Serbian Village Culture: The Meeting Point of American Anthropology and Serbian Ethnology

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Studying Peoples in the People's Democracies II

Socialist Era Anthropology in South-East Europe

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This volume is a follow up to volume 8 of this series, which explored socialist era anthropology in East-Central Europe. On this occasion the countries investigated are Romania, Bulgaria and Serbia. In all three the discipline (irrespective of its local fragmentation) originated and developed as a ‘nation-building science’. Scholars drew on the model of German *Volkskunde* and there was little or no interest in comparative *Völkerkunde* researches.

The contributors to this volume outline how this intellectual endeavor was affected by formally internationalist but in substance deeply national versions of socialism. Anthropologists were able by and large to nurture and sustain their special relationship to the nation under dramatically altered conditions, reacting more or less skillfully to fluctuating political pressures and eventually finding a secure niche for themselves in ‘national communism’. Even though it was not instrumentalized in the same way by politicians and cultural officials, this national communism was found throughout the region. Anthropology was reinvigorated and institutionally consolidated as a science deeply concerned with national issues. There was considerable investment in ‘monumental’ projects, notably the expansion of ethnographic holdings in museums and the compilation of national atlases. Inevitably the ideological constraints set by the Communist Party led to theoretical and methodological distortions, but some creative scholars were able to develop individual strategies to overcome or at least evade them.

In order to provide a broad coverage of highly differentiated local landscapes, the editors invited contributions from scholars of different generations and institutional backgrounds, including for each country a distinguished American anthropologist to present a view from outside. Those of the older generation were encouraged to include personal reminiscences whenever appropriate.

Although the focus here is restricted to the anthropology of the socialist era, the chapters also provide some landmarks for understanding postsocialist developments in the field. However, evaluations of the scientific heritage of socialist anthropology remain burdened by the fact that memories of this era are still heavily laden with emotions and, at any rate in comparison with the former German Democratic Republic, no real coming to terms with communism has yet taken place in these countries.
Chapter 16
Serbian Village Culture: The Meeting Point of American Anthropology and Serbian Ethnology

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Beginnings

Serbian ethnology was founded in the late nineteenth and early twentieth century on three different paradigms: folklore as defined by the philologist and folklorist Vuk Karadžić, common-law research by the legal scholar Valtazar Bogišić and the anthropogeographic school developed by the geographer Jovan Cvijić. These were all rooted in Romanticism and instrumental in nation and state building (revised alphabet, standardized language, canon of national traditions), delineating ethnic boundaries (spatial distribution of cultural elements, migrations), and determining ethnic/folk specifics (type of social organization, customs and other religious practices). The formation of ethnology and its rise to the status of prestigious 'national science', together with history, art history, archaeology and linguistics, coincided with Serbia’s political liberation and aspirations of uniting all Serbs who had, due to historical circumstance, lived under different foreign domains (Ottoman, Habsburg and Venetian), within a single state (the ‘Greater Serbia’ concept). From that standpoint, Herder-like ethnology, ready to delve into the past of its own people (the peasant majority) and determined to discover the true Volksgeist was a perfect fit for the goals of Serbian politics striving to restore its medieval glory and seek revenge for loosing its freedom and power to the Ottomans, as epitomized by the Legend of Kosovo.

For these reasons, as in other part of Eastern and Central Europe, faced with similar challenges, this particular ‘brand’ of ethnology was chosen over an alternative path, an ethnology grounded in the philosophical premises of the Enlightenment. A possibility for this other option, an ethnology/anthropology in the spirit of positivism looking outward, towards the world, and seeking comparisons, generalizations and laws, was offered by the Serbian educated elite in Austria, notably by one of the greatest Serbian scholars of the Enlightenment, Dositej Obradović, his predecessors and followers whose work taken together spans the period from the mid-
eighteenth to the mid-nineteenth century (cf. Kovačević 2001). However, national aspirations and international circumstances surrounding the decline of the Ottoman empire and bringing into play conflicting interests of great powers attempting to resolve the ‘Eastern Question’, influenced the decisive turn in the mid-nineteenth century from the great European tradition of liberalism, cosmopolitism and belief in the strength of reason to the other, based on ethnic nationalism and historicism.

The Absence of Development

If the ‘national science’ paradigm was the only feasible alternative while the nation was making every effort to attain its objective, did it not become obsolete once a version of the main goal, the national unity, had been accomplished? Namely, after World War I (Jovan Cvijić participated at the Versailles Peace Conference as an expert on boarder delineation) the majority of Serbs found themselves living in a single state, albeit shared with other South Slavic brethren (the kingdom of the Serbs, Croats and Slovenes, renamed the kingdom of Yugoslavia in 1929, which coincided with the abolition of constitutional government and the inauguration of royal dictatorship), the main objective of ethnology as a national science was also reached (cf. Naumović 1997: 319–51). Thus, it is a legitimate question to ask whether a change of perspective would have been appropriate within a new state that was emphatically multi-ethnic in character. This is particularly true given that each of the constituent groups had its own history of national sentiments and was quite protective of their unique identities, some also of their political autonomy, built and preserved while under foreign rule.¹ Could a different ‘supranational science’, oriented towards building bridges and defining common denominators towards other ethnicities within the first Yugoslavia have helped prevent the build-up of ethnic animosities and religious hostilities that led to the World War II atrocities? Could it have, instead, promoted the creation of secondary, alternative and truly pluralistic Yugoslav identity, rather than the enforced one designed to ‘serve to enhance Serbian domination’? Serbian ethnology is taken as an example, but, certainly the same question could be directed to other, notably Croatian,

¹ For a concise but comprehensive, unbiased analysis of the political situation in Yugoslavia in the interwar period cf. B. Jelavich (1989, vol. II: 134–57, 192–204). This two-volume work on the ‘History of the Balkans’ had a very strong impact on my own meandering path of self-searching for a more meaningful and challenging ethnological/anthropological paradigm. The book that covers three centuries of turbulent Balkan past and inter-related political, economic, social and cultural histories, together and from the viewpoint of each of its lands, provided me with a more holistic, relativistic, integral and relational grasp of processes from a bird’s eye perspective.
ethnology as defined by A. Radić and his successors. On the other hand, it is difficult to imagine a differently oriented ‘folk science’ while the government retained centralist rule and while everything that was attempted towards building a homogenized, non-ethnically based society (for example the division of the country into nine provinces, banovinas, which cut through national territories and traditional loyalties) was perceived as Serbian hegemony, the source of grievances of all other constituents (Jelavich 1989: 200).

Therefore, the mission, trends, goals and techniques of the pre-World War I ethnology were carried on with little or no innovation. Diligent field researchers, professionals and literate individuals, mostly teachers, living in small towns or villages, continued to ‘fill in the blanks’ collecting data and producing monographs of the areas that had not been previously covered, using always the same questionnaires earlier compiled by the leading scholars, never adapting them to the changed socio-economic environment or broadening the scope of inquiry. As a rule, their aim was to reconstruct the past, ‘to save from oblivion’ the former way of life, which implied ‘the right way’ of living one’s life. The repetitive accumulation of data was not accompanied by theoretical or methodological growth, which would not only

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2 'The boundaries of banovinas were gerrymandered so that Serbs formed a majority in six, Croats in two, Slovenes in one and Muslims in none. The principle offices in the state continued to be held by Serbs from Serbia, with the Slovenes receiving an adequate share. Muslims were allotted the lowest positions, whereas the Croats remained inadequately represented' (Jelavich 1989: 200–1). In effect, outside of Serbian majority, other nationalities, notably the Croats, objected to the national hegemony, political repressiveness of the regime and the existence of a unitary state. Instead, a federation of national entities with autonomous rights was preferred. Within the context of increased international dangers and with fascist and national socialist ideology echoed in the development of some internal movements within all national groups, national tensions were also growing. As an attempt of a belated remedy, the Croats, and only them, were granted autonomous status in 1939, but by then national conflicts have taken their toll. Pre-war animosities were carried into World War II and the concurrent Yugoslav revolution in which they have reached the extreme. Committed atrocities have created deep, painful and fearful memories, which could not have simply disappeared after the war. The communist regime has never successfully dealt with the problem of the past ethnic conflicts or attempted to promote a Yugoslav national identity. Instead it enforced the ‘brotherhood and unity’ concept and used the existing differences and historic memories for political manipulations and gains whenever appropriate. These same memories had been triggered off and used for the mobilization of masses during the postsocialist era ‘wars for succession’ that ended the existence of a country known for more than sixty years as Yugoslavia.

3 The first questionnaire, ‘Instructions for the research of settlements in Serbia Proper and other Serbian lands’, was composed by J. Cvijić in 1896. Cvijić continued to publish instructions covering many other phenomena (different aspects of culture, migrations, ethno-psychological traits) as did his successors, first professors of ethnology at the University of Belgrade, Tihomir Djordjević and Jovan Erdeljanović, then some of their assistants and collaborators.
have provided a framework for interpreting the collected material (Kovačević 2006: 47–50), but also targeted research towards new topics and problems. Had new research paradigms been developed, different and additional data which would have described some of the actual events and processes (for example, randomly selected, detailed analysis of the effects of cash economy, depression, penetration of political parties into village life, the function – not survivals – of magical practices based on animistic concepts in contemporary culture, urban and government influences, the changing role of women and so on) could have been collected.

The discipline was perhaps dominated for too long (almost four decades, 1906–1944), by only two professors, Tihomir Djordjević and Jovan Erdeljanović. Although those two professors may be considered as accomplished as far as their own research and publications are considered, within the chosen conceptual framework, already absolute in the inter-war period, during their time the Department of Ethnology did not grow; instead, it was more like a ‘nursery’ of talented students who worked as assistants for several years, before or after graduation, while waiting for a permanent position somewhere else to which they could be ‘transplanted’ (Rakić 1997: 304–5). Most of them created the pool of working ethnologists after World War II, some leaning more towards anthropo-geography, others towards ethnology, but all within the same established research tradition. Personally, I find the absence of development in the Department very intriguing. It seems to have been a method of controlling and preserving a single paradigm, as opposed to allowing pluralistic approaches to develop. Somehow, it is hard to imagine that no significant inspiration came from the challenges anthropology was dealing with in other countries, each in its own ways: in Great Britain (Radcliff-Brown in Oxford, Malinowski at the London School

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4 All of them, Cvijić, Erdeljanović and Djordjević, conducted impressive research, collected vast amounts of empirical evidence, published extensively and achieved some admirable results. They all studied abroad in the early twentieth century and were awarded doctoral degrees (Vienna 1893, Prague 1905, and Munich 1902, respectively). They continued to follow international literature, but new theoretical and methodological challenges as offered by contemporary British, French and American anthropology were ignored. It seems that they were selecting the works that confirmed their preconceived results. Thus, Erdeljanović (1938) learned English late in his career so that he could find writings that would support his thesis on animism as the earliest form of religion (evolutionistic approach) and prove his colleague Djordjević (1933) wrong in propagating the idea of primordial monotheism (a borrowing from German diffusionism and its Kulturkreise classifications). However, I have to note, that Djordjević deserves credit for introducing the use of archival material in the study of Serbian culture in the past, certainly a more viable source than peoples’ memories of memories passed on to them from their elders. I am not denying the importance of memories, but as a source of penetrating into perceptions and value systems, and not necessarily for accurate reconstruction of past events.
of Economics); in USA (Franz Boas at Columbia University, Leslie White at University of Michigan, Redfield in Chicago, Kroeber and Lowie at Berkeley); or France (Durkheim, Mauss, van Gennep, Griaule). Was it because they were mostly (but not exclusively) oriented to the study of 'other' cultures, or because the names of the discipline, anthropology and, in the French case, sociology, were considered distant and unrelated? Perhaps it was because younger ethnologists did not spend extended periods of time at universities abroad learning about new developments at their source. Perhaps it was because the approach of Western scholars was too radically different to be perceived as applicable to the native scientist cause.

It is not only that those modern influences did not inspire Serbian ethnology through literature, but even direct live contact between representatives of domestic and foreign disciplines did not spur any interest to try out a different approach. I am referring to the presence of American scholars who had began to exhibit an interest in Balkan cultures and field research in situ as early as the 1930s. The works of three scholars belonging to three different, but related disciplines have left a lasting legacy and an important contribution to the body of knowledge about Balkan societies and cultures presented in the English language. Those were: sociologist Irwin Sanders, folklorist Albert Lord, and historian and political scientist Phillip Mosley. Sanders began his work in 1935 in a Bulgarian village Dragaljevci, near Sofia, focusing on the study of interactions among social groups. His monograph, *A Balkan Village* (1949), based on the novel community study approach, strongly influenced future research by American scholars working in the Balkans. Albert Lord visited various parts of the Balkans, from Bosnia and Herzegovina to Bulgaria over a twenty-five year period (1934–59) and recorded a vast number of oral epic and lyrical poetry, now part of the Harvard University collection. In addition to analysing and interpreting poetry, Lord focused his research on studying the performer, 'the singer of tales', determining how he remembered, reconstructed and transmitted a poem within its living socio-cultural context. Direct contact with the culture, the search for universal patterns ('formulae theory'), comparative perspective and theoretical generalizations made Lord 'an anthropologist of oral literature'. Finally, Phillip Mosley travelled the Balkans and by using the uniform case-study method collected and analysed data on various types of

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5 As Phillip Mosley was getting ready to start studying diplomatic relations concerning Dardanelles, it was recommended to him that instead he should go to the Balkan Peninsula and study the living cultures there. Since he was coming from a different field, he consulted with Ruth Benedict and Margaret Mead at Columbia University, and most importantly, went to the London School of Economics to study fieldwork methods with Bronislaw Malinowski. With this, he became highly qualified for the research he was about to undertake.
zadruga, the flexible, alternative extended family organization. Instead of attempting the romantic and patriotic pseudo-historic search for the origin of the institution, or its ethnic and religious specifics, like native ethnologists, his goal was to determine internal structure and functions of a zadruga as a social instrument of organizing labour, providing security and adaptability to the environment in a combination of ideographic and nomothetic approaches (Mosley 1976a–c).

All of these novel ways of looking at and understanding a peasant society and culture landed on deaf ears. The only exception was Milenko S. Filipović, who had met Mosley while in Yugoslavia, and who understood the need to learn about these new, alternative approaches. In 1939 he applied for a scholarship from the Rockefeller foundation to go and study with Malinowski at the London School of Economics. The outbreak of World War II prevented him from going. Nevertheless, his desire to study abroad was granted after the war, in the early 1950s, but instead of going to England he travelled to the United States.

**The End of an Era and Continued Stalemate**

During World War II the discipline suffered considerable fatalities. The Department lost both its senior members of staff and there was no one of the same calibre and experience to take their place. In addition, the entire seminar library was destroyed in a fire deliberately set by the German occupying forces. Until Borivoje Drobnjaković, director of Ethnographic Museum in Belgrade, was hired as a professor in 1950, the entire burden of re-opening the department, replenishing its library, and teaching many different classes fell on the shoulders of Mirko Barjaktarović who was an assistant to Erdeljanović for just one academic year before the war, and who was rehired in 1947 (Pavković 1997: 1–5).

However, one of the gravest threats came from the drastically changed socio-political context within which ethnology was to operate after the communist regime came to power in 1945. There was no reason whatsoever for ethnology to retain its privileged status as a ‘national science’ any longer. The new authorities that placed themselves in charge of the definition and management/manipulation of the ‘ethnic issues’ (see Prelić, this volume) had no use for traditional ethnological research topics, such as the continued delving into the nation’s past and pursuing the research of its ethnic origins and history. This was not favourably looked upon especially if it came from the Serbian side, since the Serbs, who represented the overwhelming majority in Yugoslavia, were still associated with the pre-war hegemony and the
pro-royal and pro-Serbian Chetnik resistance movement during the war. The regime felt that it had to keep potential Serbian nationalism and everything that could revive or promote it under firm control. To make things even more miserable for ethnologists, the regime was not a big fan of peasantry either, the main subjects of ethnological research. It strongly preferred progressive industrial workers (proletariat) to backward small land holders living their own, self-sufficient lives in scattered and not always easily accessible villages.

Ethnology was not theoretically and methodologically equipped to study what was considered desirable and progressive 'from above'. Instead, sociology and, strangely enough, folklore research were the ones that met

6 Chetnik detachments were formed by Serbian officers after the surrender of the Yugoslav army in 1941. They were loyal to the King and the government in exile and they fought for the 'Serbian cause' on both national and especially anti-Croat and anti-communist platform. The rivalling guerrilla groups, the Partisans, were the major multi-national resistance movement. Under the leadership of Josip Broz Tito and the Yugoslav Communist Party, they were in the end victorious, which also meant the defeat of the Serbian nationalistic programs (Jelavich, vol. II, 1989: 267–72).

7 Sociology was first established at the University of Belgrade Law School in 1935. After World War II it was stigmatized for a time as bourgeois science and it took more than ten years to formally reinstate the discipline. First it became a part of the Institute for Social Sciences founded in 1957, and two years later it was added to the Department of Philosophy at the University of Belgrade. Very soon afterwards, it became a prominent, well-financed discipline not only because it was able to provide meaningful insights into specific social issues, but also because it was rapidly developing into a solid, theoretically and methodologically grounded science with well-defined concepts and goals. Its social and scientific relevance attracted far greater numbers of students than ethnology. As sociology was developing as a discipline, it was also 'infringing' areas of traditional ethnological research: rural sociology, demography, social anthropology. The empirical and theoretical levels attained were much higher than in ethnology. Sociologists spoke other languages, attended international conferences, encouraged free flow of academic ideas and engaged in professional networking. Therefore, it did not come as a surprise that foreign anthropologists more readily established contacts with the Department of Sociology, often completely bypassing ethnologists with whom they shared very little academic interests. It was also because ethnologists carried very little social and political weight and they could not make 'things happen' as efficiently as others could. It should be noted, however, that the roles of sociologists and philosophers were complex and ambivalent, to say the least. In the 1950s and 1960s they were mostly ideologically committed to the needs of the regime. Later on some of them continued to perform services for the government but some of their most prominent disciples were also its biggest critics, because of which they were treated as dissidents.

8 A group of folklorists in the Serbian Academy of Sciences and Art, under the leadership of Professor Dušan Nedeljković, a philosopher, was engaged in collecting new folklore creations, such as songs sung by Partizan during the war and workers while building a new socialist state after the war. Of course, the ideological loyalty and actual role of some scientists with political clout was rarely revealed to the public. There were, however, always hushed rumours, and Nedeljković was never portrayed in a positive light.
the challenge. Ethnologists on the other hand, faced with fast-paced change, strongly believed (or did not know better) that their duty was above all to record whatever was still left of the ‘pure’, ‘authentic’, ‘uncorrupted’ and ‘uncontaminated’ folk culture before it was snatched away by ‘progress’. Of course, in order to fulfil that goal they had increasingly to rely on their informants’ memories (always searching for the oldest villagers) rather than on what they could in fact see and record as an actual socio-cultural reality. In this way, and by opting to represent the idealized, conflict-free image of peasant society, in a static, timeless way, they, like their predecessors in the inter-war period, regretfully, missed the opportunity to record the invaluable evidence of how the villages were handling the change inflicted upon them and what kind of processes were evolving with the onset of rapid modernization, industrialization and massive exodus of the village population into urban areas.9

Not only were the ethnologists10 going against the tide, but they were also performing quite poorly, losing scholarly credibility with their choice of topics (‘the study of men’s drawers’ was one of the disparaging descriptions of ethnographic research), data-collecting process and presentation as crude, non-interpreted evidence. This is a very general statement, but that is how ethnology was perceived and criticized from the outside, by representatives of other social sciences. Belief that the best possible ethnologist could be somebody who was socialized within village culture and that the place of his/her geographic and social origin qualified him/her for the profession better than any training could do (confusing ethno-explication with scientific interpretation – Kovačević 2006: 55),11 did not help the reputation of ethnol-

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9 Ethnographic Institute of the Serbian Academy of Arts and Sciences, founded in 1947 after the Soviet model, had in its programmes the study of cultural dynamics and processes and effects of modernization. The largest project the Institute did in this venue was the study of the Djerdap area in Eastern Serbia where a huge power dam on the river Danube was to be built. However, the project was done very differently than similar endeavours in other parts of the world. It was more, as in the case of Jarmenovci (Knežević and Jovanović 1958), a village in central Serbia, a reconstruction of what might have been, than of what it was at the time of research. In Jarmenovci, for example, instead of studying the effects of electrification and the impact of a United Nations developmental programme that was implemented there, the authors produced a reconstruction of timeless village culture with the results that were not different than if the same village had been studied fifty years earlier.

10 As always, with some notable exceptions: for example, the work of M. S. Filipović; his Ph.D. student Manojlo Gluščević and his study of couvades; later Djurdjica Petrović who developed innovative approaches not only through foreign influences but also through cooperation with history and history of art; Nikola Pavković established bridges towards history and traditional law studies, and so on.

11 I must emphasize that in the years after World War II, the favoured ‘peasant origin’ of Serbian ethnologists was more an ideal than a reality. Before the 1970s within the university department, ethnologists of peasant or, more precisely, small-town origin prevailed, while
ogy among colleagues from other disciplines or in society at large. Hence, they were marginalized, deprived of any social influence and almost never consulted in problem-solving or conflict-resolution situations that were within their expertise. Growing insecurity about most of the aspects of their discipline, led to ethnologists’ gradual (self)-isolation from other scholarly disciplines. One of the indications that their science was ‘in a stagnant or critical state’ was that ‘in its entire history there have never been more texts about the definition of ethnology, its subject and relations with other social sciences than in the period between 1955 and 1975, when [ethnology] was clinically dead’ (Kovačević 2006: 38–9). One positive effect, however, was that the authorities never really bothered them and allowed them to ‘do their own thing’, to engage in harmless pursuit of antiquity.\(^\text{12}\) Or, the authorities after that period they represented a definite minority, almost an exception. However, perhaps as a way to reinforce the endangered ideal, several professors as late as the early to mid 1970s still professed the importance of being of peasant origin not only for cognitive but also practical reasons. Rural ethnologists could perhaps prevent the urban ones from going astray with their research topics and therefore threatening the ‘autonomy of the well-defined discipline’. Dj. Petrović was used as an example of the latter; a disparaging phrase, ‘guns and pianos’, was coined to discredit her work. She studied Balkan weaponry through a historical perspective and discussed in one article the appearance of the first piano in Serbia. Both of these were considered ‘non-ethnological’ topics.

\(^\text{12}\) Of course, in an authoritarian state (or maybe, in any kind of state) there are always at least some subtle forms of control in important parts of social life. Higher education was certainly one such area. Members of the Communist party were, I am convinced, although I do not have reliable insider information, responsible for making sure there were no dissident activities in progress. That is why every department was supposed to have one or two faculty members (approximately 5 to 10 per cent in the case of the small Department of Ethnology) who were party members. If they were to witness major infringements, I believe they would have probably had to denounce their dissident colleagues. I do not know how it was in other departments, but our ‘guardian angels’ have never publicly exercised any of their ‘rights’ or ‘obligations’ (maybe because the rest of us were so hopelessly apolitical, or at least we thought we were), but I always believed that if they had to take such action it would have been to protect us from the regime, rather than the government from us. This is especially true where there were no personal animosities, rivalry or grudges. However, as in the Kulišić – Filipović case, personal relations burdened with vengeful sentiments could lead to political manipulation. Our colleagues at the Department who also happened to be party members were normal people with strong collegial sentiments and no harm could have come from them. Nevertheless, only they can answer if either of them was in any trouble with the Party because of any of us. The majority of faculty and staff never thought of them as party members and were never ‘careful’ in their presence. After the purges in the mid 1970s when eight professors and assistants were expelled from the Department of Sociology and Philosophy for participating in 1968 student demonstrations, we all wondered if there were, especially since we had just then moved into a new building, any recording devices installed. Once I was given the following explanation: ‘Offices may be bugged because one can have secret conversations there. Classrooms are bug-free because there is no need for them: somebody from the student body (or an implanted non-student) will file a report if, in their judgement, anything went astray.’ Personally, I never had a problem during socialism, but I did have an unfortu-
could leave the ethnologists alone, because they helped their demise in the early post-war years.

It became obvious that ethnology, deeply entrenched in its fossilized paradigms, was unable to break the vicious circle in which it found itself. Insights or borrowing from other related disciplines that could have helped the change of perspective, were lacking (not on an individual, but on a collective level). The other possibility was to open doors wide to the influences coming from ethnology and anthropology as they were conceived and practised in some other parts of the world. In the mid 1950s Serbian ethnology was potentially exposed to two such options, represented by two native scholars, Špiro Kuljišić (1908–89) and Milenko S. Filipović (1902–69), who were at the opposite ends of the spectrum professionally, epistemologically, ideologically and politically. Kuljišić was the representative of a hybrid theory based on evolutionism and historic materialism (Marxist theory) which was the official theory widely used in the Soviet Union and other communist countries. While providing service to the government in attempting to enforce its socialist programme and creed, he applied his dogmatic and ideologically charged version of the theory to the construction/reconstruction of archaic study of familial, kinship, and tribal organization and ethnic relations in the Dinaric region, and the alleged agrarian and matriarchal roots of Serbian folk religion (cf. Gorunović 1997). As could be expected, he was intolerant towards other approaches in anthropology, especially towards British functionalism and structural functionalism, denouncing them, following the Soviet lead, as anti-historical and imperialistic–colonial. Although he accused other scholars of advocating pseudo-scientific, reactionary views, his main attack was focused on Filipović, whom he regarded as a strong competitor and a threat. Unfortunately, the entire conflict was not so much about scholarly polemics or even the defence of ‘progressive thought’ against ‘bourgeois survivals’ as it was about careerism and personal resentment (see Gorunović, this volume). Although Kuljišić’s ideological attempts have failed since his theory was neither mandatory nor did it secure a large spontaneous following, pointing a finger at a colleague with pro-western orientations in the 1950s, was a serious accusation which could have had grave political implications.

nate incident during the nationalist euphoria in early 1990s. I do not know who the denouncer was, nor did I ever attempt to find out, but he or she was obviously a novice to the business, or was simply trying to win some kind of points for him/herself. The ‘report’ had no bearing whatsoever to what was said or discussed in class. Still, it was a very unpleasant experience revealing to what extent things had changed: the trust between student–teacher and colleague-to-colleague communication was suddenly and sadly lost.
My discontent with the situation in ethnology and my attempts to change it so that we, Serbian ethnologists, could start to study the real life of our people, instead of antiquities, date quite a while back. However, the main result so far has been the fact that I had to leave Belgrade and retreat to Sarajevo where I have tried to do the same thing, again in vain. Because of my stand that ethnological research should be carried out in conjunction with the real life problems, I have been stigmatized as a reactionary and an exponent of Anglo-American imperialism! However, there is hope. There are some younger colleagues who want to do better. Institute of Hygiene and Bureau of Statistics are starting some interesting research. Recently the Institute of Social Sciences was formed which has in its programme the study of family, social change, especially in rural areas and many other topics. Prominent Marxist theoreticians [different from Kulišić’s dogmatic Marxism] from the Institute have asked me to collaborate with them. I have accepted, in spite of the opposition from the so-called ‘Belgrade [ethnological] School’ (Filipović’s letter to Joel M. Halpern, 17 May 1959).

Thus, it seems that Filipović’s greatest ‘sin’ was his appreciation of American (and Western) anthropology with which he became familiar through personal contacts with American scholars. Although he was Cvijić’s stu-

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13 Milenko Filipović began his career as an assistant at the University of Belgrade (1924–6). He was then an associate professor (docent) at the University of Skopje (1930–41), University of Belgrade (1941–3) and full professor in Sarajevo (1955–62). From 1945–50 he was a curator at the Ethnographic Museum in Belgrade, and Research Fellow at the Ethnographic Institute at the Serbian Academy of Sciences and Arts (SASA).

14 J. M. Halpern has retained his entire correspondence with Professor Fillipovic from 1954 until his death in 1969. The collection that consists of 151 items is now digitalized as a part of Halpern’s archive located in several institutions around the world. This is a very valuable source for the study of Serbian–American scholarly relations that covers a period of fifteen years, even though it is limited to a single ‘case study’. Only a small fraction was used in this chapter.

15 Filipović and Halpern had devised a very efficient way of exchanging news in the disciplines on opposite sides of the Atlantic, as well as publications. There are many letters in their correspondence dealing precisely with this matter. Filipović sent Halpern publications he needed while writing his dissertation, and, in return, Halpern made sure that all Filipović’s subscriptions were current so that he could continue to receive American journals: *American Anthropologist, SW Journal of Anthropology, Journal of American Folklore, and Rural Sociology*. Specifically, in a letter dated 6 August 1954, Filipović requested the following books: L. White, *The Science of Culture*, J. Herskowitz, *Man and his Works* and L. Beals and H. Hoijer, *An Introduction to Anthropology*. I was quite impressed that in the mid 1950s there could be such an unobstructed exchange of books through regular mail and that Professor Filipović was able to receive all his magazines and books, perhaps after some careful inspection at the customs.
dent and follower, and never rejected the native paradigm, he was astutely aware that alternative and, in his view, complementary approaches to the study of culture modelled on Western (American) anthropology could have positive effects on the development of ethnology at home. He was in favour of shifting research from the past to the present, from a static to a dynamic perspective, from distribution and (pseudo-)evolution to function and structure, from reconstructions to the search for causes and consequences. He went on to propose that the results of ethnological research should have practical applications in solving social issues. Filipović criticized even the most sacred aspect of all, the way of ‘doing ethnography’, recommending that, instead of random accumulation of data, information should be collected with the research objective. In addition, he pleaded, field research should concentrate on the present, for what was seen and registered as ‘now’ will be the ‘ancient past for the next generation, but it will be readily accessible to them because of our work’ (Filipović 1955: 214).

What he urged in his programmatic articles, Filipović applied to his own research. He published extensively leaving a lasting source of influence, at home and abroad. He opened up or revolutionized the approach to the study of many important issues, such as ethnic groups, the origin of tribal society in the Balkans (the so-called Vlach theory), social rituals such as ‘Zvanica, a Serbian Institution of Reciprocity in Rural Life’ (1965), ‘Suicide in the Takovo Region’ (1958), (so much for the idealized, conflict- and tension-free representation of village life in traditional ethnography) and many others. Even his monograph Takovo (1954), about a region in central Serbia (Šumadija), was different. All of his work after his initial contacts with Professors Albert Lord and Phillip Mosley in pre-war Yugoslavia, and especially after his yearlong sabbatical in the USA, at Harvard (Lord, possibly also Talcott Parsons, a follower of Durkheim and Weber also worked there at that time), with frequent visits to the Columbia University (Mosley, Arenberg and their student Halpern who Filipović cherished as a colleague and a personal friend until his death), novel in theme, concept and methodology, began to resemble the work of American anthropologists. Unfortunately, very few Serbian ethnologists at that time followed in his footsteps.

16 Filipović’s bibliography for the period 1924–60 consists of 280 monographs and articles published in Yugoslav and foreign journals, including a number of British and American publications (e.g. Man and Harvard Slavic Review). Since his death, Professors Hammel, Halpern, Lord, Echrich and Filipović’s daughter, Radmila Fabijanić, have edited, translated and published a collection of his papers, Among the People (1982).

17 During his stay in the USA Filipović also gathered material on immigrant Serbian population in America and planned to publish an extensive volume on the subject. Perhaps the manuscript still exists in his private collection (personal communication with Professor Halpern).
'Retreat' to Sarajevo curtailed his opportunity to emanate his influence at the largest centre of Serbian learning, Belgrade. However, although he may not have been able to act officially, through an institution, he was very well respected among younger colleagues who desired change, and his home was always open to anyone seeking his advice. The value of his work has passed the test of time and it is still admired even by today's youngest generation of native scholars, well versed in anthropological theory and practice, past and present, in all parts of the world. If Professor Filipović could see the changes made in Serbian ethnology/anthropology since the mid 1970s he would have been proud and content that his pioneering steps and sacrifice were not in vain. Actually, there is still so much more we can all learn from his work.

The Halperns in Orašac

What was it in American cultural anthropology that had attracted Filipović and made him believe that it was a path to consider? Since the war, the anthropologists who have spent the longest time in Serbia studying its culture by focusing primarily on one village, Orašac in Šumadija, were Joel and Barbara Halpern (see Interview, this volume). They had, in a way, combined the efforts of all three of their predecessors (Sanders, Lord and Mosley) and kept adding complexity and innovation as time went by: community study, kinship and social structure, social change, personality and culture, historic demography, life histories, expressive behaviour and oral tradition from a socio-linguistic perspective.

Through his undergraduate and graduate studies Joel M. Halpern was exposed to many different intellectual influences and anthropological

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18 He did mentor one of the best doctoral dissertations, on couvades, prior to 1970s by Manojlo Gluščević, at the University of Sarajevo.
19 I remember my mentor, Professor Djurdjica Petrović, certainly one of the most prominent enfants terribles in Serbian post-World War II ethnology, who not only contemplated change but took decisive action to alter her path, telling me how Filipović always ready to listen, advise, critique and offer support. Filipović, contrary to his arch-opponent, Kuljišić, was open to many different approaches. He did not believe that his way was the only possible way: on the contrary, as long as a researcher was breaking out of the vicious circle and aiming for higher standards. Djurdjica Petrović's path was into the study of aspects of everyday life in different historical epochs via archival documents.
20 There were many other anthropologists who had conducted field research in Serbia from the 1960s until the present (E. Hammel, A. Simić, B. Denich, R. Wagner, J. Foley, R. Hayden, M. Bakic-Hayden, E. Gordy and others). Limited space, regrettably, does not allow their superb scholarship to be mentioned here. After all, the aim here was not to offer an overview of American research but to determine what was in the theoretical and methodological package that American anthropologists brought with them in the 1950s when ethnology was in its deepest crisis and in dire need of inspiration.
schools of thought. He pursued his undergraduate studies in history and anthropology at the University of Michigan. There, the discipline was dominated since 1930 by Leslie White, who had developed a materialist, neo-evolutionist theory in direct opposition to Boas. Halpern then went to the Columbia University to study at its Russian Institute. The Department of Anthropology at Columbia was the birthplace of the American unique four-field discipline, developed by Franz Boas, a German born and educated natural scientist turned anthropologist. He dominated almost the entire American anthropology for a long time, from his first year at Columbia in 1899 until his death in 1942. Inspired by Herder’s Romanticism and the teaching of Adolf Bastian, Boas actually transplanted the German intellectual tradition to the USA, giving priority to diffusionism over simplistic and schematic evolutionism and to systematic and detailed research of particular cultures over empirically insufficiently grounded generalizations. He therefore advocated cultural particularism and relativism, but also, like his colleagues in France and Great Britain at the same time, a belief in a holistic approach according to which cultural elements could not be understood in isolation but only within their context (cf. Eriksen and Nielsen 2001: 38–41).

By the time Halpern came to Columbia, many changes had already taken place. Boas’s students, Ruth Benedict and Margaret Mead had developed the ‘culture and personality approach’ and the studying of ‘culture at a distance’, by using various sources and interviewing immigrants from inaccessible lands (e.g. Japan during and after World War II, or the Soviet Union after the Revolution). In 1946 Julian Steward came to develop his materialist, historically oriented neo-evolutionism and to end the anti-theoretical mode advocated by Boas. He was replaced by Halpern’s future mentor, Conrad Arensberg, in the early 1950s who, together with Karl Polanyi, Columbia’s economic historian, developed methods of analysing the economics of ancient empires and other issues.

The Russian Institute, jointly founded in 1946 by Columbia University and the Rockefeller Foundation and headed by Philip Mosley, also contributed to a change of emphasis in anthropological studies and their movement further away from Boas’s model at his own school. It first of all introduced hitherto almost totally absent interest in politics and economics:

21 Those are: archeology, linguistics, physical and cultural anthropology. Anthropology students in the USA start their training by studying all four fields, and then they specialize in one of them.
22 ‘The Chrysanthemum and the Sword’ on Japanese culture was created exactly in that way. Had it not been for Filipović and the possibility of having first-hand field experience, Halpern would have continued to study, as part of a large project based in Harvard, Russian culture at a distance by interviewing displaced Soviet citizens in USA or Germany.
...it was the Second World War and the post-war responsibilities of the United States as the strongest power of the free world which made clear the need for the systematic and many-sided study of major world areas outside the United States and Western Europe. One result of this awareness has been the development of 'area studies', programmes of study and research directed to enlarging our knowledge, not only in Russia, but also of the Muslim world, the Indian sub-continent, Africa, China, Japan, and Latin America ... The Russian Institute conducts an intense programme of training for graduate students (Mosley 1954: 1).

As the 'areas of study' were defined, it became obvious that there would also be a shift from investigating tribal entities to more complex peasant societies. The 'Community study approach' came to prominence as a methodological framework for analysing and describing rural and urban settlements, as segments of modern state, in a holistic, functionally interrelated and integrated way, 'from a particular point of view, rather than a series of seemingly unrelated fragments' (Halpern 1956: 1). 'The community study attempts to do for the rural or urban unit of the modern state what the ethnography did for a tribe' (Halpern 1956: 3). This approach began to be developed and applied already in the late 1920s, first in the study of homeland small towns, Middletown (Lynd 1929); and Yankee City (Warner and Strole 1945; Warner and Low 1947), and then prior to, during and after World War II, in the study of rural settlements abroad. These newer studies were definitely more ambitious as it was realized that they could be 'very effective in investigating various social phenomena and theoretical issues' (Milić 1957: 48). Those were the well-known monographs by Robert Redfield (1941), O. Lewis (1951), J. Steward (1950), and Conrad Arensberg. Professor Arensberg's study, The Irish Countrymen (1939), was a classic ethnography of life in a European rural community. As Halpern's professor and thesis adviser, he had a direct and most profound impact on Halpern's...

23 'It would seem that when anthropologists go out into the field to study culture, especially one that has been little described in literature, they have something of an obligation not only to secure data bearing to their problems or in congruence with their approaches, but to attempt to describe the culture as a whole, so that for the future we will not have only bits and fragments but at least the bare outlines of the culture unit, since it is manifestly impossible to describe any culture in toto... Since the observer is painting a picture of a culture at a certain point in time, it might be well to be conscious of the problem of including sufficient descriptive data, so that investigators in the future may be able to use the same material with perhaps different theoretical purpose in mind' (Halpern 1956: 2, 8).
research in *A Serbian Village*\(^{24}\) that has been published in three different versions (1956, 1958, 1967), plus the latest one in Serbian translation (2006).

As the subject of the American anthropologists’ research ceased to be Native American and Inuit populations living on their own turf (‘anthropology at home’), patterns of fieldwork had to change as well. Opportunities to study overseas culture *in situ*, required the kind of fieldwork inaugurated by Malinowski – long-term, stationary investigation, employing participant observation, far away from home – that was adopted and adapted by a number of American anthropologists in the 1930s. At Columbia University this type of fieldwork was promoted by Margaret Mead who gained first hand experience while studying the cultures of Samoa and New Quinine.

The goals of collecting and interpreting information from various cultures also changed. While the requirements for the ‘community study approach’ to comply with the holistic principle was in accord with Boas’s teaching, concerns about cross-cultural comparability, which implied at least a limited amount of standardization of data-gathering procedures was very advanced in relation to his beliefs. Reflective of the need of anthropology to aim ‘not only to understand cultural wholes (a Romantic enterprise), but also to dissect, analyse and compare them (an Enlightenment project)’ (Eriksen and Nielsen 2001: 12–13), was George P. Murdock’s project to create a worldwide database of cultural information that could be used for comparative generalizations. The database was named after the school in which it was housed, the Yale Documentation, or, descriptively, Human Relations Area Files (1937). In the original database information on specific topics were coded and classified on paper and, in the next phase, transferred to microfiche.\(^{25}\) Although there have been concerns, controversies and criticism about this programme, it continued to grow and diversify into several different, computerized subsystems.

Another important development in American cultural anthropology in the years before, during and after World War II, was the possibility of applying research results in solving social problems. C. Arensberg was one of the pioneers in this area, but the same aspirations can be found in the work of other anthropologists of the time, notably R. Benedict and M. Mead. While some of the work was directly related to the needs of the government, such

\(^{24}\) Halpern asked Professor Filipović to read critically the manuscript of his thesis successively as individual chapters were written. Filipović readily agreed and edited the entire manuscript very meticulously. One of his strong recommendations was to change the original title ‘Life in a Šumadija Village’ to ‘Life in a Serbian Village’ for the sake of American readers who ‘will probably have trouble locating Serbia, let alone Šumadija’ (Letter to Halpern, 10 May 1955).

\(^{25}\) Some of M. Filipović’s research results were added to the Serbs files.
as the need to understand the Japanese ‘national character’, or boost national morale during the war (for example, R. Benedict was on the Committee for National Morale), other was applied in promoting social action such as in education, ecology or the women’s movement. This specific venue of cultural anthropology could have been the source of concern outside the USA and fear that data gathered during field research in a foreign country could be used for additional, other than purely scientific needs. What is defined as patriotic within may be read as ‘espionage’ without.

In their study of Serbian village life, the Halperns employed the majority of the theoretical postulates from mainstream American anthropology, which, in my view, resulted in an excellent monograph greatly different from ethnographies written by native scholars.\textsuperscript{26} Limited space does not allow me

\textsuperscript{26} Recently, however, an opposite view has been expressed after comparing Halpern’s \textit{A Serbian Village} and the \textit{Jarmenovci} monograph by Knežević and Jovanović (1958). In this article the overall value of both of these books as an ethnographic source is questioned. Some of Kovačević’s stands require a serious dialogue, which, regrettably, cannot take place here. For instance, he postulates that native ethnologists, because of their background knowledge could gather all pertinent information in a short period of time, in ten days to two weeks, and produce an ethnography that would be equally ‘informative’ as the one generated by an anthropologist-outsider after a year of participant-observation. He further argues that the differences between the two disciplines do not really account for much because the final product of both of them, the monograph, suffers from common maladies, for example: it does not offer precise rules of how data were collected, that is well-defined methodological and epistemological standpoints; it is not precise in recording the timeframe but blurs the history and process with reporting in ‘ethnographic present’; does not distinguish between ‘norm’ and ‘ideal’, and so on. In essence, he is very much against writing ethnographies (especially post-modern ‘writings of culture’) and instead he is in favour of deliberately created ‘fragmented data that would be an equivalent to historic source’. In other words, collected datum, accompanied with detailed description of how, from whom and for what reason it was gathered, would be catalogued and printed/stored with other fragmented pieces of information in a database. Anthropology/ethnology should do away with ethnography, a written report and construction of fieldwork experience (cf. Kovačević 2006: 97–145). Obviously concerns about contextualized meanings, dynamics of interactions, or advantages of a holistic approach are not considered important. The proposed ‘data collection’ actually describes how the data, extracted from written monographs, is organized and presented in HRAF databases. The novelty is that the field researcher would only have to submit his field notes to a data processing centre without having to go to the trouble of writing a book about his findings. I will let the readers evaluate the merit of this proposal. One of my associates, while reading about this proposal remembered how a lecturer at the Department of Ethnology in the late 1960s and early 1970s, who, ironically, happens to be one of the authors of \textit{Jarmenovci}, gave us similar assignments. She would ask us (freshmen and sophomores) to study one or more of the famous anthropological monographs, but instead of expecting a discussion about their authors’ methods and results, we were required to ‘deconstruct’ the text in such a way so that we could fill in the pre-set rubrics, such as dwelling types, footwear, basket weaving, wedding rituals etc. with the corresponding data extracted from the book, and ‘learn’ about cultures in such atomized, disconnected way. I remember reading Margaret Mead’s \textit{New Lives for Old}, her restudy of the Island of Manus. I was at the same time fascinated with the concept of
to enumerate these differences. Suffice it to say that the aim of the research was not simply to describe village life at a particular time in history in an area that had been virtually unstudied by the methods of American anthropology, but also to channel analysis towards specific theoretical goals which were again a corollary of the American version of the discipline in the early to mid 1950s. One of these goals was to ‘attempt to depict a national character in terms of the community study approach ... and to determine the ‘typicality’ [of that village], relative to the rest of Serbia and even Yugoslavia’ by means of comparison (Halpern 1956: 1, 12). Another important objective was to study, again through the community study approach, cultural and social changes impinged ‘upon the village both from within and without’. The distinction refers to the changes brought about as a direct consequence of particular government policies, or processes of modernization (industrialization, urbanization), as well as to those that ‘have occurred indirectly, as part of an overall process of unplanned cultural transformations in Serbia in general during the past century’ (Halpern 1956: 17). A Serbian Village created an interactive image of village life at a certain time period. In time it became a memory of a moment long gone. It was an excellent baseline cross-section of a national rural culture from which the authors could branch out in time, both into the past and into the future through subsequent research that lasted almost fifty years (longitudinal studies). They also branched out methodologically, theoretically and thematically, specializing and following their own interests as well as the ever-changing field of anthropology: historic demography, cross-linking of linear and cyclical time, oral tradition from sociolinguistic perspective, medical anthropology and so on.

Parallel Paths Do Not Cross

Halpern readily acknowledged that Serbian ethnology and anthropogeography have influenced his work to an extent. This is especially evident in the attention he pays to material culture (clothing, shelter, tools, diet), regional differences and cultural history, usually absent from classic American monographs. It seems, therefore, logical to assume that the emphatically different approach and goals of American scholars researching the same phenomena as domestic ‘national scientists’ and on their own turf, would have aroused the keen interest of these scholars. Regrettfully, however, both before and after World War II, there was mostly polite detachment and disregard for what foreigners were doing. Native scholars, as cordial hosts,
were ready to offer information on available sources and literature, help with administrative procedures, and establishing contacts with local authorities, but lucid scientific debates, exchange of ideas, students and scholars, or collaboration on joint projects and publications were all lacking. As noted earlier, American scholars found that they had much more in common with sociologists, political scientists, demographers, folklorists and authors than with ethnologists. There were many reasons for that, one of the most important being the fact that before the mid 1970s very few native ethnologists spoke English well enough to be able to follow English-language literature, and therefore have insights into the discipline. The language barrier had another negative effect: even if native ethnologist spoke some English, they lacked the proficiency needed to conduct nuanced scientific debate. The same was true for visiting anthropologists: their Serbian was sufficient for everyday communications and field research, but often deficient for discussions on abstract theoretical concepts. For the same or other reasons (in my opinion, Serbs prefer oral to written communication in interpersonal relations), ‘Yugoslav colleagues had almost never corresponded’ (Halpern 1978: 5). Filipović was a notable exception to this rule. On the other hand, it is also possible that language was not the most serious obstacle in becoming familiar with foreign anthropological work. Rather, it may have been the lack of interest and initiative.

It should be stressed that the interest and arrangements for translating most if not all American and West European anthropologists into Serbo-Croatian have not come from the more traditional ethnologists but from those interested in literature (Halpern 1978: 6). But the situation changed in time:

It should be noted that the instructors [at the Department of Ethnology graduate students who are also fully employed as teaching assistants], unlike the older generation were fluent in English and knowledgeable of the English language literature (ibid: 9).

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27 It should be noted that the Halperns were well prepared and they delivered their lectures at SASA and other gatherings in the native language quite successfully.

28 It is interesting to note that Halpern wrote letters to Filipović in English and Filipović always responded in Serbian ‘so that you maintain the knowledge of the language (Letter to Halpern, 8 June 1954).

29 It is absolutely true that there was very little ethnologists’ involvement prior to the mid 1970s with the translation and publishing of capital anthropological works originally written in foreign languages. For the subsequent change, credit goes to the major publishing houses (‘Nolit’, ‘Prosveta’, ‘Beogradski izdavačko-grafički zavod’) and their editors. The longest lasting and most deserving of praise is the edition ‘XX Century’, conceived, published and now owned by linguistic anthropologist Ivan Čolović.
Another factor that might have played a role in impeding the cooperation that could have helped native ethnology redefine itself sooner was the fear or anxiety of possible negative consequences of contacts with foreign scholars. Fieldwork became especially difficult in the 1970s because of the political situation in the country and events that were an overture to the subsequent break-up of Yugoslavia. The 1974 Constitution, which provided legal grounds for the constituent republics to organize as separate states, made the internal borders far less permeable than before. Like many other domains, scholarly research was confined to the areas within a republic and this applied to both domestic and foreign scholars. For example, domestic scholars could only conduct research in a republic other than their own if there was an officially approved joint project in place. Similarly, foreigners needed to have separate fieldwork permits for each republic they intended to work in, to be associated with an appropriate scientific institution and accompanied by a domestic colleague while in the field. The situation grew far more serious towards the end of the decade:

It is our firm impression that at no time in our 25 years of experience visiting and working in Yugoslavia have the conditions for scholarly research been more difficult [‘worse than in 1948’, as one official has put it]. Without question, academic and scientific institutions reflect what a Yugoslav colleague has called the ‘security hysteria’. The most obvious reason for this is the impending demise of their charismatic leader Marshall Tito, who is now 86. Like all authoritarian and single-party states, Yugoslavia has no easy mechanism for passing on political power. Combined with this is a very real fear that the Russians will profit from the coming crisis and the ever-present nationality antagonism. To offset this, a policy of vigilance against all foreigners has been adopted, combined with increased emphasis on internal security (Halpern 1978: 41).

As domestic colleagues were required to file reports after talking to foreigners, and even asked to provide additional information by direct contact with a security officer, some have refused to talk to American and other scholars to avoid the turmoil. When in 1978 Belgrade daily Politika published an

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30 This is a very accurate assessment of life after the National Self-Defence concept was introduced that strived to mobilize the entire population. Drills of all sorts were not a rare occurrence. I will never forget an episode when, in the early 1980s, phone calls were placed in the middle of the night to SASA members and employees, as well as faculty and staff at the nearby schools (School of Philosophy and School of Philology), so that officials in charge of the Self-Defence programme could measure the time necessary for the employees to assemble. I think that we have collectively failed that test: we all took too long to respond. A large number of us brought books to read and papers to write, none of which were National Self-Defence related.
article and a pamphlet on ‘What is Espionage?’ (Djordjević, O. 1978: 58, 60) it was alleged that an ambiguous questionnaire was used by American scholars from Columbia University, which had a joint project with the Institute of Social Sciences. Professor Halpern’s name was also mentioned and it was implied that his work could potentially be associated with interests outside of purely scientific inquiry. Although his name was soon cleared from the ‘slanderous charges’ and he was given assurances from ranking Yugoslav officials that he would be always welcome to Yugoslavia, there ensued another unfortunate reaction on the domestic scene. Perhaps as a consequence of negative publicity, a potential deal with a publishing house in Belgrade to translate and publish *A Serbian Village*, for which negotiations had started that same year, fell apart. The book had to wait another twenty-eight years before it was finally published in the Serbian language in 2006 (Halpern 2006), but better late than never!

Was the Serbian side to blame for failed cooperation with American cultural anthropology? It cannot be denied that, when interest was displayed, American anthropologists offered help to their Serbian colleagues. Phillip Mosley used his influence to help Filipović spend his sabbatical in the USA. Halpern, Hammel and others worked very hard to publish a collection of Filipović’s papers in America and have his results added to the HRAF. It is true that Halpern tried to establish lasting collegial relationships and collaboration with Serbian scholars in the 1970s and 1980s, but it is also true that, in spite of all good intentions, there have been some misunderstandings and disappointments that have impeded the development of more meaningful and productive professional relationships. While Hammel developed a lucrative collaboration with Croatian ethnologists, which included joint projects, publications and exchange of students (American students conducted field research in Croatia, and Croatian students were offered an opportunity to complete their graduate studies at Berkley – the best of two worlds), Halpern could not boast of such successes. Was he too focused on his own research agenda to notice and support his Serbian colleagues’ needs? If this were so, that could have only been a partial answer. More important, in my opinion, was the fact that at the time when Serbian ethnology was ready for a radical change in direction, the interests of its scholars, particularly of younger generation, had considerably diverged from what Halpern was offering. The new generation needed to distance itself from traditional topics and methods of the ‘old ethnology’ and delve into something entirely different in its quest for its own novel identity. French and British anthropology, functional analyses and structuralism, revisiting old ethnographic materials and reinterpreting them from new theoretical platforms seem to have been more attractive (cf. Kovačević’s analysis of the popularity of Van Gennep’s *rites de
passage in Serbian ethnology in the late 1970s and 1980s, 2006: 75–93). Population studies, historic demography, familial and kinship structures, although theoretically and methodologically innovative compared to Serbian attempts in the same area of research, were thematically too reminiscent of traditional topoi to be appealing as a renewed paradigm in Serbian ethnology. This was an unfortunate but logical choice, and another lost opportunity to choose and use, if not the entire package, then at least some elements of the American model.

The Change

Several articles have been written describing the turning point in Serbian ethnology in the mid 1970s and its development since then (Pavković et al. 1983; Kovačević 2006: 52–61). I agree with the arguments presented there and I can only offer my personal views and experience as additional comments about the era. In the late 1960s, especially after the ‘revolutionary ’68’, social sciences became attractive not as an instrument of reinforcing the ideology and the position of the regime, but rather as a way to question the system and search for answers and deeper meanings. As a consequence there was also an increase in enrolment at the Department of Ethnology. Among new groups of students there were not only growing numbers of urbanites but also those who had lived abroad for extended periods of time with their families in diplomatic service or technical aid programmes. Fluent in foreign languages, well educated and with personal multicultural experiences, those students have introduced interest in cultures other than their own and revealed different, higher expectations from their studies. However, the response of the faculty and attempts to change the curriculum were considerably lagging behind. Students were thus left with basically two choices: to let their disappointment drive them to pursue other professions or to attempt to self-educate, through literature and/or occasional travel abroad. The socio-cultural context was very favourable. The ‘golden ’70s’ offered not only constant betterment of the standard of living (based more on foreign financial aid than on real economic growth), but also a liberal and stimulating intellectual climate that made educational aspirations possible. However, the real change, or incentive for change, within the discipline came ‘from above’, when in the early 1970s new faculty members were hired. Bringing in outsiders’ fresh views, they managed to challenge the long existing stalemate from the very beginning. For my personal development (just as I was ready to take off and continue my studies abroad), credit goes to the new

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31 I was one of them and I know how much living with my parents in Ghana, West Africa, for five years at a young age, had influenced my choice of profession.
faculty members, and especially Professor Djurdjica Petrović. An ethnologist who had acquired knowledge in history and history of art, skills in using different sources, and who had brought an end to timelessness in studying culture, insisting to the contrary on spatial and temporal precision, did not shy away from comparisons, generalizations and insightful interpretations. Secure in her own domain of research, she possessed one of the most desired qualities a teacher could have: to allow her students the freedom to choose their own paths, as long as they were methodologically and theoretically grounded. Although she had very little experience with relatively recent theories, she had no problem letting me experiment in my masters degree thesis (defended in 1978) with carnival rituals as rites of passage (Van Gennep, E. Leach, V. Turner and later T. Turner and R. Da Matta). In the next phase, while defining the theme of my doctoral thesis, I needed to establish an even greater distance from traditional ethnological topics. Consequently, I chose a subject that belonged to, at that time, the most unpopular domain, material culture and within it styles of clothing, existing mostly, before Professor Petrović began to teach that subject, at a meaningless, unattractive, descriptive level. My goal was to prove that there are no inferior subjects, only inferior approaches, and that all forms of expressive culture are equal in deepening our understanding of culture and society as a whole. Further, I selected to study the socio-cultural aspects of clothing, traditional and fashionable, again in contrast to the ‘old’ traditional ethnology immersed in the peasant society, in urban areas, notably the capital city, Belgrade, in historical perspective, in the nineteenth and early twentieth centuries. The choice of the timeframe rendered only very limited value to filed research, while the majority of information had to be gathered from various independent contemporary sources (archival documents, the press, photographs, paintings, museum objects and many others) which were overwhelmingly abundant for the period.

The freedom that we were trusted with while struggling to find our own paths, we have granted to our students as well. As a result, Serbian anthropology/ethnology, as redefined in the last quarter of the twentieth century, has developed an incredible diversity of styles and topics, preventing the discipline from falling into the trap of ‘movements’ or ‘schools’.

Epilogue

However, I have never doubted that field research, still a trade-mark of anthropological discipline, if conducted responsibly, is an extremely valuable tool in the study of culture. I am positive that even now when native anthropology in Serbia has become thoroughly ‘anthropologized’, and when there are lively discussions about the pros and cons of both modern and post-
modern ethnographies, Halpern's 'old monograph' on a Serbian village will continue to be a useful and inspiring source of information of 'how it all once was'. Personally I am very grateful for the exposure I had with the Halpers in the summer of 1978. By then most of us had realized that not only had the 'old ethnology' no future, but that we had no future with it as well. I learned so much that summer about so many things. The most shocking revelation was that I discovered so much about a subculture of my own culture because I saw it through the eyes of American colleagues. At that time, we, the native ethnologists, went on short excursion-style trips to the field. We would usually go in a group, stay in the best available hotel in the local town and radiate from there into villages. We interviewed the villagers in formal settings and everything was highly artificial and stylized. There was a definite distinction between WE, the city scholars, and THEY, the local population. We always went back to our hotel at dinner time. We observed the village life from outside and above and carefully led our informers, not where they wanted to go, but where we needed them to go.

In the summer of 1978 all my fieldwork perceptions changed. I stayed an entire month with the Halpers in their village hosts' family home, far away from the village centre, in a hamlet where the nearest neighbours were either 'down the glen' or 'over the hill'. We spent all twenty-nine days, talking, observing, participating (Carla, Halperns' young daughter, and I managed to lose pigs that were entrusted to our care and then call Mrs. Halpern, by then an experienced 'villager', to help us find them), sharing the good and the bad all day long, from sunrise to sunset. But what a learning experience! First of all I learned how it feels to go through a separation phase from my normal life and live through the liminal time of field research. I learned that one learns the most when not trying to learn but simply sharing chores with the villagers or following them around either on their daily routines, or extraordinary practices like going to the market town, or county fair, or a funeral or celebration of some kind. I learned how specific intentions can be so easily lost in translation. Some years earlier Halpern got fed-up with the chickens ruling the yard and entering the house at their own free will. He thought that it would be nice to have them contained, so he bought chicken coop wire. Our hosts were thrilled with the gift, but they used it in their own, opposite way. They fenced off a small portion of the yard next to the house to keep the chickens away from the dwelling, but allowed them to continue to roam freely beyond the wired area: people were fenced in, while the best quality production of cage-free chickens (and eggs) was maintained. I also learned to observe a bard and decode nuances of meaning imparted by non-verbal communication. I learned to see, by the motion of their heads and eyes, how a person reciting his family genealogy
was using a mental map to help him place generations of his relatives. I learned how important it is to document the process of documenting. For all that and much more, I will be forever grateful to my own special couple of American anthropologists. I felt as if I were lifted from a labyrinth in which I was lost with no hope of finding my way out and enabled to see the pattern from the bird’s eye perspective. Later on, I realized that there was also a constant, very intricate pattern of multiple insider–outsider border crossings. And finally, I wish more of us had shared the same experience much earlier. It could have led Serbian ethnology down a different path much sooner.

References


