended by forming a department of sociology and social anthropology, staffed largely by East Europeans trained in western European or U.S. universities. Native ethnography, as well as philology and history, apparently dropped out.

This experience prepared me for the picture painted by Skalník's collaborators, who show us *multiple* 'anthropologies' in Eastern Europe, all weakly institutionalized, and shaped differently in the different countries as a function of the departments from which the new programs are emerging and the disciplines of those attempting to create them (sociology, philology, philosophy, ethnography, history, etc.). Even different universities within a single CEE country have different anthropologies, as a function of local fields of power in the various cities and in their universities.

In this context, Hann's optimistic vision for a synthesis of western and indigenous anthropologies appears oddly unanalytical. First, it would be easier to realize his vision if CEE 'anthropology' were less multi-vocal and more securely institutionalized than it is so far. Second, if Skalnik's team is right, then we might ask, *cui bono* is the attempt to create Franglus anthropology in Central and Eastern Europe, and what does that imply for the form anthropology will take? It is not 'westerners' who are driving this convergence in a neo-colonial way; the incentive structures in our home contexts neither promote nor reward such an effort. It is CEE scholars, battling one another to institutionalize some kind of anthropology. The battle has not two but many protagonists, their identities and institutional positions widely variant from one country to another. In this battle *it is precisely the relationship of western-style anthropology to national ethnography that is at stake* – hardly a context in which one can simply bring both into a single department with subgroups for the different types, as Hann proposes.

It is likely that we will end with multiple national anthropologies – much like Franglus anthropology itself – each differently shaped by one or another kind of French, British, and U.S. tradition. Their histories will offer fascinating comparisons for a future ethnographer.

Reply: not a bland hybrid but a spicy consortium

Chris Hann

I

I thank all the commentators for their interesting remarks on my essay. Let me begin my defence by pointing out that the initial approach from the Editors in November 2005 was an invitation to contribute a general Introduction to this special issue. I was sent synopses of the planned articles, which seemed to provide good examples of how anthropologists can contribute to the analysis of contemporary social change, in this case the changes which have followed the demise of socialism. But rather than assess yet again the merits and drawbacks of the term postsocialism or take up some topic of current debate inside socio-cultural anthropology, I thought it might be more interesting for the readers of this sociological journal if I took the opportunity to reflect on more fundamental issues, namely the range and purpose of anthropology as a discipline and its present situation and prospects in Central and Eastern Europe (hereafter CEE). Had I anticipated that my text would be sent out for comments to such a distinguished forum of anthropologists, I would doubtless have made some points more cautiously and elaborated others in greater detail. I should certainly have tried to be less parochially British (as Petr Skalník notes I am in fact a Welshman, though that is not the main reason for my discomfort with Katherine Verdery's term 'Franglus').

However, a full year later I am not inclined to climb down on any of the main issues. I interpret most of the comments as broadly consistent with my arguments; some introduce useful elaborations and stimulating alternative perspectives. In a few cases I disagree and feel I might have been misunderstood. Since I do not have space to respond to every detail in the ten comments, I shall proceed selectively, addressing what seem to me to be the most interesting points raised by my critics, and then concluding with a restatement of my views on the future of anthropology as a discipline, in CEE and elsewhere. Let me begin with a few preliminary observations.

First, it was no doubt to be expected that most of the commentators would devote more attention to what Aleksandar Bošković calls the 'anthropology of anthropology' in CEE than to questions

concerning substantive intellectual agendas. (For example I am disappointed that only Michael Stewart seems to have noticed the kite I flew on behalf of social evolution.) I feel compelled to follow this bias in my response but I hope that readers will not lose sight of the central issues, in particular the need to address the past more adequately than has generally been the case in modern British social anthropology if we are to provide more satisfying accounts of the complexities of social transformation.

Second, the spectrum of criticism is limited inasmuch as it seems that the Editors did not see fit to invite comments from scholars who identify strongly with the traditions which I shall continue to refer to here as national ethnography (well aware that this is a simplification of a complex intellectual landscape). Unlike Zdeněk Uherek I believe that there is significant resistance to socio-cultural anthropology (also an artificial simplification). It might have been interesting to hear their voices. (Of course it is possible that the Editors did solicit the views of dyed-in-the-wool national ethnographers, but the invitation was not accepted.)

Third, the ten commentators nonetheless represent very diverse positions, some of them complementary and some of them contradictory. For example, Michał Buchowski denies that the national ethnographers were overwhelmingly preoccupied with the peasantry while Uherek confirms my view that this was the case, at least before the socialist era (in at least some places I think that this has remained the principal focus to the present day). Yet the commentaries of both Buchowski and Uherek, scholars who have pursued their academic careers in Poznań and Prague respectively, are basically in line with my own view that we should conceive of anthropology as a unified field, subsuming both national ethnography and socio-cultural anthropology. To my mind it is no accident that the commentators based in English-language institutions tend to the opposite position, i.e. they emphasize the gulf that separates ‘western’ (at the end of her comment even Verdery finds it impossible to avoid this word) anthropology from CEE traditions of national ethnography, and give at least implicit support to the view that a new discipline now has to be imported. There are, however, exceptions to this pattern. I am reassured to find that Stewart, like myself an outsider, also takes the view that, at least ideally, the various strands belong together. On the other hand Juraj Podoba, in this sense clearly an insider, insists on ‘essential disparities’ between national ethnography and the ‘modern, theoretically and methodologically elaborated social science’ that, in his view, should now be separately institutionalized. Of course some participants in the forum, notably Skalník and Bošković, have biographies that resist any simple insider/outsider classification; so let me at this point abandon the search for a neat pre-emptive sociological explanation for the various viewpoints articulated by my critics and turn instead to the details of their comments.

II

I am in full agreement with most of Buchowski’s contribution, especially concerning the present political predicament of anthropology in CEE. This is not surprising, since I have long regarded his own Institute in Poznań as a good example of how various traditions and sub-disciplines in anthropology can co-exist fruitfully within a single structure. He is certainly right to draw attention to Poland’s rich scholarly tradition. This case, like the case of former Yugoslavia, discussed by Bošković, provides plentiful evidence to refute Skalník’s blanket characterization of ‘nationalist disciplines’. My main quibble with Buchowski’s generous comments is that I do not think that to distinguish between ‘differential rates of change’ in different social domains is bound to lead to the

sin of teleology. It is surely possible to reject the simplicities of nineteenth century unilineal evolutionism and of twentieth century modernization theory, and yet still to pursue systematic linkages between different social domains in a comparative framework. For example, sociological statistics have long indicated that religious belief and practices in Poland are ‘out of step’ with changes in other domains, when compared with the patterns in neighbouring countries. Anthropological research such as that of Esther Peperkamp (2006) can give us fine-grained insights into both manifestations and causes of such phenomena.

The related issue of ‘un-simultaneities’ (this term is unfamiliar to me; I assume it is derived from the German *Ungleichzeitigkeiten*) lies at the heart of Don Kalb’s criticisms.³³ Kalb is surely right to point out that some of the best European scholarship in historical anthropology has come from Holland, and that my article paid little attention to these and other significant historicizing trends in recent decades (such as the numerous studies of South-East Europe inspired by the Austrian historian Karl Kaser, noted in the comment of Milena Benovska). However, some parts of Kalb’s commentary leave me puzzled. Given his own interests in power relations and international political economy, it is no surprise that he proceeds by targeting the field of folklore, a term that has carried a stigma for all social anthropologists ever since Malinowski displaced Frazer. Indeed what Kalb offers in his comment is basically a spirited replay of the functionalist critique of the Frazerian interest in survivals, except that Malinowski’s concern with synchronic function is now replaced by ‘local history and moral economy’. Kalb supports his critique with detailed evidence taken from a collective volume that arose out of the first round of projects in my department at the Max Planck Institute for Social Anthropology (Hann *et al.* 2003). While appreciating his attentive reading of these studies of rural decollectivization in postsocialist contexts ranging from eastern Germany to China, I think some of his criticisms are misplaced. Since they raise basic issues concerning the nature of anthropology as a discipline I shall respond in detail at each of the levels addressed by Kalb, micro and macro.

At the micro level Kalb objects to my invoking the persistence of a strong peasant attachment to the land in explaining why the villagers of Tázlár resented the consolidation of fields by the management of their socialist cooperative in the mid-1970s. My main reason was simply that this is what people told me at the time and later, and I take their explanations seriously (which is not to deny that local accounts may be misleading, inadequate or downright false by the criteria of a comparative social science). How can Kalb interpret this as an argument for the survival of a primordial peasant ethos or mentality? In my monograph (Hann 1980) I outlined the formation of this community in the late nineteenth century. Even in the short co-authored comparative article cited by Kalb the property ideology is given temporal and regional grounding. It is essential to appreciate the historical context in order to understand why attachments to the soil were so much stronger among those born before the socialist era than among those who reached maturity after collectivization in 1960. Of course the farmers who protested in the 1970s also had an economic interest in holding on to their use rights over the land, which the cooperative wanted to remove from their control; but it was impossible for the fieldworker to overlook the strong emotions

³³ Both Kalb and Buchowski, suggest that I add Marshall Sahlins to the list of anthropologists who have engaged seriously with history. While I have much respect for Sahlins, I prefer the materialist evolutionism of his early writings to the structuralist idealism that has dominated his work since the 1970s. His work on Hawaii celebrates the enduring power of structure and leaves little space for changing political economy. I am much more sympathetic to the work of Eric Wolf, also cited by Kalb, and I agree that Katherine Verdery has been a splendid representative of this tradition in her work on Transylvania.

expressed by those peasants. At that point in time their material and emotional motivations were congruent. I don't know how a vulgar materialist could explain the behaviour of some of the same farmers when they sought to regain full ownership rights over the same fields in the early 1990s. In the adverse economic climate of those years it was clear that land in this region of poor soils would be a liability rather than an asset; yet some old men prevailed over their wives and sons, insisting that they had a duty to their forefathers to regain ownership of *their* land, regardless of economic rationality. The appreciation of this sentiment, so puzzling to most economists and the bureaucrats in the capital who drew up the blueprints for Hungary's decollectivization, is an example of how the fieldworking anthropologist can provide insight into issues with far-reaching consequences, in this case for the postsocialist agrarian economy, on which the livelihoods of millions continue to depend.

As I mentioned in the essay above, my appreciation of such factors in the case of Tázlár has been enriched in recent years through my reading of the work of Antal Juhász. This national ethnographer has provided invaluable contextualization for resettlement processes in this region over the *longue durée* – very much the 'local history' that Kalb himself in one breath seems to praise. The property values which crystallized in the capitalist migration processes of the later nineteenth century were transmitted for most of the twentieth century, such that even after forty years of socialism they were still strongly present in certain groups. If it is 'folkloristic explanation' to point to such factors, then I am a folklorist! It seems to me that, if we look around us, most human communities, including the communities of academic institutions, have comparable continuities and comparably uneven patterns of transmission. But perhaps the shining novelty of the Central European University prevents its staff from developing the scholarly equivalent of the peasant moral economy (incidentally I am not aware that Karl Polanyi ever used this term).

Kalb is no less dissatisfied with our volume at the macro level, since the authors failed to report on the demise of peasantries in other parts of the world. Obviously it was incumbent on me in my introduction to frame the postsocialist case studies by outlining 'the general story', and my brief discussion of the changing agrarian economy worldwide was insufficient. Kalb is entitled to that view, but the fact is that the more space anthropologists devote to the level of international political economy, the less space we have left for ethnography of the kind I just illustrated, which presents glimpses of those elusive 'flesh and blood' actors that other disciplines ignore. As David Scheffel notes, I certainly do not wish to give up on fieldwork. I think this has been the great strength of western anthropologists who have worked in the CEE countryside, enabling them in the best cases to tell ethnographers in the major cities something new about their own society. As it happens I find the global scenario sketched by Kalb highly compelling. He might be interested to hear that some villagers in Tázlár do as well. Indeed, much of his diagnosis would be considered commonplace by those who have to struggle with the resulting 'ambivalences' in their everyday lives. Of course there are all kinds of ways in which anthropologists can join in debates about the current directions of neoliberalism. I argued in my essay that we should pay more attention to history, but I think that our contributions to understanding details of the 'global marketization process' must also remain grounded in our fieldwork, including enquiries into highly localized refractions of the theory of political economy. However excellent the work of David Harvey, I do not think that he or many of the other scholars cited by Kalb would claim to be anthropologists.

Like Kalb, Verdery declares CEE traditions of national ethnography to be radically different from the discipline in which she works; she also seems to favour similar practical strategies. I am

grateful to her for drawing attention to the early article by Halpern and Hammel, but like Buchowski I take the view that all of these Americans have drawn the lines rather too sharply. In any case I did not argue that the two traditions were the same and am happy to assent to Bošković's alternative diagnosis of 'closely related'. But even if the epistemologies of the great majority of national ethnographers are radically different from those of the majority of 'Franglus' anthropologists, why should that have to lead to separate institutionalization? The epistemology of Ernest Gellner was undoubtedly very different from that of, say, Clifford Geertz or Marshall Sahlins, or Eric Wolf, but that does not prevent us from recognizing them all as having made contributions to socio-cultural anthropology. When I was a junior colleague of Gellner in Cambridge in the 1980s I don't think anyone in the department embraced the epistemology and vision of its head: but this made for a highly productive department. (Incidentally Podoba is wrong to suggest that Gellner supervised significant empirical research in CEE or indeed anywhere else in the socialist and postsocialist worlds.)

Though I have never experienced the sort of unacceptable treatment that he reports in his comment, I share Scheffel's basic concern for the future of anthropology as a discipline, in CEE and elsewhere. I am grateful to him as well as to Uherek and Skalník for expanding my knowledge of the history of anthropology in Bohemia and for helping me to place the 'struggles' of recent years in historical context. My principal quibble with Scheffel's comments is that I feel that he (like Skalník) goes far too far in condemning the 'parochialism' of the local scholars. Buchowski gives contrary examples from Poland, Bošković from former Yugoslavia, and I can think of others in Hungary. To take an extreme case, the East German Marxists who theorised modes of production in the framework of a Marxist philosophy of history were rather isolated internationally, but I would not call them parochial; their counterparts in the *Volkskunde* tradition were arguably even more innovative (Noack, Krause 2005).

The main argument of Uherek's paper seems to be that Czech scholars have yet to develop adequate methods to deal with social change. I sympathize with his complaint that the concept of 'postsocialism' is sometimes used loosely and crudely. One cannot reduce all social explanation to the legacy of the preceding era. Of course we need more nuanced accounts of the present, and we must recognize that the long years of 'normalisation' do not have the same meaning for those who are too young to have experienced them. At the same time Uherek himself gives some indication of just why the 'shadow' of socialism may remain more significant in the Czech case than in some others.³⁴ As for his thumbnail sketch of disciplinary history, it contained two surprises for me. First, I find it a little hard to accept that the founders of the Czech discipline were entirely dispassionate professionals, 'not influenced by nationalism' but merely making their scholarly contribution to its genesis and development. Second, I was surprised to read that the *národopisci* 'lost direction' under socialism to such an extent that they were a 'non-existent opponent' for the advocates of a new anthropology in the 1990s. This rather contradicts the picture I have gleaned over many years from Skalník (e.g. 2002, 2005a), and which he restates in his critical comment above.

³⁴ However, I cannot entirely endorse Uherek's analysis of Czechoslovakia under socialism. I visited Prague for the first time in 1972 and I followed the scholarly literature on 'normalisation' in the years that followed. Conditions were certainly bad, especially for intellectuals, and I am well aware that the fieldwork which I as a Westerner was able to carry out with considerable freedom in the 1970s in Hungary and Poland would have been impossible in Czechoslovakia. Still, one only had to look at the Supraphon catalogue, from Bach and Bedrich Smetana to Bob Dylan, to realize that even the official cultural scene was far from 'sub-standard'.

The contributions of Skalník and Podoba appear to contradict the pattern I suggested in my preliminary remarks, since these ‘local scholars’, based in the Czech Republic and Slovakia respectively, are severely critical of national ethnographers and favour the institutionalization of a new discipline. Helpfully, both scholars divulge enough personal information to give the reader insight into how they have come to reach this position. It appears that Podoba has had a change of heart on the key issue as a result of ‘very specific behavioural strategies adopted by Central European ethnographers’, by which he means that his colleagues in Slovakia have resisted his own efforts to modernize (i.e. ‘anthropologize’) national ethnography. This leads him to assert ‘fundamental differences’ between the two camps. Yet both Podoba and Skalník believe that good anthropologists must be attentive to history and can find much of value in the works of national ethnographers. Podoba hails western anthropology as a modern social science but argues somewhat schizophrenically that to illuminate certain contemporary issues it is important to delve back into the pre-communist era; that may not quite be *longue durée* but it is rather more than most ethnographers of postsocialism tend to offer. I regret that institutional factors and bureaucratic inertia have frustrated the efforts of these scholars to create departments more in tune with their own predilections (which are also my predilections); but I see no reason to believe that the overnight creation of rival new departments with new names would bring a solution to their problems.

Skalník asks ironically how much I know about the folklore of my native Wales. The answer is: virtually nothing. I am not proud of this. When I began to study social anthropology in Cambridge in 1974 Alan Macfarlane was lecturing on witchcraft and kinship in England. I found this very stimulating, though it was very unusual in British anthropology and Macfarlane himself eventually moved on to other themes. When Skalník insists on the achievements of the ‘revolutionary theory and method’ of Malinowski, I am basically in agreement with him. The shift, in simplified terms a move away from history and folklore towards social science, brought abundant fruits in succeeding decades; but, as Skalník himself reminds us, synchronic functionalism was never without its problems. Contrary to Stewart’s reading, my essay was not meant as an unqualified ‘hailing of the Frazerian legacy’.³⁵ I have never argued that today’s students of anthropology should be obliged to read *The Golden Bough*, not even the abridged edition; but like Stewart I do indeed think it is worth attempting to resume a dialogue with some contemporary evolutionary theorists. In short, it is time to take forward the dialectic set up by Malinowski, and to re-incorporate what was valuable in the positions he so flamboyantly rejected.³⁶ Despite their polemical tone it seems to me that, at least as far as history is concerned, even my sharpest critics in this forum endorse a similar position.

In addition to her stimulating comments on historical methods, Benovska points to a basic practical consideration. In Bulgaria (as in most of CEE) there is little or no tradition of research in ‘exotic’, non-European societies. A lack of qualified staff and resources precludes any speedy emergence of anthropology departments with a global range in teaching and research. The result in Bulgaria, according to Benovska, is that two opposing camps are obliged to co-exist, because they are too poor to afford a divorce. But this couple only came together a few years ago, and perhaps

³⁵ It would of course be a mistake to assimilate the comparative armchair enquiries of Frazer with the researches of the national ethnographers, most of whom have always had a strong commitment to fieldwork.

³⁶ It is worth pointing out that Malinowski maintained close contacts with the national ethnographers of his day in Poland, published much of his early work in their journal, and appears to have considered accepting an appointment at the Jagiellonian University in the early 1920s. This fact should caution us against exaggerating the gulf between CEE and the west: the history of our field would have been very different in both sectors had the London School of Economics not come up with a more attractive offer.

they should be wary of hasty decisions. Benovska's own work suffices to show younger scholars that it is certainly possible to combine interests in local folklore with comparative research into the burning contemporary issues of 'postsocialism'. She personifies my argument that bridge-building is both possible and necessary. However, let me make clear that I did not mean to advocate that to be a good anthropologist it is necessary to master the full range of all traditions. Rather, taking the university department to be the key academic unit, I argued that different strands can flourish alongside each other. Considerable 'blurring of genres' (Bošković) has already taken place as some socio-cultural anthropologists pay more attention to local history and many ethnographers adapt new theories and give up 'self-exotisation and the aesthetisation of one's own culture' (Benovska); but there is no reason why sub-groups should not maintain their distinctive identities for many years to come within a unified departmental structure, and that is what I argued for in my essay.

III

At several points in the preceding section my discussion slipped inevitably away from the intellectual issues to more pragmatic questions. Stewart's comment raises this dichotomy most explicitly. He agrees with me that the western anthropologists and national ethnographers belong together in principle, but in the end, like Verdery and Kalb, he concludes that building the new anthropology is a matter of selecting the most suitable local allies (backing 'winners', as Stewart puts it, 'without prejudice as to where their intellectual home is'). Thus in Sofia the partner might be Cultural Studies, while in Cluj it appears to be Sociology; in Szeged and Pécs some colleagues think it should be Media Studies, and perhaps in some obscure places there could even be a liaison with folklore. Verdery expresses the enthusiastic hope that out of all the current academic politicking a rich and variegated landscape will be formed, to be studied by future ethnographer-historians. But I worry about the consequences of such *ad hoc* alliances. I have experienced a little of the factional struggles to which Verdery makes reference and I share her impression that CEE scholars of varying hues have been engaged in a struggle for custody or possession of 'anthropology', without always caring sufficiently about its intellectual content.³⁷ But how long will the prize be worth coveting if anthropology comes to mean something quite different, even between different institutions within the same country? How can such an anthropology maintain what Podoba lauds as its 'unambiguously clear and comprehensively defined methodical and methodological framework'?

I would like to agree with Uherek when he suggests that, far from the widely reported polarization of separate intellectual communities, we should be able to look forward to a future in which 'all that there will be (are) good or bad anthropologists in their fields'. But I think the conclusion that most readers will draw from other comments in this forum is that the present situation of anthropology in CEE remains confused, since both the name and the content of the discipline are hotly contested (cf. Skalník 2002). Several of the commentators reinforce the binary perspective that there are at present basically two categories of anthropology and anthropologists in

³⁷ I agree with Verdery that, in the present battlefield, it makes more sense to acknowledge the internally competitive energies of the local scholars than to accuse western anthropologists of seeking to impose their paradigms. In the essay that she cites by Buchowski, he insinuates that western anthropologists have no business writing about CEE at all unless they engage carefully with the works of local anthropologist. This, as Scheffel and Bošković point out, pushes in the direction of the strong claims made in various forms of Native Studies (Aboriginal Studies etc.). I am unsympathetic to this stance. On the other hand I take seriously the concern expressed by numerous commentators above (notably Skalník, Podoba and Bošković) that some Western authors pay no more attention to the works of new cohorts of local socio-cultural anthropologists than they did to earlier generations of national ethnographers. Needless to say the problems associated with these 'hierarchies of knowledge' are by no means unique to CEE.

this region (and implicitly in many other parts of the world). In the one camp we find the so-called 'local scholars', those whose training and research expertise is overwhelmingly confined to their own country. If they make use of foreign works at all, it is likely to be those of a very few 'classical' figures whose works are available in translation. In the second category are the cosmopolitan anthropologists who effortlessly master foreign languages and know how to develop theory and comparison. But in my experience such a dichotomy is hardly an accurate description of the present situation. The basic requirement for a western PhD student working on CEE is that she or he spend a year or two 'in the field' gathering data and write a dissertation that addresses some current theory or debate in the international literature in order to satisfy the requirements of a motley campus committee. Local linguistic and historical competence is often limited (some candidates spend much or even most of their fieldwork time in a large city where they communicate primarily in English). Neither the theory nor the method of Malinowski provides any touchstone nowadays, and few examiners attach much significance to the criterion of comparison. Is the dissertation which emerges at the end of the day, usually referencing overwhelmingly English language sources, so much less parochial than its 'home' equivalent?

A genuinely cosmopolitan anthropology would be different. We should at least be able to dream of a world in which a student embarking on a field project in the Czech Republic, regardless of whether she or he has Czech as a native language, might take theoretical inspiration from Poles such as Czarnowski and Dobrowolski and consult studies undertaken in Hungary and Romania for comparable empirical materials. Of course this is not quite realistic. Initiatives such as that of *European Association of Social Anthropologists* to make major works written in 'obscure languages' more readily available through translating them into English are to be encouraged, but they are not going to solve the problem.

I argued that the consolidation of mixed departments can help to bridge the present gaps (I would add here, not only the gulf between CEE and the west but also divisions within CEE). Several commentators dismiss my perspective as utopian. But my arguments are not predicated on any assumptions of altruistic behaviour by scholars (*pace* Stewart I suspect that altruism is perhaps a rarer trait among anthropologists than among the people we study). On the contrary, to expand the profile of the traditional departments of ethnography (and not merely to re-label them) is obviously in the interests of all concerned, at least beyond the short term. If the trend reported by Benovska for Bulgaria is confirmed elsewhere and students prefer to enroll for courses that reflect the new western influences, then certain national ethnographers will sooner or later have to give up their obdurate resistance: they will have to appoint new staff with different orientations and modify established curricula, or else face the dissolution of their departments. It ought to be possible to facilitate this process by dispensing with disparaging labels such as 'nationographers' (Skalník) or 'local scholars', and accepting that the diverse intellectual communities of the national ethnographers are no less legitimate than the diverse agendas of outsiders. Closer inspection often reveals that the trumpeted novelty of the new western product has significant local forerunners. Holding all the various groups together in one institution seems to me the best way to ensure cross-fertilization.

Anthropology will surely remain a fluid and essentially interstitial discipline, but there needs to be some core agreement concerning intellectual agendas. The discipline cannot define itself solely with reference to the fieldwork method and continue indefinitely 'happily producing ever greater numbers of local studies' (Stewart). Of course what Verdery calls 'multi-vocal' anthropologies

have always been shaped by multiple elements of contingency. More generally, the ways in which internal and external boundaries have been drawn by the various social and historical sciences reek with arbitrariness. I have made my own intellectual background very clear. British social anthropology was an intellectual community that poached creatively across all kinds of boundaries for much of the last century without losing its own sense of identity. That is the community with which I identify most strongly, and I like to think that some of its work deserves to find a place in the core curriculum of anthropology programmes anywhere. But if I were asked to design a department of anthropology in CEE (or anywhere else in the world), it could not replicate this British model. Instead, I would begin by stipulating that several members should have historical expertise in the ethnography of the country in which the department was located. Other colleagues would have other regional and thematic specializations: some might be well read in global political economy, and there might indeed be room for a psychological and/or biological anthropologist, as requested by Stewart. But if just one post were vacant and I could appoint only one of these specialists, then despite my personal interests both in the questions of class and power which fascinate Kalb, and in the universal foundations of human sociality which Stewart urges us to prioritize, I would opt for the ethnographer (but in the hope that the Department of Political Economy, along with others such as Archaeology, History, Sociology, Psychology and Biology, would not be located very far away, so that I could attend their seminars when I wished; there is no doubt that all have much to contribute to anthropological agendas).

I conclude by reiterating my conviction that the future of anthropology in CEE lies in unified departments that will build on existing foundations in ethnography/ethnology/folklore. Of course there will be difficulties, but it seems to me that we have here the unusual situation in which ethical, practical and intellectual considerations all point in the same direction. It is ethically preferable to respect the accomplishments of existing communities of scholars and seek to build upon them, rather than to sweep them aside through the imposition of a new western hegemony. It is pragmatically rational to consolidate the institutional position of our field by working from the existing base, since the alternative exposes the subject to different alliances in every institution, is very demanding in terms of the time costs of building those alliances and persuading all the necessary committees to approve the new curricula, and will lead to a fragmentation that a relatively small discipline cannot afford. Finally, I have argued that there are also compelling intellectual grounds for favoring this path: in particular, the availability of in-depth expertise concerning the history of the country in which one lives should be of benefit to all, in both research and teaching.

Perhaps 'synthesis' was not the best term. What I would like to see emerge in CEE is not a bland hybrid but a creative consortium of distinctive clusters of scholars – which in my experience is exactly what the best anthropology departments in the English-speaking world tend to be.

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Appendix

A Response to David Z. Scheffel's Paper *The Past and Future of Anthropology in Central and Eastern Europe: notes on Chris Hann's vision*

In a critical comment David Z. Scheffel made on Chris Hann's text he selected me as an example of the persisting negative tendencies in Czech anthropology and ethnology, which emerged out of the reviving national aspirations in the period of Romanticism, became petrified during the Communist period, and to date continue to contribute to 'bending the results of science' and relinquishing the longing for 'truth'. The reason why I was identified with the above-mentioned 'post-socialist' specters constraining the spirit of criticism in the Czech Republic was my article 'Teaching and Learning Anthropology in the Czech Republic', which was published in *Educational Histories of European Social Anthropology* (edited by Dorle Dracklé, Iain R. Edgar, and Thomas K. Schippers) in 2003 by Berghahn Books. There were no scientific aspirations behind this short, seven-page text, which was created directly in response to a request from the editors. I did not regard it as the outcome of any real 'scholarship', and it was not even presented as such. The request, as I understood it, was to present a summary of contemporary anthropological and ethnological institutions in this European country, extending into the local historical and intellectual context, and to describe the challenges, tasks, and problems of the two disciplines. The text of 'Teaching and Learning Anthropology in the Czech Republic', which is accessible at www.ksa.zcu.cz, comprises a total of eighteen paragraphs. In the first paragraph I take a critical view – citing László Kürti – of the stereotypical 'homogenisation' of the central and east European space; in the second, through Ernest Gellner, I discuss the generally well-known nature of central European Romanticism and its reflection in local man-centred sciences, and in the final paragraph I make the first explicit mention of the article by Josef Kandert and David Scheffel published in 1994 in *Anthropological Quarterly*. Here I unfortunately committed two gross formal errors – I mistakenly cited the year of that work as 1992, and in the final version I did not notice that the editors had not included (I don't know whose fault this was) all of the endnotes in my manuscript. So, for example, it occurred that there was no reference that the authors of the translation of citations from *Český lid* and of the work by Jan Erazim Wocel are Josef Kandert and David Scheffel. I must self-critically admit that it would probably be impossible to make more mistakes in less space (to paraphrase Woody Allen, whose exact words I cannot unfortunately recall). I deeply apologise to Josef Kandert, David Scheffel, and other authors originally mentioned in my endnotes. The discussion that Kandert and Scheffel's article provoked at the time struck me as interesting and indicative of how the social sciences were taking shape after the fall of communism. I was convinced that it should not be omitted even from this brief excursion into the history of Czech anthropology and ethnology for non-Czech readers, little acquainted with the topic, and therefore I explicitly made reference five times to Kandert and Scheffel's article. The remainder of the chapter in the volume, which was a kind of 'yellow pages' of European anthropology, is made up of references to Lubor Niederle, Jindřich Matiegka, the Prague Linguistic Circle, Petr Bogatyrev, and the fate of the domestic academic community after 1948. Considering when the article 'Teaching and Learning Anthropology in the Czech Republic' was written, I rephrased Kandert and Scheffel's comments in a paragraph on the 1990s, and I also cited the title of their joint study 'Politics and Culture in Czech Ethnography'. In the conclusion of the text I listed across two pages the main

research topics and presented an overview of the most important Czech universities and academic institutions. The article 'Teaching and Learning Anthropology in the Czech Republic' clearly offers nothing new in the way of knowledge, and it is not the result of any research activity. Its purpose is purely informative.

Ivo Budil

Commentary

Prof. Ivo Budil's "Selected publications" found at <http://www.ksa.zcu.cz/osobni.php?IDWorker=73> (as of 12 February 2007) mentioned the article in question without casting any doubt on its scholarly status until the moment when Prof. Budil was alerted to this fact. Seven out of eighteen paragraphs contain passages copied from the article by Scheffel, Kandert 1994, and they go well beyond mere quotes of translations. The sections pertaining to L. Niederle and P. Bogatyrev were lifted from our article as well.

David Z. Scheffel