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Recalling Fieldwork

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RECALLING FIELDWORK

People, Places and Encounters

edited by

Raluca Mateoc and Franc;ois Ruegg

LIT

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A month after I drafted this introduction, the very day I sent it to him, Christian Giordano passed away in Vilnius. We were going to write it together, he said he would have some time during the winter holidays. I shall much regret the absence of his contribution to this introduction. I always welcomed and appreciated his critical and sensitive remarks and suggestions. We worked and taught together, specifically in Romania, for the last 15 years. His presence and intellectual companionship will be terribly missed.

This volume is devoted to his memory.

Introduction

Fran9ois Riiegg

It i s always meaningful to look back at the source and context of one's anthropological inspiration, particu larly if one can compare them with those of colleagues who come from different academic trad itions and political contexts which motivated and shaped their fieldwork.

This reflective moment is also a consequence of the reflexive trend, initiated in the late 1980, to speak about one's own field experience, which includes giving more space to the 'voices' of interviewed people in the final ethnography. In the present book, some authors mention and quote their diary; others refer to Malinowski's posthumou s diary, revealing a strong discrepancy between the results of his research and his feelings. We still live in a moment when expressing one's feelings whi le doing ethnography is seen as being not ju st human, but also an intrinsic part of the process, a movement that leads some researchers to create or participate in the elaboration of an anthropology of emotions. This is not our point here however. In reflecting on and recalling our fieldwork we wanted to unveil the various motivations that brought us to do fieldwork in this part of the world, during the communist era as well as afterwards. We wanted also to see commonaliti es or differences in our respective approaches, training and methods.

It is in th e *Academica* hotel, here in Bucharest, that I am reading the contributions we received for the present volume, this November 2018.

I used to pick up my monthly scholarship money, amounting to some 1000 Lei, just next door at the *Facultatea de Drept* (Faculty of Law) during the 1972- 1973 academic year. With my *Universite de Fribourg* colleague Christian Giordano we taught twice a year at the Faculty of Philosophy in Grozaveti, where I once used to lodge in a private room in the student 'bloc ' for foreigners, among many Vietnamese and Latin Americans groups and a couple of lost philologists from Northern Europe, Russia and the United States. The very building where we taught used to be my canteen. Apart from this switch of function for the building and the Carrefour shopping center, very little has changed here. The heating system is still on for the winter at a fixed date with no ways to control it. Granted, there are now air conditioning systems, but the rest - the porter 's lodge, the furniture in the classrooms, the bathrooms, the ambiance -is pretty much still the same.

I went back to the COS and the *Casa Universitarilor,* restaurants for academics to which we had access as foreigners, as well as to the Military Circle, a pre-communist style institution near the university of Bucharest, that rather refers to the interwar period and to the Vienna Opera! They too are unchanged. I have always wondered about what exactly was due to the communist regime and what was just inherited 'bourgeois ' legacy. Similarly, Kogalniceanu has replaced Gheorghiu-Dej on the square and has given his name to it and to the Boulevard. An Italian cafe *to-go* stands now where the old Turkish coffeeshop *la nisip* was, a nice and hot place in the winter because of the hot sand it used to heat the *ibric.* Otherwise, the same streetcars still hiss around the garden of Cimigiu ...

Among the texts received, there are five recalling Romania (Geana, Kligman, Rostas, Sampson, Verdery), two Bulgaria (Creed and Silverman), one Tchequo(slovaquia) and South-Africa (Skalnik), and two Romania and Bulgaria and other places (Giordano, Rliegg).

Spied Spies

What first struck me in reading these essays, are the commonalities among the North - American scholars' narratives, who constitute half of the contributions to the present volume. Contrary to us Western Europeans, their fieldwork took place in the framework of a plan, an organized 'political exchange', strongly marked by the Cold War. Their stay was highly institutionalized , prepared sometimes even by their professors and very carefully conceived, even supported by their Embassy. In some cases, the

fieldwork was somehow also monitored (Kligman, Sampson, Verdery). This was less the case for those who did their fieldwork in Bulgaria (Si l-verman and Creed) and had such a hard time to get permission to study the group they had chosen and to go where they wanted. Thus the 'political ' stamp of their research , or more precisely, the references to espionage and secret police are omnipresent in their account. It may be linked with the fact that they had a longer stay in one single village and had therefore to establi sh much more formal relationships with the local authorities than those of us who made shorter fieldwork or multisite ones.

Similarly, their files at the secret police constitute in some cases an additional source for ethnography, after having been a matter of anxiety. In this context, I must confess that I haven 't yet cared to look up my fi le at the Securitate. [1] I still believe that, being from a small and neutral country, Switzerland, I was not such an interesting subject to spy on. I missed also the opportunity to check my Swiss file when the scandal of the Swiss secret :files arose in 1989. Visiting and carrying out research in a socialist country was probably suspect and my successi ve academic scholarships in Eastern Europe were a clear sign of obstinacy. Only once was I overtly and literally pursued by a soldier after having inadvertently taken a picture in a military zone in Dobrudja.[2]

Forbidden Anthropology

By the same token but on a different register, the diverse com munist re-gimes hindered not only foreigners (see particularly Creed) but also their own scholars to practice social anthropology. Peter Skalnik in particular , but also Gheorghita Geana, were prevented from practici ng exotic ethnog-raphy, at least as long as the regime lasted. We all remember how difficult it was for our colleagues in the Warsaw-pact countries to get access to any foreign literature, scientific or not.

For many years after the fall of the Berlin wall, we tried to intro-duce social anthropology in Romanian Universities (as well as in Bulgaria) with little success. It worked in Timi oara for a couple of years, it works

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[1] Since then, I have received confirmation from the CNAS that there are no files under my name at the *Securitate.*

[2] The film was developed and returned to me in a perfect condition. But it is true that my car was confiscated after I had forgotten to ask for Romanian plates.

now in Cluj and in some ways in Bucharest, as well as in Sofia and Plov-
div. This was an important factor in the imbalance and sometimes lack of
understanding, to say nothing yet about methods, between our Eastern col-
leagues, ethnograph ers and folklorists, and us. The contribution of Zoltan
Rostas explains it a contrario (see Rostas, this volume). Depending on the
years, the Ceauescu regime was more or Jess lenient. I had the fortune of
living the last 'liberal ' years, before 1974, at least from the point of view
of freedom of movement in the country and the availability of food.

Village Ethnography, Political Anthropology

Another commonality is the long-stay method of participant observation
(attributed to Malinowski) and the village ethnography performed by the
American anthropologists represented here either in Bulgaria or in Roma-
nia. This time spent in the countryside, interpreted by many of us both
'native' (Geana) and foreign, as an initiation rite, also had a social dimen-
sion. Having to share primitive accommodations, rural food and walking
in the mud in the wintertime -not to speak about the ability to drink *fuica*
or *rakiya,* affected people differently, depending on their previous experi-
ences.

      Some contributors were in the field alone, others were with their
spouse or with colleagues working in a nearby village. From my memory
of my time in the field, the Romanian ethnographer teams, had a harder
time to adapt to the country life. They kept their distance by sleeping in
hotels and dressing in a city-like code. More recently in Bulgaria, I noted
the same phenomenon: Sofia female students still wore delicate shoes and
trousers to go on summer school fieldtrips in the muddy embankments of
the Danube river.

      It is also necessary to remember that 'native' ethnographers and
folklorists in Eastern countries used to do massive fieldwork, nicely called
'team ethnography ' (Rostas; Ruegg). In my view, this is not very different
from what Marcel Griaule practiced in French Equatoria l Africa in the
1930s, a policelike examination of 'tri bes ', accompanied by a razzia of
ethnographic and sometimes sacred objects, disturbing the normal daily
way of life of the village. I mentioned my appalled reaction to this invading
method in my own diary, when accompanying an ethnographic expedition
of the Ethnographic Institute in the autumn of 1973 in Oltenia.

      Another common aspect is the specific angle under which most –
but not all - of the authors, regardless of their home country, looked at

'their ' villages. They concentrated on the socio-economic life: collectivi-zation, change, agrarian policies and reforms, informality and bureaucracy under and after communism or late socialism. I think it is fair therefore to call it political anthropology. This was also the focus of Christian Giordano, after socialism, referring to his past anthropological experience in Sicily.

Peter Skalnfk and Gheorghita Geana, on the other hand, were con-fined to practice 'village' or rural ethnography. In addition, some authors were more sensitive to or concentrated on minorities, Pomaks or Roma/Gypsies, Germans (Saxons and Schwabs), Hungarians (Szekler) or Lippovans (Old believers), even though the respective Socialist republics rarely mentioned them officially at this time (Silverman, Ruegg).

For those who did not chose political anthropology, specific themes were treated such as sorcery (Kligman), folk-music (Silverman) or rural architecture (Ruegg).

Anticipation of Forthcoming Topics

Among the topics dealt with by the authors, some became central in the years following the fall of the Berlin Wall. Minorities, whether ethnic or religious, would come to occupy a broad segment of social anthropology of Eastern Europe, going along with (or against!) neo-national isms and ethnic revivals, not to mention ethnic cleansing. In Romania it started with Transylvania and the 'Hungarian' minority, to later shift to the 'Roma', and sometimes the Tatars, the Turks and the Lippovans. In Bulgaria the Turks, the Pomaks and mostly the Roma attracted much attention on the part of anthropologists. This is clearly linked with the development of NGO networks advocating for minorities and parallels the trend of 'social construction of one's self .

On the contrary, religion , which became a central research topic in the aftermath of socialism, was hardly studied during 'our' time, apart from its ethnic aspect.

As mentioned previously, the political dimension which dominates this collection continues to interest anthropologists today: collectivization, agrarian reforms and more general themes of 'transition' are still being harshly debated, especially since, as Giordano stated, 'transition never ended'. In post-socialist studies, the understanding through daily life ex-

periences of the internal contradictions and actual adaptations of communism especially in its later stage (Creed), would become topics named corruption, clientelism and infonnal economy (Giordano).

Conclusion

In this volume, it was not possible to give a complete view of anthropologists working in and on Eastern Europe during socialism and shortly after. However, we were able to acquire good insight into the conditions in which anthropology was possible, both for insiders (not to use the word native) and outsiders. We learned about the difficulties they encountered in the field, the tradition they came from and followed (Rostas), the support they received or did not receive, the topics they chose and even their personal feelings in the location.

I wish to thank Raluca Mateoc for having inspired, launched and carried out this very exciting project. She spared no effort to complete this book.

We also thank the authors for sharing their stories. These stories represent a fine contribution to the history of anthropology in this part of the world.

Spring 2019

Ethnographies across
Field Sites

© François Rtiegg. Oltenie, portrait.

© Peter Skalnik. Ethnographer with head teacher R. Barlok,
Nizm\ Sunava, September 1970.

From Sicily to Malaysia via Bulgaria:

Finding the Common Thread of One's Fieldwork

Christian Giordanof

Introduction: One Anthropologist, One Field?

There is a widespread cliche, if not indeed a myth, that anthropologists are identified with their researches in a specific field by their colleagues and especially by outsiders, thus spawning the misleading narrative of an intimate and nearly exclusive link between a given anthropologist and a given field, viewed almost as his own property on which others cannot or will not tread. This has led to the fiction of *one anthropologist, onefield,* usually in a markedly exotic location hard ly reachable by regular means of transportat ion.

Through emblematic examples drawn from the professional biographies of anthropologists who rose to fame also because of their apparent communion with a specific field, I intend to show that these *maftres a penser* of our discipline developed or experienced diverse fieldwork in different parts of the world. I will reference principally four anthropologists who directly or indirectly affected some of my choices, not so much geographically as in terms of theme: namely, Bronislaw Malinowski, Edmund Leach, Raymond Firth and Oscar Lewis.

Malinowski is famously associated with his fieldwork on the societies of the Trobriand Islands (Malinowski 1922; Malinowski 1926). In fact, the binomial Malinowski/Trobriand is familiar even to those with sketchy notions of anthropo logy. One tends to forget, however, that Malinowski , who had emigrated to the US, died suddenly in New Haven on May 16, 1942 wh ile making thorough plans for fieldwork in Oaxaca, Mexico. Consequent ly, Malinowski meant to conduct a new research that would have been both theoretically and thematically very different from the one in the Trobriand Islands.

Then there is the case of Edmund Leach, renowned for his researches on the cyclic modifications of political and power structures

among the Kachin in the northern highland s of Burma (present-day Myanmar) (Leach 1954; Kuper 1993). One tends to forget that Leach also covered other important fields, such as the analysis of the social and economic organization of a Kurdish community in Iraq (Leach 1940; Kuper 1993: 155) and research es in Pu! Eliya, a peasant village in Ceylon (present-day Sri Lanka), in which Leach analyzed the relation between symbols and kinship (Leach 1961).

        Even more interesting in terms of plurality of fields is Raymond Firth, celebrated as the most distinguished representative of British economic anthropology but principally for his researches at Tikopia that dealt with the social and economic life of a small isolated community of Polynesian origin in the Solomon Islands archipelago, which instead is culturally Melanesian (Firth 1936; Firth 1939). Besides this study on a society untouched by Western modernity , thus regarded as primitive , Firth conducted two other intensive yet currently less renowned fieldworks. The first one, in relation to his doctoral thesis, focused on the economic practices among the New Zealander Maori (Firth 1929), whereas the second one, coming after his researches in Tikopia, revolved around an analysis of economy among fishermen comm unities in the sultanates of Kelantan and Terengganu (in present-day eastern Malaysia). Following this specific fieldwork experience, Firth wrote the brilliant but still, little-known book *Malay Fishermen* (Firth 1966). In my opinion, this vol ume is even more interesting, both theoretically and methodologically , than his celebrated research on Tikopia. Lastly, Firth also supervised an urban anthropology fieldwork on kinship relationships among London's middle class (Firth Hubert; Forge 1970).

        Oscar Lewis, one of the most distinguished exponents of US urban anthropology, is renowned for creating the concept of the *culture of poverty* (1966: 19 ff.). Specifically, he studied the representations and social practices of actors with kinship ties who dwelt in the slums of large Latin American cities (Lewis 1966a: 19 ff.). At first, Lewis focused on a specifi c field, i.e. on the society and culture of these built-up areas of Mexico City and, by mean s of a specific version of the autobiographical method, reconstructed the precarious existence and the resulting everyday survival strategies of two Mexican families in a constant state of poverty on the fringes of society (Lewis 1961; Lewis 1964). Oscar Lewis then travelled to Puerto Rico, thus expand ing his empirical knowledge of the *culture of poverty* thanks to this new field (1966 b). His sudden and premature death brought his researches to an end, but one can reasonably assume that Lewis would

have extended his fieldwork to other societies, Latin American or other-
wise, marked by the *culture of poverty.*

My choice of these four authors does not have the ambition to be
exhaustive, since my picks are rather arbitrary due to my selective, yet not
totally irrelevant expertise. In fact, several other distinguished anthropol-
ogists who did not limit their research to a single field could be mentioned.
Considering the history of anthropology, therefore, the famous equation
one anthropologist, one field is rather a fiction spread by people unfamiliar
with this discipline's past than a methodological reality. Indeed, one could
add that if anthropology seeks to be defined as a comparative social sci-
ence, then this discipline's empirical research must perforce opt for multi-
ple experiences in diverse and contrasting fields. This is the only possible
way to draw sound comparative parallels between societies and cultures
on the strength of dialectical or even divergent empirical experiences.

My firm endorsement of a distinctly empirical pluralism, however,
is also a legitimate justification for my personal choices since, in fact, I
have not circumscribed my researches to a single field. Over the course of
about half a century, I have had the chance, luckily I would add, to conduct
long-term empirical research in three different locations, i.e. Sicily, Bul-
garia and Malaysia, along with other fieldwork experiences, such as more
temporary and less systematic stays in Spain, Portugal, Paraguay, Greece,
Turkey and Poland, which, though certainly shorter, minor and relatively
on-the-surface, were not utterly fruitless, unjustified and arbitrary.

First and Fundamental Fieldwork Experiences in Sicily

My first fieldwork in Sicily was linked to my university studies in sociol-
ogy (not anthropology) at the university of Heidelberg. At this renowned
German university, sociology and anthropology were joined in a single
department, thus there were several interaction s, at times also confronta-
tional, between the two disciplines' professors and students. The sociolo-
gists believed they were far more avant-garde in the field of social theory
and viewed anthropology rather as a descriptive discipline, low on concep-
tual ambitions, whereas the anthropologists considered sociology an
overly dry and abstract subject. As a foreigner witnessing these debates on
the differences between the two disciplines, which I regarded as very Ger-
man given their methodological, theoretical and conceptual rigidity, I con-
cluded instead that striving for a greater unity while taking into account

the undeniabl e differences between the two disciplines would have been much more stimulatin g.

I thought it would be interesting, therefore, to theorize on a possible interdisciplinary *unity in separation.* Sociology, as an essentially German discipline, fascinated me primarily because of its theoretical conceptual- ization potential dating back to the founding fathers of this subject, pri- marily Max Weber, Georg Simmel, Alfred Schiltz and his pupils Peter Berger and Thomas Ludemann (Weber 1956; Simmel 1908; Schiltz 1931; Berger and Luckmann 1966). On the other hand, social anthropology, of British origin, appealed to me chiefly because of its empirical pragmatism, thus because of its indispensable fieldwork, as set out in the classics men- tioned in this article's introduction. Therefore, I believed it would have been interesting to determine whether certain theoretical reflections of so- ciology would be corroborated by fieldwork or whether anthropology 's empirical observation would lead to a different result, which in turn might lead to modifying sociology's theoretical abstractions.

I must admit that to this day I am still and always between a rock and a hard place, i.e. between Scylla – sociology – and Charybdis – an- thropology. Though this stance may seem tricky and all but shocking, I find it rewarding precisely because of its multiple and dialectical perspec- tives. At times colleagues ask me, 'so, which side are you on?' and I truly do not know what to reply because I am on neither side or perhaps on both.

Coming back to the matter of fieldwork, i.e. this article's subject, my first opportunity, i.e. the opportunity to lose one's methodological vir- ginity, came up when I took part in an exercise in situ in Sicily organized by the Institute of Sociology and Ethnology of the University of Heidel- berg in the late 1960s. It was precisely during this stay that sociologist and ethnologist Wilhelm E. Miihlmann, as my supervisor, made me appreciate the theoretical significance of Husserl's phenom enological reduction and consequently of setting aside one's own world with its values and certain- ties (Husserl 1986). At the same time, this unconventional and outstand- ingly erudite exponent of German social sciences also made me aware of the importance of the Weberian interpretive approach, which Clifford Geertz would later employ in his researches in Bali and in Morocco (We- ber 1968; Geertz 1973; Geertz 1995).

Once I had grasped these two fundamental theoretical and method- ological teachings, Mtihlmann proposed me a doctoral thesis theme. It in- volved studying the social and political role of workers, artisans and

peasants' associations in Sicily over the forty years following Italy's unification (1860-1900) through archival documents. This proposal had a distinctly historical quality, yet I accepted it because I intended to use a genuinely anthropological approach based on the *network analysis,* which was very popular at the time and favored by notable exponents of the *Manchester School* then headed by Max Gluckman (Boissevain, Mitchell 1973). Therefore, mine was a rather unusual attempt to apply an anthropological method of analysis to a reality of the past that historians had studied through other methods based on these organizations' formal aspects. Instead, I would focus on the informal, thus clientelistic dimension of these associations dominated by politicians, including ones linked to the Mafia, who exploited them for electoral purposes (Giordano 1975).

I firmly believe that the archival documents ultimately proved my point. In fact, my second research in Sicily, financed by the Swiss National Science Foundation (SNF) in the late 1970s, i.e.after passing my doctorate exams and having become a research fellow at the Institute of Sociology at the University of Basel, was based on an analysis of the contemporary activities of cooperatives, mainly agricultural ones, in three Sicilian provinces (Giordano, Hettlage 1975; Giordano, Hettlage 1979). In this case, too, I focused chiefly on the relevance of networks and personalized relationships, on the structure of informal coalitions between the associations' members, their leaders and politicians, and finally on the reciprocal and often clientelistic services rendered.As in the case of workers, artisans and peasants' societies, the politicians in particular exploited the cooperatives for sheer electoral purposes. The noteworthy difference between the historical workers, artisans and peasants' associations and the contemporary cooperatives was essentially that the former revealed personalized clientelistic services, whereas the latter revealed a widespread clientelism based on a form of welfare allocation involving state or EU subsides. Ultimately, thanks to the good offices of politicians, members of the cooperatives would receive financial contributions for their economic activities via the cooperatives in exchange for the votes of entire families or kin groups (Giordano 1982). These transactions were possible thanks to the intercession of the cooperatives' leaders who in turn gained more prestige and local power. It was a well-oiled system that for the most part ran smoothly and at the time was practically generalized. In brief, Rome and Brussels were their cash cows.

My fieldwork in Sicily continued into the early 1980s when I became assistant professor at the University of Frankfurt. Thanks also to the

exceptional collaboration with Ina-Maria Greverus, head of the Institute of
Cultural Anthropology and European Ethnology, I was able to broaden my
empirical knowledge of the Sicilian reality as well as significantly expand
and improve by theoretical resources, especially the anthropological ones.
During this time, in line with previous political anthropology researches
and also with the students ' assistance in fieldwork, I focused on how the
State was perceived by the population of a number of Sicilian municipali-
ties. This may well be an infinite theme since through a qualitative meth-
odology we were able to piece together specific social representations of
the State. In turn, these representations have a significant influence on eve-
ryday behaviors. The results, rather unsurprising but still astonishing,
clearly revealed skepticism and at times also contempt and rejection in
connection with the State, which however was viewed as an important pro-
vider of economic resources given the reallocation of welfare at the time.
Consistent with the terminology proposed by Max Weber (Weber 1956),
these Sicilian experiences led me to theoretically conceive the cleavage
between the legality of the State, legislature, government and bureaucracy
on the one hand, and the severe deficit of legitimacy of public institutions
and politicians on the other. Yet, this cleavage can be interpreted anthro-
pologically solely through other concepts such as public mistrust, person-
alized relationships and informality, which would become a veritable
leitmotif in my political anthropology contributions, not only in those con-
cerning Sicily but also and foremost in relation to theoretical reasoning.
This cleavage between legality and legitimacy in the minds of citizens is
precisely what made me understand why they tend to informalize folmal
public structures by means of highly personalized networks.

From the Fall of the Berlin Wall to the Discovery of Bulgaria as a Subject of Anthropological Research: Fieldwork Continuity and Discontinuity

The fall of the Berlin Wall at the end of 1989 opened the borders of the
by-now former socialist countries also to anthropologists. This change,
which occurred around the same time I was appointed full professor of
social anthropology at the University of Freiburg (Switzerland), gave me
the opportunity to start fieldwork in Bulgaria, regarded as the nation most
loyal to the Soviet Union . However, I already had some fieldwork experi-
ence in Poland, moreover rather superficial, thanks to a collaboration with
the University of Torun's rural sociologists.

Poland was famously the less Sovietized country of the Eastern bloc; its agricultural sector had only been partially Sovietized, i.e. the land's collectivization had been incomplete and limited to large landed estates, whereas small holdings in particular had been left nearly intact (Giordano 1988: 177-198). In fact, in the years following WWII, peasants and smallholders, along with workers, were viewed as the backbone of the new socialist society.

Within this context, I focused principally on the phenomenon of peasant-workers *(chlop-robotnik).* These were people who worked in the privatized agricu ltural sector and concurrently employed in factories as workers. Interestingly, these social actors gave priority to the informal agricultural sector, which guaranteed a markedly higher income, over their formal activity as workers in a factory.

This led to my interest in field research on the re-privatization of agriculture in the post-socialist scenario. Consequently, I chose the most Sovietized society of the now dissolved Warsaw Pact: Bulgaria, Moscow's most loyal ally owing also to linguistic and cultural affinities. Thanks largely to a colleague from Sophia, we decided to carry out an anthropological research, thus a qualitative one, on rural decollectivization in Dobrudzha, a region in the country's furthermost northeast on the Black Sea. During the socialist era, this very fertile region, regarded as Bulgaria's granary, had undergone a rapid collectivization because along with small parcels of land it comprised properties that were viewed as extensive, though they were not actual latifundia as the ones of the boyar in nearby Romania.

Under socialism, therefore, a system of large collectivized cooperatives took shape especially in Dobrudzha, yet a concurrent policy of forced industrialization involving a process of urbanization of a fair chunk of rural population was prioritized.

The post-socialist policy was a sort of *back to the future,* i.e. the new elite in power had a rather populist and unlikely vision of a return to the land since the land reform in the early 1990s provided for the restoration of property boundarie s to what they had been before the socialist era. In the meanwhile, however, the former owners' children or grandchildren had no intention to renounce their property's restoration, but neither they were willing to start off from a situation dating back forty years to become the farmers of tomorrow , given especially the far more interesting and above all more remunerative job opportunities in the urban industrial and tertiary sectors.

This socioeconomic context was the setting of our fieldwork in Do-brudzha. Who were the new social actors in this region's new/old agricul-tural sector was the fundamental question of the research.

The findings of our fieldwork were at first unexpected, yet ulti-mately logical and above all rational from an anthropological point of view. To begin with, we were dealing with a reprivatization of agriculture with no peasants because the existing social relations mirrored the socialist ones but looked like capitalist ones. In sociological terms, the social actors of the old system were, rather surprisingly for us researchers at least, the same ones of the new system (Giordano, Kostova 1995: 57 ff.; Giordano, Kostova 2001: 5 ff.). More specifically, the social structure of the old so-cialist cooperatives along with its relations of power had replicated them-selves in the post-socialist era. In other words, the old leaders of the socialist cooperatives had turned into capitalist entrepreneurs, whereas those who cultivated the land for the new owners were the same people, with due exceptions, who prior to 1989 had been employees or much more often agricultural workers in the collectivized agricultural enterprises. The networks of personalized relationships born under socialism tended to re-produce themselves in the post-socialist era.

For an anthropologist from a Western capitalist country, it was very interesting to see that old managers and new entrepreneurs had quickly picked up the basics of capitalism and most importantly had gained control of large parcels of land (in one specific case, nearly 5000 hectares) in part purchased and in part leased from the new owners.

Consequently, our research focused on the new activities of these agricultural entrepreneurs, knowing full well that we were aiming to achieve an anthropology of the new elites in rural Dobrudzha. Our inter-viewees, well aware of our curiosity, were very friendly and open about our questions. The free-form interviews, thus without a questionnaire, were very thorough and detailed especially in regard to matters concerning the new relations of property. In the end, we decided to study a small num-ber of new agricultural entrepreneurs and selected a particularly forthcom-ing one who later became our primary informant. Thus, we were able to reconstruct a very interesting and biographical *case study,* as often occurs in anthropology (Lewis 1964).

These favorable circumstances allowed us to observe the field up-close: no easy matter in Dobrudzha's rather closed society. Our brand-new agricultural entrepreneur, and former head of the now dissolved collecti-

vized cooperative, even showed us around what may be defined as his personal *latifundium,* consisting chiefly ofleased parcels and to a lesser extent of purchased ones. This explained why these actors are locally known as *arendatori,* i.e. as *tenantfarmers,* not as *landlords.*

Our *arendator,* however, had built himself a luxurious home located right in the middle of his lands since he was positive that quite soon he would have been able to take over also the remaining parcels up for lease. What amazed me during this tour, given the person's ideological background, was to come upon an Orthodox chapel in the garden surrounding the villa. Even more surprising was the spacious meeting hall inside the villa where the employees of our *arendator,* i.e. some of the collectivized cooperative's former members, could have a meal and relax under the watchful eye of a portrait of Che Guevara. Moreover, this room contained a small photographic display of the highpoints of the now dissolved cooperative founded by our host's father during the collectivization period and which our interviewee had practically *inherited* thanks to the support of the local Communist Party. This was proof that, despite everything, amongst the people there was a somewhat nostalgic feeling about the past and a highly skeptical feeling about the present.

Finally, we need to add that our *arendator,* with whom we met several times also at a later date, not only went on to become a successful entrepreneur, but has also been socially acknowledged, thus allowing him to join the formal circles of the new economic and political powerful.

At this point we can justifiably ask what theoretical lesson may be drawn from this fieldwork. I believe this research throws into relief the social process that I had already observed and highlighted during my fieldwork in Sicily. There is a conclusively significant continuity between past and present that is distinctly observable at a micro-social level also in Bulgaria and specifically in the case of Dobrudzha's *arendatori.* Ultimately, there was a continuity of power relations during the transition from the Soviet-like socialist system to the capitalist one of Western origin. There was, indeed, a change of system, but the former social actors stayed in place along with their roles and networks. Just as in Sicily, in this case as well I witnessed an informalization of formal structures.

Thus, it was thanks to fieldwork, i.e. through empirical researches concerning the replication of hierarchies, that a few years later, when I was more familiar with the field, I was able to formulate a paradoxical theoretical observation; *the transition is over because it never started* (Giordano 2005: 7-23). Moreover, the fieldwork experiences in Bulgaria brought to

mind Sicily and made me think of the famous quote from the insightful novel *fl Gattopardo* by Giuseppe di Tomasi di Lampedusa: *if we want everything to remain the same, everything must change* (Tomasi di Lampedusa 1958). A new and specific form of *Gattopardo-like stance* could be observed among these novel large tenants who to some extent had also become major landowners: a socioeconomic tactic peculiar to those who, having been part of the past regime's local establi shment, decide to adapt to the new situation by adopting radical capitalist practices that allow them to maintain their social standing and increase their economic prospects.

From a Leisure Trip in Southeast Asia to Fieldwork in Malaysia: The City of Penang as a Metaphor of Urban Multiculturalism

In the spring of 1991, soon after being appointed full professor of social anthropology at the University of Freiburg, I went on my first trip to Southeast Asia starting out from Singapore. The itinerary included a short stop in Penang before reach ing Sumatra, specifically Lake Toba, famous especially for its close link to the Batak culture, renowned also in Europe for its *Rumah Bolon,* i.e. the architecturally striking and richly decorated characteristic houses. After this first leg, the trip continued to Java and especially Bandung and Yogyakarta, and finally Bali and Lombok .

Yet, I was particularly struck by Penang, moreover because my secondary subject during my sociology course at the University of Heidelberg had been South Asian history. Professor Dietmar Rothermund, an internationall y renowned Indologist, introduced me to colonial Malaysia and especially the British presence in this country, thus to the three *Crown Colonies* of the *Straits Settlements:* namely Singapore, Malacca and Penang. Penang in particular had fired my then strongly exoticizing and orientalistic (Said 1979) imagination, not to mention the role played by Emilio Salgari's novels, i.e. *The Pirates of Malaysia* and *The Tigers of Mompracem*, as well as *Lord Jim,* one of Joseph Conrad's masterpieces, even though these novels' plots had nothing to do with Penang.

On reaching Penang I was stunned by the seriously dilapidated architectural beauty of the *clan kongsi* in the first place, i.e. temples of the powerful kin groups, related by blood but also by adoption or ritual, which had monopolized the region's opium trade during the 19th century. Equally stunning was the deterioration of the urban British colonial architecture around the port area, which, if not totally derelict, was by now nearly vacant. We need to bear in mind that this is a tropical zone where humidity

causes serious damage over a very short time. This decline was chiefly
ascribable to the federal government in Kuala Lumpur, wh ich for reasons
of political opportun ity had revoked Penang's free *port* status to grant it to
other more politically loyal localities. Jn fact, Penang, for reasons that we
will delve into later, was viewed as barely compliant and scarcely loyal to
the ru ling power after Malaysia was granted independence in 1957 (Wat-
son Andaya, Andaya 2001).

My interest in Penang as a location for a systematic *fieldwork,* and
n o longer solely as a tourist site, became more intense during two trips in
late 1995/early 1996 and in late 1997/early 1998, and one other trip in the
summer of 1998. After which, these trips occurred much more frequently
but my actual fieldwork began only in 2003, when UNESCO officially
recognized Penang's historic district as a candidate to the World Heritage
Site list. For political reasons however, this honor was granted only in 2008
together with the twin city of Malacca, wh ich, contrary to Penang, was
held in high regard by the federal government in Kuala Lumpur. This par-
tiality towards Malacca stemmed from two facts: the sizeable ethnically
Malay popu lation compared to Penang, and the commercial role played by
Malacca in precolonial times, thus before the Portuguese, the Dutch and
finally the British, when this independent sultanate was an important meet-
ing place for exchanges between Arab and Chinese merchants who traded
chi efly in spices and fabrics. In short, Malacca was viewed as a small
homeland, more autochthonou s and authentic than Penang that had been
founded by foreigners, i.e. by British colonialists.

Th is honor, therefore, was the poli tical outcome of a typical com-
promise between the different ethnic compon ents that constitute the coun-
try's social structure and characterize the national and local political
system. I will delve further into this aspect because this political backdrop
is crucial to understanding the workings of this culturally and socially very
complex country.

My decision to perform fieldwork in Malaysia and more specifi-
cally in Penang ought to be viewed in counterpoint to the situation in Eu-
rope throughout the 1990s, predominantly in the Eastern part of the Old
Continent. In fact, on arriving in Penang I immediately noticed the city's
ethnic, cultural , religious and linguistic diversity. Yet, I also noticed a co-
existence between groups, which, if not exactly harmonious, was charac-
terized at any rate by mutual tol erance. Penang's three numerically larger
groups, i.e. the Chinese who made up the majority, the Malays who were
a very substanti al minority and the Indians who were the smallest of the

three communities, did indeed live separately with occasional tensions, but apparently without conflicts.

I found this surprising and it sparked my interest as an anthropologist given that my field experiences, especially in Southeast Europe, pointed in a different direction. Bear in mind that the 1990s were blighted by interethnic war in former Yugoslavia, especially in Bosnia, while other countries chiefly in Eastern Europe were beset with tensions such as those in Bulgaria between Bulgarians and ethnic Turks especially, in Romania between Romanians and Hungarians in Transylvania, in Albania between Albanians and Greeks, in Greece between Greeks and Macedonians on the one side and Greeks and Turks on the other et cetera, et cetera. These tensions and disputes are still extant but perhaps to a lesser degree. Moreover, the age-old issue of the recognition of the so-called *gypsy* populations , discriminated against in any European State, was still unsettled. This rather gloomy scenario was the actual background of my anthropologist's curiosity regarding Penang and Malaysia that led me to tackle this field.

I set out by asking myself what the reason behind this very complex yet not impossible interethnic and multicultural coexistence could be. Thanks also to discussions with local experts, I realized that to this day Penang could be viewed as a *plural society,* in line with the concept coined by the perceptive British colonial functionary John S. Furnivall back in 1939, an author I was familiar with ever since my studies at Heidelberg (Furnivall 1944). Despite his Eurocentrism , Furnivall showed how in Indonesia in particular, though his observations also apply to Malaysia, the different ethnic groups lived separately without becoming a single political entity (Furnivall 1944: 446). Nonetheless, nowadays in Malaysia, and in Penang as well, the so-called *immigrants,* i.e. Chinese and Indians, display a markedly more hesitant patriotism compared to the Malays who are also known as *bumiputra, i.e. children of the soil.* Consequently, in the analyses of my empirical data I speak about a social cohesion based on the principle of *unity in separation* in a *rainbow nation,* paraphrasing Nelson Mandela's renowned definition (Giordano 2012: 35 ff.).

We ought to bear in mind, though, that between *children of the soil* and *immigrants* there is a significant social asymmetry. In fact, whereas the former can profit socio-economically from *affirmative action, immigrants* cannot. Since the Malays are by far among the country's least affluent, they can rely, contrary to the Chinese and Indians, on specific economic and social help from the State. We need to point up, however, that *affirmative action* has also generated some discontent, especially

among the less affluent Chinese and Indians who are barred from it due to their *immigrant* status despite having full citizenship rights, political ones included. Yet, *affirmative action* has overall lessened socio-economic differences, thus lessening the chances of conflicts. Finally, we need to mention that due to its economic success Malaysia is becoming an immigration country. This will create new problems, including political ones that will be worth studying thanks to a new *fieldwork* . But this is music for the future.

From 2006 on, having headed three *doctoral schools* (2006, 2008, 2010) together with two distinguished and insightful colleagues from other Swiss universities, Mondher Kilani and Ellen Hertz, and thanks also to the crucial support and proficiency of local experts such as Khoo Salma Nasution, Abdur-Razak Lubis and Gwynn Jenkins I was able to delve deeper into my fieldwork experiences . Thus, I was able to investigate specific phenomena of Penang's multiculturalism, of a political nature in particular, and of a public one in general, much more systematically as well as critically.

I focused primarily on two themes:
- the linguistic and religious differences within Penang's three largest and important cultural communities (Malay, Chinese and Indian).
- the management of multiculturalism , thus also the construction of collective identities by institutions and individual actors active in politics and more in general in the public sphere.

Concerning the first theme, through informal discussions with experts and my own observations I realized that Malaysia's famous *ethnic trinity* (Malay, Chinese and Indian) was more of a theatrical pretense for political purposes than an empirically verifiable reality.

In Penang especially, the Chinese, who constitute the majority of the city's population, are subdivided to this day into at least five groups: Hokkien (the largest group) Cantonese, Teochew, Hakka and Hainanese (Giordano 2017: 1-9). Language is the main difference since each group has its own idiom that the other groups can scarcely understand. Moreover, as a rule, though less so nowadays, Penang's Chinese do not speak Mandarin; therefore, any communication between members of the various groups can solely be in English or more precisely in *Manglish* (Malaysian English).

The Indians as well, though the Hindu Tamil are in the majority, are not culturally homogeneous from a linguistic, religious, and social

point of view. Moreover, the Hindu Tamil are structured i n   accordance
with a caste system   whose bound aries are   practically   unsurmountabl e.
Other relatively small Tamil communities but of Muslim faith live in Pe-
nang, such as the *Chulia* who by tradition are   rather   affluent jeweler s.
There are also Punjabi, Sikh, Kashmiri as well as Indians from other re-
gions, and finally Catholic Indians chiefly from Kerala.

The Malay, probably the most homogeneous community in terms
of religion, are however linguistically differentiated. In fact, the national
l anguage *Bahasa Malaysia* is not fully standardized, unlike *Bahasa Indo-
nesia,* and has several local variation s with specific intonations.

This greater cu ltural differentiation has helped me better under-
stand the political and institutional construction of multiculturalism and its
actual political and institutional management. In my opinion, the greater
complexity of differences has helped me avoid a misleading essentializa-
tion of cultmal diversity in the research. Thus, we were able to constantly
relativize discourses of a political nature. In fact, if we were to take the
political vision of the cultmal, lingui stic and religiou s diversity at face
value, then, drawing on Eric J. Hobsbawm and Terence Ranger, we could
rightly speak of a rather rigid and static invention of difference, if not in-
deed of a counterfeit of reality (Hobsbawm, Ranger 1983).

Therefore, by political or insti tutional management of cultural di-
versity I am referring to public strategies, including ritualistic ones, whose
purpose, in principle at least, is to soften the inevitable ten sions between
the various ethno-cultural components of Penang in particular , and of Ma-
laysia in general.

We ought to remember that in the now distant 1969 a bloody con-
flict largely involving Malays and Chinese led to massacres with most cas-
ualties among the latter. Nowadays, these incidents have fortunately
become the country's *negative myth,* sti ll and always evoked when ten-
sions between the various ethno-religious communities are on the point of
erupting. By *negative myth* I am referring to the well-known definition by
German politician Joschka Fischer, i.e. that specific events such as the
Holocaust must never occur again. Malaysia, instead, must never again
witness the 1969 interethnic violence and whoever violates thi s taboo risks
pu blic stigmatization, thus political ostracism .

Consistent with the logic of this *negative myth* as a deterrent to any
further similar incidents, a full array of political rituals hinting at the coun-
try's unity while underscoring the communities' differences has emerged
in Malaysia and in Penang in particular. The outcome may be defined as

the principle of *unity in separation* (Giordano 2012) characterized by a specific cultural intimacy (Herzfeld 1998), i.e. where individual members of each group have the right but not the duty to live in their own cultural environment. Accordingly, any reference to ethnic ghettoes would be misleading.

Agai nst thi s backdrop, the political elite, Penang's in particular, performed political ritual s and ceremonials in whi ch the staging of both unity and separation may be observed empirically. This political management's intent is to lessen tensions between communitie s by means of ri tu als acknowledging each collective identity while underscoring national unity. As an empirical example of these practices, I will mention the *heritage walk* of the *Chief Minister* of the State of Penang in July of 2013, i.e. on the fifth anniversary of Penang's inscription on UNESCO's *World Heritage List.* During this *heritage walk,* in fact, the *Chief Minister* paid crucially symbolic visits to three highly significant places of worship: the *Han Jan Ancestral Temple,* seat of the *Penang Teochew Association,* the *Kapitan Keling Mosque,* place of worship of Ind ian and Muslim *Chulia,* and finally the *Malay Mosque* (Giordano 2018: 489-492).

In fact, one could wonder why the *Chief Minister* neglected visiting a Hindu temple, given the numerical relevance of the Tam il community of that faith. The reason is quite simple. During the 2008 celebrations to promote Penang as a *World Heritage Site,* the *Chief Minister* visited Penang 's oldest Hindu temple, the *Sri Mariamman* (Giordano 2018: 490). Therefore, opting for the *Kapitan Keling Mosque* was once again a strategic choice.

In conclusion, my *fieldwork* should be viewed as an endeavor to realize an anthropology of the local political elites as skilled managers of *unity in separation.* Since I am not an idealist, I would also add that the politi cal role of guarantor of *unity in separation* is based on a shrewd political management of diversity, thus also geared towards an often cunnin g electoral strategy that can reasonably be expected to gamer support from all ethnic communities. In Penang in particul ar, this entails securing the crucial votes of the Chinese in the first place along with those of the Malays and Indians.

Conclusion: Consistency and Inconsistencies between Fields?

To the casual eye, my three fields may seem somewhat unrelated, thus might seem the product of random choices. I would draw attention, instead, to an overall thematic and theoretical coherence, albeit not a geographical one. In fact, all three fields deal with highly different subjects, but are linked by a political anthropology of the elites, a sub-discipline that the current anthropological *mainstream* still scarcely appreciates.

Nowadays, in fact, there is a tendency to study excluded and discriminated social segments, which in my opinion is an indication of misery-spotting and intellectual populism and in some extreme cases even of anthropological voyeurism with a penchant for poverty and marginalization. Actually, I have always been very critical of this do-goodism, which I perceive as misleading and above all ethnocentric despite its alleged commitment towards the socially and economically disadvantaged.

Ultimately, the three fields dedicated to the political anthropology of the elites (Sicily, Bulgaria and Malaysia) incorporate two main leitmotifs that characterize the scope of my empirical research.

The first leitmotif focuses on empirical research concerning informal social relations, coalitions and asymmetrical networks that emerge in specific situations in which the citizens' public mistrust in the proper and fair management of the common good by the bureaucratic and political apparatus is predominant. As the cases of Sicily and Bulgaria reveal, this indicates a deep gap between legality and legitimacy in which extensive and highly personalized networks based on a combination of kinship, instrumental friendship, clientelistic and frequently also Mafia-like relationships can thrive.

The second leitmotif, instead, focuses on fieldwork concerning cultural diversity, and more specifically ethnic diversity. In this case, fieldwork is about the multicultural coexistence of different communities with highly distinct cultures in terms of ethnicity, religion, language and race. As the case of Penang reveals, we can reasonably postulate that this coexistence is permanently accompanied by interethnic, interreligious and inter-linguistic tensions and conflicts, yet also by difficult but not impossible negotiations and compromises. My experience in Malaysia has convinced me, also in view of other fieldwork, that harmony between culturally different communities, thus with different if not indeed incompatible social representations and practices, is largely a chimera stemming from do-

gooder delusions, which unfortunately are far too prevalent in present-day anthropological discourses.

My *fieldwork,* therefore, has also taught me to question these policies, which may be defined as consociational (Walzer 1997:22-24; Lijphart 1977) and which for the time being have worked well enough to ensure internal cohesion and a significant economic success, because possible conflicts between ethnic communities are always just around the corner and can flare up at any time with devastating consequences.

Yet, the question of trust and especially the problem of mistrust (Muhlfried 2018: 225) between groups that are not only ethnically different, but also hierarchically ordered is pivotal also in these researches that study interethnic relationships.
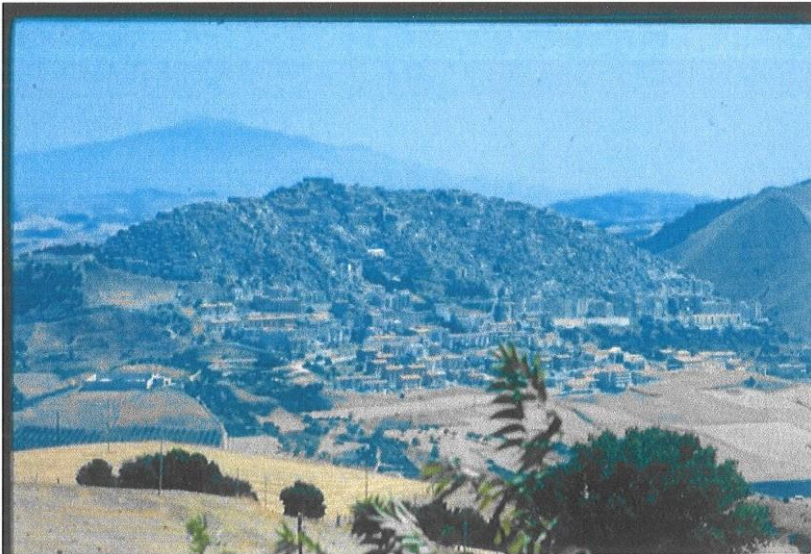
© Christian Giordano. Sicily, agrotown, first fieldwork.

© Christian Giordano. Malaysia, Penang, multilingualism.
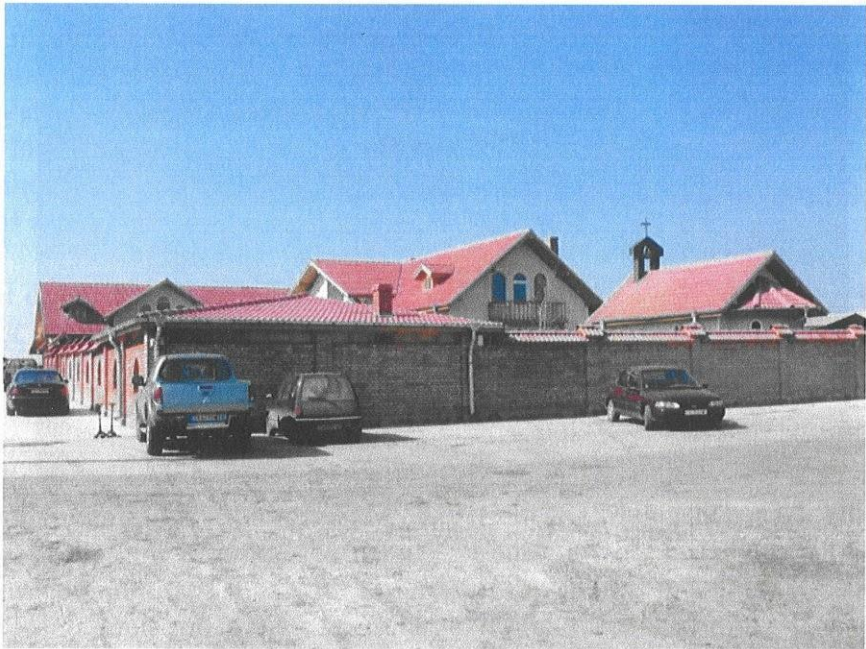
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New house of a powerful *arendator*.

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Pas de chez soi pour l'anthropologue /
There Is No Home for the Anthropologist

Franr;ois Riiegg

Paris 1970, terrain par defaut

En 1970 a Paris, l'ethnologie classique, exotique, battait de l'aile. A moins d'etre le *poulain* d'un ma1tre – de qui il fallait assurement porter la serviette- ou de ce qu'on appelait comme etudiant avec un rien de mepris, un *mandarin,* Jes chances d'obtenir un credit de mission pour faire du terrain sans etre un chercheur avance, affilie a une ecole africaniste, oceaniste ou americaniste, etaient maigres. C'est aussi que la revolution de mai 1968 avait entraine une reforme des universites, une multiplication de leurs sites et une banalisation des noms, ou plutot !attribution de matricul es. Certes la Sorbonne existait encore *de facto* mais s'appelait dorenavant Paris III ou Sorbonne nouvelle ou encore Pantheon Sorbonne, pour Paris I.

      Pour des raisons administratives qui m'echappent encore aujourd'hui, j'atterris a Paris VII, autrement <lit Jussieu, lieu de sa premiere implantation ju squ'en 2007, universite toute nouvelle dont les tours n'etaient pas encore achevees, devenue depuis Paris-Diderot. Il fut question un temps aussi pour moi de Paris V Censier, partie de la Sorbonne nouvelle, situee a une station de metro de Jussieu. En fait l'enseignement de l'ethnologie etait associe au professeur qui dirigeait l'UER, soit l'unite d'enseignement et de recherche, exemple du nouvel arsenal de sigles dont les Parisiens raffolent encore. Ainsi devais-j e finalement atterrir chez Robert Jaulin (1928- 1996), l'enfant terrible, africaniste et americaniste, plutot que chez Jean Guiart (1925-), le classique ethnologue oceaniste. Les luttes entre les ecoles battaient leur plein. Chez Guiart on apprenait la parente, chez Jaulin 'l'ethnocide' et la defense des popul ations indigenes. Au-dessus de la melee tronait Claude Levi-Strauss, dans la tour d'ivoire du College de France. Me voici done livre sans le vouloir a l'ethnologie dissidente, sans meme savoir ce qu'etait l'ethnologie bienpensante.

      Il est difficile pour qui a termine ses etudes d'ethnologie avant 1968 ou vingt ans plus tard d'imaginer le doux chaos qui regnait dans Jes

universites parisiennes de ces annees. Les greves, les graffitis, les vols de
livres et les nuages de fumee de Gauloises bleues durant les cours n'en
sont que des facettes, facettes auxquelles le malheureux neophyte que
j'etais avait peine a s'habituer. Je venais de passer deux ans a la Faculte
des Lettres de l'universite de Geneve en Histoire de l'Art et n'y avait rien
vu de la 'chienlit' deploree par De Gaulle! Dans le campus de la cite uni-
versitaire du 14e arrondissement ou je logeais – plus precisement dans le
pavillon suisse construit en 1933 par Le Corbusier, les factions cambod-
giennes s'entretuaient, tandis que les Nordiques s'exposaient a leurs fe-
rn tres au moindre rayon de soleil et que fleurissaient partout les comites
de gestion.

Geneve, l'histoire de l'art et les Alpes

Revenons quelque peu en arriere. Etudiant d'histoire de l'art a Geneve de-
puis 1968, je m'impatiente face au conformisme des champs d'etude: la
France, l'Italie et presque rien d'autre, sauf pour l'art contemporain qui ne
m'inspire guere. J'avais toutefois effectue avec enthousiasme un premier
'terrain' en Catalogne, a la recherche de l'art roman, sujet classique, mais
terrain exotique. Je dois reconnal'tre pourtant que grace au Professeur
Georges Besset, les ecoles d'art allemandes m'avaient ouvert un peu]'ho-
rizon au-dela de ce champ clos, notamment brs d'un passionnant voyage
d'etudes dans 1'Allemagne du sud. Mais l'histoire de l'art demeurait terri-
blement academique pour un amateur de plein air.

Interesse deja par l'architecture vernaculaire des Alpes dans les-
quelles je pratiquais assidument 1'alpinisme, je collaborai brievement pour
arrondir les fins de mois a l'elaboration de 1'atlas ethnographique helve-
tique, dirige par un ethnographe balois. Encourage par 111011 ancien profes-
seur de philosophie, je me decidai a etudier l'ethnologie, chose impossible
a Geneve. Pour l'etudier en Suisse romande, francophone, a Neuchatel
done, il fallait avoir etudie deja pendant deux ans la geographie! Paris sem-
blait done la meilleure voie. De l'academisme genevois a l'improvisation
parisienne de l'apres 68, le choc fut celui que l'on attend en general de son
premier terrain exotique. Je m'en remis en traduisant de l'allemand un livre
d'histoire de l'art 'holiste' traitant de trois clol'tres catalans d'un point de
vue musicologique, le *Singende Steine* de Marius Schneider. Les exercices
d'observation participante locale dans un cafe, l'ethnologie du monde mo-
derne, n'avaient rien de tres enthousiasmant. Mais les anthropologues de
terrain revenant qui du grand Nord -Jean Malaurie, qui de 1'Amazonie –

Robert Jaulin, faisaient rever, de meme que d'autres figures emblema-
tiques telle celle de Georges Charachidze, eleve de Dumezil, professeur
d'anthropologie religieuse ou encore (le Prince) Dika-Akwa nya Bonam-
bela, pour les religions africaines, a savoir le *Nyambeisme* qui ressemblait
etrangement au thomisme, ou enfin Yvette Grimaud pour l'ethnomusico-
logie et particulierement la polyphonie de Georgie. Un ethnologue doit re-
ver!

Place cependant devant la necessite de choisir un terrain pour mon
travail de maltrise, nous sommes en 1972 et dans la situation que j'ai de-
crite plus haut en ce qui concerne Jes missions sur des terrains exotiques,
je proposai d'entreprendre une etude, inspiree de !interpretat ion structu-
raliste, de l'espace d'une vallee laterale du Rhone en Valais, le Val d'An-
niviers, connu pour son ancien nomadisme saisonnier. Je pouvais
facilement y sejourner et disposer de materiaux d'archives et de travaux
anterieurs dedies a cette vallee, deja potasses par le sociologue Bernard
Crettaz qui les mit a ma disposition. A propos de ce premier terrain, on
peut dire qu'il s'agissait d'anthropologie *at home,* mais egalement d'an-
thropologie de deuxieme main, au vu des etudes existantes. L'exemple le
plus frappant en etait que mes 'informateurs' citaient des sources ecrites.
Toutefois Jes visites de caves des maisons *bourgeoisiales,* c'est-a-dire des
maisons appartenant en communaute aux autochtones et leur servant de
-lieu de rassemblement, de deliberations et de festivites, donnaient a l'exer-
cice du piment. Je participai ainsi a 1'une de ces assemblees annuelles a
Grimentz, lors de laquelle on evoque Jes morts dans la cave, mange du
fromage de l'alpage du village et boit le vin de la bourgeoisi e, cultive dans
la plaine du Rhone et savamment conserve dans un tonneau en altitude que
l'on remplit a chaque vendange, nomme vin des glaciers. L'histoire etait
de la partie pour expliquer la presence de monuments d'architecture mili-
taire et religieuse, comme aussi les legendes et la 'revolution blanche' des
annees soixante du 2oe siecle. Elle amena la prosperite avec le tourisme
d 'hiver. Je ne me fis pas d'illusion, la societe actuelle ne gardait que peu
de traces de son espace autrefois clos, sauf a en interpreter les vestiges.

De Paris a Bucarest : un choix aleatoire, un terrain mobile

Que faire ensuite, car ma soif de terrain exotique n'avait nullement ete
etanchee? Mon interet pour !habitat restait entier. Mon professeur d'an-
thropologie religieuse, Georges Charachidze, me mit en contact avec Anca
Stahl, femme de Paul-Henry Stahl, fils du celebre sociologue roumain

Henri Stahl, qui dirigeait des etudes à I'Ecole des Hautes Etudes de Paris.
Je m'inscrivis ainsi à son seminaire sur !'Europe du Sud-est et sollicitai
une bourse d'echange de l'universite de Geneve avec Bucarest. Muni des
conseils de ce chercheur roumain, lui-meme specialiste de !'architecture
dite populaire, je passai un an en Roumanie, une Roumanie aussi exotique
que possible, car a la fois demeuree rurale et fennee par le rideau de fer.
Quoiqu'en Europe, ce sejour m'obligeait a passer le rite d'initiation bien
connu des ethnologues: se plonger dans l'ailleurs, apprendre la langue, ob-
server du plus pres qu'il est possible.

　　　　Du fait que man objet, I'habitat, etait physique en grande partie,
mon etude ne suivit pas le trace ni la methode habituels. II ne s'agissait pas
de constituer la monographie d'un village en s'y installant pour plusieurs
mois, en en decrivant minutieusement les habitants, leurs fermes, leurs pra-
tiques agricoles, leurs relations sociales en y suivant les rites lies aux sai-
sons, mais bien d'avoir une idee plus generate de l'habitat du pays et de
ses differentes regions.

　　　　Heureusement pourvu d'une (petite) voiture, je pouvais circuler
dans toutes les provinces et en reperer les styles d'habitat, classes par les
ethnographes, comme d'habitude, dans le contexte administratif et poli-
tique existant, a savoir les provinces historiques d'un cote, mais aussi le
socialisme national de l'autre, le paysan, faute de proletaire, etant encore
et toujours l'icone de la culture nationale. Le defi a relever etait ici, comme
ailleurs en Europe, de sortir de l'approche purement descriptive, technique
et folklorique. II suffit d'ailleurs de consulter la table des matieres de n'im-
porte quel ouvrage d'architecture rurale pour comprendre exactement ce
que je veux dire.

At Home?

Les deux caracteristiques principales de ce terrain furent la mobilite, pas
d'ancrage de longue duree dans un lieu, et la spontaneite. Je veux dire par
la que je n'etais nullement prepare à affronter ce terrain. Je n'en avais au-
cune connaissance livresque et historique, je ne parlais pas la langue, en
un mot je le decouvrais en meme temps que la societe socialiste qui en
constituait le contexte, projete de mes etudes exotiques sur les banes de
Paris VII dans la Roumanie rurale et communiste de Ceauescu.

　　　　Au moins avais-je l'avantage de n'etre pas embarrasse d'idees pre-
conues. On peut à peine parler d'un *choix* de terrain et certainement pas
d'un quelconque lien avec celui-ci, ni familial, ni affectif, ni linguistique.

II n 'etait pas plus question d'etre *native,* que de le devenir. Une immersion totale dans un monde etranger et ferme entraine forcement une demarche lente, des decouvertes qui n 'en sont pas, des enthousiasmes et des deceptions. Je travaillais en revanche en partie avec des ethnographes de la capitale, ce qui me permit a la fois de progresser plus rapidement dans la connaissance de mon objet d'etude et d'etudier, au deuxieme degre, des pratiques institutionnelles qui n 'etaient pas sans evoquer la lourdeur des expeditions ethnologiques coloniales, des villages etant quasi requisitionnes pour les besoins de l'enquete. Ainsi travaillaient d'ailleurs Jes pontifes de l 'ethnologie fran9aise visitant leurs collegues roumains et recouran t aux services de petites mains locales. Pour moi je n'etais qu'un etudiant et j 'avais le choix entre participer a des enquetes formelles du genre de celles que l'on entreprend pour constituer les Atlas ethnographiques nationaux en Europe, et faire mes propres recherches, sans guide ni filet. Je pratiquai les deux approches.

En parallele, ce fut la decouverte du socialisme reel, dans la cite universitaire de Grozaveti entre autres, mais egalement dans les villes de province. C'est dire Jes dortoirs bondes des etudiants roumains alors que nous avions une chambre individuelle, les queues pour acheter de la nourriture, le travail benevole *(munca patr iotica)* des etudiants a l'automne, les banderoles de Ia propagande sur les edifices publics, les magasins du Parti, la censure etc. Je n 'avais avec le regime aucune affinite ideologique, mais j 'etais prevenu au contraire par un dissident, mon professeur a Paris, des difficultes que j 'aurais surement a affronter, oriente et introduit aupres de telle institution plutot que telle autre, l'Institut d'histoire de l 'art en l'occurrence ou je commen9ai ma recherche aupres de collegues folkloristes tres aimables, accueillants et comprehensifs.

Ma plus grande surprise fut probablement de voir que la campagne, du moins dans les zones montagneuses, avait ete si peu ou pas touchee du tout par la collectivisation -du moins a ce qu'il paraissait, et que le paysan continuait a vivre en autarcie, 'pauvre' comme avait ete le paysan de montagne dans les Alpes avant la deuxieme guerre mondiale. C'est justement je crois le besoin et le fait de comparer des situations analogues qui sauvent du nativisme ou du nationaiisme (ethnocentrique), qui empechent de croire a '}'exception roumaine' en l'occurrence, et, en ce qui concerne l'architecture rurale, de tomber dans le piege de ']'architecture populaire roumaine ', ainsi que s'intitulent articles et ouvrages consacres. C'est le cas en realite dans toutes Jes nations europeennes, ne serait-ce que parce que les institutions qui s'en occupent sont etatiques. Je m'en suis explique dans un petit

ouvrage (Ruegg 2011). D'abord ii fallait se rendre compte de la relative
jeun esse de la nation roumain e dans ses frontieres actuelles et ensuite du
fait que les types d'habitat observes, bien que n'etant pas determines par
la seule geographie non plus, se repandaient bien au-dela des frontieres
politiques, comme j 'en avais !'intuition et plus tard la certitude. Je ne fis
done pratiquement point usage de mes carnets d'esquisses de plan de mai-
sons rurales, patiemment releves lors de mes enquetes, a la recherche des
maisons plus anciennes.

De Belgrade à Vienne, en passant par Zagreb et Cracovie

C'est pourquoi mon deuxieme terrain fut celui de l'ancienne Yougoslavie,
pays relativement neuf Ju i aussi et dont]'habitat rural etait deja fortement
influence, comme je pus m'en apercevoir en circulant dans Jes vallees ap-
paremment Jes plus recu lees du pays, voire carrement transforme par les
constructions des emigres revenus recemment de sejours de travail en Al-
lemagne. La encore le terrain fut mobile, comportant des sejours en villes
de Belgrade et de Zagreb aupres des institutions specialisees dans ce do-
maine, ainsi que la traversee du pays de bout en bout dans ses diverses
parties que traversait I 'ancienne frontiere entre les empires ottoman et au-
trichien. J'avais rencontre a l 'EHESS Sacha Popovic, le grand expert de
l'lslam balkanique dont je suivis le seminaire. II m 'introduisit a Belgrade
et devint plus tard un ami, mais il ne pouvait diriger mes travaux ! La visite
de la zone frontiere de la Roumanie, la Backa et la VoYvodine, m 'ouvrit
Jes yeux a la fois sur la composition multiethnique des population s qui y
habitaient et sur la similarite de leur habitat dont je comprendrai plus tard
qu'elle etait due en partie a la colonisation autrichienne. L'heritage otto-
man de la Bosnie-Herzegovine ajoutait par ailleurs une autre touche cultu-
relle historique au te1rnin. En franchissant la frontiere des anciens Empires
ottoman et autrichien, on changeait de reference concernant le rapport a
l'espace et son occupation. D'un cote, un espace marque par le feodalisme
militaire ottoman, fluide, avec des maisons a cour interieure, d'un autre,
un espace marque par l 'illuminisme autrichien, avec ses obsessions de
clarte, d'economie et de securite. Mais en meme temps, la geomorphologie
jouait un role visible sur I 'habitat, de l'Adriatique aux Alpes juliennes,
transfrontalier. Aussi, dans les zones plus elevees, I 'architecture en bois
ressemblait fort a celle des Carpates et Sur la cote elle etait adriatique, en
p1erre.

Elargissant encore le cercle comparatif, je me rendis en Pologne du Sud pour trois mois toujours avec le secours d'une bourse d'echange, cette fois entre Geneve et Cracovie. Adoptant la meme methode de survol, sans omettre la visite des nombreux musees en plein air, j'obti ns, en plus d'une belle archive photographique, une vision generale que confirma la lecture helas trop tardive d'un de ces savants ouvrages allemands. Bruno Schier (1966) avait en effet dresse un tableau systematique de tous les aspects de I 'habitat rural de I'Europe centrale orientale, de la forme du toit a celle de la cour de la ferme, en passant par les systemes de chauffage. Ma seule consolation fut de pouvoir confirmer *a posteriori* mes intuitions.

Je devenais ainsi sans le savoir un anthropologue europeaniste, mais totalement isole, car confine dans un sujet habituellement traite par des architectes et des folkloristes locaux. Je pressentais certes la necessite d'aborder la question d'un autre angle que du seul angle ethnographique (national). La difficulte qui se presenta alors fut de trouver un 'patron' a]'esprit assez large pour englober plusieurs nation s et sans attaches parti- culieres, nationale ou ideologique. Gardant mes relations avec l'EHESS et a l'INALCO j e postulai pour une bourse de releve aupres du Fonds natio- nal sui sse de la recherche scientifique. Cette bourse devait me permettre de remonter aux sources, du moins aux sources occidentales, celles de I'Empire autrichien. Le prochain terrain devait etre le cabinet, celui des 'archives principalement, autre poste solitaire. De ce point de vue, il est vrai que mon itineraire de recherche se fit a l'envers, du terrain aux sources ecrites.

De l'ethnologie a l'histoire et a la litterature de voyage: une anthropologie interpretative *emique* du discours

Tres loin physiquement et intellectuellement des institutions academiques parisiennes , agitees par la decolonisation et le besoin de redefinir le terrain de l'anthropologue comme terrain du proche, du quotidien et de la ville (cf. Anthropologie, Etat de lieux, 1986), j e me plongeai dans l'histoire de la colonisation autrichienne des frontieres orientales de !'empire recon- quises sur Jes Ottomans a la fin du 18e siecle par le Prince Eugene. Je de- couvris simultanement la litterature de voyage, essentiellement d'expression allemande, qui decrivait Jes pays que j 'avais parcourus deux siecles plus tard. Le discours primitiviste de ces recits ne pouvait que me frapper, allant parfoisju squ'a comparer textuellement les paysans du Da-

nube aux sauvages americains. De meme pour l'habitat, les temoins ocu-
laires ne parlaient que de miserables huttes, de cabanes primitiv es enfu-
mees dont les attributs correspondaient a ceux de leurs habitants, ignorant
hygiene et civilisation .

A cent lieues des etudes ruralistes, je me plongeai avec enthou-
siasme dans ce que !'on appelle desonnais l'imaginaire et Jes representa-
tions sociales. Pour un eleve des relativistes culturels, critiques de toute
intervention dite de developpement dans le Tiers-Monde, ii devenait evi-
dent que les campagnes de civilisation entrepri ses sous le regne de Marie-
Therese et de Joseph II dans la seconde moitie du l8c siecle ressemblaient
etrangement a celles que Jes Nations Unies promouvaient au 20e siecle, de
!alphabetisation a la promotion des droits de l'homme, en passant par
!hygiene et la sante corn;ues selon les canons de la medecine occidentale.
Le meme *bonisme* inspirait les entreprises coloniales et post-coloniales, en
Europe centrale comme dans les colonies lointaines. De la naquit mon
idee, mon leitmotiv, que rien n 'a change fondamentalement dans le dis-
cours et Jes pratiques, qu'il s'agisse des sciences politiques ou du develop-
pement, mais davantage encore dans le processus de 1'elargissement de
I 'Union Europeenne , ce sur quoi je me pencherai plus tard, fort de ma con-
naissance du terrain. *L'Aujldaerung* n'avait pas cesse d'imposer ses ca-
nons depui s Kant !

Du point de vue de la m6thode,j e decidai d'emprunter aux temoins
eux-memes, les voyageurs, leur classification toute teintee d'ethnocen-
trisme et de jugements de valeur car elle avait l'avantage de restituer une
representation sociale bien plus large que Jes categories architecturales
elles-memes, techniques ou ethniques. Se dessinaient a travers ces repre-
sentations trois mondes. Le monde *eclaire* du voyageur-temoin, le monde
primitif vu de I 'exterieur et enfin celui des deux *cultures* en opposition,
l 'allemande* et la *turque* pour reprendre les expressions des temoins occi-
dentaux. Cette interpretation avait l'avantage de respecter mes sources et
d'en degager deux visions du monde opposees, des types radicalement dif-
ferents de conception et d'occupation de l'espace. De ce point de vue on
peut dire que j e sollicitais, analogiquement du moins, le savoir local
(Geertz) ou plutot contemporain des voyageurs, celui qui a produit les ca-
tegories du primitif et du retarde.

Sans avoir besoin d'en faire un catalogue de pri ncipes et sans que
ce soit une verite bien nouvelle, il s'avere que tout ethnologue rencontre
t6t ou tard la necessite de se pencher sur I 'histoire de son objet et bien plus
encore, sur le contexte ideologique qui y a preside. De meme ii ne peut

ignorer non plu s Jes travaux non seulement de ses predecesseur s, mais aussi de ses voisi ns de d iscipline, hi storiens, psychologues sociaux, sociologues et lettreux.

De l'habitat a l'environnement en passant par le multiculturalisme

Apres une longue parenthese consacree a des activites professionne lles dans Jes milieux internationaux non gouvernementaux, pour lesquelles la vision critique que donne l'anthropologie est plus qu'utile,je revins a l'anthropologie. Les annees 1990, soit l'apres-socialisme, ouvraient des portes a la recherche, notamment dans la nouvelle configuration des pays de l'Europe centrale-orientale, avides de rejoindre I 'Europe. La question <lite des minorites, du multiculturalisme et du neo - nationali sme occupait de nombreux anthropologues. La Roumanie fournissait la aussi un intere ssant terrain d'etud es en raison des diverses minorites qui s'affirmaient dans le nouveau contexte politique de l'acces a)'Union europeenne. Tandis que les Allemand s avaient rejoint en bonne partie l'Allemagne qui les avaient rachetes deja du temps du communi sme, la minorite magyare occupa le devant de la scene. Le maire roumain de Cluj-Kolosvar en fut l'un des acteurs les plus visi bles, ayant fait peindre les banes publics aux couleurs de la Roumanie. La vi lle etait marqu ee par des symboles nationaux bien distincts monum ents dedies aux heros, egl ises et noms des rues, tandis que les anthropo logue s du monde post-sovietique s'occupaient de ce qu 'etaient devenues les statu es sovietiques renversees, demontees ou deplacees, de Lenine a l 'ouvriere rnodele en passant par le soldat sovietique. Je ne decouvris que bien plus tard qu'au Kirghizstan, le camarade Leoine passant pour le heros civilisateur de ce peuple nomad e sedentarise par les Russes, iietait normal de J ui reserver un etage du musee national a Bichkek. Apres tout on celebre aussi en France les heros de la Revolution fran9aise et la guillotine au Musee Carnavalet.

J'obtins alors le financement d'un projet de recherche. Une bonne partie de cette recherche *interculturelle* que finan9a le Fonds National suisse de la Recherche Scientifique fut menee par Jes collegues roumains, davantage orientes vers la sociologie que l 'anthropolo gie sociale inexistante en Roumanie comme dans la plupart des pays satellites de l'URSS, pl us enquete aupres d 'informateurs qu 'observation participante (Rilegg & al ii 2006). C'etait le prix a payer pour pouvoir reprendre des activites de recherche sur le terrain. Cependant mes recherches interculturell es subsequentes dans la Dobroudja rournain e et bulgare, particulierement dans le

domaine de l'anthropologie religieuse, devaient s'averer plus fructueuses et satisfaisantes.

La 'question rom' revint egalement sur le tapis eu egard a l'ouverture des frontieres europeennes en 2007, colncidant avec l'elargissement de Union europeenne. Ce theme fort controverse et mine par les ideologies et l'actualite devait me foumir !occasion de mener quelques recherches directes sur le terrain, soit dans le domaine de transformations survenues dans cette population, notamment en Republique de Moldova, soit dans celui de leur nouvel habitat, les palais tsiganes qui defrayaient la chronique. J'eus par la meme occasion le loisir de constater la naivete avec laquelle en Europe de l'Ouest, on considerait les Rom, sans aucun recul historique. Par ailleurs, la conversion massive des Rom aux neo-protestantismes me permettait de renouer avec mes etudes d'anthropologie religieuse. C'est ainsi que fut lance le projet de recherche 'Nomades et parlementaires ' (2005-2008) dont le titre illustre bien le but: montrer que la population rom est multiple et qu'on ne peut pas la confondre avec 'pauvrete et retardement' comme le font trop souvent les associations non-gouvememementales diverses et les organisations internationales intergouvernementale s. Les anthropologues qui sont tombes dans le piege miserabiliste ou exotique romantique sont la majorite. Le titre 'Ma vie avec les Tsiganes discrimines' resumerait !orientation des nombreuses publications faisant echo aux media et aux reportages journalistiqu es du type 'Enterrez-moi debout' de Madame Fonseca dont le public occidental raffole. Cela sert a mieux mepriser les Roumains qui traitent si mal leurs minorites. Voila le theme repris par tant de 'valets du capitalisme' comme aurait dit la propagande communiste, a savoir la defense de la veuve et de l'orphelin sur un registre faisant appel essentiellement aux sentiments et a l'ideologie qui veut diviser le monde entre bons et mechants.

Un des aspects nouveau et important de la recherche etait la conversion massive des Rom/Tsiganes aux neo-protestantismes. Si le baptisme s'etait deja installe du temps du communisme, le pentecotisme, plus recent, dans ses formes charismatiques, remettait en cause la religiosite decriee des Rom/Tsiganes et leur statut social. Cependant les chercheurs locaux se concentraient davantage sur cette religiosite, bien connue en Europe de I 'Quest, qui etait pour eux nouvelle, que sur Jes transformations sociales qui en decoulaient. Le discours des nouveaux convertis n'est guere different la-bas ou ici. La conversion sert de justification au changement de comportement de l'individu et de sa famille, !integration des marginaux dans la societe majoritaire, la valorisation de l'individu face au

groupe et le succes economique de ces nouveaux *businessmen.* Ce que deux cents ans d'efforts pour !integration par la *raison* n'ont pas reussi, le charisme nee-protestant l'a realise en une vingtaine d'annees, pour ceux qui ont suivi cette voie et ils sont nombreux. Paradoxe difficile a integrer pour les sciences sociales, pour ne rien dire des organisations intergouvernementales et non gouvernementales qui rivalisent de plans de developpement et de programmes d'aide sans aboutir davantage ici que dans leurs territoires traditionnels exotiques.

Quant a mes recherches a propos des *palais tsiganes,* elles me ramenaient a mes travaux sur l'habitat a travers les representations sociales. La methode elle aussi demeurait la meme, a savoir une tentative d'interpretation des transformations sociales et de !habitat tsiganes, en critiquant le discours habituel ethnicisant, denigrant ou empathique, mais toujours miserabiliste. Le propos des interesses, du moins en Republique de Moldavie (Rilegg 2008) varie peu et explique les changements survenus dans le statut economique et social, et par consequent I'habitat, par la conversion religieuse, ainsi que nous venons de le dire.

Il fallait sortir de cette perspective (emique) tout aussi monolithique que le discours miserabiliste (etique) et placer le phenomene dans une perspective non ethnique. C'est pourquoi nous avons choisi celle des nouveaux riches, favorisant le statut socio-economique plut6t que l'ethnie. Au lieu d'observation participante au sens traditionnel du terme, ii s'agissait a nouveau d'entreprendre ici un survol plus large, a savoir etudier un contexte geographique et sociologique relativement nouveau et neglige, celui des nouveaux riches des pays ex-communistes. S'il existe plusieurs ouvrages et articles concernant ces palais, tant6t pour les critiquer a cause de leur mauvais gout, tant6t pour s'insurger contre leur irruption dans la ville et leur visibilite choquante, la perspective adoptee demeure ethnique, sans que Jes auteurs se soient pose la question de la comparaison. Les Tsiganes ne peuvent construire que des palais tsiganes! Ce faisant on perd toute reference au contexte socio-psychologique, celui de la *competitivite economique* et celui de la *revanche sociale* notamment (Rilegg 2015).

L'habitat conduit naturellement a etudier l'environnement et le rapport a l'espace. Plus specifiquement d'ordre symbolique, ce rapport est indubitablement lie aux visions du monde des groupes sociaux concernes. Habituellement, celles-ci se formulent a travers les appartenances religieuses. Dans le sud-est de I'Europe, ce sont Jes traditions chretiennes orthodoxes et musulmanes sunnites et alevies qui se melent. Mais comme

pour l 'habitat, on observe des traditions communes transethniques et trans-confessionnelJes. La *dendroldtrie* ou culte (aupres) des arbres en est un exemple, comme Ies pelerinages sur les tombeaux des Saints pratiques en islam alevi (Rilegg 2013). De fait les cultes qui prennent l'arbre ou la fon t comme support sont repandus bien au-dela et fondes, en ce qui concerne le christianisme, sur Jes textes aussi bien de l'Ancien Testament que du Nouveau, ainsi qu'il ressort des analogies faites entre les arbres du Jardin d'Eden et de celui dont a ete tiree la croix du Christ.

Quant aux pratiques de guerison effectuees dans le contexte des pelerinages, elles ressemblent fort à des pratiques trad itionnelles que I'on pourrait assimiler au chamanisme, si ce terme n'etait pas aussi galvaude, aussi bien en Bulgarie qu'en Roumanie et plus generalement dans les Bal-kans.

Aussi, le terrain du syncreti sme religieux, tel qu 'on peut encore !observer dans le sud-est des Balkans, dans la Dobroudja notamment, s'inscrit pour moi dans le meme souci de sortir des identites construites, ethniques, politiques ou religieuses, telles I'islam et I 'orthodoxie chre-tienne pour proceder systematiqu ement a la comparaison. *Observer / 'Islam* (Geertz) n 'est plus guere possible ni dans son titre ni dans son ob-j et, tant notre perception en est plus fine aujourd 'hui et tant il est a la fois politise et caricature, acculture et mondiali se. En revanche observer Jes pratiques de guerison independamment de leur pretendue origine reli-gieuse nous force à nouveau a comparer d'une part, et a identifier des pra-tiques fortement semblables, quelle que soit l'ethnicite de ceux qui guerissent ou qui sont gueris. C'est ce que nous avons pu observer ces dernieres annees dans la Bulgarie du Nord et la Dobroudja roumaine.

Conclusion

Si ce que l 'on decouvre par soi-meme a l'epreuve du terrain est plus labo-rieux, ii reste que la satisfaction intellectuelle n'en est que plus grande et que cette epreuve du terrain forge en quelque sorte une conviction. La me-thode comparative et la methode interpretat ive ont ainsi ete mes princi-pales ressources pour rendre la recherche interessante, c'est-a-dire significative, stimu[ante, au-dela de toute pretention a l'objectivite ou a l'exclusivite. Il n'est pas de mon ressort de recommander telle ou telJe me-thode ou approche, mais l e fait est que l e terrain et !observation critique a partir de ce terrain me semblent devoir occuper une place au moins aussi

grande que les entretiens dont on sait les possi bles derives a tous points de vue.

What is home?

Revenons un instant sur la discussion au sujet de 1'indigeni sme du cher-cheur. Quoique la tendance, voire la mode, soit de defendre la perspective familiere d'une ethnologie du proche et par le proche, je continue de penser qu'une distance est salutaire et que l'exotisme bien compris est a la fois un stimulant et un garde-fou. Nativisme et exotisme (vulgaire) sont en effet les victimes de la politisation de la recherche, la question ne se posant d'ailleurs reellement qu'avec la decolonisation . Ainsi dans la perspective demagogique et politiquement correcte dominante depuis les annees 1980, notamment dans Jes *cultural studies* , il faut que le sujet observe s'exprime, qu'il soit present et represente, sinon majoritaire, s.elon le vieux slogan qui veut 'donner la voix aux sans voix'. La volonte de compensation pos-thume, la culpabilite coloniale, d'un paternalisme penible parce qu'ignore, correspondent bien au miserabilisme ambiant de l'anthropologie contem-poraine. Non seulement il faut donner la parole aux sujets de l'enquete, mais encore choisir exclusivement ceux-ci en fonction de Jeur marginalite , de leur pauvrete, de leur exclusion , ce qui ne manquera pas de souligner le caractere oppressif de leur situation, et de dresser un tableau le plus noir possible des autres, des dominants, des mechants en un mot que sont les Occidentaux, riches et exploiteurs.

De meme l 'exotisme est-ii voue desormais aux gemonies (Bensa 2006) parce que lie indissolublement au colonialisme, mais egalement gal-vaude par le tourisme de masse, la litterature a bon marche et ronge par la mondialisation. On rappellera a ce sujet la vibrante defense de l'exotisme bien compris par Victor Segalen (1908), tout en sachant que chaque gene-ration depuis longtemps, se plaint de la disparition des differences et du 'monde d'hier' (Stefan Zweig). Or il me semble que sans un minimum d'exotisme, c'est-a-dire essentiellement *d'etonnement,* ii ne peut y avoir de recherche fructueuse. Sans tomber dans le sentimentalisme deja de-nonce il y a longtemps par Geertz (1988) a propos d'une ethnologie empa-thique, ii faut neanmoins que transpire un minimum d'enthousiasme, ne serait-ce que pour convaincre le lecteur et cela meme si cet enthousiasme est une strategie rhetorique. Or la langue de bois de certains tenors des *cultural studies* me rappelle fiicheusement celle de la sociologie fran9aise obscure des annees 1970.

Si l'on comprend bien la critique que peut faire tout 'indigene' gene de se voir objet d'etude exotique à l'explorateur etranger *(nous ne sommes pas des sauvages!),* la revendication du monopole de la parole par l'indigene a trop souvent des accents revanchards. II y a davantage: l'anthropologie *at home* presente a mon avis deux dangers: celui de la paresse d'un cote et celui de l'aveuglement d'autre part. Paresse, parce qu'on se connait trop bien et que les choses vont de soi. Aveuglement parce qu'on ne se preoccupe que de soi (Rilegg 2014). On perd ainsi le contexte et retombe dans une autre forme d'ethnocentrism e ou de narcissism e. A quoi bon alors l a longue critique de celui-ci, si c'est pour retomber dans le meme piege, celui qui tente J es etudiants d'entrer en psychologie parce qu'ils pourront s'etudier ainsi eux-memes comme en entrant en anthropologie ils etudieront leur famille ou leur voisinage. Et qu'en est-ii de la comparaison, dans la perspective indigeniste?

There is no home

Plus importante que la question de l 'indigenisme, la question de la comparaison et de l'ampleur de la perspective adoptee me semble etre centrale. La frilosite des recherches actuelles, concentrees sur des objets minimalistes et politiquement corrects est une des consequences de la sempiternelle autocritique regnant en anthropologie depui s la decolonisation et le postmodem isme. On n 'ose plus voir grand, adopter des perspect ives larges, nouvelles provocantes tant on est conscient des principes moraux dominant la recherche: donner l a voix au sans voix, adopter la perspect ive de l'autre qu 'il soit femme, musulman ou noir!

Une des constatations qui me semble evidente c'est qu'il n'y a, pour la plupart des anthropologues qui ont voyage, pas de 'patrie' parce que l'exercice de l'anthropologie lui-meme vous detache de vos racines, ne serait-ce qu'intellectuellement. Le cosmopolitisme est une qualite inherente a l'ethnologue et plusieurs s'en sont inspires ou l'ont souhaite (Appadurai, Kuper). Ce n'est bien sur pas une raison pour attribuer ce meme cosmopolitisme aux personnes observees et interviewees, par souci d'egalite ou sous pretexte qu'elles appartiennent a une nation europeenne dans notre cas, ainsi que le travail de terrain nous l'a enseigne. En effet, sous les apparences superficielles de la mondialisation, se cachent, pour peu qu'on prenne le soin et le temps de soulever le voile, des localismes vigoureux qui continuent d'impregner la vie et les comportements des communautes et des individus qui les composent. Cependant le cosmopolitisme

du chercheur, ce que Germaine Tillion appelait la vue aerienne, permet de garder partout la necessaire distance, celle qui garantit la rigueur sans detruire pour autant l'enthousiasme du chercheur. Sans une ouverture cosmopol ite enfin, il n'est guere possible de pratiquer la comparaison, qui, au risque de me repeter, est inherente a toute demarche anthropologique qui pretend sortir de la simple ethnographie.

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My First Proper Social Anthropological Fieldwork - Sunava, Northern Slovakia, 1970-1975[1]

Peter Skalnik

The political framework of my field experience was determined by the fact that I was growing up in the communist-ruled Czechoslovakia, a country located in central Europe, directly bordering with capitalist West Germany and Austria (so-called Iron Curtain). Czechoslovakia, unlike neighbouring Poland and Hungary that experienced fierce resistance to communist rule, was a docile vassal of the Soviet Union which did not allow liberties forced out by oppositionists in those two countries. Actually during the 1960s and 1970s I would go first to Poland and later to Hungary to breathe fresher air of access to information and generally liberal atmosphere. When finally reformist liberalism arrived to Czechoslovakia, it was crushed by Soviet allies' military invasion in August 1968 and subsequent occupation of the country for more than 20 years. These tragic events also thwarted my attempts to continue my studies in the United States (Northwestern University) or Western Europe (Cambridge, Bergen). In 1969 I was accepted for a 6-months post-Master study at Northwestern University in the United States but the new collaborationist Minister of Education refused to recommend me for an exit visa (for more see below).

I started my university studies at Prague's Charles University in 1962 my majors being African studies and history. I chose not to study 'ethnography and folklore' because as it was my mother's subject, I knew only too well about the theoretical poverty of the study of one 's own popular culture. In Prague, my mentors were two self-made social anthropologists, Lad islav Holy (Africanist) and Milan Stuchlik (then specialist on Indonesia and Oceania). In 1962 Dmitri A. Ol'derogge, famous Soviet ethnographer and Africanist, visited Prague and I was instantly charmed by

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[1] This chapter is a revised version of my paper published in Czech (Skalnf k 2001). Its oral version was presented at the conference 'Local Societies in Field Research ' that took place in Prague's Vila Lanna in 12-13 June 2000.

his personali ty.[2] I wanted to study with him in Leningrad. At that time study abroad was reserved tojust few in my country and scholarships were limited to the countries of the Soviet bloc. I applied for scholarship for the study of African studies *(afrikanistika)* at the Leningrad State University. I was eminent student but also probably because my parents were communist party members, I was awarded Czechoslovak Ministry of Education scholarship in 1963. Still as a student I had a privilege of attending the 7th International Congress of Anthropologica l and Ethnological Sciences that took place in Moscow and Leningrad in summer of 1964. At the congress I could listen to Meyer Fortes and Germaine Dieterlen vividly discussing Dagon and Tallensi using interchangeably English and French, witness papers read by Leslie White, Roman Jakobson, Masao Oka.

In Moscow I had a long exchange of opinion with my teacher Ladislav Holy whom I told that I am decided to devote my professional efforts to social anthropology . Indeed, my yearly seminar works, my M.A. and PhD. theses were all written on West African topics, especially state formation. After completing my studies, I wanted to walk in the steps of Holy and carry out my own fieldwork in Africa. My plan was to go to Northwestern, where Herskovits founded the oldest African studies in the United States, for a preparatory period and then continue to Ghana to carry out fieldwork among the Nanumba, a small northern Ghanaian traditional 'state' that remained unstudi"ed (Meyer Fortes, Jack Goody and Susan Drucker-Brown recommended this to me). This goal was 'missed' by a hair's breadth. The Czechoslovak border was closed for free travel in October 1969 and I was still waiting for a decision about financing of my study at Northwestern. When it came and I was to fly to Chicago on 4[111] January 1970, it was too late because Minister Hrbek and pro-invasion communist vice deans Jan Petr, Josef Haubelt and Antonin Robek at the Faculty of Arts prevented me from leaving, arguing that if it were other country than United States I would have had a better chance. My subsequent applications for scholarships for study of social anthropology in Cambridge and Bergen were rejected already on university level. Going to

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[2] The occasion was the meeting of the Permanent Council of the International Union of Anthropologica l and Ethnological Sciences, held in Prague, where my mother, Olga Skalnikova, was one of Czechoslovak delegates while L. Holy fulfilled the role of secretary. Beside Ol'derogge I had opportunity to meet famous African ist anthropologists such as Melville Herskovits, Meyer Fortes, Daryll Forde, Antonio Jorge Dias and Vinigi Grotanelli. Other personal ities who also came to Prague were Andre Leroi-Gourhan, Sergey P. Tolstov, Henri Vallois and others.

Africa to do research was then utopia but my library research was entirely devoted to Africa.

Another theoretical influence was my former Prague professor Jan Pecfrka, specialist on ancient Greece, who suddenly came to Leningrad early in 1964 and told me about the French Marxist interest in the Marx's concept of Asiatic mode of production (AMP) that was rejected by Soviet historians and Orientalists back in the 1930s in favour of Eastern feudalism.3 I purchased the relevant issue of the French Marxist review *La Pensee,* issue 114, in the kiosk in front of the university and followed its echo in Soviet scholarship (Skalnf k and Pokora 1966). Discussions on AMP prompted my interest in the state formation because I was not satisfied by the endogenous, gradualist and Eurocentric theory of the origin of the state as expounded by Marx's closest colleague Friedrich Engels in his *Origin of the Family, Private Property and the State* that was a compulsory read ing at Leningrad and considered a canon never to be challenged. But 'rebellions of evidence' (Godelier) collected by anthropologists working in non-European areas suggested that social classes and class struggle were not always and perhaps never present and causing inequalities leading to the formation of the state.

Fieldwork in Slovakia instead of Africa, Methods and Techniques, Ethical Problems

In this misery I received an invitation from *docent* Jan Podolak[4] to engage me externally as a lecturer of 'vseobecna etnol6gia' (general ethnology) in his department. I accepted. This was also with the hope that I could carry out a long-term fieldwork without which I would not become social anthropologist. It would be in Europe, in Slovakia, and following the example of my fellow Africanist colleague Josef Kandert who under the supervision of Holy carried out social anthropological fieldwork for his Cand idate of Science dissertation in one of the villages of Slovak Ore Mountains in the years 1967-1969. At that time, I had experienced only

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[3] Pecirka (1926-1993) was one of the editors of *Eirene. Studia Graeca et Latina,* an international journal published in Prague since 1960 that first reported in German and English about the renewed discussions about the AMP. He edited an influential volume of translations *Rane formy civilizace* [Early forms of civilization) (Pecirka 1967).

[4] Jan Podolak (1926-2017) was a leading Slovak specialist in *narodopis* (nationgraphy) who founded Kabinet etnol6gie at the Faculty of Arts of the Comenius University as a research unit. He promoted 'general ethnology'.

two short field sojourn s. In 1966 I was for two months in Central Asian Soviet republic of Tuva as a member of an archaeological expedition (I collected more than 100 specimens of traditional culture of central Tuvinians that are today deposited in Prague's Naprstek Museum of Asian, African and American Cultures). In 1967 I spent one month studying politics, kinship and religion in Northern Ossetia, in one of the valleys of the Soviet Caucasus.

The question was where and how to start. The opportunity offered itself soon. Already in February 1970 I accompanied together with *docent* Podolak a group of students during their winter fieldwork praxis in Liptovska Teplicka under the Kraiova Hol'a Mountain. Teplicka is a large village but we were too many even for it. Podolak decided that I and seven students will go to Sunava. Overnight we stayed in Teplicka, but the day we spent in two adjacent communes of Ni:Zna Sunava and Vysna Sunava. Every student obtained a topic according to classical nationgraphic key and they visited houses with their questionnaires. They were supposed to collect data on material culture such as agriculture, forestry, animal husbandry, crafts, traditional food, but also childbearing, marriage, death and related subjects. The students interviewed the dwellers, took photographs where appropriate. They were supposed to write short reports from the ten days research. I could do what I liked, except giving advice to the students. People whom I then met in Nizna Sunava were very friendly. But they kept in their chests a bitter wrong dealt to them in 1950 about which they told me. In that year, two years after the communist takeover, their village was attacked by armed workers militia who dragged away the priest and several other men. In the nearby town of Svit they beat brutally dozens of them. Twenty years later one could feel their defiance. After all, one house in the middle of the village bore the inscription 'Dubcek' [5] and a half ofland owners were not members of the cooperative. This was fascinating for me and I decided to do my major fieldwork in both Sunavas, with emphasis on the Nifoa that impressed me with its non-conformism. Starting with July 1970 I was half-time employed in Bratislava as a research assistant in the Kabinet etnol6gie (a small institution with six employees), part of the Faculty of Arts at the Comen ius University. I became a member of the Slovak Nationgraphic Society and with its support I threw myself into fieldwork still in summer 1970.

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[5] First Secretary of the Communist Party of Czechoslovakia, leader of a liberalisation movement, suppressed by Soviet military invasion of Czechoslovakia in August 1968.

There was no language problem since I spoke Slovak very well, thanks to Milica Linhartova with whom I spent a lot of time speaking exclusively Slovak in the years 1966-1969. But another *sine-qua-non* of social anthropological research – I mean, as long as possible stationary fieldwork -was difficult to fulfill. Nobody from my super-ordinates, even those who understood my quest, would have sympathy or did not know the way to approve official trip longer than one month. I am afraid that so it remains until today. Altogether I managed to spend a little bit more than a half year in the field. I went to Sunava in different parts of the year as I concentrated on activities of economic and ceremonial cycle. This I managed in spite of teaching obligations in Bratislava and teaching African studies and anthropology at two Prague universities and preparation of my Candidate of Science dissertation on early states in West Africa. In the beginning of 1972 when I transferred entirely to Bratislava, I fell in love with a Slovak girl who lived in Prague. I married her in June the same year. So until the end of my Bratislava engagement I moved between Prague and Bratislava with temporal rides to Sunava. I calculated that I travelled several times around the globe in mostly night trains. Prague Orientalists teased me: I am the only Czech Orientalist who commutes to the Orient.

Politically - motivated Challenges

I was however very content in the Bratislava semi-exile because I was taken seriously there, could teach more or less what I wanted from social anthropology and if I didn't try to defend my dissertation and of course if there were no 'complex evaluations' (de facto regular check-ups of political loyalty) I would have been in Slovakia till today. But from Prague came cadre materials, also on my father and mother. The Soviets discovered my commentaries in *Current Anthropology* and told it to the Slovak colleagues and then even in Slovakia I wasn't left in peace. My research project was under the heading 'social relations and change' still included into State Research Plan of 'Socialist Way of Life', but it was no more possible to publish the results of my Sunava research. My paper presented at Smolenice Castle conference in 1972 on 'Transformations of Popular Traditions in the Present' was originally accepted for publication in a two-volume collection under the same title, but was subsequently removed from the manuscript. It was published in Bratislava after 17 years, unchanged (Skalnik 1999). But I was fortunate to attend the 9th International Congress of Anthropological and Ethnological Sciences held in Chicago

in 1973 and a Hungarian Ethnographical Society conference in Szolnok in 1974 where I presented versions of the paper (Skalnik 1979 a, b).

Methods and Techniques, Topics Researched

My research was conceived as social anthropological, that is it followed the principle of holistic knowledge of social relations gained during the time of research. The synchronic approach enables social anthropologists to gain what is most valuable: authentic set of data. This presupposes as much participant observation as possible. I concentrated therefore on events that happened spontaneously in front of my eyes or those about which I knew in advance because they were included into the ritual or agricultural calendar or otherwise. So I participated in masses, weddings, annual meetings of the cooperative, elections, theatre performances, funerals, Christmas, Easter and Whitsun, sowing, hay harvest, sheep husbandry activities, harvest, potato collection. I would frequent pubs alone and with my interlocutors. First what I tried to document were kinship relations. Through in-depth interviews I managed to collect a host of genealogical schemes and namely in Nifoa Sunava I soon orientated myself in the network of kinship and ritual kinship. Most of all I was interested in politics or political economy. As I spoke to people an event after event emerged, conflicts and even tragedies that were significant for my informants. To such like the 'Sunava rebellion' many have returned, others were of neighbourhood character or completely individual. Very important for the people was emigration to North America or their relations with those relatives or neighbours who lived there, visited Sunava or even decided to return for good. With interesting individuals I concentrated on their life history.

Local Tensions

Perhaps most interesting from all in the first phase ofresearch (1979-1976) was the tension between cooperative members and private holders in Nifoa, while in Vysna all but three farm holders were members of the cooperative. Ithad direct consequences for the development of the two neighbouring communes. Nifoa considered itself a victim of communist licence and therefore its inhabitants low level of development of public service blamed on vengeance of state and Communist party organs in the district and region for the events of 1950. Vysna, in contrast, was a united commune and quite proud for that it could deal with 'masters above' and profit

from various advantages. Poverty was still in the memory of middle and older generations. It prevailed under the Tatra Mountains at least until the emergence of Bat'a works in Svit in the 1930s, but in substance was liquidated only during the era of 'people's democracy' and 'socialism ' after 1948 communist takeover. Better understanding of both village communes as a whole required to study not only parish registers and chronicles deposited by commune offices, but to merge into archival documents concerning villages and the whole district. This I did in the Poprad district archives and in the Hungarian State Archives in Budapest. I studied also historical and social geographic works on Spis Region and Slovakia as such. I became interested in human ecology. Most important for a broader perspective proved to be comparative study of village communities elsewhere in Europe and namely in mountain areas. I used my research periods in two Caucasus communes and the study of literature on Alpine communes written by Eric Wolf and John Cole (Skalnik 1974).

Dilemmas of the Fieldwork

In 1974 both Sunavas were admini strati vely merged under the simple name Sunava. This was actually a Solomon 's solution of the then dilemma of central communes. Nearby Strba, much larger than Sunava was made a ·central commune'. However, by unification of the two Sunavas a commune emerged that had more than 1500 inhabitants and thus it was not possible to ignore it like before. Moreover the 'Communist-Catholic' leadership of the united Sunava knew how to deal with the superordinate authorities. For me monitoring of the tension between the former independent communes turned into examination of some survival of separate identities in behaviour of inhabitants of the now united commune. Until the end of the first phase of research the problem I was facing was unsatisfactorily short sojourn in the field and therefore impossibility of concluding the research by an integral book-length text. A compromise was publication of articles that could only begin after I refused to sign politically motivated 'complex evaluation ' in 1975, followed soon afterwards by the end of my job in the Faculty of Arts at Comenius University with the explanation that 'Slovakia does not need an Africanist'. Because I was told by the Dean Samuel Cambel that I would not be allowed to work in the 'ideological sphere' (meaning education, culture, mass media) I had no choice than leave for good into full exile, in the Netherlands and later South Africa, where I gradually processed the results of my research into

several articles. I wrote two comparative studies, in wh ich the data from the Sunavas was compared with Caucasus and Alpine mountain communities (Skalnik 1982, 1986).

Results of the Research

The data, collected throu gh research were of differing quality. On the one hand hard facts on kinship or landholding, on other subjective narrations about conflicts among neighbours, conflicts with outside powerholders and similar that could not be verified by the study of, for example, police records. The handbook I used in my anthropological research was 6th edition of the well-known *Notes and Queries on Anthropology* and Myrdal's book on objectivity of social research (Royal Anthropological Institute 1964; Myrdal 1970). At that time, I did not know either Pelto's handbook on anthropological research or methodological book by Berger and Luckman on social constru ction of reality. Social anthropology was still ruled by structural functionalism of Radcliffe-Brown. I was under the influence of Meyer Fortes whom I knew personally. He, though student of Malinowski, was a close younger colleague of Radcliffe-Brown at Oxford. The collected facts -data about social structure were then considered objective – and eventual binary structures of Levi-Strauss, about which I also knew, did not seem to me useful in field research. Most important was to be *there,* to control utterances through real behaviour and viewpoint s of others. In order to reach as objective quality of recorded material as possible I used tape-recording and photographic documentation. Tape-recording cassettes were however lost after my escape to exile. What was preserved is all written documentation, for example fieldnotes, genealogies, re-written and ordered notes, thematic cards and also reports. Also, photographic positives and negatives and slides survived. As if nothing stands in the way of complex monographic write-up of my data, perhaps only auto-censorship.

A Sort of Re-study

Soon after my final return to Czechoslovakia in December 1990 when I returned to my alma mater at the Faculty of Arts, Charles University at Prague, I decided to continue in the Sul1ava research. This village community has obviously changed in the meantime, but I recognized a lot there and lot of peopl e remembered me. True, I had to acknowledge with regret

passing away of a number of good acquaintances who often were my informants (today we might say research associates), but entry into the village community was very easy. Finally, we had freedom and the uncertainties of the post-com munist period were not yet apparent. I was interested in continuity between communist socialism and post-communist democracy. I wrote an article for the Slovak weekly *Kulturny iivot,* in which I for the first time named the village with its proper name (Skalnf k 1991).[6] This time I had a powerful tool with me, namely a video recorder, with the help of which I recorded some narration s and activities which I would not be able to record during the communist rule. Thus far I stayed in Sunava several times, last time in 1998 and 2000. I passed through the village very briefly in 2016. My plan has been to study political culture and I still hope to carry it out. It is not easy to get free for fieldwork. Even ifthere is money, time is lacking. Advantageous is the direct rail connection Prague-Tatranska Strba or Poprad-Tatry that takes no more than 8 hours. This allows to complement data more or less continuously . What is disadvantageous is the impossibility of a long-term stationary fieldwork. It is very exciting to look forward to an opportunity to evaluate socio-political development of the Sunava village community during the 20th and the first quarter of the 2 1st centuries. After all, Sunava inhabitants had to brace seven political regimes in those 125 years.

Ethical Problems

In the initial phases of the research one had to be very careful. The topic of the research was officially known as 'social relations' or 'social transformations during socialism ', and even as such it was suspicious. Of course, I had always with me a special letter from my employer or from the Slovak Nationgraphic Society. But I most importantly tried to build relations with important and less important people that did not require any official recommendation. First, I stayed in Ni:Zna Sunava in the house of an old couple and after a year moved to the house of the popular cooperative zoo technician in Vysna. Both houses were located in the middle of the village. Trust was the most important principle. This means that I never referred to one informant what I learned from the other. I never used alcohol for getting information. I was also never seen drunk. Because I came

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[6] Enlarged English version was published soon afterwards (Skalnik 1993).

not for few hours I could exercise patience and information I was expect-
ing would come as if by itself. However, I have had some problem s with
suspicion of some pro-comm unist villagers which I softened or eliminated
by postponing publication of the research results in Slovak. My research
interests were very sensitive and I could not bring into trouble those who
supplied me with information about their pli ght during the communist dic-
tatorship. Thus, most results have come out in English in foreign publica-
tion s.

The second phase after 1989 change of the regime is different be-
cause I came to the village not as a researcher but as an old acquaintance
or a friend, or a visitor coming for vacation. In the meanti me, I carried out
researches in West and Southern Africa as well as New Guinea and now I
am in an altogether different category of an elder man. My role of a re-
searcher I understand also as a reciprocity. Several time I stayed in Sunava
with members of my family or with friends, always I distributed photo-
graphs from a previous sojourn. I think that the researcher has always to
keep in mind that he/she is not only a sort of intruder in the place of re-
search but also that her/his role is a mystery for the studied populatio n.
One should thus make oneself more accessi ble, speak about one's home
conditions so that one looks to people as normal. I strived always to adhere
to this principle, in distant Africa or New Guinea as well as in Sunava.
Ethical elements are in social research ever more important, people can
read what the researcher wrote about them. A researcher should try to com-
pare her/his viewpoints and interpretations with opinion and folk models
of the researched. I am certainly one of those who would not like to rela-
tivize everything and discount everything as credible when one could re-
ally observe it and in which one participated. But I am against
objectification of the informant, to deal with her or him as non-entity that
is good only as a source of information.

Epilogue

The rest of my career was filled with various further fieldwork experi-
ences. The major one was among th e Nanumba of Northern Ghana since
1978 (Skalnik, f.c.), southern Africa since 1983, two field sojourns in
Lihir, Papua New Guinea (1988, 1990), from which several video pro-
grammes emerged. Finally came two Central European field researches.
Both were re-studies in communes that were studied before by rural soci-
ologists. The first was in Dolni Roven located in East Bohemia, Czech

Republic (Skalnik 2004), the other in Dobrzen Wielki (Skalnik 2018) in Opole Silesia, Poland (2006-2018). In both of them students were involved in the research, the second culminated in four years group research by a team of seven experienced fieldworkers under my leadership (2015-18). All of these fieldwork efforts were intermittent, the longest non-stop stay in the field being Ghana for six full months. The New Guinea research remained a torso (Skalnik 1991b).

Starting with the Slovakia research I have developed *a life isfieldwork* philosophy or attitude meaning that I perceive my life as a continuous fieldwork experience. This approach to life has taught me how to face unexpected changes, brace unfair treatment but also to be attentive to the human and natural environment. The spectrum of fieldwork situations was broad, geographica lly and topically. Even though my topics were most commonly political, I have never been in prison and never felt really to be in danger of life. This I consider a good luck. Not all of my colleagues were so fortunate. I am looking forward to more field experience even if the radius of my ventures might shorten with age. The Slovak research experience was never ignored in subsequent fieldworks. Most important was always rapport nay friendship with those whose knowledge I was listening to and using in my writing. If my career was the result of my anthropological fieldwork, then I was always gratefu l for it to my fieldwork acquaintances and friends.
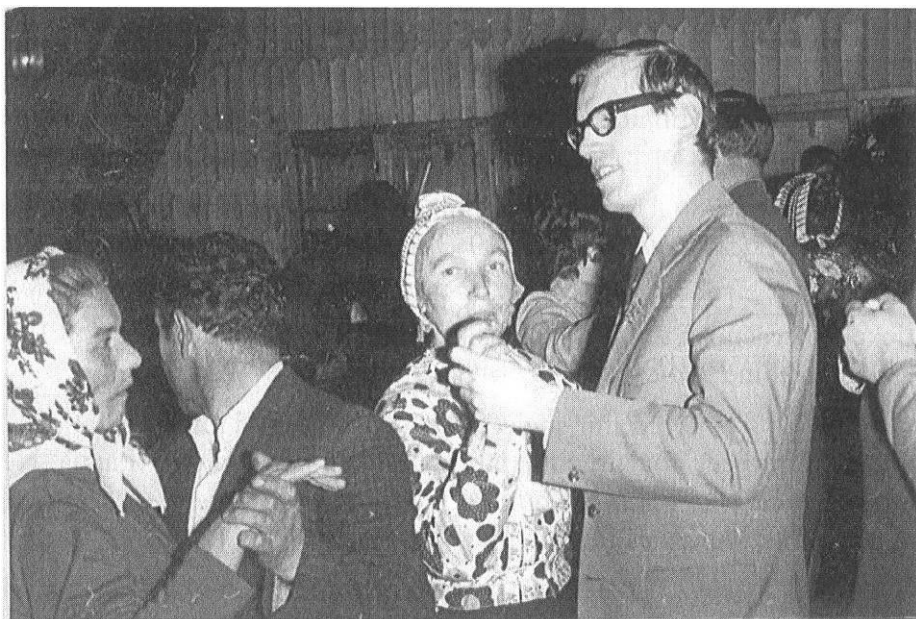
© Peter Skalnik. Nifoa Suilava with High Tatra Mountains in the background, private fields are very narrow, winter 1971.

My First Proper Social Anthropological Fieldwork

© Peter Skalnik. Easter Monday water bath, 12 April 1971.

© Peter Skalnik. Peasant dance evening,
ethnographer dancing with Ms. Zemkova, 1971.

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*Single-Sited
Ethnographies*

© Katherine Verdery. Uncle Petru Bota.

© Steven Sampson. The author with villagers in Maramure, 1976.

Fieldwork and Social Relationships
in Aurel Vlaicu, Romania

Katherine Verdery

When I recall my first fieldwork, which took place between November 1973 and December 1974 in the Transylvanian village of Aurel Vlaicu, two images flash into my mind. One is of the village cemetery, where I spent several weeks in a cold February writing down the information on the tombstones, so I could be 'working' but would not have to talk with people-something I found daunting, if not overwhelming. From the data the tombstones had provided I began sketching out genealogies of village families. The second image is of the woman I first stayed with, Maria, who helped me to overcome my panic about interviewing because she was always delighted to spend time with me and answer my questions. Moreover, she sent me along-or took me herself-to specific people for further conversation. This form of 'snowball sampling' was far preferable to the statistical methods I had learned in graduate school at Stanford, for it meant I showed up on people's doorsteps with a letter of recommendation: Maria's good word. That was a tremendous help to an American plunked down, at the height of the Cold War, in a communist country where she had no reason to think people would be anything but suspicious of her. (As I show in my memoir *My Life as a Spy* [1], it proved to be a reasonable concern.)

These two images suggest a great deal about my first fieldwork. On the one hand, a rather shy person who is not at ease meeting people, I had had (like most in my generation of anthropology students) almost no training in field methods and was basically terrified of being in the field. I had read plenty of theory but had very little idea of how to operationalize it, so those tombstones were a way of getting usable, maybe even quantifiable, kinship data. Added to that was the rumored omnipresence of the

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[1] Katherine Verdery, *My Life as a Spy: Investigations in a Secret Police File* (Durham, NC: Duke University Press, 2018). Much of the material I present in this essay can be found there.

Romanian secret police, or Securitate, who might intimidate those I visited (if not, for all I knew, me myself). On the other hand, I found many Romanians very friendly, and once I got into a conversation I was fascinated by what I could learn, even if I was not sure how I would use it. As time passed and people like Maria introduced me to their friends, my interest overcame my reticence. I continued to work in Aurel Vlaicu (Vlaicu, for short) or the neighboring settlement, Geoagiu, for the next 30 or so years and to return to it for visits until the present.

Before I get into my story, I should note for younger readers that I was formed at the height of the Cold War, that period of time when the United States and its allies faced off repeatedly with the Soviet Union. Powerful anti-communist sentiment pervaded the atmosphere in the U.S.; the Soviet bloc was presented in grey colors as a grim, joyless space in which everyone feared the secret police and stood in endless lines to acquire goods. Although part of my motivation in seeking to go there for my doctoral dissertation was to see what life was really like behind the Iron Curtain, I was not untouched by this atmosphere. I had been in the fourth grade when the Soviet Union launched Sputnik, setting off the space race in which the U.S. would seek to demonstrate its come-from-behind preeminence. Our concern about being better than the Soviets permeated my childhood. At some level, I learned to believe in the overall superiority of my own way of life despite being willing to expose that view to another reality, and I also believed the form of social science I was pursuing-heavily oriented to theory-was superior to what I found in Romania. It is surprising to me now how little my anthropological training affected that attitude.

In 1972 I applied for and received a research grant from the International Research and Exchanges Board, a U.S. organization founded in 1968 to promote scholarly exchange with the Soviet bloc that would not be largely dependent on government funding, as Fulbright fellowships were. At that time IREX grants were locally administered by Romania's National Council for Science and Technology (the Fulbrights were under Romania's Ministry of Education, a much more conservative institution). I was assigned a research supervisor, Professor Mihai Pop, director of the Institute of Ethnography and Folklore, who recommended that for my project I should go to the county of Hunedoara. He even accompanied me to the county capital, Deva, to work out exactly where I should settle so as not to cause any trouble. County officials recommended the commune of

Geoagiu.[2] Although they apparently assumed I would stay in the commune center, this had not been spelled out.

How, then, did I get to Aurel Vlaicu? In this essay I will answer that question and describe how once there, I gradually learned to do fieldwork, from a fairly shaky start. As part of that process, I expanded my circle of social relationships and began finding conversations with people more interesting than the theories I had started out with. I end with some comments about the relative advantages and drawbacks of being a 'foreigner' as opposed to a 'native' researcher.

'Choosing' a Field Site

My choice of Vlaicu was in some sense foreordained. Jn my first month in Bucharest, I found myself watching TV one day with some acquaintances. Among the programs was a report about a Romanian born in this village in 1882 named Aurel Vlaicu, one of Romania's two geniuses in the field of aviation and the inventor of Romania's first airplane. I was not well informed about Romanian history but at least now I knew who this fellow was, when I eventually showed up there. Although I had preferred other locations, those turned out to have military bases that I would have to stay away from (as I learned from driving into one). I had spent a good bit of tim e trying to decide upon a field site-trave ling around in the county, looking up places in a compendium and in the most recent census, and so on. But being assigned to Geoagiu com mun e nullified all that effort. My only contri bution was to pick Vlaicu out of the commune's eleven villages, because it was neither too big nor too small (915 people) and had its own collecti ve farm, unlike most of the others-and because I had heard of it. Moreover, 20% of Vlaicu's inhabitants belonged to Romania's German minority. So much the more interesting, I thought.

'Choosing' Vlaicu, then, was largely serendipitous, and it had consequences well beyond those I might have imagined. Once it became clear that my assigned location would make it impossible to do the project I had come with (concerning Romanian folk culture), Romanian-German relations moved to the center of my research, along with the history of the Transylvanian serfdom from which the ancestors of my villagers had emerged. These were topics about which I had read, but they were not the

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[2] The commune is an administrative unit, as in France. It usually contains 5-10 villages oriented to a central settlement.

project I had come to do. In brief, I did not initially choose my field site at all, except in the most limited fashion. I chose to return to the same place for subsequent projects because I now had invested a lot of myself in it and knew I could do better work there, in consequence.

I completed my first fieldwork in Vlaicu in December 1974, re-turning once again for four months in 1979-80 before publishing the results of my work, in 1983.[3] Another book followed in 1991, based largely on library research rather than fieldwork (see below).[4] After the regime changed in 1989, I went back to Vlaicu to study the process of decollec-tivisation-the restitution of land to its former owners-, spending the ac-ademic year 1993-94 there as well as several shorter field trips during that decade. It was the most extensive fieldwork I have done in Romania-as well as the most enjoyable by far.[5] By this time I was no longer afraid of either fieldwork or the Securitate: frequent visits and the accumulated wealth of data from my earlier research made it possible to work with many people I already knew well, and everyone realized that I could no longer be put off with superficial answers. Indeed, it was this history (in-cluding documents I had not used in my earlier book) that made me decide to return to the same village for this project rather than going elsewhere. Similarly, I chose it once again for my portion of a joint project on the collectivization of agriculture in the 1950s.[6]

My only exception to working in Vlaicu occurred in 1984-85, when I did some interviewing in Geoagiu, the administrative center, for a project on understanding s of national history in the rural population. For this pro-ject I needed a more varied sample (economically, occupationally, and ed-ucationally) than Vlaicu offered. I already knew some people in Geoagiu and was used to working with the People's Council there. In the end, how-ever, I was unable to use the material I collected owing to interference from the local police, who -according to gossip-were intimidating any-

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[3] Katherine Verdery, *Transylvanian Villagers: Three Centuries of Political, Economic, and Ethnic Change* (Berkeley and Los Angeles: University of California Press, 1983).

[4] Katherine Verdery, *National ideology under Socialism* (Berkeley and California: Uni-versity of California Press, 1991).

[5] The book it generated was *The Vanishing Hectare: Property and Value in Postsocialist Tramylvania* (Ithaca, NY: Cornell University Press, 2003).

[6] Results of this joint project with Gail Kligman and 17 other scholars, most of them Romanian, were published as *Peasants under Siege: The Collectivization of Romanian Agriculture, 1949-1962* (Princeton, NJ: Princeton University Press, 2011).

one who spoke with me. When I learned thi s, I left Geoagiu and completed the work as a library project instead.[7]

Learning about Fieldwork

As I said above, I had very little training in ethnographic methods. My first serious lesson was provided by my Romanian research supervisor, Professor Mihai Pop, who proved to be a marvelous help. Either an intuition about my lack of experience or just his everyday professionalism led Professor Pop to do something extraordinary for me. Once we had resolved my location, in Deva, he went out to Vlaicu with me to the host family I had been directed to, whom he persuaded to give a small party that first evening and to invite their parents, some neighbours, and the director of the village school. After they had poured the wine and served the cakes, the professor started interviewing those present, offering me a splendid example of precisely what I lacked. Maintaining an alert but also relaxed, friendly pace, he talked with them about village history, marriage and kinship, internal migration, and many other topics. I noted his unfailing smile, his courtesy, the way he addressed questions in a down-to-earth way of speakin g, reducing the social distance between him and them. It was the best possible lesson for a novice fieldworker.

Regrettably, his lesson was not fully learned.

A skilled ethnographer must keep her ears open all the time, to understand what the world looks like from the viewpoint of her interlocutor. By contrast, I tended to listen only up to a certain point and then to start expressing my own ideas, which kept me from discovering theirs. Here is an example. One day I stopped to visit one of the women I had made friends with, Veca. I caught her in a bad moment, as she was lying in bed on her stomach and her si ster-in-l aw was applying suction cups to her back. The sister-in-law held a small glass in her hand; with the other she used a candle to light a little stick wrapped in cotton and soaked with alcohol; she put the burnin g stick into the glass, and immediately after, the glass on Veca's back. The little glasses stuck there and bruises began to develop under them. When I asked them why they were doing that, they explained that Veca had caught a cold and the glasses would pull the cold out of her -as they were already doing, through the bruises. Having never heard of this practice (called cupping), I looked at them stupefied and

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[7] Verdery, *National Ideology under Socialism.*

started to talk about aspirin, cough syrup, maybe antibiotics, and so on. I didn 't try to explore their ideas, their practices of popular medicine: I told them this wasn't right. To this day I am still ashamed of myself.

Over the years, however, I did begin to change my style. For instance, I had begun my project about national history by asking people what they could tell me about one or another major historical figure. It wasn't going well. Finally, one man said, 'You shouldn't be asking us this question, you should ask the schoolteacher!' I reali sed that he was absolutely right, and that my research design had a major flaw: why shoul d collective farm peasants, many with only four years of schooling, be expected to know the national history, especially to tell a university professor who was much more educated than they were? So I changed my conversational strategy: instead of asking questions about famous figures, I would make a statement such as, 'I was driving through Deva the other day and I saw a huge statue of someone on a horse, but I don't know who it was. I didn't go to school in Romania, and I don't know a lot about your history'. In most cases my interlocutor would immediately rush to help me out, supporting my exalted status as a professor with the information I had admitted to not having. The conversations went much better after that.

In that case I had consciously changed my strategy, but in a more general sense, I had begun to treat my respond ents with more respect, in contrast to the way I had treated Veca with her cups. This meant listening to them no matter what they said and then seeking clarity. Although you might think I should know that already, I had not had much experience with that kind of listenin g-which is a skill, requiring practice-either in my graduate trainin g or in my previous upbringing. Initially, then, I simply did not know how to do ethnography. To support this self-critical opinion, I cite from my Securitate file a part of a telephone call between two colleagues, 'F' and 'N', whose conversation the police overheard with listening devices.

> F: – I have the impression that the ethnography she does picks and chooses from a number of domains without going into depth in the European style. I don't know if this is good or bad but I've seen . . . like with her discussion of Philosophy: three words from here, four from there...

> N : – Pretty much. Probably it's an excellent instrument for informing the public over there, because she synthesises

things admirably, you know... It's just that it's as if taken
<u>from an airplane.</u>

This seems a fairly good representation of my research style for my
first two books. It reflects a certain arrogance, a lack of concern for the
details and particulars that ought to mark the anthropological enterprise.
Perhaps it also reflects the stance of people raised during the Cold War,
the medium in which my research occurred: a taking of positions, rather
than an open exploration of one's interlocutor's ideas.

In any event, it was after the end of the Cold War that I believe I
finally began to do better ethnography . I returned to Vlaicu in the summers
of 1991 and 1992 to decide upon a project, and for the academic year 1993-
94 to carry it out. My topic, the restitution of collective farm land to private
ownership, had come from the villagers themselves during my summer
visits: this was what people were interested in talking about. Everybody
wanted to tell me their story about getting their land back. The experience
of working on this topic validated something important that one of my col-
leagues had once said: the research goes better when the topic is of interest
for the researched population too. If in the past some vi IIagers avoided me,
with this project everyone wanted to stop me in the street to tell me who
had broken into their ploughed field, what a nightmare the measurement
of their gardens had been, their problems with putting together the money
to pay costs of production, etc. This enthusiasm, I believe, was the result
of my having a theme that came from the villagers themselves, not from
my books and theories at home. Moreover, I remember several villagers'
telling me that they enjoyed my visits because they didn't have to worry if
the Securitate would be coming to ask them questions afterward.

In this research, the experience they were living was so historically
novel, the moment so open, that no one had certain answers to anything-
definitely not I. Exploring villagers' ideas about ownership, what they
thought it meant and how they felt about it, was fundamental to grasping
this new social process. I was finally learning to listen and no longer had
to copy from tombstones. It is true, though , that times were different from
before. I now knew a large number of people in Vlaicu, who had watched
me come and go for twenty years and were easily approachable, and none
of us assumed that the Securitate was still following me.[8]

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[8] This would change toward the end of the 1990s, when my friends began telling me that
the same officer as before had been asking questions about me. The difference, of course,

One day I went to visit Marioara, an elderly woman with whom I spoke often. A marvelous raconteur, she had taught me a great deal in a superb story-telling style that I had wanted to tape record, but she refused. After learning that a college student doing a master's thesis had taped her, I asked why she had accepted him but not me. First, she said, his interview was very superficial, unlike our conversations, but second, 'You can't promise me that things won't change again here, makin g it like before. I don't want to make trouble for myself .For me, this marked the shift away from the open research climate I had enjoyed throughout the 1990s, during which time it had been possible to do good ethnography.

Forming Social Relationships

A central feature of Romanian society-and, I believe, many other parts of the world as well-is the overwhelming importance of social relationships in people's lives. In Romania, particularly under socialism, relationships were crucial to getting anything done: to lower-level Party officials needing patronage, to higher-level Party officials needing clients, to city-dwellers needing reliable sources of food, to villagers needing help with weddings or funerals or with schooling their child in town, and so on. Writing in *Trade and Markets in the Early Empires* (1957), Karl Polanyi proposed that socio-economic life in any society is handled by some mix of three principles: markets, redistribution, and reciprocity. [9] In a socialist society i n which market forces had been distorted and redistribution was working out rather poorly, much of the work of living would depend on reciprocity.

I will attempt not to prove this assertion but only to draw its consequences for fieldwork. IfI was going to make any progress with my research, I wou ld have to create social relationships like everyone else. It took me a while to figure this out, but gradually I learned to bring little gifts to people I went to visit-some chocolates, for instance, a package of coffee, or for someone special, a pack of Kent cigarettes. Finding people happy to see me was one reward of this practice.

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was that now they told me openly. See Verdery, *My Life as a Spy,* for evidence of my continued surveillance.

[9] Karl Polanyi, Conrad M. Arensberg, and Harry W. Pearson, *Trade and Market in the Early Empires; Economies in Hist0ty and Theo1y* (Glencoe, Ill., Free Press, 1957: 250-253).

More important than these gifts, however, was the friendship I established right at the beginning with my first landlady, Maria, whom I introduced in my opening paragraph. On the whole, Romanians are warm and cordial from the outset but take a long time to establish true friendships-indeed, they think U.S. views of friendship are much too superficial (and they are right). Developing a friendship may start with a warm and positive feeling about someone, but it becomes a friendship only after much testing of that person's trustworthiness, many exchanges of favors or assistance, common friends who confirm the person's reliability, and so on. My friendship with Maria was an exception: we bonded with one another right away. An unusually intelligent woman whose seventh-grade education belied her great intellectual qualities, she had a warm and welcoming presence. Maria was my first and most enduring social relationship in Vlaicu. [10] Through her I learned about relationships in the village: who was related to whom, who was born in Vlaicu and who came into the village from elsewhere, who drank with whom, who was respected and who not (and why), the nicknames of everyone in the village as well as their godparents (a very important social relation in Romania), and later, who was sleeping with whom. I could do a lot with all this information in my conversations with villagers. Moreover, she would take me to one or another person she thought might be interesting for me-in other words, she herself was a social relationship 'factory', creating connections for me many of which would last for decades and contribute mightily to what I learned in Vlaicu. Thus, by having a strong relationship with Maria, I learned about most of the social relationships in Vlaicu. But most important of all, I could go into her courtyard or kitchen any time to drink a coffee together, share a bit of gossip, and enjoy myself with her. This is a great thing for members of a profession in which you leave home for a very long time and live among strangers.

Over the years, Maria recast our connection into the kinship idiom so basic to human societies (especially rural Romania), saying that she loves me as if I were her own daughter. Indeed, for a time I took on her daughter's role. In a documentary film in 2012 (by which time we had known each other for 40 years), she explained the situation as she saw it:

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[10] As of April 2020, she is still alive, age 93. We talk on the phone every three or four weeks.

> From the moment I saw her I really liked her. My daughter
> had just gotten married and moved to Bucharest and in my
> heart I felt a sort of hole because she had gone. And so when
> [KV] came, I thought to myself that it was good to have
> someone here with me.

Until that interview, I had never really understood how we could
become such good friends so easily.

The importance of forming these relationships stems from the re-
search methods of western anthropologists: with few exceptions, we work
individually, not in teams, and this fact has important methodological con-
sequences. Except for those who come with a spouse or family, we are
compelled to create relationships with people in order to serve our needs
as human beings. Without making such relations, we would be crippled by
loneliness. But once we make them, we are to some extent pulled away
from our normal way of being towards the world of the relationships we
make for ourselves. In short, our principal work instrument becomes our-
selves and our capacity to form relations with people. As a method of work
it is at the same time both enjoyable and very difficult. Essential to its suc-
cess is the receptivity of the people we work with. Because in my experi-
ence Romanians have a true genius for creating social relations with others,
including foreigners, it was lucky that I went there. Vlaicenii enabled me
to make use of my principal work instrument: myself.

Theory and Data

Learning to do better fieldwork meant adjusting the relation between the-
ory and empirical data in my writing. What separates my first two books
(on ethnicity and national identity) from the others (two about villagers'
relations with the land, and two about the Securitate) is the weight of the-
ory in relation to field data. The first book, *Transylvanian Villagers,* was
based on Fredrik Barth's theories about interethnic relations, alongside Im-
manuel Wallerstein's 'world-systems' theory-a huge framework in
which the villager from Aurel Vlaicu could easily be lost. More than half
of the book came from research in libraries and archives, not from relations
with living people. *National Ideology under Socialism* was similar: a lot
of reading in libraries and fewer conversations, all organized by some the-
ories of Pierre Bourdieu and Michel Foucault.

But in my book about decollectivisation, *The Vanishing Hectare,* the words of Vlaiceni and others were the overwhelming source of my analysis and presentation. Because the theoretical part did not overwhelm the empirical material, I consider it my best book-the one in which I truly became an ethnographer.[11] One can see this even in my final book, *My Life as a Spy,* about my relations with the Securitate. It is full of people's words-my friends and associates from Vlaicu, Cluj, and Bucharest; excerpts from the documents in my file; conversations with some of my officers and informers; etc.

For someone who started off with the most fashionable macro-sociological theories, the fact that I end up writing about kinship and clientelism (very old topics in anthropology) is, to say the least, surprising. But here is where my life and research in Romania brought me: to the overwhelming importance of the social relations that construct not only people's lives, but also our knowledge about them. This may seem a modest achievement, but it is of the essence, and it is something I learned from doing fieldwork with Romanians. Having started with macro-models and a certain intellectual arrogance, I gradually learned that ethnography demands a continuous desire to listen to people, and at the same time to use myself-my reactions, my sentiments-as an instrument for knowing. These practices distinguish ethnography from other social sciences.

I wish to highlight the trajectory of how I formulated the central research problems of my four field-based monographs. At the beginning, in the 1970s, I came with research problems established in the U.S., as was the theoretical apparatus through which I analyzed my material. The theme of my second project, in the 1980s, derived from the reactions of a number of Romanian intellectuals to my first book, but my interpretation of my material once again rested on theories from west European thinkers.[12] The balance shifted with my third book, *The Vanishing Hectare:* the theme of decollectivisation came from conversations with Vlaiceni, and the analysis was a mix of ideas from Anglo-American anthropologists, on one hand, and Romanian scholars working on decollectivisation, on the other. The theme of the fourth book, *Peasants under Siege,* was the result of interactions with Romanian colleagues from our research team, and the interpretation came from our collaboration.

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[11] Others seem to have agreed with this judgment: the book won the J. I. Staley Prize for 2011, anthropology's most prestigious book prize.

[12] See Verdery, My Life as a Spy, chapter 2.

In other words, all these research relationships pulled me away from my initial tendencies and domesticated me. With the help of my Vlaicu respondents and Romanian colleagues, my goals came to grow out of the realities I was studying and the research relations they entailed-making me, I believe, a better anthropologist.

'Native' versus 'Foreign' Ethnographers

These observations lead me to compare fieldwork by foreigners and native researchers: were there significant differences between the ethnography possible for those of us coming from the U.S. and Romanian social scientists? Romania has a long tradition, particularly rich during the interwar years, of social science research in rural areas. Typically, it consisted of teams of several researchers working together in a given settlement for a few weeks, each concentrating on specific aspects of rural life. They would then produce a collaborative analysis, sometimes involving repeat visits to the site, usually written by one person from the input of the group.[13] Gail Kligman and I created a different version of this joint research: in 1999, to study the collectivisation of agriculture in Romania during the 1950s, we fomled a team of nineteen historians, anthropologists, and sociologists, five from the U.S. and U.K. and the rest from Romania. Each was to conduct research in a community or region usually where they had already been working then write a case study to be included in an edited volume. Kligman and I, the two organizers, then produced a synthetic analysis from these and our own research.[14]

Members of our research team had very different relationships to their research sites, and this had an impact on the kinds of oral information

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[13] For outstanding monographs produced by this method, see Ion Conea, *Clopotiva: un sat din Hat eg* (Bucharest, lnstitutu l de $tiinte Sociale al Romaniei, 1940); Francisc Josef Rainer, *Dragu§ . un sat din Tara Oltului (Fagaras)* (Bucure$ti, Institutul de $tiinte Sociale, Tnstitutul de Cercetari Sociale al Romaniei, 1945), and Henri H. Stahl, *Ner ej , un village d'une region archaique.· monographie sociologique* (Bucharest, Institut de Sci- ences Sociales de Roumanie, 1939).

[14] Two books resulted from this, each in English and Romanian. Dorin Dobrincu and Constantin lordachi (eds.), *Tiiranimea :jiputerea: Procesul de colectivizare a agriculturii in Romdnia (1949-1962)* (la$i, Polirom, 2005), was translated into English as Constantin Iordachi and Dorin Dobrincu, *Tramforming Peasants, Property and Power: The Collectivization of Agriculture in Romania* (Budapest, Central European University Press, 2009). Kligman and Verdery, *Peasants under Siege,* was translated as *Tiiranii sub asediu: Colectivizarea agriculturii in Romdnia, 1949-1962* (Iai, Polirom, 2015).

they were able to obtain. Five members had little or no prior familiarity with the communities in which they worked, though most had contacts of some sort who had led them there. Two others had conducted previous research in their research sites during the 1990s. Five members were born or have close kin (grandparents, affines) in the communities they researched for our project, and the role played by those kin in the collectivization process had important implications for their research results. One of our colleagues, for example (Calin Goinea), was the grandson of the first collective farm president in his natal village, though his grandmother's family had been opposed to the collective. These facts influenced people's perceptions of his interviewing. Another (Daniel Puiu Latea) found that his local knowledge from growing up in the community he studied facilitated his research in important and time-saving ways: he knew names, genealogies, conflicts, and past and present feuds. Armed with this local knowledge, he could use relationships to his advantage, playing naive, pretending he didn't know about something, and then following his interlocutors' reactions. A third team member (Iulianna Bodo) worked in her husband's natal village, where she has affines and friends as well as neighbors who know her. From these connections and other research projects she and her husband have conducted in the village, her familiarity with it is almost 'native'.

Other than those born in their research sites, however, none of the Romanians on our team equaled Kligman's and my longevity in the field: over twenty-five years each, in stays sometimes lasting a year. We U.S. anthropologists came to Romania and then would not leave. We stayed in a single community for a whole year or even more, usually in the home of some family; some of us came back in later years as well. People could lie to us for a time, but then they would forget what they had told us earlier and say something else; the discrepancies often proved ethnographically illuminating. People would gradually get used to us. This lengthy exposure helped transform our potentially problematic identities as 'foreigners' into 'familiars' and enabled us to benefit from the local knowledge and dense relationships we had formed and maintained, without which we would have had much more difficulty doing this kind of work. By contrast, Romanian researchers who lack ties to a community (as kin, through networks or experience, etc.) are considered nearly as 'foreign' as those of us from abroad. Villagers are not necessarily more likely to prefer working with them instead of a foreigner they have known for years.

The difference between natives and foreigners is affected, of course, by the political environment in which the research takes place, as evident in Marioara 's comment above concerning my inability to guarantee that things would not change. Central to the political environment during my research in the 1970s and 1980s was secret police surveillance. As I have realised from working in Romania's Securitate files since 2000, officers' default assumption concerning U.S. researchers and Fulbright lecturers was that they were spies and would have to be closely watched. In Vlaicu, the rumor that I was a spy (encouraged, if not indeed planted, by the police through their informers) circulated throughout the socialist period and even afterward .[15] Surely such a reputation would inhibit villagers from talking with foreign spies? In 2009, Romanian oral history researcher Cosmin Budeanca interviewed some Vlaicu villagers about local history; among other things, he asked what people had thought of me.[16] Several of them replied more or less like this: 'They said she was a spy, they did. But after a while they got used to her. She stayed a long time, and they got used to her'. Another elaborated more fully: 'People said she was a spy, but if she was one they wouldn 't have let her into the country. And she didn't have anything to spy on, 'cause we just talked about the collective farm. So if she has permission from Bucharest to be here, why would she be a spy'?

It is easy enough to imagine that a foreign researcher might be a spy, but perhaps less likely that a 'native ' Romanian researcher would be thought one. On the other hand, an unknown Romanian researcher coming to the countryside from some city might easily be suspected of being a Securitate informer, and this would make people no more likely-if not indeed less so-to be willing interlocutors for a native scholar seeking interviews than for a long-known foreign one. This is key: except in the case of researchers born in the community where they worked, Romanian researchers in general come for fairly short periods, unlike foreigners with research grants enabling them to stay for up to a year or more.

My conclusion concerning the relative advantages of local as opposed to foreign scholars as interviewers, then, is that this is not a categorical opposition with inevitable effects on one's ability to do research. More

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[15] For an extensive account of this issue, see Verdery, *My Life as a Spy.*
[16] I thank Dr. Budeanca for sharing his interviews with me. He had been recommended directly to specific people by the village priest, whom he had known at university. Therefore, he was not coming into Vlaicu 'cold'.

important is the nature and length of one's contact with the community. Whether one is native or foreign is less significant than the longevity of one's exposure, which enables interpreting responses from 2000-2003 (for example) according to the social relations and attitudes of villagers in the 1970s and 1980s. Equally important is the distinction between ethnographic research and more strictly interview-based work, which is usually short-term and does not allow the researcher to evaluate responses in broader discursive fields of daily communication and interaction over time, in the way that long-term ethnographic research does. [17]

Fieldwork can be a difficult, lonely activity-especially in the early phases, the 'tombstone' moments, before one begins to give oneself over to developing social relationships with people in the field, like my Maria. As one develops those relations, however, one begins to become a different kind of person, testimony to the fact that we are above all social beings. Perhaps this is less true of those who do short-term fieldwork in one or multiple locations. In my case, lengthy and repeated exposures to my field site substantially changed my ways of thinking and being-in a positive direction. That is a wonderful justification for doing extended fieldwork.

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[17] For further discussion of these issues, see Kligman and Verdery, *Peasants under Siege,* Appendix D.
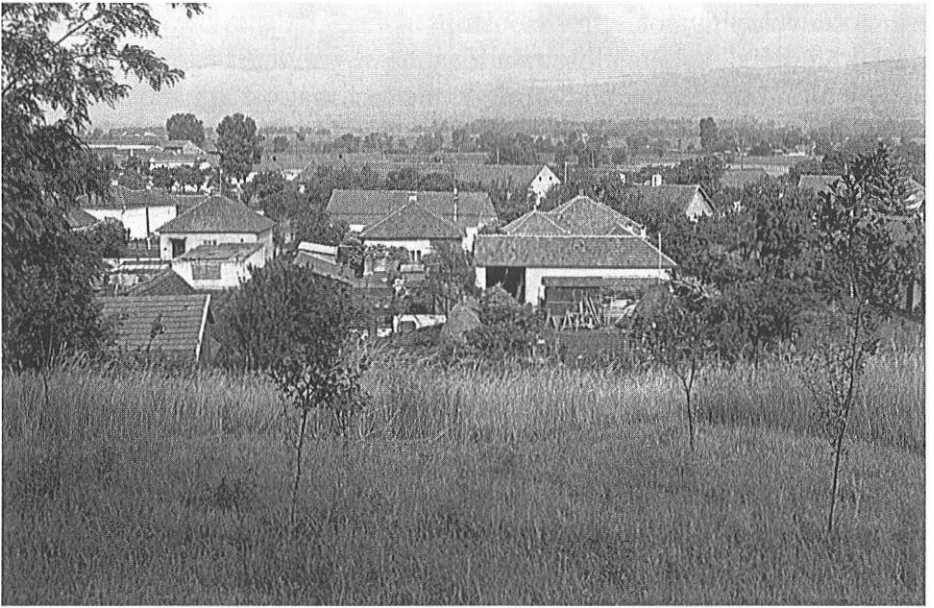
© Katheri ne Verdery . A view of the village of Aurel Vlaicu.
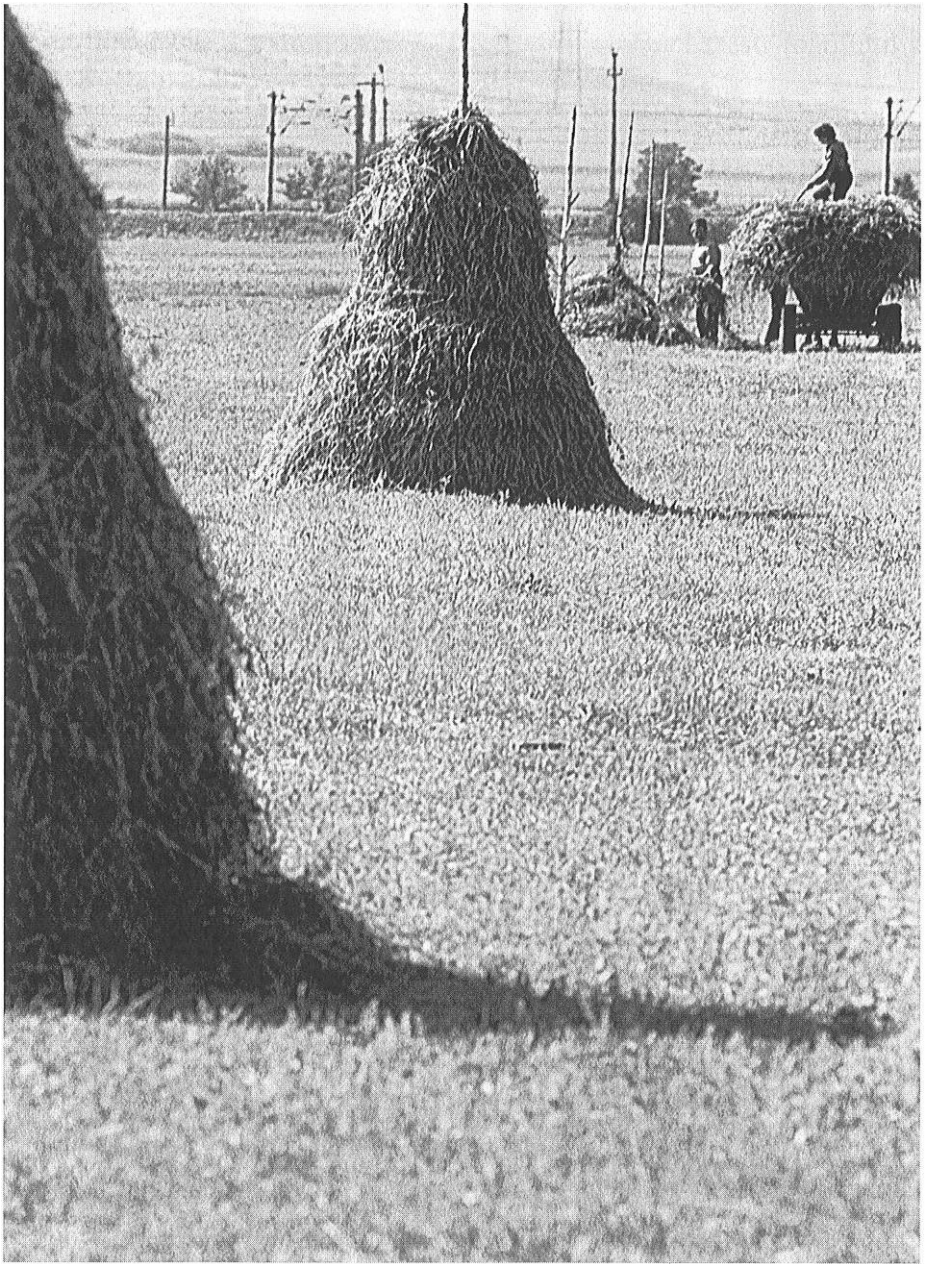
© Katherine Verdery. Gathering hay for transport.

© Katherine Verdery. The author and her favorite respondent, 1977.

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Reflections on Fieldwork in Maramures: Identity as a Category of Practice[1]

Gail Kligman

'Why Romania of all places?' Family, friends, and colleagues have repeatedly asked me why, absent family roots, I was originally drawn to the country and what has kept me coming back during fourty years of ethnographic research. In this paper, I recount how Romania entered my life in such an enduring way and then reflect on various of my experiences in Maramure, a region in the far north, from the end. of 1977 to the collapse of the Ceau escu regime in 1989. In particular, I explore three key aspects of the social construction of my identity: as an American, a single woman, and a secular Jew.[2] These three categories of practice (Brubaker and Cooper 2000) became differentially salient throughout my fieldwork in Maramure , shaping my personal interactions and the course of my research in fundamental, and, at times, unexpected ways.

The Socialist Republic of Romania and Mararnure

I knew little about the Socialist Republic of Romania before I went to college. While an undergraduate at the University of California, Berkeley, to which I had transferred from Reed College in Portland , Oregon, my interest in Eastern Europe piqued. In 1974, as a sociology graduate student at Berkeley, I served as a research assistant for a visiting professor from Romania. Mihai Pop was an eminent folklorist, ethnologist, and semiotician at the University of Bucharest and director of the Institute of Ethnography

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[1] This paper draws from Kligman 1988, 1998, 2011, 2017. References are minimal and do not reflect the rich scholarship in and about Romania.

[2] This reflection is not an autoethnography (see, for example, Heider 1975; Khosravi 20 I O; Moors 2017), but rather is in the style of a memoir, broadly informed by thinking about methodological issues in ethnographic research and the reflex ive turn in sociology and anthropology (see among many others: Bourdieu and Wacquant 1992; Burawoy 2003; Clifford and Marcus 1986; Emerson 2001). It also sheds light on doing fieldwork during the Ceauescu period.

and Folklore. An enormously inquisitive, multilingual and erudite East European intellectual, Professor Pop's optimism and 'joie de vivre' were inspirational. I had no idea then how instrumental and influential he would become in my life. Later that summer, I visited Professor Pop in Bucharest and met some of his family members. The World Population Conference was being held there, which, at that time, was not of great interest to me. Yet years later, its focus on demographic issues found resonance in my research agenda and my understanding of life in Ceau escu 's Romania (Kligman 1998, 2007).

That brief stop in Bucharest in 1974 would be the first of countless trips. I returned to Berkeley to formulate and defend my dissertation proposal on the medical system in a modern izing socialist state. I expected to conduct field research in what was then the Socialist Republic of Macedonia in the Socialist Federal Republic of Yugoslavia. Few of my peers in sociology were doing research abroad, and my foray behind the 'Iron Curtain' contributed to a persistent miscategorization of my professional identity as that of a cultural anthropologist.

During that era, for Americans, research in the eastern bloc was managed through bilateral agreements between the U.S. and the respective countries. When I applied, the Yugoslavs were suspicious about the work of two other Americans, in consequence of which I was informed that while I would be welcome to carry out fieldwork there, I would be accompanied while doing it. That hardly seemed the best way to undertake sociological or ethnographic research, which caused me to panic about my doctoral project. Romania came to the rescue.[3]

In the 1970s, Romania, unlike a number of other socialist states, was relatively open to the presence of foreign researchers. One factor that contributed to Romania's 'openness' to the West and its positive image in western countries was Ceau escu's refusal to participate in the Warsaw Pact invasion of Czechoslovakia, which solidified his reputation as a maverick in the bloc. The West looked favorably upon him[4], even though his

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[3] While Romania welcomed western researchers in the 1970s, the country's official representatives were not as forthcoming about surveillance practices as the Yugoslavs had been. See Kligman 2017; Vcrdery 2014, 2018.

[4] Verdery (1991: 105- I 06), like others, noted Roman ia's 'declaration of independence' in I 964 signaled the beginning of growing trade relations with the West. The U.S. granted Romania Most Favored Nation status in 1975, which it maintained unti l 1988 when Ceauescu renounced it in anticipation of the U.S.' withdrawal of Roman ia's MFN status due to human rights abuses.

courageous stance in 1968 simultaneously foreshadowed the national ism that marked his politics thereafter.[5]

Although I was doing my doctorate in sociology, I was assigned to Professor Pop at the Institute of Ethnography and Folklore. Awaiting my first official meeting with him, I was astonished when the young woman who exited from his office came up to me and inquired: 'Aren't you Gail Kligman?' Itwas Katherine Verdery, whom I had met during my freshmen year at Reed College but had not seen since. She was a graduate student in anthropology at Stanford University and had ju st completed her dissertation fieldwork as I was about to begin mine. That encounter marked the beginning of what has become a lifelong friendship and professional relationship.

During my dissertation research, I was based primarily in Bucharest.[6] Soon after my arrival, Professor Pop introduced me to a French anthropologist, Claude Kamoouh, then doing intensive field research in Breb, a small village in Maramure . He suggested I come visit for the Chri stmas celebrations there and in Sighetu Marmatiei, the major city of what is considered historic Maramure . He thought this visit would broaden my exposure to Romania, which, from my vantage point in Bucharest, I understood to be a modernizing socialist state. Maramure , he remarked, wou ld make me feel like I had gone back in time, even to an earl ier centu ry. Indeed, it was rad ical ly different from anything I encountered in Bucharest or in the south of Romania. I returned for Easter with another colleague from the Institute of Ethnography and Folklore, Radu Rautu, who was th en doing a folklore project on sorcery. These initial excursions to the region shaped my future research.

Upon completion of my doctorate, I returned to Maramure for postdoctoral research on sorcery. Party tenets about scientific socialism repudiated superstition, sorcery, and other-world ly beliefs. Yet, under the cover of the darkness of night, local Party mem bers continued to surreptitiously seek the sorceress' advice. Unfortunate ly, the practitioner I had previously met died soon after I settled in Maramure , so T had to change my research project. I could not try to find another person involved in activiti es the Communist Party held in contempt; it would have been too

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[5] See Kligman 1998; Verdery 1991, among others.

[6] See Kl igman 1981, 2017. My doctoral research involved sem i-structured interviews, group discussions, and observation during short visits in villages and towns across the southwest of Romania.

compromising for someone who had not already met me. Moreover, my research—where, with whom—had first to be approved by the authorities. I was not free to do entirely as I chose. Whenever I arrived in a village, I was expected to register with the local police, and Romanians were supposed to report their interactions with me. As in any authoritarian regime, fieldwork in Romania posed ethical issues for researchers and Romanians alike. How such concerns were negotiated varied greatly.

After a short period in the village of Glod, Professor Pop's birthplace, I relocated to Ieud, where I spent thirteen months doing ethnographic fieldwork on life cycle rituals (Kligman 1988). My research consisted of intensive participant observation, semi-structured in-depth interviews, and daily informal discussions.[7] Living with a family introduced me to the general concerns of everyday life in a way that living on my own could not have (nor was it an option). As my knowledge of the vicissitudes of daily life in a communist regime deepened, gossip became a vital source of information on topics of interest to leudeni that were not necessarily central to my research but which broadened my overall understanding.

While not all of my research in Romania has since been in Ieud[8], I will focus on various of my experiences there during the communist period, reflecting on interrelated aspects of the ongoing construction of my identity by others and myself. Researchers doing ethnographic fieldwork must in one way or another engage the socio-cultural constructions of their personae and their own presentations of self. How did I become a quasi-kin member of the Romanian family with whom I lived? How did these villagers in the far north of Romania make sense of my presence there? I was surely a curiosity to everyone. They had no familiar cultural categories into which I could readily be situated.[9] First and foremost, I was an American, simultaneously idealized yet mistrusted. I was also a single woman, thereby challenging their familiar gender expectations. And I was a non-

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[7] I recorded ritual events and interviews unless asked not to. In lieu of recording, I took detailed notes. It is important to keep in mind that my research was done in the context of a surveillance state.

[8] I returned to Ieud in the summer almost every two years until the regime's collapse.

[9] Most family, friends, and colleagues at home reciprocally lacked cultural categories through which to comprehend my daily experiences in Romania.

religious person living in the midst of a community in which religious tensions between Orthodox Christians and Greek Catholics (Uniates) were simmering, although not readily evident to outsiders. [10]

I had no idea when I first arrived in Ieud that Aunt Juji *(Miituii Juji)* and Uncle $tefan *(Unchefu* $tefan), the parents of what would become my surrogate family, had been reluctant to accommodate me. An American residing in their house would invite unwanted attention, not only from other villagers, but from the secret police, Romania's notorious *Securitate* (Deletant 1995; Verdery 2018). This family already had a difficult history. Labeled *chiaburi* or wealthy peasants, they were deemed 'enemies of the people' and had their property confiscated in the 1950s. (See footnote 31.) According to State Decree nr. 225, it was illegal for an unrelated foreigner to reside in the home of a Romanian; to do so required approval from the highest authorities in Bucharest. At the insistence of Mihai Dancu, the Director of the Ethnographic Museum of Maramure, who was also Aunt Juji's first cousin, my soon-to-be host family finally agreed to a one-week trial period. Dancu understood that staying there would be beneficial not only for my research, but to me personally. Among her various qualities, Aunt Juji had a way with words, was a good cook, and a good source of information about traditional customs. People were always dropping by, meaning that word about me spread rapidly via the traditional communication system-the village gossip network.

Thus began the social negotiation of my identity as an American in their midst, a single woman, and, unbeknownst to them until two decades later, Jewish as well. These three categories of identity shaped who I was perceived to be in Ieud and my presentation of self as well. Being American and a single woman were always present in some way. My religious identity was context-specific (e.g. in a conversation, at a ritual event such as a baptism, wedding, or funeral) and was constituted for the purposes of my fieldwork, as I discuss below.

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[10] The Communist Party outlawed the Greek Catholic (Uniate) Church and tried to force it to merge with the Orthodox Church. Ieud was home to prominent Uniate priests and resistance was strong. Priests who refused to switch were imprisoned. This period left a lasting divide that erupted into the open after 1989 when confiscated church property became a hotly contested issue.

The American *(Americanca)* [11]

The family's skepticism about hosting me for a year was understandable. Aunt Juji was then taking care of her bedridden, semi-paralyzed mother-in-law and was not eager to have responsibil ity for me too. During the week, her husband and children lived in an apartment in Sighet; he commuted home every weekend. Why did she need the burden of an American who would undoubtedly have pretensions to a lifestyle they could not provide? They could offer me a bed with a straw mattress and a warm woven sheep's wool blanket in the room used to receive guests, which connected the kitchen to the family's bedroom. Privacy was limited at best. There were no 'urban' amenities: no electricity, ju st an oil-burnin g lamp over the table; no running water although water from the well was heated on the wood-burning stove in the kitchen that served as the source of heat. The outhouse was in relative disrepair. As Juji emphasized: 'we don't have condition s for *domni'* . [12] She had her own ideas about the world I inhabited. While I would have preferred a private room and a bathroom , the advantages of living there far outweighed such inconveniences. The ailing, widowed mother-in-law was an esteemed godmother in the village whose godchildren visited her often; Uncle tefan and Aunt Juj i also had many godchildren who stopped by. Juji 's mother had been midwife, adding another if overlapping group of women who learned of my presence. When Uncle tefan was home, men came to chat and asked him about me too. As an outsider, where *I* lived hastened awareness of my being there and made it easier for me to meet peopl e.

        The local authorities were also interested in the 'condition s' I allegedly expected. The mayor wanted me to live in his house, which had electricity; I wou ld have a modicum of household comfort and he wou ld receive rent transferred from the National Council for Science that administered my IREX grant. I politely refosed; staying at an official's house

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[11] As mentioned, I first lived in Glod. Accompanied by Mihai Dancu , we walked to this relatively isolated village because snow made the dirt road im passable by car. As we neared, someone shouted: 'Here comes the American?' 'Here comes the American' resonates with an historica l reference that has shaped the category of American both positively and negatively. In the early years of communism, 'the Americans are coming' was a widely circulated sal vationist rumor that offered hope that communism would be temporary. But the Americans never came, which raised serious suspicions and doubts about Americans. See Kl igman and Verdery 2011, 277-278.

[12] Domn refers to the gentry. It connotes class and status distinctions pertaining to those who are educated, have 'manners,' and are accustomed to 'creature comforts'.

would have greatly limited my incorporation into village life and made it much easier for the mayor and others to report on with whom I met, when, and why. [13]

Settling in was a dynamic process, which involved negotiating who I was to be in the field, my possible or plausible self, and how I would find my way of being in an environment so different from what was familiar to me. People there had no realistic basis for understanding my life in the United States. Anti-American propaganda combined with American television shows such as Dallas and Columbo, which could then be watched at the homes of those few who, by the mid-eighties, had electricity and a television, created an idealized image of America. My attempts to temper overblown convictions about the wonders of America were futile; I was constantly lectured about how life was in the U.S. by those who had never been there. Their 'knowing' better than me underscored a partial void that I could not transform, a feeling of loneliness that was existential rather than social due to the geopolitical and economic constraints that made visiting the U.S. impossible for most Romanians before 1989. While I would become actively involved in the lives of my Romanian surrogate family and friends, they could only know me through our interactions there. My identity was, in consequence, a partial one, distinctly 'situated' in time and space.

Because of the *Securitate,* on the one hand, and local gossip networks, on the other, it was widely known in that part of Maramure that an American was living in Ieud. That was how people of all walks of life referred to me (and still do[14]): *Americanca,* the American. [15] In Ieud, there were variations on who identified me in this way. In the extended family, I became 'our American', for example. 'American' marked my status quite clearly. It also conveyed an implicit warning to be careful about what was

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[13] While villagers had to report on interactions with me, surveillance activity there was less intrusive than in cities. It was easy to know where I was at any moment. In urban environments, the Securitate often blatantly followed me (Kligman 2017).

[14] After the recent death of a woman I've known since 1978, the priest read a condolence message on my behalf. He publicly identified me as the Americanca.

[15] While the category 'American' was often interchangeable with that of 'foreigner from the West,' being an American carried more ideological weight than, for example, being French. Officials initially wanted me to commute to Ieud, an impossibility given the extremely limited means of transportation; I argued that French colleagues were allowed to live in villages and they too were from the West, that discriminating against me went against Romania's privileged MFN status, etc.

said to me. After all, what was I really doing there in this isolated northern area a few kilometers from the Soviet border? Cold War tropes lurked in the shadows of those of us from the West and could readily be invoked (Verdery 2018).

Being an American mattered in other ways I could not have fathomed. During my stay, I was bitten by a shepherd's dog. Due to unusual allergies, I could not have rabies shots. However, once the local doctor had seen me, knowledge of my plight reached Bucharest. I was summoned to the capital where, after the long train journey with an aching wound in my calf, I was met at the station by Professor Pop and, unexpectedly, representatives of the American Embassy. In Ieud, the owner of the dog and local authorities feared that I would initiate a lawsuit, thereby creating an international incident. I was uncomprehending. A lawsuit? My worry was rabies. Had I not been an American, and officially in Romania supported by an IREX grant, it is unlikely that this experience would have attracted much attention.

In Ieud, 'American' encompassed the class and status distinctions associated with 'dornni', whether local, from urban cities, or foreign countries. Deference and respect were due me, which was manifest in how my hosts and others thought I should be treated. But 'American' also referenced the political tensions of the Cold War: America was the number one enemy of the communist bloc-politically, economically, and culturally. Being an American in this context in a village near the Soviet border meant that I was always the 'Americanca', in one way or another.

A Single Woman

Compounding the category of being an 'American' was being a single woman, who in their cultural idiom, should have been married by then. Most academic women in Romania were married; why wasn't I? Village women wondered what was wrong. They missed few opportunities to ask when I intended to marry, why my partner did not visit (having little understanding of how difficult it was to get to Ieud) [16], why I did not marry a Romanian, and so on. There was endless gossip, innuendo, and matchmaking fantasies, which I often deflected through joking responses: how could

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[16] Following the long flight from the U.S. to Bucharest was a fourteen-hour overnight train to Sighet. Buses to Ieud were infrequent and cars were few. Herds of animals and horse-drawn carts also travelled on the two-lane main road.

I allegedly be involved with or interested in so many different men and still be a respectable woman? At times, their insistent questioning was simply tiresome.

My gender raised other concerns. As I settled in, I was not perceived to be as pretentious as 'domni' are assumed to be, but how would people acknowledge my status as an intellectual and foreigner when addressing me? My youth and openness did not fit the customary formality of most terms of address. 'Dr.' in recognition of my doctorate was much too awkward. Most people settled on addressing me as *domnioara,* Miss, which di mini shed the hierarchical, formal aspect that adding my last name signified, but was not as informal or familiar as using my first name.[17] I understood that this was a reasonable compromise that satisfied cultural and linguistic expectations, even though it was jarring to my feminist sensibilities.

Understanding gender ro les was illuminating and provided the framework against which I negotiated my own gender identity and confronted some of my own assumptions about gender relations.[18] Women were responsible for almost all domestic labor. [19] But as a guest with status, I was not allowed to perform household chores. Aunt Juji or her daughters were to make my bed and to serve m e meals, to be eaten separately in the room for guests, where I worked and slept. I was profoundly uncomfortable with all of this and began a process of negotiating what would allow them to show appropriate respect and me to feel more at home and less lonely (e.g. I mad e my own bed, ate with the family). Yet what was possible in the fam ily, into which I had become integrated as a member, was not in publ ic. Once, Aunt Juji and I both unwittingly overstepped the public boundaries of gender propriety . With much preparation to be done before Easter and the arrival of colleagues from Bucharest, I offered to don rubber boots and take the pots and plates to the stream to scrub them along with the other women doing the same in anticipation of the spring holiday. Those women, however, were scandalized that Aunt Juji would take advantage of me in this way, a foreign guest, who was neither daughter nor

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[17] Domni oara references single women of the 'domn' category. My host family's children eventually agreed to address me by my first name although never in publ ic. That was deemed too culturally disrespectful. However, it was cu lturally appropriate for me to address my hosts as Aunt Juji and Uncle   tefan, signifying our 'kin' relationsh ip.

[18] On being a single woman in the field, see, for example, Wolf, 1996: 9.

[19] The traditional division of household labor persisted; the Party 's promotion of gender equality did not extend into the private sphere.

daughter-in-law. Criticism spread of Aunt Juji's transgression; I refrained from any further assistance outside of the house. Incorporating me into the family entailed an ongoing dialogue about gender and generational relations with extended family members, but not more generally with other villagers.

Early on, I had to confront many of my own taken-for-granted views about gender relations, especially with respect to domestic violence. Women internalized the belief that they are inherently evil, *a dracului,* of the Devil, an original consequence of Eve's acquiescence to temptation. Wife beating was accepted practice. While women did not relish it, they also did not question it as their due unless their husbands were alcoholics. Ritualized expressions during weddings in which married women instructed a new bride on what awaited her included admonishments that 'your husband isn't your brother, don't think he won't beat you'. Luckier ones boasted, 'Since I married, not one palm has he given me'.[20]

As I became increasingly entrenched in village and family life, I also became privy to discussions l had not anticipated, which deepened my understanding of the far-reaching effects of the regime's draconian pronatalist demographic policies (Kligman 1998). These polices did not affect me, but they did Romanians of reproductive age, especially women. I had to reconsider my assumptions not only about young women's knowledge of their developing bodies, but also about unmarried and married women's experiences of unwanted pregnancies that transformed their own bodies into sources of self-betrayal. As I became better known and increasingly trusted, being a foreigner whose mother was an obstetrician-gynecologist made it acceptable for people to seek advice regarding 'female matters' despite my unmarried status.[21] When the youngest daughter in the family experienced her first menstrual period, Aunt Juji asked to speak privately with me, having first consulted with her husband. In a shortage economy

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[20] Kligman 1988: 134-35. In another village, as an educated foreign woman, I was asked what 1 thought about a husband who had beaten his wife soundly. My culturally inappropriate response was that she should leave him. For the aggrieved, the problem was not her husband's violence, but rather that her father-in-law had held her, meaning she could not defend herself.

[21] My mother's occupation and my status served as a foil against which questions about my own intimate experiences could be avoided. Surely, I was aware of contraceptives and how to obtain them, etc. These assumptions about me provided an important entree into issues of the body, intimacy, and reproduction (Kligman 1998).

and especially in the village, cotton was hard to come by.[22] What, she discreetly inquired, did we use? I showed her a tampon to be sure she understood that these had to be inserted. I could imagine that this might pose misinformed concerns about a possible effect on her daughter's virginity, and while I was happy to let her daughter try one, I thought Aunt Juji should first discuss its use with her husband. She did and, certainly to my surprise, asked me to accompany their daughter to the outhouse to help her through this first ordeal. I could hardly refuse. My own mother did not assist me when I 'became a woman'; the ensuing shared experience of embodied intimacy created a bond between us of 'younger and older sisters'.

The deliberate unavailability of contraceptives in Romania, part of the arsenal of pronatalist measures, was of a very different order than the unavailability of feminine hygiene products. Sexual activity among both married and unmarried couples had become rife with anxiety lest an unwanted pregnancy result. A married couple was among many who spoke confidentially with me, desperate to avoid another pregnancy. They already had two children, lived in a small apartment, and were barely making ends meet despite each being employed full-time. The wife feared that if she rejected her husband's sexual desires, which she also felt was not right to do, he would seek satisfaction elsewhere, which she could not bear. They sought my assistance, hoping I could somehow procure contraceptives for them. Regretfully, there was no possibility that I could do so in any effective way.

Gender and being an American were inescapable everyday categories that shaped my public persona and were the source of ongoing adaptation on my part. Religion was another, not as evident, yet in many respects, much more problematic, to which I now turn.

Religion and a Strategic Deception

When I announced that I was planning to do a postdoctoral project in Maramure$, my beloved maternal grandfather was appalled. How could I, after what had happened to the Jews there? While he could never accept my choice, doing research and living there provided me with a more palpable understanding of the terrible horrors of the Holocaust in a way that exposure to the scholarly and autobiographical literature, documentary and

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[22] Cotton was preferable to rags, which many used. Sanitary pads were unavailable.

feature films, oral historical testimonies and museum displays could not.[23]
But I did not go to Maramure to study Jewish history—Romania's or my
own. To the extent that religion plays a role in my life, I am culturally
Jewish and secular. Yet in this northern corner of Romania, religion was
ever present, communist regime notwithstanding. Religious affiliation was
an identity marker. Ieudeni were either Orthodox Christians or Uniates.
The ongoing religious tensions between them festered and, from time to
time, erupted into physical violence.

   In view of the local salience of religion, Ieudeni wanted to know
about my religion. In that religion is not of primary importance in my own
life, l had not anticipated that it would come to play a role in my fieldwork.
But indeed, it did, and my religious identity became the source of a per-
sonal 'hidden history', one that only surfaced after the regime's demise in
1989.

   As mentioned, I was first located in Glod, where I initially encoun-
tered the significance of religion in daily life. One day, I was summoned
to the house of regionally famous folk musicians originally from this small
village. Word had reached them of 'the American' living there. I was ex-
cited to meet them. They came directly to the point, saying that staying in
Glod was not in my best interest. I came to later understand more fully that
their reasons were sound. Glod had become a village with numerous
pocaifi, a pejorative reference to neo-Protestants, upon whom neither the
regime nor many Romanians looked kindly.[24] Someone could eventually
accuse me, an American whose country was associated with promoting
such 'sects', of proselytizing and surreptitiously distributing Bibles. I was
privately amused by the thought but appreciated their counsel.

   Not long after I moved to Ieud, as villagers recounted their local
history, the deportation of its Jews was mentioned, along with accusations
I had heard in Glod too - that the Jews had killed Christ, that the Jews had
brought communism. While Ieudeni had admired 'their' Jews for their re-
ligious behavior and taken pity upon them as they were forcibly removed
from their midst, decades later a latent antisemitism remained just below

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[23] At the Elie Wiesel Memorial House in Sighet, a wall with a floor to ceiling list of the
Jewish population in the villages of Maramure before and after WWII starkly illustrates
the ravages of history.

[24] Other than the banned Greek Catholic church, religion was tolerated although neo-
Protestants and Roman Catholics were nevertheless persecuted. See Kligman and
Verdery 2011: 222-223.

the surface .[25] As mentioned, questions about my own religious affiliation repeatedly came up, to which I would fumble a response that I was not a practicing religious person, and then try to avoid further attention to my beliefs.[26] I nevertheless needed to figure out what my public persona and response would be to this inevitabl e question. After considerable consultation and deliberation, especially with an older Jewish colleague in Bucharest whom I greatly admired, I decided it would be best to conceal my Jewish heritage, a decision that was predicated on a 'strategic lie' .[27] He had pointedly observed that I had to decide whether I wanted to conduct my research and understand the human condition better or be a martyr . The ethical irony of my conscious deception while in a state that fostered duplicity was hardly lost on me (Kligman 1988, 1998).

Emphasizing that I was not actively religious seemed to have quelled the curiosity about my religious life. The only person who somehow 'knew ' or intuited that I was Jewish was one of leud's Gypsies.[28] Once, rather inebriated, he stopped me on the path and announced that after they took away the Jews, they [the Gypsies] had to bear the brunt of d i scrimination on thei r own. He then walked off and never again said another word.

Hidden histories (such as mine) have a way of eventua lly coming to light although how that occurred was not something I could have anticipated. I had addressed this deceptive aspect of my public persona or village identity in *The Wedding of the Dead* published in 1988. A year later, the Ceauescu regime collapsed. In 1998, *Nunta mortului,* the translation of that book, was publ ished, in consequence of which my 'secret' of two decades was revealed in print. I was mortified to think what those who had

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[25] See Kligman 1988, 365, footnote 31.

[26] In Ieud , asking about rel igious affiliation situated individuals along an historical conti nuum. A joke captured the latent antisem i tism: 'Who did you meet on the road, Joa?' 'Just a man and a Jew' (quoted in Kligman 1988:333, footnote 42).

[27] On deception in fieldwork, see Wolf, 1996:I I - 13, for example. My decision was not unproblematic. I had no idea about the doctrinal differences and historical grievances between Orthodox and Uniate Christians, which daily i nfused the underlying rel igious tensions in Ieud (Kligman 2011). Nor did I know that how one crossed oneself signaled the faith to which one adhered. I also had to learn the formulaic greeting and response then practiced in Ieud as peop le passed each other on the path or entered a house (e.g. We praise the Lord, Jesus; Forever, Amen). When Jews lived in Ieud, th is greeting was a publi c marker of their difference. By the late 1970s, few elsewhere in Romania exchanged this greeting.

[28] Very few were aware of the term 'Roma' or self-identified as such.

become so very dear to me would say when next we met. My heightened anxiety proved largely self-flagellation. After so much time, the responses to this revelation were quite muted, shrugged off with comments such as 'It's not your fault you were born Jewish'. Yet one discussion assuaged my guilt about having lied in the same ironic way that the insinuation that someone could perhaps accuse me of proselytizing had. Uncle Ştefan's brother, himself a Uniate priest who 'came out' after 1989, understandably wanted to speak with me.[29]

I was prepared for a stern lecture. What ensued was anything but. His mother was Aunt Juji's mother-in-law who had been bedridden when I arrived to live with them. She had stopped eating, a cause for great concern in the family. One day, I asked if I might try to spoon feed her. It seemed unlikely that a woman so culturally respected would refuse this commensal gesture offered by a foreign guest in the house. She did not. Her son's comment to me so many years later was that while he regretted I had felt compelled to hide my religious background from them, Jewish or not, in his eyes, I was a saint. I was deeply appreciative; his words helped lay to rest my lingering anxiety about having been deceptive.

Today, questions about my religious preference have subsided and anti-Semitic comments have largely disappeared. Few people there refer to the Jews using the more colloquial, pejorative term *(jfzi)*. Someone living near the Jewish cemetery on the outskirts of Ieud is paid to keep it up and lead visitors to it. That said, there are no Jews living in Ieud and almost all of the people with any living experience of Ieud's Jewish population are themselves deceased. Not surprisingly, in Ieud, religious tensions continue to center on the struggles among and between the Orthodox and Greek Catholics, not on Jews.

A Summary Reflection

Since all researchers must negotiate the social construction of their identities when conducting ethnographic fieldwork, what then, if anything, distinguished my experience of the identity categories discussed in this short reflection? After all, where one is from, and with what gender or religion one identifies are general categories of practice. Fieldwork in Romania in

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[29] Many people-local intellectuals and peasants alike - read the translated volume. One of the greatest honors bestowed upon me, following upon the published translation, was being made an honorary citizen of Ieud in 2002.

the 1970s and 1980s, however, heightened certain aspects of them. The geopolitical backdrop of the Cold War was always 'there', and shaped research throughout the years of an increasingly repressive regime. Being an American colored both officials' and locals' views of me, especially since I was doing research near the Soviet border. The eyes and ears of the secret police circumscribed what I could discuss; prior to my arrival, villagers were instructed not to speak with me about the collectivization period, for example, of which I became more explicitly aware just before I left the field that first time (Kligman 1988, 324, fn 40, 2017). As I gradually established my trustworthiness, I was introduced to matters that were not discussed openly or, if they were, were communicated through ritualized poetry. In these ways, the official silencing of history was subverted (Kligman 1988; Kligman and Verdery 2011).[30]

Being an American was also a privilege, which, for me, meant an ethical obligation to 'do no harm', as best I could. Unlike Romanians, I could leave the country with little more consequence than being denied reentry. But such ethical concerns came with research constraints, not being able to probe about particular issues, or not having access to certain individuals or data, compromising the depth and breadth of analysis (Kligman 1988). Today, to conduct research in Romania, bilateral agreements are unnecessary. Access to data, as anywhere, can be problematic but the reasons are no longer primarily determined by the state (although access to various state archives is).

In Ceauescu's Romania, the pronatalist policies remained ever present in daily life and served for me as an important backdrop to understanding the challenges Romanians were forced to confront. Living with a family exposed me to the regime's penetration into the intimacies of people's emotional, sexual, and reproductive lives. Pronatalism in a shortage economy deprived women of hygienic products and safe abortions, couples of contraceptives, and families of being able to adequately feed the children the regime demanded. Gender equality brought women into the labor force but did little to reduce their household responsibilities, resulting in women's infamous double and triple burdens.

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[3] Collectivization was one such hidden history to which I eventually became privy prior to1 989 through peasant poetry. I had no way of knowing then that that history would inform a major research project that Katherine Verdery and I co-directed (Kligman and Verdery 201 1).

For me, being a woman and single was something about which I was more constantly aware in my everyday interaction s in the village than in my official interactions. Still, I was unable to rectify my brother 's visa problem when he got to Maramure because I refused a sexual exchange as a condition.[31] The Securitate often used such encounters for instrumental purposes, which also acted as a brake against pursuing more intimate relationships with Romanians.[32] With the passage of time (and aging), gender-related issues in relationship to me have receded.

Religion, however, was the identity marker I had least expected to become so prevalent in my fieldwork. It was rarely of importance when I was in Bucharest or among urban intellectuals. But in Maramure , and in leud specificall y, the religious differences between the Orthodox and Uniates were relived on a daily basis. My research on life cycle rituals also entailed religious practices: children were baptized in their family's faith, couples married in church (and were not considered really married until then), and the deceased were issued the last rites and buried by an Orthodox or Uniate priest. Religion was th us a much more salient category there and in my research than in my experiences in Romania's cities.[33]

Today the dominance of the Orthodox Church is manifest in the construction of the Romanian People's Salvation Cathedral *(Catedrala Mantuirii Neamului)* in Bucharest. Churches of every denomination have been built in urban and rural environments, some competing with others to demonstrate their importance through the size of their creations. The Orthodox Church has vociferously supported the recriminali zation of abortion and denounced the decrimina lization of homosexuality demanded by the European Union. Jewish tourists from Israel and elsewhere have begun to visit the remnants of Romania's Jewish past, and Romanians, not necessarily Jewish, have instituted Jewish Studies departments and research units. Religion is no longer actively suppressed. While antisemitism without Jews is a known phenomenon, it is not currently prevalent in Ieud.

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[31] This happened in Bucharest also. The price of access to data was not worth it.

[32] Romanian women were often subjected to such sexual conditions at work, the university, etc. The Securitate often cultivated potentia l informers by threaten ing to reveal sexual indiscretions.

[33] It was also salient in my research on reproductive politics.

What the future may hold as antisemitism rears its ugly head in parts of eastern Europe is another matter.[34]

When I began my fieldwork in Ieud, I arrived a complete stranger, an outsider. Yet over the decades, I have, for many, become more than an 'outsider within'.[35] Ongoing involvement with Ieudeni has broadened my situated identity across generations.[36] The Romania I encountered during the communist period is quite different from Romania today. Romanians are no longer confined to their country's territorial boundaries. Colleagues and academic friends can travel, and we have met here and elsewhere to work jointly on projects or extend scholarly networks or deepen friendships. And since Romania entered my life in the unplanned way that it did, Romanians have unexpectedly populated and enriched my life in the U.S. as well. Why Romania? Perhaps it is best understood as a fortuitous accident of the geopolitics of the time. The rest is a long story.

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[34] On August 3rd, Elie Wiese's childhood home in Sighetu Marmatiei was desecrated with anti-Semitic graffiti (Gillet 2018), which seems an isolated act. On June 20, 2018, Romania's Chamber of Deputies passed anti-Semitic legislation, the enforcement of which will be telling.

[35] Patricia Hill Collins' classic discussion (1986) has inspired considerable work on and elaboration of this term, especially among feminists and ethnographers. Etic/emic perspectives have long been explored in ethnographic analyses. The Romanian word for stranger, *strain,* has multiple meanings in village culture (e.g. being a foreigner, a stranger, living among strangers, etc. See Kligman 1988, 2017).

[36] In Ieud in August 2017 I ran into someone Ihad not seen in decades who has lived in Italy since the 1990s. Her father, a prominent musician, had been enormously helpful to me during my first fieldwork and Iwas close to the family. Much to my surprise, her daughter keeps in her cell phone a picture from the late 1970s of her grandparents and me!

© Gail Kligman. Aunt Juji (left) and her friend Aunt Gasie.

© Gail Kligman. Carved wooden gate and smaller door entrance,
leud, 1978.

© Gail Kligman. Display of an unmarried woman's woven and embroidery skills
which became part of her dowry.

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Tattoos and Ankle Bracelets:

Recalling Fieldwork in Romania

Steven Sampson

In the fall of 1973, having completed my first semester of graduate school at the University of Massachusetts-Amherst, I was invited by Professor John W. Cole to join a research group of graduate students who would conduct ethnographic research in Romania. At the time, UMASS was one of the few American anthropology program s focusing on Europe, an area with little attention or status within socio-cultural anthropology. UMASS had an ongoing European Field Study Program, where a professor and grad students could conduct pre-dissertation field research in Europe. My own interest was in socialist Eastern Europe, especially Yugoslavia, which at the time was relatively open to anthropologists. However, the chance to go to Romania on fieldwork, together with John and five other UMASS grad students for a pre-dissertation semester (with a 1500 dollar stipend) was a fantastic opportunity. John had studied under Eric Wolf at Michigan. John's research had been in the Italian Alps/South Tyrol on issues of ecology, ethnicity, household inheritance and capitalist modernization (see Cole and Wolf 1974). Eric Wolf, a generation earlier, had participated in Julian Steward's 'Peoples of Puerto Rico' project. John was inspired by the Puerto Rico project, and he envisioned our research in Romania as a continuation of his own interests in ecology, economy, ethnicity and modernization that he had studied in the South Tyrol. Our Romania project would examine these variables in a setting of socialist modernization.

Some months prior, in the summer of 1973 John, David Kideckel and Sam Beck had carried out a reconnaissance trip to Romania. John, David and Sam met Romanian ethnologists in Bucharest and Cluj, and they traveled around the country. In consultation with Romanian colleagues, they decided that we would all be placed in the Braov region of southeastern Transylvania, known as Tara B'irsei/Burzenland. The historical comparative basis between the South Tyrol and Southern Transylvania was striking. The South Tyrol was a multiethnic area where Italians and Germans lived in villages and had migrated to market towns, pursuing both

agriculture and wage labor. The South Tyrol had great variations in altitude, making for diverse farming/herding adaptations, differences in inheritance patterns and variations in market access. Until World War I, the South Tyrol had formed the *southwestern* frontier of the Austro-Hungarian empire, after which it was incorporated into the new Italian nation-state. By comparison, southern Transylvania, and especially Tara B'lrsei, had been the *southeastern* tip of the Austro-Hungarian empire until being incorporated into an expanded Romanian state after World War I. Tara B'irsei was also multiethnic: Romanians, ethnic Germans (Saxons), Hungarians, and Gypsies (Tsigani/Rromi) all lived there. Both the South Tyrol and Southern Transylvania had highland and lowland settlements with different relations to agricultural production and to markets. Braov (population 200,000 in the 1970s) was a major industrial and trade center as well providing jobs for commuting peasant-workers. In Romania, variables of class, ethnicity, modernization, and state formation could be investigated and compared with the culture and history of the South Tyrol. Our project became one of studying explicitly socialist modernization in an area that had been all but closed to Western anthropologists, or where the explicitly socialist component was not at the center of their research. [1] So in early 1974, John and his family, and five grad students ended up settling in five villages for six months, until August 1974. A year later, all five students returned to research our dissertations, thanks to Fulbright and IREX grants. I settled in Feldioara, a large village 22 km north of Braov.

With this project began my interest in Romania, which now, 45 years later, has remained. In the remainder of this article I will focus on the context around and my initial field experiences in Feldioara (Marienburg/Foldvar) (pronounced fel-dee-WAH-ra).

That First Project

Every scholar I know – and not just anthropologists - is marked by their first major research project. This mark is much like a tattoo, in that it goes deep into our skin, marks our identity, generally lasts a lifetime in terms of emotions and memory, and like a tattoo, can be removed only with much pain. This research tattoo has a special significance for those of us who are

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[1] Besides the UMASS group, there were other anthropologists working in Romania in the early and mid-1970s, among them Katherine Verdery, Gail Kligman, Joel Marrant, Mitchell Ratner, and Claude Karnoouh.

anthropologists. Un like other scientists, our research topic is not tied to a statistical data or questionnaire, nor to a laboratory, nor to a set of one-off interviews. We do our fieldwork in strange places or in closed milieu s where we normally have not lived or entered. We settle in and try to live or act like the people we are studying, either in a village, or an organization or some kind of social milieu. Most important is that as ethnographers, our primary research instrum ent is our very selves. This makes our research a 24/7 endeavour. It i s more intense, but at the same time more mundane, since we are 'out there' day and night, even if we are using the evening hours for ourselves, writing up notes, etc. Field research, and especially field research for the dissertation, is a rite of passage par excellence. We have the first culture shocks, intense relationships with certain informants, unfamiliar confrontations with bureaucratic authorities who may be much older than us, confrontations with elites who think they know our topic better than we do, and we often live i n a strange dwelling with a family who may treat us like a child, a financial resource or even a spy. We have the trials and tribul ations of gathering data, analyzing our material , and figuring out our focus. Back at university, we are dealing with thesis advisors, the writing up process, the pred ictable delays and writer's blocks, and 'getting finished' -forget about getting finished on time. The pressures of dissertation fieldwork and writing up are of a unique kind. For most of us, it is the first time that we will write something really long, hundreds of pages, and the first, and perhaps only time that it wi ll be closely assessed by a group of professors at our own institution whom we know (as opposed to the blind referee). The dissertation research process is a truly surreal experience, both intellectually and emotionally. I think i t is so intense that it ends up being a tattoo that stays with you for life.

These pressures were also present for me in Romania, where my wife and I spent a total of 18 m onths (6 months in 1974, 12 in 1975-76), mostly in the village of Feldioara. Since the first fieldwork, I returned to Feldioara several times for short periods. After completing my dissertation, I did further research on local vi l lage elites at the Romanian Communist Party Training School, Academ ia tefan Gheorghiu, where I also visited several other villages for short field stays to watch local leaders at work. Between 1985 and 1989 I was blacklisted from entering Romania. From 1990, I return ed to Romania for visits, fieldwork and as a consultant, working inside Romanian government institution s as part of their EU accession process.

This article is based on three sets of recollections: mindnotes, field notes and the written reports of another institution who was intensely interested i n my work: the Romanian secret police (Securitatea). The mindnotes are the selected memories that float in your head, the images, smells, conversations and sensations that an anthropologist has when they have lived among a group of people intimately for a long time. To jog these mindnot es are my own field notes from Feldioara. These notes were written in the pre-computer era, entered in notebooks and typed on Unisort analysis cards. Unisort cards are large 5 x 7 inch cards with holes and small numbers written around all four edges, from 1 throu gh 80. After typing your notes on the cards (I used my Hermes Baby portable typewriter), you punch out the holes in the numbered areas, where each number could denote relevant categories (e.g., kinship, inheritance, history, the collective farm, etc.). The Unisort cards are still in the basement of my home. They remain unscanned.

Besides mindnotes and field notes, the other set of archives about my work are the 600 pages of secret police repo lls about me and my activities gathered by the Romanian security services, Securit atea. This archive ends abruptly in July 1985, when my family and I were detained on entry at the Bucharest airport and expelled as persona non grata. The reason for my expu l si on, I learned, was that I was con sidered to be conducting hostile activi ties against Rornania; these activities apparently included published and unpubli shed papers that I had wri tten about Romania and presentations at international conferences. The papers covered topics such as Romani a's und erground economy, bureaucracy and corruption, Roma n ians' emigration, analysis of rumours, even a paper about the Securitatea itself (Sam pson 1982- 1989, also accessible on my website www.steven-sampsontexts.com). I had also written journ alistic articles, some of which were translated i nto Romanian and broadcast on the Romanian services of the BBC, Voice of Amer ica and Radio Free Europe, and I was interviewed about Romanian developments on several English and Scandinavian language media. My Securitatea dossier includes reports from local people in the v illage, from various other Roman ian friends and acquaintance s, Romanian academics in Bucharest, Romanian scholars whom I met at conferences abroad and Romanians who stayed as gu ests in my home (first in Amherst, Massachusetts and later in Copenh agen, where I now live). It also includes assessments by Romanian embassy personnel in Denmark , where they speculate who I really am, whom I have met, whether I could be useful in providing them with information about Romanian defectors in

Denmark, and whether my public and journalistic activities were hostile to Romania.[2]

Millions of Romanians had these kinds of files, and so did all foreign researchers. Reading the reports by Securitatea officers or informants (most of whom I could identify, despite the use of pseudonyms), I found some that were accurate, others nai've and still others mean-spirited. Nevertheless, they help shape the kind of recollections and memories that I have about my initial field research.

Despite not being able to return to Romania in the late 80s, I followed events in the country. I participated in conferences on Romanian developments held in Europe and in the U.S., and in Denmark we organized a Romania heari ng. Through the 1980s, I was occupied with the issue of whether the regime would collapse. I wrote articles with titles like 'Is Romania the next Poland ' (1983a), 'Muddling through in Romania' (1984a) and 'Romania: House of Cards' (1989), and an unpublished paper on the anthropology of the security apparatus itself.

In 1990, following the overthrow of the Ceauescu regime, I was able to return to Romania, where I covered the elections for a Danish newspaper. By chance, one of my wife's work colleagues was organizing 'democracy visits' to Denmark. I arranged for him to visit Feldioara and a group of local teachers visited Denmark in 1991. Suddenly, my vi llage informants were in our kitchen sitting and talking.

In 1992, I was invited to join a Danish consulting firm to help reorganize the Romanian Ministry of Environment in preparation for EU accession. This led to other work in the Romanian central government, on issues of administrative reform, civil society, social impact of mine closures, public commun ication and NGO legal framework. This Romania consulting work led me to further jobs in other Southeast European countries, in areas such as civil society organizations, project assessment, human rights and democracy export. Between 1992 and 2013 I commuted between my academ ic employment in Copenhagen and Lund and consulting job s in Albania, Kosovo, Bosnia and Romania. I began to understand the ins and outs of what I called 'The Social Life of Projects' (1996) and the mechanics of exporting of Western models to the post-commun ist east. I learned how to wri te feasibility studies, consulting reports and executive summaries in the particular jargon of project management. I negotiated with government officials and Soros Foundation directors, and I learned to

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[2] For more on the anthropologist as spy, see Katherine Verdery 's recent book (2018).

deliver 'outputs' within days, even hours. In many cases, the project work – for Scandinavian NGOs, consultancy firms or government aid offices- gave me further insight into the workings of bureaucracy and project life generally.

The Formal and the Informal

In this commuting between academic research, university teaching and consulting projects, my fundamental interests have been in the workings of bureaucracy, informal systems and corruption. After years in Romania, both pre- and post-1989, I wanted to learn how organizations work and how they do not work. The workings of organizations -efficiency in some cases, muddling through in others, the distortion of goals and the coɪrUp- tion of means – became a kind of an obsession of mine.

My interest in organization, bureaucracy and 'getting things done' is a direct result of my research and field experiences in Romania: watch- ing the local leaders operate in the village, interacting with the Romanian bureaucracy, and in the year after my dissertation, observing training at the local party schools. The initial research has thus left its indelible tattoo. The tattoo is my obsession with the bureaucratic and the informal. The fieldwork left its mark in other ways, of course: I still swear in Romanian, perhaps because these were some of the first words I learned; and if a res- taurant is serving *mamaliga* or ca caval *pane,* I will order these on reflex.

My initial Romanian research thus ignited a kind of flame that burns inside me. The standard name for this kind of professional project is 'research interest'.For me it is a tattoo. But the tattoo of that first fieldwork is also a limitation. Sometimes it acts as an electronic ankle bracelet. Like a criminal under house arrest, if I stray from my immediate surroundings, the alarm rings. The alarm forces you back to your starting point. For me, that first fieldwork had this electronic ankle alarm character. The fieldwork created the tattoo which is the research interest. But sometimes it is also an ankle bracelet. I am a prisoner for life.

Being Special in Romania

I did not have these interests in bureaucracy, informality and corruption when I began graduate school. On departing for Romania, I had originally intended to conduct sociolinguistic fieldwork. The village of Feldioara had a multiethnic, multilingual population where I could investigate language

use and language choice i n particular settings. But as usually happens to anthropologists, immersion into the specific field situation changes one's perspective. The people among whom you have settled end up being pre-occupied with different kinds of issues. The main issue was access to re-sources in an austere, bureaucratic, authoritarian regime. Romanians in vil lages and towns had to live their lives coping with interminable shortage of basic necessities , the political mobilization of the Ceauescu regime, and a bureaucracy that was at once demanding, arbitrary, repressive and inefficient. It was a system that required people to spend their time and energy trying to figure out how to cope, how to influence a local official confronted with vague or contradictory regu1ations, and how to find a way of getting around restrictions. The Romanian word for this is *descurca,* meaning 'maneuver'. For example, vi llagers had official plans requiring them to sell their farm produce to the state at command economy prices. Technically, they could not slaughter their own pig for a wedd ing or sell it privately. This led to strategies of false reporting of animal holdings or payi ng off a veterinarian to report that one's pig was sick and could then be slaughtered. On other occasions, bureaucratic regulations could be dis-obeyed or ignored. A local official explained to me one day that there are some regulations he carried out, and others that he just put into the desk drawer. On other occasions, the system was downright brutal , with sudden campaigns putting Romanians off balance. Romanians thus had to maneu-ver their way through the system . They had to use networks, subterfuge and bribes to obtain even basic necessities such as meat, fuel or medicine ; they had to pay off the local veterinarian to be allowed to slaughter their own pig; they had to know someone to get powdered mi lk for their baby, to get their children into university or to obtain an urban residence permit. As a researcher, I too had to confront the Romanian bureaucracy in order to obtain documents, enter certain meetings and ensure my research ac-cess.

     In this system, we American anthropologists, we were special guests. But we were also at the mercy of the Romanian bureaucratic sys-tem. We were Romanian -speaking researchers roaming around, unaccom-pani ed by an official escort, living outside Bucharest, among the villagers. We were foreigners in a country where it was in fact illegal for a Romanian to even speak to a foreigner without authorization, nor overnight in a Ro-manian home. As foreigners and as foreign researchers, we were subject to intense surveillance: letters to my professors at UMASS, and to my wife in Denmark about paying the electric bi ll, were precisely translated. My

room at the party school was periodically searched, where it was noted that I had a copy of *The Gulag Archipe lago*. In the village, a rumor spread that I had left dollars and a golden pistol behind. As foreigners, however, we also enjoyed certain privileges compared to Romanians. We could legally spend our dollars in the notorious 'dollar shops' and buy luxuries for ourselves, or gifts for others (Kent cigarettes, liquor, Swiss chocolate, electronics, etc.). Packs of Kent cigarettes were the maj or bribe currency in Romania and were especially useful for obtaining minor services or discounts in hotels, trains, or for car repair. As foreigners, we had these small perks. Most importantly, however, we could do what no Romanian could do: pack up and leave for the West without applying for an exit visa. We were freewheeling, unwilling representatives of the West, roaming around i n a nasty communist dictatorship run by a cynical party apparatus and a brutal secret police. We were never physically harassed. Our informants, friends and acquaintances, however, were monitored, interrogated, pressured and blackmailed to write reports about us. Looking back, I can only wonder how naive we really were.

Initial Fieldwork in Feldioara

Prior to entering Romania, John had cultivated a few important personal connections. The American sociologist Daniel Chirot had been to Romania doing historical sociology a couple years earlier. He visited Amherst and told of his experiences. And on the 1973 reconnaissance trip, John met the Romanian ethnologist Romulus Vulcanescu and folk lorist Mihai Pop, as well as sociologists Henri Stahl and Mihail Cernea. In the Fall of 1973, John had settled on how we could organize our UMASS group project. Two of us, Steven Randall and Sam Beck, would be placed in Paltin and Poiana Marului, two v illages high in the mountain s. These were herding villages where there were no collective farms. John and his family and David Kideckel settled in the villages of Mlndra and Hireni, respectively, where there were collective farm households but also industrial work in nearby Fagara. Finally, my colleague Marilyn McArthur and I were placed in the large (pop. 3000) multiethnic village of Feldioara, which had some local industry and commerce and where many residents commuted by train to the industrial town of Braov, 22 km away. John knew of my interest in lingui stics and with his interest in ethnicity, we saw Feldioara as a great choice of field site. The village had a maj ority Romanian population, but also Saxon German, Magyars and Romi/Gypsies. Walk down

the main street of the village, and four languages were being spoken. My colleague Marilyn, a fluent German speaker, would focus exclusively on the Saxon community, most of whom lived in the v illage center. I ended up living with a Romanian family, l iterally down the hill, and started out by investigating peasant life, household structure, the col lective farm and daily life.

The Political / Organizational Context

American anthropologists in the 1970s, l ike all foreign researchers, needed special permissions to enter Romania and do fieldwork. Through the US exchange programs IREX and later Fulbright, we received official desig- nation as visiting foreign researchers. (For further discussion of these aca- demic exchanges during the Cold War, see the recent work of the historian Justine Faure [2019, 2020], includ ing her interview with Katherine Verdery [2011]). For each of the dozen or so Americans who came to Ro- mania each year -usually to study literature, history, or folklore -Roma- nian s could go to the United States, usually to study engineering, hard science, or economic topics. We were part of this diplomatic game, and we knew it. Our presence in Romania was thus approved at the highest levels, and as the Securitate records show, our activities were closely monitored . We needed to be careful about what we did and who we met, and especially what we said to whom; obviously, any sort of poli tical discussions were unwise not only for us, but especially to any Romanians with whom we talked, including those who were uninhibited in criticizing the regime. The close monitoring was also carried out by the U.S. embassy, where we were invited for informal gatherings, and more intimate meetings where we could discuss our work. Embassy staff, including the U.S. ambassador, visited me in the village. I will not speculate on who among embassy staff was working for The Agency. But we had some 'value'. Being far from the centers of politica l intrigue in Bucharest and in the Central Committee, we could tell the embassy people how ordinary Romanians were getting along. For the Romanian authorities, however, gathering and disseminat- ing knowledge of how Romanians coped with everyday tri bulations, the vaunted 'local knowledge', this was the equivalent of espionage. For an American to find out how Romanians really lived, and to reveal it to others abroad, was subversi ve stuff. (For more on anthropologists and espionage especially in Eastern Europe, see Verdery [2018] and a more critical re- view by Le Textier [2019]).

Arrival and Set up

After two weeks in Bucharest, on a cold, day in February 1974, my wife and I, together with Marilyn, arrived in Feldioara. We parked my car in front of the local town hall and walked into the mayor's office. In a few minutes, the mayor had managed to find a letter in his desk that had been circulated some weeks before, announcing our arrival. Standing in the office was a local village man, about 40 and dressed in farm clothes. He was apparently getting some papers signed and stamped by the mayor. As we discussed accommodatio n with the mayor in my halting Romanian, the man offered to accom modate my wife and I in his home. We accepted and settled into the guest room in a modest three-room house down the hill from the center of the village. Boria worked on the collective farm as a tractor driver, and like most villagers, kept a few cattle, pigs and chickens at home. Horia's wife Geta commuted to Braov to work in Factory No. 2 (they made screws). Also living with us was Geta's mother, who was retired and kept house. My wife and I stayed in the 'fine room' of the small house while Boria, Geta and the mother-in-law all slept in the kitchen. We negotiated a rental payment from my stipend, and we took meals by ourselves, purchasing bread, salami, cheese and jam in the local grocery shop or in Braov. After some weeks, however, the accommodations being a bit too intimate, and without a bathroom, we looked for somewhere else to live. Attending a wedding celebration, a young gymnasium pupil mentioned that his aunt and uncle had a nice modem house. We ended up moving into this house, with better access to kitchen and bathroom facilities. Here we remained through the summer of 1974 and again for a year from August 1975 to August 1976. Zinca and Enache were an older couple, and they cared for their two grandchildren while their mother lived in Brasov. An unexpected benefit was that Zinca's brother was head of the local collective farm. Moreover, the local physician, well-connected among the vi l-lage elite, was the godmother of Zinca's grandchild, and a frequent visitor in her home.

In my 'Cultural Ecology' course, which I took at the University of Pennsylvania with Professor Robert Netting, I recall him saying, 'If you don't know what to do, take a census'. Wh ich is what I did during my first months in Feldioara. I also tried to collect data on language use, which in this case meant observing how people switched languages in various pub-l ic forums. However, the language switching was generally confined to the Saxon Germans, who were also fluent Romanian speakers, and to some of

the local Magyars, who could speak both Hungarian and Romanian. It was late January. We had spent only two weeks in Bucharest getting organized, and we were out in the villages, in the snow and mud. By comparison with the bureaucratic experiences of other anthropologists, in other parts of the world, this was an incredibly rapid entry into the field setting. The Romanian bureaucracy either did not know what we were doing, or they were kindly allowing us to do what we want in the hope of using us later. Or both.

My main introduction to the village population came through being invited to some weddings, most of which lasted two days. This public visibility of 'the American guy with the beard', and his blond wife, led to several invitations to families' homes. During the first few months in Feldioara, I concentrated on a general survey of local life. This meant interviewing and hanging around with three generations of people: the elderly who could talk endlessly about household structure, agriculture and the years before collectivization; the ordinary workers and their families, some of whom worked on the collective or state farm but most of whom worked in the factories of Braov; and the younger people in their twenties who also worked but hung out in the local cafe or had parties in their homes or wedding celebrations. Being a large village, Feldioara had a school and even a gymnasium. I became friendly with several of the local teachers and was able to extend my networks and discuss more general research concerns with them.

Finally, I was fortunate to have nearly daily encounters with my UMASS colleague Marilyn, who dwelled in a large house in the center of the village, where the remaining Saxon Germans still dominated. We could meet at her house or in the local cafe and discuss the latest developments and gossip, as well as planning to see archives in Braov or the weekly trip to town for a meal, and meetings with my other UMASS colleagues.

Being an *Etnograf*

For an American walking around the village with blue jeans and an Icelandic sweater, accompanied by a blond Danish wife, driving a German-made car with foreign plates, there was no way that I could be anonymous. The issue, however, was one of impression management: trying to convince the villagers why exactly I was there. Most Romanians knew what an *etnograf was.* An *etnograf was* someone who studied folklore and folk traditions. But Feldioara was a worker village. It had few of the picturesque

tradition s or ritual s found in the more traditional regions of Roman ia such as *Maramure* . The Romanians in Feldioara worked in factories, went to the local bar, tended their gardens, or sat home on Saturday nights watching programs like 'Dallas' or 'Colombo'. In the meanti me, I had to convince local officials that I was a researcher and not some kind of snoop. Why, asked the mayor, did I as an *etnograf want* to see the vi llage household registers (Registru Agricol)? Why would an *etnograf want* to attend the local People's Council Meeting, or meetings of the collective farm, much less the local communist party organization (which brought together most of the local elites). In the end, however, largely because I hung around for month s, I was able to attend nearly all these meetings except those of the local party committee. My connections with most of the professionals in the village (Rom. *intelectuali)* tended to keep me abreast of developments. l became close friends with some of the schoolteachers, and I pl ayed chess with the local dentist. I took lunchtime meal s at the canteen of the local board ing school or the collective farm, where I met visiting officials. Eventually, I settled into the role of being a *sociolog,* a term with which most Roman ians were either vaguely familiar or which seemed to be harmless.

Since anthropologists often want to see documents, and since documents were always suspect in Romania, much of my time was spen t negotiating and waiting for responses to my requests. Marilyn and I managed to obtain access very old church records in Braov, showing family and household structures from centuri es earlier. Itwas exciting to see the same fami ly names pop up in church record s from the 1700s. We also managed to copy village household records from the 1950s and 1960s, when households and their land hold ings were expropriated and collectivized. In Feldioara, this process was even more drastic, as the Saxon Germans after World War II had been sent to labor camps in the U SSR, only to return some years later to find that their land, an imals and even homes had been taken over by Romanians, some local, other immigrants or refugees from other areas. Soon after that, the 'class struggle ' period began. With collectivization of agriculture, the Saxons and Romanians all lost what remaining lands and animals they owned, except for small private plots and gardens.

Research Themes: Migrants and Urbanization

As I hung out in the village, two predominant themes arose which marked my research: the migration into the village from other parts of Romania and its planned urbanization. Feldioara had plenty of industrial work-places. Down the road was a brick factory. Just west was a uranium pro-cessing plant, called 'Factory R', which was in fact quasi-secret. Also nearby was a large pig-rais ing complex with 30,000 pigs. Feldioara had a state farm and machine tractor station, all of which employed skilled work-ers. Finally, several hundred Feldioarans and new migrants commuted to Braov's factories, while Braov professionals commuted to the school, the gymnasium, the state farm, and the medical clinic. The village was integrated into a larger industrial economy. It was a village of wage work-ers and functionaries, with a residual workforce of older women working on the collective farm. Villagers lived in their houses and had gardens and plots on the collective farm. The immigrants lived more modestly, often in rented accommodations or prefab apartment blocks of four stories.

For the foregoing two decades before my arrival , Feld ioara had en-dured an in-migration of people from more underdeveloped areas of north-ern and eastern Romania, primari ly Moldavia. These people settled in Feldioara to work in local industry or in the factories of Braov. Obtaining a residence permit for Braov was difficult, so the Moldavian workers of-ten settled Feldioara and then commuted to Braov by train. Young Mol-davian men and women brought their families, or married each other, or they married local Feldioarans. Some of these migrants were also teachers, functionaries or party officials who had found work in Feldioara instead of more remote v i llages. During my time in Feldioara, the mayor (who was also head of the local party committee) was himself a Moldavian, and the chief village clerk was a Magyar from Braov. The mayor who succeeded him was also an in-migrant from Braov.

The major social schism in Feldioara was not so much an ethnic division between the local Romanians and Saxons. It was between locals and the new arrivals *(localnici i venetici).* And despite having been in the village for decades, these Moldavian immigrants were still considered new arrivals, or even interlopers. This contrast would form a major theme of my Ph.D. thesis (published in revi sed form in 1984, Sampson 1984b).

The second theme which came to mark my research, and which would foln the core of my thesis, was the planned conversion of Feldioara into a small town. As part of a major plan to restructure the Romanian rural

landscape, Romania's 13,000 villages wou ld be reduced and consolidated. Three thousand small hamlets wou ld be elimi nated and their residents moved to larger settlements with infrastructure and services. Three hundred villages had been selected to be developed into small urban centers of approximately 5000 residents. One of these was Feldioara. This national settlement restructuring plan, called systematization *(sistematizare)* was on the minds of the local leaders and the party committee, who looked forward to receiving new resources and infrastructure. The coming urbanization was discussed at People's Council meetings and in the party committee gatherings. It was mentioned in the national press and by Ceauescu in his speeches. Implementing systematization was one of the tasks of regional planning officials in the Braov County administration. The plan to urbanize Feldioara, including the building of two hundred apartment units for incoming workers and extension of industry and services, would commence in 1975. Here was an opportunity, serendipitous as it was, for me to do a different kind of anthropology: an anthropology of socialist planning at the local level. As I ended my first fieldwork in Feldioara in the summ er of 1974, the systematization project came to occupy more of my time and thinking. An anthropology of socialist planning! Why not?

Back home at UMASS, in preparation for the dissertation fieldwork the coming year, I settled on making socialist planning and systematization my thesis topic, with Feldioara 's urbanization as my case study. In August 1975, I returned to Feldioara to conduct fieldwork on systematization. My UMASS colleagues returned with me (now with IREX and Fulbright grants) to the same villages where we had been previously. My colleague Marilyn McArthur returned to Feldioara's Saxon German community, observing the veritable demise of the Saxon German ethnic group as one family after another emigrated to West Germany, encouraged by family members who returned from Germany on vacation.

One might have expected ethnic conflict in this situation of Saxon German emigration, but aside from Roman ians' jealousy about Saxons being able to emigrate, and a history of prosperous German farmers looking down on Roman ians who tended to be poorer or even landless, ethnic relations were rather cordial. On several occasions, Romanians told me how they learned from the Saxons (Feldioara had had an agricultural school decades before). Romanian families sent their children to the German language school classes, hired Saxon German teachers as private tutors or enrolled their children in the German gymnasium in Braov (Romania's current president is himself an ethnic German). Both Romanians and

Saxon Gennans were aware of certain kinds of cultural differences (Saxons had formal associations, Romanians had networks; 'those Saxons eat sweet, we eat sour', Saxons had pri mogeniture, Romanians had partible inheritance). However, these differences remained in the background as Feldioara endured the immigration of strangers from the poor northeastern regions, living in apartments right in the center of the village.

Me and Them versus Them and Each Other

In a critique of Geertzian anthropology, Frederik Barth once wrote that anthropologists should not focus so much on how we relate to the people we study, but on how we relate to each other (1989: 22). Barth's idea was that we should stay in the background and watch the drama of social life unfold. This is what I tried to do in Feldioara, but often my own presence and research priorities affected the situation. With a focus on Romanian socialist planning, I had no research model to find among other ethnographers, in Eastern Europe or elsewhere. I realized that I needed macro data about planning, regional data about the systematization plan, and local data about how the plan was being implemented. I therefore came to the realization that there were some kinds of data I absolutely needed and other kinds of data that would be just nice to have.

The data that I needed concerned everything about Feldioara's conversion into a new town. This involved obtaining the actual urban planning documents, conversations with planners in Braov, and the decisions about what to construct. In socialist Romania in Cold War times, all such documents were lestricted, or even secret. Moreover, as is common in many urban planning schemes, Feldioara's plan also involved the expropriation ofland and dwellin gs for the building of apartments and diversion ofroads. These kinds of measures involved conflicts, and I tried to identify and perhaps track some of these critical cases. In this sense, I was inspired by Gluckman's work on the case method. Getting hold of the actual plans was complicated, since p lanning documents were considered strategic, and besides, Feldioara also had that secret uranium factory. Fortunately, I developed a cordial relationship with the top administrator for Braov County, Stefan Bucur, who allowed me to hang out in the regional planning office. Here I met the architects, engineers and planners who were to implement the systematization scheme for all vi llages in Braov County. For these experts, there was no problem with a researcher trying to study how they applied their experti se. They enjoyed the attention of an American student

watching how they work. In many ways, it was easier working with them than with the suspicious local leaders, who were always looking over their shoulder and who were sensitive to signals from the regional party apparatus about the danger of a foreigner in their midst.

And what of the plan? As in so many cases of planned change, and as was typical for Romania, things did not go according to plan. Some systematization plans in Romania were brutally enacted, especially in Bucharest, where thousands of dwellings and even churches were destroyed to make way for Ceauescu's monumental constructions. But in Braov county, the systematization plan was delayed several times. In Feldioara, some apartment blocks were indeed built during the period of my fieldwork, but the other promised developments to improve village infrastructure never really took place. Not then. Not ever.

On leaving the village in August 1976, the plan to urbanize Feldioara had yet to be implemented. In the years that followed, I returned each year for short stays, watching the urbanization plan stand still. Some apartments were built. A culture house was completed. In the end, I wrote a thesis on the idea of planning and improvisation, and how the discourse of planning and future development was as important as the actual implementation. I ended up demonstrating what so many development experts and local villagers already know: that nothing goes according to plan. But that people still believe in planning.

Tattoos and Ankle Bracelets

My study of the unfi.1lfilled scheme to urbanize Feldioara left its mark on me. My struggles to obtain access to various documents and to attend meetings began to tattoo itself on my entire approach to how I understood life in Feldioara, how I understood Romanian planning, and eventually how I understood East European 'real socialism'. Much of my fieldwork time involved observing how Romanians negotiated an economy of shortage and bureaucratic regulations, as well as my own interactions and confrontations with bureaucratic actors, efforts to obtain access to documents or attend a meeting or make a visit. Just as ordinary Romanians used their energies to obtain access to resources that were important for their lives (food, housing, medical treatment, exit visas), I was also preoccupied with struggles to obtain the resources I needed as a researcher. What emerged from my 18 months of initial fieldwork in Romania was an understanding of how Romanians use informal networks to negotiate their world. This

kind of approach wou ld inform my work from then on, both in my mono-
graph on planni ng and improvisation , in my analysis of system rationality
and irrationality, and in my articles on bureaucracy and corruption, official
and underground economy, communication and rumors, and even on the
not-so-secret police. Hence, a couple years after my thesis, I was writing
about the Danish und erground economy, and most recently I have been
interested in Danes who 'snitch' to the authorities on the neighbours who
cheat on taxes.

     Ifmy fieldwork resulted in some kind of mark that sticks with me
all the time, a tattoo that I could not wash off, it is this und erstandin g of
'the informal' as a strategy, a tactic, even an ethos. The informal in Roma-
nia was known humorou sly as PCR, the i nitials of the Romanian Com-
munist Party, but also *pile, cuno$fiinfe $i relaf ii:* short-cu t, acquaintances
and connections *(pile* means 'file', as i n filing one's way out of jail). The
informal , as I would write about later (Sampson 1986), had several func-
tions. It was a lubrication mechani sm that kept society going, enabling Ro-
mania to muddle through (Sampson 1984a), while other East European
countries were exploding (Poland). However, informal relations also un-
dermined the formal system, making the planned economy even more im-
practical and irrational. Any job in the socialist economy offered resources
for informal plunder, either material or simple social access. The real Ro-
mania had a cumbersome bureau cracy, but the real Roman i a was also the
informal Romania. Informal, non -in stitutional structures enabl ed families
to propser while the collective farms on whi ch they worked remained poor.
Finally, aside from its lubrication and undermining effects, informali sm
was also a regime tactic: the system tolerated sloppiness, petty corruption,
pilferage and embezzlement, allowing people to experience some kind of
control over their lives, and to mom entaril y forget the repressive control
of the Ceau escu regime.

     It was this kind of pervasive informalism that has remained for me
as an ankle chain. The i nformal remained an orientation that I deployed in
researching various topics. And it became an orientation in my daily life.
My first fieldwork gave me a Roman ian orientation to life, such that no
matter what the problem , what the regu lation, I could 'find a solution',
'find someone', 'get a connection '. Some months after returning from
Feldioara to Denmark, for example, I missed a deadline for an application
that I had posted the night before. I did what any Romanian would do: I
walked into the admin istrator's office to complain that the postmark was
valid, but that my application had not been delivered. I wanted special

treatment in a face-to-face manner. But this was Denmark, not Romania . This was a Danish bureaucrat not a Romanian one. My application was late. There was nothing he could do. 'You 'll just have to apply next year', he said. He asked me to leave the office. No relation was started, no bribe solicited, no special favor offered. End of story.

We anthropologists, perhaps because we do so much face-to-face research, have retained our faith in the power of informal solutions. In our ethnographi c description s, we someti mes communicated an optimistic message about how socialism could 'work ' by allowing peopl e to play the system (e.g. Wedel 1986). However, we anthropolog i sts tend to forget that despite all these informal strategies, tactics and networks, socialist Eastern Europe was also full of people standing in line, for hours or days, unable to get what they desperately need ed. In other words, these societies were also full of people whose informal resources were still inadequate to fulfill even basic needs. This lack of basic resources led to true tragedies in Romania: illegal abortions, the sad state of orphanages and street children, the poverty of those in outlying communities, and medical neglect of the elderly. While East Europeans' informal systems were celebrated as models of ingenuity, the East European revolutions were in fact fueled by those who were tired of waiting in line, those million s who did not have the right connections.

In 1979, while living in Denmark and writing up my dissertation , I attended an international sociology congress in Uppsala, Sweden. Here I fell in with a delegation of Romanian sociologists. During those days, I provided some of them with cash, and one of them, a professor at Ştefan Gheorghi u, the Romani an Commu nist Party Academy, ended up staying a few days with me in Copenhagen. I even helped him to purchase a highly prestigious set of matching blue jean s and denim j acket for his son *(costum de blugi).* These favors, and our extensive conversations, helped me to enter the Romanian party academy as a researcher. At the party academy, which contained Rom ania's only remaining sociology department, they translated my d issertation on socialist planning in Feldioara into Romanian (for internal use only, although I managed to procure a copy). I attended seminars on 'management science', which was becoming popu lar within the party training schools. I visited local party schools and villages to interview vi llage leaders about the problem of administering regulations i n a local environment. I was often accompanied by a Romanian sociologist

colleague, who served as a valued intermed iary, an escort, and who re-
ported back to the security organs of my activities. Generally, however, I
was free to travel and conduct the research for these visits.

For many years, despite Romania's economic decli ne and brutal
repression, I naively assumed that anything was possible if you could try
hard enough and could find the right connection. But alas, like other Ro-
manian s, I also ended up running up against a brick wall. On December
15, 1984, a decision was handed down by the Securitatea that I was con-
sidered an 'undesirabl e person ' and would henceforth be denied entry into
the country - for a period of exactly five years, until 31 December 1989.
Some of the sociologists at the party academy had reported that my articles
'd istorted the realities of Romania'. They also reported on discussions we
had had on the Polish workers rebellions taking place at the time. Some of
my papers reached the wrong hands, or were found among other American
grantees. Add itional reports on me were provided by Romanian diplomats
in Denmark, one of whom had grown up not far from Feld ioara, while I
had met other diplomats at meetings of the Danish-Romanian Friendship
Association or in visits to the embassy. With these data about me, Roma-
nia's security organs concluded that I was conductin g activities outside the
country. On July 20, l985, on landing at Otopeni airport with my wife and
two small children, I was curtly i nformed that I was *persona non grata,*
detained in the transit hall, and sent back to Denmark on the plane the fol-
lowing day. Further efforts to obtain a visa by addressing Romanian em-
bassy personnel proved fruitless. Right up until my application in the
spring of 1989, Romanian diplomats in Copenhagen were send ing reports
about my public activities to Bucharest and interrogating friends and ac-
quaintances in Romania , telling them that I was a spy.

Ceau escu was executed on Christmas Day 1989. I returned to Ro-
mania in March 1990, watching the 'transition' unfold . I have revisited
Feldioara several times and have also worked in various consulting tasks
in the new Romanian government. But that is another story. What left its
mark, however, was my experience in watching Romanians negotiate a
chaotic, improvisational, repressive society in which even the smallest
daily tasks were considered a victory over the system. My own fieldwork
experiences of obtaining data and access were also small victories over the
system.

Our anthropologica l faith in the power of the informal, what we
now call 'agency', an agency that triumphs over 'structure', has a way of
staying with us. For me it is the tattoo. But it is also the ankle bracelet of

my initial fieldwork. Living in Denmark, a well-ordered welfare state where there are also moments of chaos and bureaucrati c stonewalling, I often find myself thinking much more like a Romanian than an American or a Dane. I look for avenues and channels. I try to arrange the coveted face-to-face appointment. I do not trust the state institutions. I look for workarounds for regulations. I try to figure a way out, or a way through. *Descurca* remains my favorite Romanian word and to be *descurcare/* is certainly the most eminent trait a person can have.

I have a Romanian suspicion of public authorities. And I have a faith that with the right connections or the right approach, preferably face-to-face, that anything can be done. It is a kind of naivete, I admit. My 'discovery' of the relation between planning and improvi sation, about a plan that never happened, about the power of the informal, these became both the tattoo that marked my work and the ankle bracelet that kept me in line. If I try to escape, the alarm will ring.

© Steven Sampson. Migrant youth hanging out on a Sunday, 1975.

© Steven Sampson. In the local town hall and party headquarters, some days before the May 1st holiday, 1976.

© Steven Sampson. A wedding celebration
in the village center, 1974.

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Ethnographic Fieldwork with Bulgarian Roma, 1970s-2018: Strategies and Challenges

Carol Silverman

Passion for Balkan folk music and dance, the naivete of youth and perseverance were all factors guiding my early fieldwork. How these developed into a research program combining documentation of Roma with advocacy and activism is the subject of this chapter. Exploring fieldwork spanning over forty years in Bulgaria, I begin by recalling my early fieldwork trips, delve into institutional constraints during communism, and chart my focus on Romani music. Finally, I analyze the transnational and participatory turn in my work after the fall of socialism. Throughout I intersperse significant encounters that shaped my work.[1]

Why the Balkans?

I began doing research in the 1970s as an undergraduate in the City College of NY because I was a devotee of Balkan folk music. I became an avid Balkan folk dancer, spending four to five nights at various clubs; my social life revolved around Balkan folklore, learning to sing, and learning Balkan languages through song texts. I first travelled to Bulgaria in 1971 to learn to sing and dance, and then I went to graduate school in Folklore at the University of Pennsylvania to acquire the skills to analyze the materials I was already studying. I was lucky to take my hobby and make it my career.

Festivals, which were frequent and government funded during socialism, served as a place to network with performers. My first Balkan trips in the 1970s included the Bulgarian national festival in Koprivshtitsa as well as the Croatian Smotra Folklora festival in Zagreb and the Macedonian Ilindenski Denovi festival in Bitola. I would seek out singers whom I had heard on recordings, and surprisingly, sometimes they would generously invite me home to their villages. 'I was often the first westerner to

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[1] This contribution builds on Silverman 2000; Silverman 2008; and Silverman 2019.

visit these remote locations. In those early days I had many naive and ro-
mantic concepts about village life-about artistry, harmony and beauty,
but I quickly learned that conflict and politics infused every art form in
every locality' (Silverman 2008: 397).

One of my first lessons in the politics of Bulgarian folklore in-
volved the songs of Pomaks (Bulgarian speaking Muslims). State policy
in the 1970s and 1980s supported mono-ethnism and Bulgarization and
severely regulated the display of Muslim ethnicity. The official
Vizroditelen Protses (regeneration process) dictated that there were no mi-
norities: everyone was Bulgarian. This policy enforced name changes and
the prohibition of religious and cultural observances among the country's
Muslim minorities - Turks, Pomaks, and Roma. The policy was enacted
among the three groups of Muslims at different times and with different
consequences (Neuberger 2004).

During visits to villages such as Sveta Petka and Draginovo in the
western Rodop mountains, I saw firsthand how Pomaks were subject to
mandatory name changes and prohibition against Muslim elements of their
music and costume, as well as political repression. The villagers told me
they had two names: 'their Slavic name for official purposes-and their
Muslim birth name for private life'. They also had to change the names in
their songs when they performed at festivals, so, for example, instead of
singing about Aishe they sung about Maria. Some Pomaks could not recall
their official names because they were assigned to them by administrators
and they used them rarely, only for documents. I realized 1 had to learn
their official names in order to write to these villagers; I regularly sent
photographs and New Year's cards via the postal system, and could en-
danger them by using the wrong name. Pomak women were also prohib-
ited from wearing their traditional aprons; in response, they took off their
aprons for bus trips to the city; they also substituted western pants for their
prohibited wide leg pants *(shalvari).*

I hadn't planned to study the role of the socialist state in Bulgaria,
but the state was everywhere, and was viscerally affecting my Bulgarian
musician friends, so I shifted my focus. I formulated my research to ex-
plore what the state endorsed, and what its censured and why. I analyzed
what became official folklore, for example what was presented at folk fes-
tivals, and what was omitted. My 1980s articles showed how nationalism
and socialism informed forms of state-sponsored culture such as music
festivals, ensembles and schools, as well as collection activities. Soon after
I began studying how excluded groups, especially minorities such as

Pomaks and Roma resisted state categories and developed unofficial forms of expression.

Institutional Constraints in Socialist Bulgaria

My research bridged the disciplines of folklore, ethnography and ethnomusicology, thus I did not fit into the usual mold of a researcher with a singular focus and singular affiliation. As explained by Bulgarian authors (Mihailescu, Iliev & Naumovic 2008), ethnography was considered a historical science whose aim was to study the culture (ethnos) of the Bulgarian people, which was assumed to be identical to the Bulgarian nation/state. The study of culture was based on Marxist principles and thus, it was mandatory to extol the socialist consciousness of folklore. Folk songs were seen as demonstrating the revolutionary spirit of the nation, and partisans' and workers' songs were given special attention. The main focus, however, was on peasants as hard-working people and as the true bearers of traditional culture.

Imagined as industrious and stoic, the nation was pictured as withstanding the onslaught of outside forces, especially 'the Turkish yoke', meaning 500 years of Ottoman 'slavery'. This narrow vision ignored all minorities and all neighboring nations, denied any cultural exchange and posited 'pure Bulgarian folklore' located in rural life. Researchers were focused on the 'classic village', an artificial construction that presumed stasis and isolation. Rural folklore was the defining site of nationalist pride, and it provided the evidence that Bulgaria had resisted Ottoman domination. Many researchers sought to prove that folklore was a means to repel foreign influence, inspiring the fight for independence. 'Foreign' was a gloss for Turkish, thus routinely researchers had to make the absurd claim that there were no traces of Turkish music in Bulgarian folk music; it had remained 'pure' and 'authentic'. Some actually believed this claim; their belief may be somewhat understandable because they were not allowed to study Turkish music. I was constantly surprised at how little some of my colleagues knew about other Balkan cultures; some honestly believed that every aspect of Bulgarian culture was unique to Bulgaria. Others, however, were hungry for comparative materials but could not easily obtain them due to travel prohibitions. Because I was simultaneously travelling to Yugoslavia, I often brought books on comparative Balkan materials and on western ethnographic theory as gifts to scholars.

Ethnography sought to explain the ethnogenesis of peasant customs and way of life *(bit).* Bit was defined as the material life of peasants. The Institute of Ethnography was located in the ethnographic museum, and its researchers often used the superb collections housed there, including costumes, house types, work implements, etc. I was also interested in costume and spent many hours with the exhibits and behind the scenes. The minutiae of peasant life were collected and catalogued; collection and cataloguing were themselves legitimate goals. The study of bit was often combined with the study of spiritual culture (customs) in many volumes labeled *Bit i Kultura.* Materials were organized by geographical region and scholars became specialists in regions; collections, publications and exhibits were organized by region. Ethnicity, on the other hand, was routinely ignored because the ethnic minorities were problematic for the model of the homogeneous nation. Gender was similarly ignored.

The Institute of Folklore, intellectually inspired by literature, was concerned with verbal art, with variation and creativity and with folk artists, whether singers or storytellers. Perhaps because it was relatively new, the Institute of Folklore hosted more lively discussions, and l gravitated to scholars there. I benefited from conversations with Radost Ivanova, for example, and admired her book *Bulgarska Folklorna Svatba* (1984), a thorough ethnography of wedding ritual based on many years of fieldwork; when it was translated into English, I used it in my university classes. But I was disappointed that it did not include anything on contemporary weddings that I was studying (see below). I became friendly with Ivanova's family and later sponsored her son to study at the University of Oregon.

Ethnomusicologists, housed in the Institute of Music, dealt with the sound aspect of music; they too collected and transcribed. Since my husband Mark Levy is an ethnomusicologist, and we often did research together, we had close ties to ethnomusicologists such as Vergiili Atanastov. Coming from the highly interdisciplinary field of folklore in the United States, I thought it was unproductive to divide culture into these three subdisciplines. When studying music at a wedding I had to read folklorists to learn about the song texts, ethnomusicologists to learn about the melodies, and ethnographers to learn about the customs. Of course, these divisions, though upheld in most publications, were not rigidly respected in individual researchers' minds; in truth, many researchers were knowledgeable across the cultural spectrum.

Although I was critical of institutional constraints, I was dazzled by the encyclopedic knowledge of folkloric materials that my colleagues

had at their fingertips. The sheer volume of collected materials was overwhelming; I pored over song collections and costume pieces and was awed by the time and eff0lt collectors had put into classifying texts and objects. In hindsight I am very appreciativ e of the painstaking collection work of my Bulgarian colleagues. Furthermore, I believe that immersion into the materials of folklore and ethnography is necessary before theorizing about them. I have seen too many non-Bul garian ethnographers generalize too glibly about Bulgaria as a whole without a deep knowledge of ethnographic materials.

In spite of my admiration for the depth of knowledge exhibited by my Bulgarian colleagues, I was frustrated because most of them did not fully understand the western :fieldwork methodology of participant observation. Whereas they typically made sh0lt trips to villages and sought out elderly informants from whom they elicited texts and memories of an earlier era via questionnaires and surveys, I wanted to live in a village for a long period oftime and observe and participate in contemporary ritual and mu sical li fe (also see Verdery 2018:17). There were major problems with my goals, namely, we could not obtai n permission to live in a village, especially a village with a sizeable mi nority population or a village too close to the border. These prohibitions were predicated on the assumption that we should not be allowed to roam around unsupervised , especially in multi-ethnic regions; the authoriti es were suspicious that we were spies, troublemakers, or journ alists. This theme has been expertly documented by Katherine Verdery (2018), and I explore it below.

Spy or Folklorist: A 1980 Vignette

From 1979 to 1980 Mark and I were doing fieldwork (he with bagpipe players and I with singers) in the Rodop mountains in the vicinity of the village of Gela. Even getting to this village was a major accomplishment. Even though Mark's approved grant stipulated living in a vi llage, the government would not grant us permi ssi on. The state did not want foreigners wander ing around rural areas where there was less police superv ision - it would be harder to monitor our activities, which were *defacto* suspicious, since we were surely spies. Mark and I waited and waited in Sofia for permission to live in a Rodop mountain village, and when a month went by, and we realized we would never get permission, we compromised by agreeing to live in a hotel in Smolyan, the regional capital. We rarely slept in the hotel, however, and instead often slept in the homes of our friends.

This brings up a second problem, namely that Bulgarians were not supposed to socialize with or offer lodging to people from the West. We would beg and plead with our friends to accept the fact that we could not sleep over, but, due to their Balkan hospitality , they insisted. I remember the horror when a knock was heard at midnight and a police officer entered a home and ordered us to leave. Most of our nightly sojourn s were undetected, but a few friends were fined a month's salary for hosting us, and when we offered to pay the fine, they would not accept our money . I felt unethical and guilty causing these problems.

Thirdly, we were told that fieldworkers had to be accompanied by a Bulgarian researcher - this unwritten 'rul e' was intermittently invoked to d iscourage us from going to isolated rural areas. We were also told that we could not film unless a Bulgarian camera person from the Institute of Music was next to us, filming the same thing at the same time. We were too intimidated to film in the Rodops during Mark's initial grant period, but when we realized how ridiculous this agreement was, and how unsupervised we really were, we ignored the agreement and filmed. Fourthly, endless bureaucracy was required for every move. For example, instead of giving us our stipend for a few months at a time, the Academy of Sciences required us to drive back to Sofia every month; this, of course, was a tactic to disrupt our rural research. We surmounted this rule by living for a few months on one month' s stipend. It also took two months to receive special permission to visit villages in the *granichna zona* (border zone) such as Gela, where Mark's key musician collaborators lived. Even Bulgarian villagers needed permission to visit these villages bordering Greece and Turkey, and inhabitants had a special pass. From the government's point of view, they didn't want to risk the poss ibility of Bulgarians escaping to 'the West' over the mountains. For us, however, the rules made no sense since we did not need to escape to Greece! On the other hand, the Rodop border region is very isolated and is dotted with military installations which we were not supposed to see; fmthermore, many border villages are inhabited by Pomaks who were persecuted rel igiously and culturally by the government.

In hindsight, the most significant challen ges doing fieldwork in socialist Bulgaria were neither the hassles nor the surveillance I endured; rather they were the inconvenienc es and suffering which l caused for my col l aborators: their surveillance, fines, interrogations, and emotional stress. I knew that if I did something wrong, I could always leave but this

option was not possible for Bulgarians. I recal l one encounter when our passports were confiscated and we were questioned by the police for five hours after we took a hike into the border zone without the proper documentation ; I knew that we would not be jailed. But when a friend was warned of dire consequences if she continued to associate with me, I was really worried for her.

The most stark juxtapo sition between what villagers had to do and what they wanted to do occurred in Gela in sprin g 1980. Mark and I had been saying in a *hizha* (mountain hut) for several weeks. Kina the proprietor, a woman from Gela, knew us well from our frequent presence at village events, and knew we had obtained an *otkrit list* (permission to enter the border zone). Yet we arrived late one evening to find the hizha locked with all our possessions inside. We went to Kina's house, saw lights on inside, and knocked, but no one answered. We knew something was wrong, but had to sleep in our car that night despite the chilly mountain weather. The next morning the hizha was mysteriously unlocked and we vacated quickly. Years later, at a saint's day celebration in Gela, Kina embraced us and cried as she confessed that in 1980 she had locked us out of the hizha on the orders of a superior. A directive had been circulated that there were spies i n the vicinity and we, as Americans, were under suspicion. In an emotional apology, she said she knew were innocent, but she 'had no choice'. Her poignant declaration illustrated the conundrums Bulgarians faced in dealing with us. Verdery similarly discusses the dilemmas of Romanians who interacted with her (2018).

Why Roma?

I was planning to write dissertation related to Bulgarian folklore; however, after experiencing the challenges of doing official research and also hearing about others' horror stories e.g., tapes being confiscated and prohibitions against fieldwork in rural areas, I began to consider other options. Fortuitously, I became a volunteer teacher in a Romani alternative school in Philadelphia, and I decided to do my dissertation with Roma in the United States. My research with the two largest Romani groups in the United States, Kalderash and Machwaya, dealt with identity, ethnic boundary maintenance, gender, and the pollution and taboo systems. Having migrated to the Un ited States from various parts of Eastern Europe during the 1880s to the 1920s, many knew very little about their distant relatives in Europe. Among the few tangible things I was able to give to them were

historical information and cassette tapes of East European Romani music, and we had lively discussions about cultural differences.

After immersing myself in American Romani culture, and gaining some fluency in Romani language, I was anxious to pursue Romani fieldwork in Bulgaria. My husband received two grants for his etlmomusico logy dissertation fieldwork in Bulgaria during 1979 to 1980, and [accompanied him. At first I lamented that not having an academic affiliation would hinder my research, but the opposite was true; Mark had the hassle of obtaining official permission to do everything while I, being 'an accompanying spouse', was more free to pursue my research because I often went unnoticed.

My first entry into Bulgarian Romani culture was meeting Sonya, who was fifteen years old in 1979 and was a vocal student at the Music High School in Shiroka Luka, in the Rodops. I too was also a vocal student, attending classes as well as studying music education in relation to state ideology. Strikingly ironic is the fact that Sonia was the only Romani pupil in a music school of hundreds of students. Despite the fact that many Romani children are particularly talented due to continual exposure to live music from infancy, they faced discrimination in the educational system; they were not guided toward the state music schools, and, when they applied, they were often rejected.[2] Sonya passed as a Bulgarian, and she told me not to speak about Roma in front of other students. She was especially interested in my recordings of Balkan Romani music from the 1950s to the 1970s. Because Romani music was banned from state recordings, my tapes were very valuable, and we often listened to music together. When she invited me visit her home to attend her cousin's wedding in Septemvri, Thrace, I was thrilled. This visit began a long association with her family.

Simultaneously, I began to document the racist remarks Bulgarians (even intellectuals) regularly made about Roma, using the term *tsiganin* as an ethnic slur meaning lazy, dirty, and untrustworthy. I was warned by many Bulgarian scholars and friends that I would surely be robbed or even be killed if I ventured into a Romani settlement. In desperation, I concealed my involvement with Roma from them. Evidence from a 1992 survey indicates that levels of prejudice against Roma in Bulgaria exceeded levels

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[2] Bulgarian Romani musician Rumen Shopov also narrated how his talented son faced overt ethnic discrimination even after socialism when applying to a music high school. There are still very few Roma in the state music schools.

of prejudice against Blacks in the American South in the 1950s. More spe-
cifically, 41% of Bulgarians polled (excluding Turks and Pomales) indi-
cated that they would not want to live in the same neighborhood as Roma;
27% indicated that they would prefer not to live in the same country as
Roma (Kanev 1996).

When I arrived for Sonya's cousin's wedding in April 1980, I im-
mediately felt as if I were in another world. The *mahala* (neighborhood),
which was on the outskirts of Septemvri, consisted of run down houses
and shacks, unpaved streets, outdoor sanitation, and a dense population of
approximately 2000 Roma, all of whom seemed to be in the streets and
courtyards. The smells, sounds and textures were vastly different from the
Bulgarian and Pomak villages I had visited, and people were much poorer.
Romani and Turkish music could be heard everywhere, children of all ages
in various stages of undress played outdoors, and body distances were
much closer. Street life was active, noisy, and multi-generational. Sonya's
family greeted us in true Balkan fashion - we were treated as royal guests
and her father slaughtered a lamb for us. Her father was the *baro Rom* (big
man of the neighborhood), someone who was very respected and main-
tained good relations with state officials.

From Sonya and her family I first learned about discrimination in
employment and school, tracking into classes for the disabled, housing
shortages, and regulations about the practice of Islam, including the clos-
ing of mosques, prohibitions against circumcisions, and against wearing
shalvari (Silverman 1989). I also learned about Romani economic adapta-
tions to socialism, especially in the informal economy (Silverman 1986) -
Sonya's relatives, in addition to having statejobs, were involved in the sale
of building materials, clothing, horses, brooms, and foodstuffs at various
times during a decade of socialism. I also learned about gender relations
by seeing the choices women had while growing up, marrying, and having
children. I investigated women's choices in relation to generational, class,
and ethnic differences. I observed the significant role of women in ritual,
dance, and song.

I rarely did formal interviews, as official encounters smacked of
surveillance and alarmed Roma who were trying to avoid state control. At
first I thought my visual documentation would be conspicuous, but spon-
soring families told me that the photographs and videotapes I gave them
were very valuable; they also hired a local cameraman to videotape their
celebrations. They frequently helped me interpret my visuals and we had
many conversations about the 'Romani way of doing things', in the light

of being both Bulgarian and Romani. Discussions often turned on the role of the state in their lives and I realized that the state was a defining feature of my research. Wedding music emerged as the nexus between performance, Roma and state policy; it was very important in the lives of Roma, both as musicians and as active patrons.

Music: State Control and Resistance

I tooled my research to focus on Bulgarian wedding music, a fusion genre influenced by both the west and the east; it combined rock and jazz with Turkish and Romani music and was created mainly by Roma. I did not pick this topic in advance; it demanded my attention because it was the most popular and controversial music in the 1970s and 1980s. As we were travelling around villages, we noticed the craze for wedding music especially among the youth; people would travel hundreds of miles to crash weddings to listen to live performances of the stars; their bootleg recordings cost a month 's salary. I too became a fan, charted the repertoire, met the stars and their families, and documented dozens of weddings. All of this research was unofficial because wedding music was prohibited by the state for being impure, kitsch and containing 'foreign ' elements. In addition to name changes, the state enforced bans against circumcisions, Turkish and Romani language and music, and Muslim costume and custom. But the reaction on the ground was the opposite: the musicians who were jailed became countercultural heroes. I thus studied resistance to the state in musical, artistic, and economic terms.

By the mid-1980s wedding musicians faced a coordinated program of prohibitions, fines, and imprisonment. The police targeted the most famous musicians such as members of Ork. Trakiya, as a warning for other musicians. My conversations with them detailed how they were repeatedly harassed; their cars or license plates were confiscated and they were fined, beaten, and jailed; in prison their heads were shaved and they were forced to do menial work such as breaking rock and digging canals. Ivo Papazov, founder of Trakiya, vividly remembered that legal charges of 'hooliganism' were filed against him because no law existed about Romani music: 'There was no evidence - they had nothing to charge me with! I hadn 't broken a law - there was no law about music I had broken! They charged me with political propaganda, that I didn't respect their laws, that I was spreading propaganda -as if I were a terrorist ' (2012a: 142- 143; Silverman in press)!

Wedding music was prohibited from official state-sponsored channels (schools, ensembles, festivals, restaurants, television), but its popularity soared. Students in the folk music schools, for example, wou ld regularly sneak out on weekends (facing punishments) to play or to listen to the prohibited music at weddings. I made the following journal entry while attending the Shiroka Luka school in October 1985:

> All the students talk about is wedding music. They are infatuated with it, and they test us to see what we know: "Who is the accord ionist with lvo now?" They live for this music but they are not allowed to listen to it or perform it. Playing weddings is strictly prohibited. The administration recently issued uniforms and confiscated all of their "ci vilian"clothing so they can't sneak off and pass unrecogni zed. Some students have no warm clothing now.We met a vocal student from Thrace who does weddings on weekends, but she has to sneak off or take sick leave (Silverman 2012a:144).

In fact, many students told the legendary story of being threatened about wedding music by their music teachers, of ignoring these threats and sneaking out to a wedd ing and seeing these same teachers at the wedding (Silverman 2012a: 145)!

Ivo Papazov explained that because official spheres were closed to him, he concentrated on weddings:

> So we started to play illegally. We played at weddings because these are private and nobody could tell you what to play. People would record us at weddin gs and sell these tapes, and we became very famous.... We wanted to work in restaurants but they wouldn't let us. We still played Romani wedd ings even though they prohibited us from playing Romani music. It is absurd not to play kyucheks at a Romani wedding. So they hound ed us; they wou ldn 't let us play that type of music, but it is impossible to omit this type of music.... And after we were in jail we weren 't allowed to play at festivals. They followed us everywhere so we had to stop playing wedd ings for a wh ile. I didn 't want to be arrested a second time. There were so many wedd ings

that we couldn't play-we bargained for weddings three
years in advance (Silverman 2012a;143)!

But weddings were also infiltrated by the police and wedding spon-
sors were arrested. This drew my attention to analyzing how the private
sphere was deconstructed during social ism. Musicians developed creative
tactics for avoiding detection, for example, at village celebrations, family
members kept watch (often from the roof) for approaching police officers.
One tactic was for musicians to park their cars in private garages during
weddings; another was to run and hide when the police approached, as
Yuri Yunakov described to me in detail (Silverman 2012a: 227-228). If it
was too late to hide, a musician could morph a Romani kyuchek[3] in pro-
gress into a traditional ethnic Bulgarian melody.

I approached the complicated issue of resistance from the ground
up, taking my cue from musicians: Yuri suggested that the bravest re-
sponse would have been to continue playing kyucheks and face the harsh
consequences. But resistance is never simple: musicians, though brave,
were survivors - they did not seek to become heroes for the sake of dissent
or fame - they already had fame and they were not 'activ ists'. They defied
the state because of economic rather than moral imperatives. Music was
their profession, and they made a living by serving their patrons who re-
quested kyucheks. So, they defied prohibition s when they had to, but also
compromised when they had to. For example, Ivo performed sanitized ver-
sions of his Bulgarian music (purged of j azz and rock elements) on state
television and recorded them for *Bal/canton,* the state production company,
saying he had no choice.

Resistance in Bulgaria surfaced even the most official sites. As de-
scribed above, the teachers at the folk music schools lectured their students
about the evils of wedding music but sometimes broke rules to patroni ze
it. Ivo recalled that some of his most ardent fans were police officers, and
he even played at their private events. In 1985 at the bapti sm of Romani
musician Matyo Dobrev's daughter at his home in Straldzha, one of the
guests of honor, who was a local police officer, danced kyucheck with
abandon. These examples helped me think though Herzfeld's point that
cultural intimacy with the state is highly nuanced (1997). Herzfeld himself

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[3] Kyuchek is the most characteristic Romani musical genre in Bulgaria, sometimes em-
ploying Turkish-der ived scales *(makams);* it also refers to the solo dance with torso move-
ments associated with this music (see Silverman 2012a:27-30).

commented on my last example above by pointing out, 'for a brief instant we see the official representatives of state ideology as human beings capable of wincing at the absurdity of what they must nevertheless proclaim ' (2000: 226). He further explained that despite the external formality of states, they can be viewed in social terms as 'intimate apparatuses'. The state embodies 'potentially disreputable but familiar cultural matter' which is 'the very substance of what holds peop l e together.... Some of that substance even includes resistance to the state itself' (Herzfeld 2000: 224). On both sides, the official and the unofficia l, there were cracks in Bulgarian dogma. Police officers arrested musicians but secretly loved kyuchek; wedding musician s not only resisted but also accommodated to the state. In the cracks in official ideology, then, wedd ing music thrived (Silverman 2012: a 145).

Another stri king example of music policy in relation to ethnicity was the banni ng of *zurna* in 1984 from festivals, med ia, and even privat e parties. Zuma is a double reed conical -bore instrument played in pairs (one drones and one plays melody), and has been a Romani niche in the Balkans since the 1300s. The official reason for the ban was the claim that zurna was a foreign, specifically Turkish, instrument; however, varieties of this instrument type are found from India to Spain. In 1980, when I attended a Pomak wedding in Avramovo, Veli ngrad district, where Romani zurna players were hire d despite the ban, family members served as guards, watching from the roof of a house to warn if any officials were approaching. This underscor es how both Roma and their patrons resisted prohibi - tions.

When zurna was prohibited from the 1985 *Pirin Pee* (Pirin Sings) folk festival, in a region where it is the quintessential outdoor instrument, government officials substituted *svirki* (flutes) that are much softer in volume than the zurna. Audiences failed to show up at the stages where dances were performed to svirki, and when they did, they found the dancing uninspired. Despite the ban , Romani zurna players arrived at Pirin Pee and played for dancing in a meadow above the festival. They attracted a large crowd and dancers tipped them generously; people of all ethnicities danced vigorously until the musicians were chased away by the police. Despite prohibitions and having to accept a state job as a gardener for a hospital , zurna player Mancho Kamburov of Razlog managed to perform surreptitiously and even teach his son. These examples show how Roma and their

patrons subverted the socialist system of musical management. This re-
sistance allowed zurna bands to survive until 1989, when prohib itions were
lifted and they emerged as a vital trad ition.

Institutional Dilemmas: 1980s Vignettes

By the mid 1980s Roma did not exist according to Bulgarian state policy,
thus I could not officially study them. They were referred to in official
contexts as *grazhdani s novo-bilgarski proizhod* (citizens with new Bul-
garian ancestry). Most of my colleagues knew this was nonsense but they
had to conform. They might say they didn't exist but then they would wink.
I did all my fieldwork unofficially on tourist visas, and I had to conceal
my research because of the charged nature of the topic. For example, in
1985 I received a small grant from the *Centur za Billgaristika* (Center for
Bulgarian Studies) where I had an official archival project but surrepti-
tiously attended Romani weddings every weekend.

I tried to avoid the Institutes and colleagues, but conflict sometimes
erupted about wedding music and the entire Muslim issue. In this period ,
ethnomusicologists, folklorists and journalists wrote polemical attacks
against wedding music. Some scholars were, in fact the architects and the
enforcers of music regulations. For example, revered ethnomu sicologist
Manoi Todorov was heavily involved in the *kategoriya* (category) system
in which performers were tested to determine their level of expertise and
their mastery of 'pure' Bulgarian m usic; and to dictate their fee scale. Mu-
sicians, however, circu mvented the system by charging the official fee
over the table but requiring more money under the table. Some were fined,
but wedding musicians stubbornly clung to the free market domain.

In 1985 I was invited by Manoi Todorov to attend one of his uni-
versity classes. I knew he was a critic of wedding music and an architect
of state regulation and I wanted to document how he framed these issues
for students. When he said that jazz and Turkish elements were 'ruining
wedding music', Icould not longer sit still. I stood up and countered that
they were creative innovations and that Bulgarian folk music had never
been pure. I felt I had to speak out so that the students could hear an op-
posing view. As a foreigner I had the luxury of being able to criticize and
leave; in contrast, Bulgarian researchers who were critical confronted their
superiors and suffered severe consequences. I also received sharp criticism
from a visiting Bulgarian scholar after a lecture I delivered at University
of California, Los Angeles in 1988 on repression against Muslim Roma

and their cultural resistance to it. Publicly this scholar felt obliged to defend state policy but privately she told me that she agreed with me; moreover, as a member of another minority (Jewish), she said she herself suffered from the repression of the state.

Given its popularity, it was perhaps inevitable that the state initiated a direct hand in trying to shape the direction of wedd ing music. The Stambolovo festivals (1985- 1988), involved both promotion and regulation, and again Manoi Todorov was at the forefront. Scholars like him began laud ing the talent of wedding musician s wh ile they simultaneously crafted policies that dictated what could be played at the festival. Twenty thousand fans attended the first festival in 1985; but no first prize was awarded because, accord ing to Todorov 'no one played pure Bulgarian material '. Kyucheks were prohibited , but even in the Bulgarian musical material judges said there were foreign polluting elements from the west and east. Directly after the competition Todorov held a meeting with band leaders where he lectured them about how they had corrupted Bu lgarian music. But by 1985 even M. Todorov wrote of wedding bands that: 'We would be too hasty to deny them a place in contemporary sociali st life just because of a few weaknesses' (1985:31). After 1989 wedding music was liberated from state control but faced new challenges from competing forms of music such as *chalga* (pop/folk) (Silverman 2012a: 155-163).

Post-1989 Transnational Fieldwork

The most dramatic changes for Roma in Bulgaria during the postsocia l ist period are the decline in the economic situation, the increase in violence against them, the mainstreaming of xenophobi a and the possi bility of migrating (Dancheva 20 18). These alarming trends are accompanied by the rise of a human rights movement and the flowering of Romani musical culture. Although I cannot detail all these topics, I emphasize that they all dramatically made fieldwork more engaged with current politics. In sum, 1 tried to use my professional and privileged position for advocacy and collaboration. 'Wherever I went in the Balkans, I was now considered a link to the West. I was asked to arrange emigration, university education, concert tours, CD production, book publication, and invitati ons to conferences' (Silverman 2000: 209). Simultaneously, the field of anthropol ogy was changing to embrace reflex ivity and analysis of positional ity in terms of epistemology.

Harraway 's concept of 'situated knowledge' argues that knowledge emerges from particular positionalities that are informed by hierarchies. People in dominant positions, shape 'truths' and thereby relegate the experiences of women and minorities to invisibility and inaudibility. This has been the situation of Roma until recently, both in scholarship and in representations in discourse and image. One reason is that few Roma have been allowed to occupy intellectual seats of power; there have certainly been grassroots intellectuals and community leaders but they rarely were permitted to climb the official structures of authority. Now more Roma who are well educated with official degrees and certificates are claiming visibility and audibility. Re-centering scholarship with Romani voices as primary is a necessary paradigm shift. My work thus moved from documentation to collaboration and advocacy.

In the 1970s and 1980s, I was critical of the government in my writing, risking future entrance into Bulgaria; perhaps this was my first activist move (Silverman 1983, 1986, 1989). But at that time I was operating solo – no Romani NGOs existed. Since the fall of socialism, I have critiqued the abhorrent human rights situation of Roma in Bulgaria, and have also advocated for musicians by arranging numerous US tours and albums for them . I helped Bulgarian Romani saxophonist Yuri Yunakov prepare his successful US asylum case and became his booking agent, tour manager, and vocalist for his ensemble. I have also introduced Balkan Romani musicians to American audiences via lectures and performances at camps and festivals. In all of these venues, I am committed to collaboration, specifically ensuring that Romani voices are heard center stage via educational panels and program notes about artists' life histories. Along these lines, I have worked with Voice of Roma a Romani-led NGO in the USA that sponsors music festivals and tours that have a strong educational component.[4] In this work I uphold the tenets of engaged anthropology 'by producing texts, films and exhibits for public consumption, and by actively

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[4] VOR mission is as follows: 'it is the mission of Voice of Roma to promote and present Romani cultural arts and traditions in a way that counters both romanticized and negative "Gypsy" stereotypes, and in so doing, to contribute to the preservation of Romani identity and culture. VOR also works to heighten awareness of human rights issues faced by Roma in today's world, and to support efforts by Roma to (re)build and maintain their communities, improve their lives, and to strengthen the Romani voice both nationally and internationally. Our mission is accomplished through organizing and implementing cultural arts, educational, economic development, and charitable projects for and about Roma' (voiceofroma.com).

engaging *with* people on the ground to make change through research, education, and political action based on dialogue' (Beck and Maida 2015: 1-2) (see Silverman 2019).

As the Romani human rights movement emerged in the 1990s, I struggled to combine activism and scholarship and was alternately accused of neglecting one for the other. Whereas American Romani activist Ian Hancock advised me to forego music and instead concentrate on activism re: human rights abuses, some of my colleagues in academia said I was spending too much time on activism. Many Balkan Roma in New York agreed with Hancock's sentiment, and advised me to forgo a music focus because it promotes stereotypes; they suggested that I focus on middle class educated Roma to counteract the ubiquitou s of 'Gypsies' as poor and as performers of music. I have taken their advice and have published on education, work and gender (2012b) with collaborative projects planned. My newest project centers on music and activism under the direction of RomArchive, a Romani-led digital initiative (romarchive.eu).

Conclusion

My research has gone through several phases. In my early period I considered myself a documenter and analyst, and in no way an activist; there were few published works about Roma to consult and few models of engaged research to follow. I became politicized because ethnicity and religion were politicized in Bulgaria. More specifically, the Romani music I was studying was prohibited and I could no longer ignore state repression. Migration also emerged as a theme in my research. I have worked on migration since the 1970s, first with American Kalderash, then with and Bulgarian and Macedonian Roma in the US. Today I am involved with Balkan Roma in a transnational network including the US and several countries in western Europe. I now approach research from multiple locations with the collaborative guidance of community members who send me to visit kin. In New York City, for example, I am involved in mentoring the second and third generation of Balkan Roma as they enter higher education and the professions.

In hindsight, I realize that a significant strength of socialist ethnographers were their voluminous collections and thorough descriptions. Yet this rich record revealed a gaping hole-the absence of documentation of minorities, ethnicity and gender. Not only did nationalism and totalitarian-

ism stifle open discussion and support political repression, but it also pro-
hi bi ted research on the diverse cultures of the ethnic minorities and their
interplay with m aj ority cultures. I regret the absence of significant work
that could have been done by colleagues during the socialist period to an-
alyze the mosaic that makes up Bulgaria. Today in the institutes and uni-
versities, there is a more healthy attitude toward research on Roma, but a
great deal still needs to be done.

© Carol Silverman. Carol Silverman with Mancho Kamburov (zurna player)
and his family in 1984 in Razlog, Bulgaria, photo by the author.

© Carol Silverman. Romani woman and her son selling black market scarves, 1983, Haskovo, Bulgaria, photo by the author.

© Carol Silverman. Romani wedding 1980, Septemvri, Bulgaria, photo by the author.

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Worth the Wait:

Lessons Learned Getting to the Field

Gerald Creed

I spent 20 months in Bulgaria in 1987-88, most of them in the northwestern village of Zamfirovo. The research I conducted there provided the basis for my understanding of life under late socialism, but in some ways the effort to get to the village, and my early struggles once I got there, also taught me a lot about the socialist system. These insights were certainly expanded and enriched by my subsequent research, but my sometimes traumatic experiences getting to that point likely informed my subsequent conclusions. I came to this realization from a review of my journals. I kept a record of my experiences with near daily entries totaling six volumes. I had never referenced them previously because, like Bronislaw Malinowski (1989), I used them primarily as therapy and intentionally restricted my entries to personal reflections that did not qualify as 'data'. The latter I iecorded separately on note pads and subsequently typed up as 'field notes'. The invitation to write this chapter inspired me to read my journals for the first time, and while I was generally just embarrassed by my anxieties and personal insecurities, I was surprised with how my later conclusions about the nature of socialism mapped onto some of the frustrations I tecorded.

The book that came out of this fieldwork and subsequent follow-up research argued that socialism was a system of conflicting complementarity. With this apparent oxymoron I attempted to capture how total state responsibility and intense integration of all areas of life under state direction forced authorities to accommodate actions and activities that might be ideologically suspect, or even counter to some plans, because they were helpful in some other arena, or furthered some other objective for which the state was also responsible. Over time, the accumulated accommodations, many of which started as grassroots adaptations and responses to state plans, eventually domesticated socialism into a more benign and tolerable system. The book presents evidence of this dynamic from my observations and my communications with village residents, but reviewing

my journals made me realize how much my own early experiences with the socialist state may have planted the seeds of my subsequent conclus10ns.

So, this chapter returns to my early experiences and links them to my subsequent understanding of socialism. Where relevant I have included unedited excerpts from my journal as block quotations with the date of the journa l entry. I did not continue the journal on my many return trips to Zamfirovo in the 1990s. Those experiences mostly confirmed my prior understandings of socialism, but they also revealed new elements and helped me better appreciate the dynamics that had shaped the qualities of late socialism I experienced earlier. I conclude by recounting some of those discoveries.

Preparations

I never should have pursued research in Bulgaria. At least that was the opinion of some specialists who I consulted in the early years of my graduate train ing. It was the early 1980s and no American anthropologist had been allowed to conduct extended fieldwork in the Bulgarian countryside, which is where I wanted to be. In that context, the time and effort required to prepare for dissertation research seemed like a risky investment. A more reasonable path wou ld have been to study Hungary, Poland, Romania or Yugoslavia where American anthropologists had already established a beachhead. Yet, the lack of research in Bulgaria lent my proposal greater significance, and the difficulty of access was actually one of the qualities that attracted me to Bulgaria. For a chi ld of the cold war, it was 'the communist' rather than 'the primitive ' that constituted the quintessential 'other' of my anthropological curiosity. I wanted to understand life under communism, and that grandiose objective required a conventional case, not a gou lash version. To the degree that isolation from the West was a measure of communist orthodoxy, I needed to go to a place that did not welcome Americans. I failed to recognize that I had concocted a classic double bind . Instead, I fortified myself with the counter examples of Roger Whitaker (1979) and Eleanor Smollett (1980), who had recently published research on Bulgarian vil lages. The former was actually a sociologist who did not live i n a village to do his research, and the latter was a Canadian citizen with political pedigrees that I lacked, so I should not have taken that much encouragement from their examples.

My persistence earned me preliminary visits to Bulgaria in 1983 and 1985 to attend the annual summer Slavonic Studies Seminar-a one-month intensive language school organized by the state for foreigners. In retrospect, this experience should also have dampened my commitment to future research. Among the dozen or so Americans who attended were senior scholars whose prior experience working in the country made them skeptical of my plans, and again some advised me to seek a different location. My own perceptions were also d isheartening: regular surveillance of participants was obvious, as was the state's intent to minimize contact and interaction between western participants and Bulgarians (apart from the teacher s and organizers). It seemed unlikely that this state wou ld approve me living a year in a village. I learned of two ethnographers who had been denied permission for thei r projects. Ifall this was not enough to discourage me, I'm pretty sure I contracted TB during the first visit, which later nearly cost me a lung. None of this convinced me to abandon my plans.

The only way for an American graduate student to get into Bulgaria for any type of research at this time was through an existing official exchange program, of which there were two: the Fulbright programs and the International Research and Exchanges Board (IREX) fellowships. The former had exchange agreements with the Bulgarian Ministry of Education and the latter with the Bulgarian Academy of Sciences (BAN). IREX had funded my trips to the Slavonic Studies Seminars and had also provided support for area and language training in the United States. The only two institutions I could find at the time with regular classroom instruction i n Bulgarian were UCLA and Ohio State. I chose Ohio State for both its closer proximity to New York City, where I was a graduate student, and its interdisciplinary strengths in east European area studies. The year there proved formative in multiple ways. In addition to the Bulgarian language I studied the economics, political science, history and literature of Eastern Europe and consol idated an interd isciplinary commitment to area studies that shaped my subsequent career.

After the year at Ohio State I received both Fulbright and IREX fellowships for my fieldwork. I chose the IREX specifically because it connected me with the Academy of Sciences.

The award was contingent upon approval of my research program. I had decided to focus my attention on how communist agricultural pol i-cies affected village household economies. My hypothesis was basically what became known later as the atomization thesis (Kideckel 1993, Rev 1987), but my proposal predated these publications, and I referred to the

process as the 'individualization' of households. Collectivization of agriculture seemed a quintessential element of communist transformation and thus a good window into communist life in general. Not coincidentally, Bulgaria had pursued an extreme variant of collectivization, not only consolidating village holdings into collective farms, but further combining the latter into multi-village agro-industrial complexes. Agriculture was more concentrated there than in any other country in the Soviet orbit (other than the Soviet Union itself). This was another factor in my choice of the country. State sponsored publications presented the agricultural sector as a major success story, which I hoped would make the authorities more receptive to my project.

My focus on changing agrarian regimes was grounded in the precepts of economic and political-economic anthropology in which I had been trained, which foregrounded economic factors as the primary forces shaping life experience and cultural arrangements. I was persuaded by the profusion of anthropological and historical research on households in the early 1980s that the household was the best place to find the economic impacts I sought (e.g. Netting, Wilk and Arnould 1984). My approach to socialism, however, was probably shaped more by political scientific analyses than the limited anthropology of socialism available at the time. I envisioned my work as part of the growing challenge to the persistent totalitarian model of socialism associated with Friedrich and Brzezinski (1956, see also Arendt 1951), but in some ways my expectation of household individualization was exactly what the totalitarian model might predict, and contrary to some of the early anthropology of socialism (Cole 1976). While my attention to the household inverted the top down perspective of the totalitarian model, the local responses I anticipated were compatible with its premises. My eventual conclusions, however, were the opposite of totalitarian expectations and more in line with the alternative models of socialism that I had found attractive when studying political science. These included Skilling's (1970) emphasis on the continuing impact of group conflicts at all levels within communist societies, which he argued required the Party to adapt, and Lowenthal's (1970) attention to the contradictions created by the Party's dual commitment to both utopia and development. Looking back, I can see the seeds of my notion of conflicting complementarity in these efforts to account for the accelerating pace of change across communist polities from the 1960s onward.

My application to the Bulgarian authorities did not specify a particular field site other than the requirement that I live in a village for a year.

My project could be adapted to most rural locations, but my decision not to be more specific was calculated to avoid political sensitivities and to signal receptivity to the advice of local specialists. I subm itted the request and waited. There was no reply for months, which made me anxious, especially as the proposed time of my departure drew nearer. I was elated and relieved when the approval came through , and since I had specified that I needed to live in a vi llage, I was encouraged that I might be allowed to do so. Although, at least one other scholar had gotten similar approval only to be stymied after getting into the country. So, I was only guarded ly optimistic.

Sofia

I arrived in Sofia in early January of 1987. I had been there twice during the summer, but wi nter was a totally different experience. Besides the bitter cold, which struck me as harsh for 'southern' Europe, there was a dull grey quality to the atmosphere.The residents also appeared more subdu ed, inward and stern. Even as I began to adjust, it hit me that the dismal setting was exactly the American stereotype of communism. This realization made me wonder which other elements of that stereotype might also prove to be true. I was housed in the Student City which was a collection of highrise cement dormitories on the outskirts of town . It was perhaps one of the dreariest neighborhoods in the city, but I was at least famil iar with the area having stayed in the same location during my sum mer visits . I also had a roommate who was more than a little bitter about sharing the room he had to himself before I arrived, and which he insisted was supposed to be his alone. Apart from my bed and desk, he refused to free up any space in the room, notably shelf space for books or my portabl e coffee pot. He eventually cleared one small shelf of a bookca se for my use. I was basically living out of a suitcase which I kept under the bed. I was told I'd get my own room in two to three days. That turned out to be six weeks. I learned that the expression 'two to three days' was more like the indetermi nate 'in a whi le' and actually closer to 'it will happen when it happens'.

> I can't believe this, it's been a month almost and I'm still in a little corner living out of a suitcase (3 pairs of pants and 3 shirts is all I've worn). All the winter clothes I brought are going to be worthless because by the time I'm able to unpack it will be spring (6 February 1987). I wake up so

angry about my room and living situation that I can hardly think, much less work; and it's casting a real shadow of doubt over my ability to relocate to a village. Ifit takes this long to find another room in the same block how long will it take to find a village to go to; find a place to live there and get moved? Enough to make the mind spin. Unable to make coffee as all the electrical plugs-that is those that can be reached-are being used by my roommate. What a life. (7 February 1987)

I made regular inquiries with the building administer as to my room assignment, all of which were brushed off with the same d ismissive, 'two to three days', sometimes with an added explanation for the latest delay, sometimes not. What I did note from all these trips is how often my room-mate (a history student in the university) was in her office when I arrived. I hoped he was also complaining about my being in his room, but I figured he was also likely reporting on me and my activities, and that the delay in getting a private room was exactly to allow for close observation. I have no doubt that there was a sh01tage ofrooms (as they were incredibly cheap and there was a waiting list). Occupants rarely left and arriving in the mid-d le of the academic year meant there was not much turn over, but it was also a convenient way to get a preliminary report on me from close quar-ters. This was in fact a common quality for socialism, a negative element often had benefits. This may have been my first experience with conflict-ing complementarity .

l do not know if my roommate was a willing or unwilling accom-p lice, he certainly seemed unhappy about my presence . My assumption of his cooperation was complicated when he insisted I accompany him to a Corecom outside of Sofia. Corecoms were hard currency stores where for-eigners and Bulgarians with legal access to foreign currency could pur-chase western goods not available to others. There were numerous such stores in Sofia at much more convenient locations, but he said they didn't have the brand of cigarettes his girlfriend liked. Besides, he had a car, which was unusual for a Bulgarian student, making the trip relatively easy. He also wanted to exchange currency with me, but since my official sti-pend was provided in Bulgarian leva I had a reasonable excuse to avoid this illegality. At the time I saw these actions as evidence that he probably wasn't reporting on me, but I would later see it as a perfect lesson in the

duplicity that had infected the system, with the person reporting on me also using me in an illicit way for his own benefit.

I had learned to work on multiple angles as well, although with less prowess. While I continued pushing for my own room in the dorm, I also began advocating with academic advisors to get out of Sofia altogether and into a village. Since I had no idea if and when that might happen, I knew I needed to improve my living situation in the city as it had me in a state of near despair. So I pressed on both fronts.

My formal affiliation was with the Institute of Sociology. My interest in the contemporary village economy was more aligned with Bulgarian sociological research than it was with the interests of the Institutes of Ethnography and Folklore, which focused on traditional expressive and material culture. My *otgovornik* (responsible person) was Veska Kouzhouharova, a noted rural sociologist who was also an avid member of the Communist Party; her husband was a renowned folklorist, and I was told an advisor to the Central Committee. Her ideological commitments were evident in her work and advice, but she was extremely knowledgeable on the topic of Bulgarian village life. I was also assisted regularly by Vladimir Vladov, an associate at the Institute who spoke English. He accompanied me on the seemingly endless visits to multiple government offices in order to get my 'internal' passport, which was required of foreigners on extended stays (mostly foreign students from third world socialist countries studying in Bulgaria).

Fieldwork of an anthropological type, with long-term residence and participant observation was not common for any research in Bulgaria at the time, which no doubt made my request more suspect. Luckily many researchers, including those shepherding me, were knowledgeable of Anglophone anthropology and aware of its research methods, so they understood why I wanted to live in a village. But to complicate matters further I had also requested to live with a village family. This was an added difficulty since it was illegal for a foreigner to spend the night with a Bulgarian citizen. These were steep obstacles to overcome and I had been preconditioned to be anxious. After a few weeks in Sofia, I began asking Veska about choosing a village. I was told it was premature as there was a lot for me to learn from relevant specialists in Sofia. I agreed of course, and in retrospect realize the time I spent speaking with scholars in Sofia was very helpful, but I had been primed to interpret such reasonable hesitations as delays. We eventually discussed the village selection and seemed to agree that the south central plain around the city of Plovdiv was a good location

because it was a rich agricultural area noted for vegetable production . It was also not too hard to reach from Sofia, a factor that elimi nated Dobru- dzha, the fertile breadbasket of the country located in the far no1theast. I worried that proximity to Sofia might have ulterior motives, but it did make sense as I had to return monthly to collect my stipend.

I got both encouraging and disconcerting news on February 20th when my supervi sor said that after d iscussing my project with officials at NAPS (the National Agro-Industrial Union, which was the name at the time for the Ministry of Agriculture), they decided that I should go to a village in northwest Bulgaria, somewhere around the county seat of Mihailovgrad. She said the change was due to better living conditions there. This was mysterious since the area around Plovdiv was a rich agri- cultural area and the villages there some of the mo st prosperous in the country, whereas the northwest was poorer and less developed. I was en- couraged that there was discussion of my village location, but anxious that the decision did not seem to prioriti ze the foci of my project, and especially leery that the ju stifi cation did not make sense. Five days later I got word that the selected village was Zamfirovo.

> I found out what village I'll be going to. Apparently, it has been decided since the decision to relocate to Mihilovgrad but didn 't get thru to me.... Anyway, the village is Zamfi- rovo. It 's about 100 km from Sofia and about 20 km from Mihilovgrad. 1977 statistics put it at around 2,300 resi- dents. Bigger than I wanted but the size is not an important point for my methodology. A wheat, grape and livestock raising area with some local 'industries'. It's exciting. Now all I have to do is actually get there and try and convince them that I have to live with a family. (25 February 1987)

The hope that this decision signaled an imminent relocation was short-lived as I didn 't hear anythin g further. Fortunately, two days earlier, I had finally gotten my own dorm room so I was at least more comfortable in Sofia. After the euphoria dissipated a bit, however, I began to wo11'y that my new fortune might be an indication that they expected me to be residing in that room for some time, so I renewed my inquiries about my village relocation. This time I was blind sided by the response: I could not go to the village until I submitted my research program. This was the first I had heard of an official program beyond my original proposal that had been

approved in order for me to get the visa. It seems I had to complete a more
specific program in the institute's format, and more ominously, have it ap-
proved by the Director of the Institute who I had met only briefly and did
not know. I had been in the country for over six weeks asking about my
field site and no one mentioned this requirement. I later recognized this as
a typical pattern of socialism in keeping processes or procedures opaque
so that citizens could never be certain that particular actions would produce
desired results. You completed one step as instructed, only to be told you
needed to do another, or that you had missed an earlier step, and so it con-
tinued. Some have suggested that socialism is best understood as a gigantic
bureaucracy (e.g. Hirszowicz 1980), which I appreciated, but it was a bu-
reaucracy of a particularly indeterminate and opaque character: you don't
know what might be required, or when it might produce results, so you
keep going through the requirements, which may or may not be accepted
or work, but the next step/task could be the charm, so you keep at it.

> Spent the better part of the day and night typing my 'Pro-
> gram'. What a pain. I borrowed my x-roommate 's type-
> writer for this purpose and pecked at it all day (4 March
> 1987). Spent the day at the Institute having my Program I
> had spent so much time perfecting corrected by Veska. I
> was about to cry (to myself of course) at the thought of hav-
> ing to retype it, when a few of the young people volunteered
> to retype it for me. I was so relieved... Anyway this en-
> deavor has taken most of my time the last week. Of course
> it has to be accepted before I can go the village. I didn't find
> this out till a week or so ago when I was pressuring about
> when I could leave and was told after I got my 'program'
> finished, we could start working on it. I worked on it every
> day and thought it was finished, but found out otherwise
> today. Anyway today's changes should be the final ones. (5
> March 1987)

I submitted my revised program and waited. I don't recall, and
didn't record, if/when I got official notice of the Director's approval.

> Leaving the Institute I ran into Veska who was going on
> about something. I ask[ed] her if she talked to the sociolo-
> gists from Mihilovgrad about my work and she said yes cer-
> tainly, followed by a long discourse of which I understood

> nothing except something about it being too cold. I said
> something about having to go anyway & she retorted w/ a
> statement about finishing my Bulgarian lessons. Wow she's
> using my Bulgarian lessons as an excuse. They are only to
> help me while I'm waiting & somehow they've been incor-
> porated as part of my program which must be completed
> before my research. Go figure! Didn't know what to say so
> I said that I had finished. (12 March 1987)

On the 19th I noted in my journal that I would be leaving for Zam-
firovo, but did not specify when. I must have left out 'in a week' because
I eventually left on the 26th. I remember clearly that I asked Veska, as I
had done many times before, when I could go, and instead of another ex-
cuse I got something to the effect of 'whenever you want'. I was dumb-
founded but managed to say I was ready now. She replied nonchalantly
that we needed to reserve a car and driver with BAN and we set a date for
the next week. What had seemed impossible ended as routine. This was a
quality of socialist practice I would later discover and document in Zam-
firovo (1998:184).

As I had grown more convinced that I'd actually get to a village, I
had started pushing my need to live with a family. My project concerned
domestic economies so l figured I would need intimate interaction with a
family. I knew this was not allowed, but neither was living in a village,
and that seemed to be happening.

> I am also concerned about my living arrangements. Vladi-
> mir says they certainly know it if they've reserved the car,
> but they won't tell me ->me to believe that it's not w/ a
> family as I asked for (18 March 1987). I dropped my paper
> off today at the institute and asked Veska directly if she
> knew where I would live - she said no that we'll have to
> wait and find out & then proceeded to tell me how I
> shouldn't worry because it's easy to make contacts in the
> village, just walking on the street. ... whenever I hear 'don't
> worry' I really start worrying. (19 March 1987)

Of course, even with the date set and the car reserved I still found
reason to fret. Vladimir told me on February 22nd that the official respon-
sible for foreign scholars at the Academy of Sciences was upset about not

being informed earlier of my relocation, and I learned on the 23rd that BAN wanted more information about my program and plans. I was gearing up for another delay, but my contacts at the institute apparently handled it as I did not hear anything further and departed for Zamfirovo as planned on March 26th. I left behind a dorm room that had been without hot water for 3 days and with a toilet that had been running nonstop for more than two days (after I reported it). I was happy to get away from all that and excited to finally 'start' my fieldwork.

Zamfirovo

Getting to the village was another lesson in Socialist protocol. I was ac-companied by both Veska and Vladimir, for which I was grateful. After crossing the Balkan Mountains, which were still snow covered on the northern face, we drove past the turnoff for Zamfirovo and continued on to Mihailovgrad. There we first met with the head of the People's Council *(Naroden Svet),* and a cultural worker for the county who was to be my regional contact. She showed us around the town and then treated us to a private meal in the restaurant of the People's Council.

> I should comment on how funny the lunch scene was in
> Mihailovgrad, the day we arrived. We (the five of us) sat
> around a lg square table (banquet size) that should seat
> about 25-30. It was absurd and like some comic caricature
> of the filthy rich setting miles apart at some gigantic table.
> (29 March 1987)

After lunch we set off for Zamfirovo now accompanied by the county cultural attache. We returned to the turnoff we had passed before, but the village was some distance off the main road. Upon arrival in the village we went directly to the Mayor's office, located, as in most villages, on the central square. She was a young and pleasant woman approximately my age, and immediately put me at ease. After introductions and some pleasant chit chat she suggested we go see my accommodations. I was to live with a retired couple, both of whom still worked part time, the wife in the home on a knitting machine, the husband for the village cooperative. Our entourage, now numbering six (seven with the chauffer), could not fit in the car so we set off on foot for my new residence, the car following with my luggage. After meeting my new landlords, who were perhaps as

anxious as I was about meeting them, our Mihailovgrad guide collected their passports and we got back in the car to head to Berkovitsa, a small older town near the foot of the Balkan Mountains that served as the municipal center for its surroundin g villages, which included Zamfirovo. The purpose of the trip was to register me with the local police. It was much smoother than any of my experiences with this routine in Sofia, but the cultural attache from Mihailovgrad handled it all. At this point it hit me just how many people and places had been engaged to make my village residence possible. Ifl had appreciated fully what was required I too would have concluded it was too improbable to attempt, and definitely too much to expect my contacts to do for me.

Getting to the village proved no panacea. A conversation with the Mayor two days after I arrived made it clear that I needed to consult with her about the families I would interview, and a subsequent meeting with the Mayor and the village Party Secretary a few days later produced a list of nine families that had been selected for me to study. I realized my research was to be constrained in new ways I never suspected. Moreover, it became increasingly evident that even the approved households were not going to be easy to access.

> It has taken over a month, but his morning I realized just how hard it is going to be to get any data here. When I asked my land lord this morning about the first family I would interview (he had said that I could start today 2 days ago) he said that everybody was busy now and over the holidays and I would have to wait ti ll after the holiday week-end ...Even the supposed teacher of Bulgarian has disappeared, or is too busy or something. (30 April 1987)

Families were not anxious to be interviewed for good reasons. They did not yet know me and were no doubt suspicious or leery of my motives. They also did not have the free time for multiple and extended interviews, or understandably preferred not to spend their limited spare time that way. They had not volunteered but been chosen and instructed to cooperate. The fact that they managed to avoid doing so frustrated me, but it was perhaps an unrecognized lesson in how villagers worked around state/Party demands or used some official expectations to avoid others. They had a hundred good reasons, many of which were village or Party priorities, for why they were not available at any given time, without ever

having to refuse outright. They could deploy these excuses to authorities with impunity. This was one avenue for domesticating socialism.

I soon learned that my landlord was a longstanding member of the Communist Party, had served a term or two as Mayor and was still a force in village Party activity. This replicated on the village level the dynamic I experienced with my handlers in Sofia and I was coming to appreciate the primacy of the political (i.e. Party) on even very mundane levels. I was always under the observation and guidance of not only Party members, but rather loyal and relatively powerful Party members with significant ideological conviction. While these choices were likely intended to make sure my research remained within politically acceptable parameters, each of these decisions also helped facilitate and widen my research, as the influence each of these individuals had as a result of their Party positions and history turned out to be helpful once I had established positive social relations with them. This also explains why the processes were attenuated, as it takes time to establish significant social relationships. I am sure Veska's (and perhaps her husband 's) advocacy was essential for my getting to a village, and also being able to live with a family. There were indeed a few apartments in the village in a newer building near the village square that housed the pharmacy on the first floor. I learned that it had been planned for me to reside in one of them, but my request to live with a family was conveyed with enough authority to motivate local authorities to find an 'appropriate' place to live (meaning one with adequate accommodations and politically reliable), despite the fact that no one wanted to take me in. I am equally sure that my subsequent ability to work more freely in the village was a product of my relations with my landlord and the other village leaders who began as my monitors. Relations that were restrictive could develop into helpful ones.

Over time the early barriers and rigidity I encountered dissipated, and I was able to complete most of the requisite 'interviews'. In part this just confirms the rationale behind long-term anthropological fieldwork, but it also revealed something about the nature of socialism and its susceptibility to social relations that I would later appreciate as crucial to how villagers domesticated it. During my early weeks in the village my focus on the research steps I had outlined in my proposal/program kept me from recognizing my 'lack of progress' as itself revelatory, but in retrospect I believe these experiences laid the foundation for my later conclusions. In rereading my journal, I was struck by how often I was engaged in social

events and economic activities of various sorts with vi llagers and how, de-
spite all that activity, 1still worried I was not making sufficient progress.
Jn the end, I learned far more from the multitude of casual conversations
and interactions I had with villagers than from the formal interviews by
which I measured my progress. Ironically, I never even transcribed all the
interviews I recorded.

On reflection I believe the process of my integration into the village
milieu was so gradual that I was unaware of my own increasing incorpo-
ration. It started with my landlords and village officials and expanded to
include their networks of relatives and friends. It eventually included my
neighbors and their connections, and some single men around my age who
became drinking budd ies. The connections extended beyond the village,
which provided insight into the rural and urban economic exchanges that
were essential to the economic functioning of the household and the coun-
try. As I have noted elsewhere (1998:26), one event did stand out as a
turning point: when I volunteered to take over as goatherd to relieve a
neighbor whose mother passed away while he was at pasture. As news of
this favor circulated, I found villagers more engaging.

l felt completely at home with my landlords nearly from the start.
Although both had health concerns that sucked me into more discussions
of the socialist healthcare system than my project required, they were fairly
active retirees, with vegetable.gardens and vineyards, a goat and chickens
(but no sheep). The landlady was more infirm than the landlord, so the
usual gendered division of labor was upset, but this also opened up oppor-
tunities for me to assist in an array of activities. The atmosphere of the
household was busy, but generally relaxed, with a regular routine orga-
nized primarily around meals. I was brought into every discussion of fam-
ily and village dramas, in addition to the daily discussions of work. I had
a separate sleeping/working space to which I could retreat when I needed
to work or when my incorporation into social dramas became overwhelm-
ing.

My interest in the articulation of household, village and state econ-
omies justified participation in a variety of work activities, including work
brigades for the collective farm stacking straw bales and picking fruit.
Some of this work occasioned conversations about the limitations of the
socialist economy, its outmoded or aged technology, as well as its overde-
pendence on chemicals (pesticides, herbicides and fertil izers). Some older
workers would volunteer comparisons with pre-socialist practice, usually
with the present on the favorable end for its greater mechanization, but

sometimes with the past winning out for better maintained fields producin g higher yields. I also volunteered regularly to help villagers I knew in their so-called 'personal ' economic activities, this was mostly agricultural work (shepherd i ng, hoeing, pruning or harvesting), but sometimes included other types of work villagers with skills and access to materials wou ld practice on the side. All these experiences helped me appreciate the link between the socialist state sector of the village economy and the personal and household economi es of villagers.

I was also common ly invited to public ritual s. These events, especially weddings and christenings, were especially helpful for understanding household economic activ ity since they required an extensive mobi l ization of resources and labor for the celebration. However, when I think back on my experiences the context that is the most iconic of my fieldwork was the d inner, the same experience that Buchanan (2006:52) describes in her urban fieldwork in Sofia by the Bulgarian expression, 'going as guests'. The reasons for such invitations varied from special occasions such as holidays and birthdays, to just normal sociality. If I assisted anyone with agricultural work or chores, I would always be invited to dinner. Even if there was no reason for celebration, my presence as a guest made it a special occasion for the hosts. With the work of the day done, in the comfort of their own homes, lubricated by the homemade brandy and wine that was the pride of every household, and enjoying a banquet of good food occasioned by a guest, the atmosphere was usually unreserved, energetic and enjoyable. In this context villagers were more expressive about the struggles, challenges and joys of their lives. They complained plenty about various difficulties of village life, but in a mode and atmosphere that conveyed significant contentment and security. The affective experi ence of these events helped me appreciate the domestication of socialism. The same security was not so evident by the 1990s.

Postsocialism

I left Bulgaria in November 1988 exactly one year before the Berlin Wall was permanently breached. I did not return until 1992 so my dissertation was based solely on my experiences under socialism. The analysis included some of the insights about socialism discussed above, but with the erosion of that system I was able to see characteristics that were less evident when it was operating smoothly, and I incorporated those into my monograph on Zamfirovo.

As I noted in that book, the retreat of the socialist state made my work easier in expected ways, but more complicated in unexpected ways. Villagers who had avoided me before seemed more approachable and in a couple of cases even approached me with explanations for why they had hesitated to engage with me earlier. They had been concerned that doing so might bring official suspicion or questions about what they had shared with me. I assumed all villagers with whom I interacted were questioned about my interests, so I certainly understood this. Villagers I had previously spoken with or interviewed were sometimes more vocal about the problems of socialism, and I was able to collect additional information on previously sensitive topics, such as resistance to collectivization. I also felt a bit liberated, as I no longer had to worry that my questions or interests might get me or others in trouble. Ironically after some period of research I realized I really wasn't probing or delving into many areas I didn't investigate earlier, although it was a relief not to have to think about it. If anything, the limited difference in my sense of interpersonal relations between 1988 and 1992, and the few revelations about the past I learned in 1992, gave me more confidence in what I had experienced and learned during the socialist period.

The greatest consequence of the transition for my research was that I felt less constrained in what I could write or publish. The linchpin in my understanding of village socialism was the expansive role of informal activities, and that was the one sensitive topic I had investigated in my earlier research, not because I had planned to, but because it proved to be central to the domestic economies I was studying. While villagers would not report such activities in a formal interview or survey, they did not try too hard to hide them from me in other contexts, sometimes even conducting them in my presence. Their minimal concern with me knowing about these clandestine and often proscribed activities had been the gateway to my appreciation of the socialist system, but I worried about which of these findings I could report without endangering my interlocutors. With the collapse of Communist rule this concern was lifted and my ability to include the wealth of material on informal activity was crucial to being able to support my argument about socialism as a system of conflicting complementarity that underlay the domestication thesis.

Unfortunately, the freedom to write came with a cost. I could write what I wanted but my motives became more suspect. As noted above, socialism promoted the primacy of the political. Rather than being diminished by the collapse, politics became the primary prism through which

the transition was viewed. Certainly, research had been restricted extensively by the Communist Party, the most obvious example being the lack of attention to Roma or the Turkish minority, but within sanctioned topics there were no competing ideologies. Scholars had to work within the same general political framework, which constrained critique but made it possible for me to work with and benefit from the advice and input of an array of scholars. By 1992 that kind of collaboration was impossible. Scholars were assumed to have a political agenda and every project and publication was evaluated as either pro- or anti-communist. For example, almost any criticism of the process of privatization and land restitution was read as support for communism and the Socialist Party.This made it very difficult to work across the new political divide. In some cases, I was pulled in different directions by scholars who had assisted me in the past and assumed that I would be on their side of the new political divide. There was no place for neutrality. Social relations remained paramount , but political divisions could sever them.

The collapse of single party rule also made it more difficult to get support from local leaders or bureaucrats no longer obligated by the state to assist m e and who suspected political motives behind my requests for access or help. Luckily, I was not as dependent on their cooperation, but it was still frustrating. More upsetting were the few cases in which people J had known well changed their perception /reception of me. The most bizarre of these was a village friend who had helped me with my work but decided the timing of my stay in the village and the collapse of the communist government one year later was too coincidental. J must have played a role in what he now saw as a disastrous development. The new political context required me to consider the possible motives behind the new information I was getting. For example, new reports of prior communist resistance from people who were fearful of talking about it in the past, had to be evaluated in a new context that granted significant political and social capital to resistance pedigrees. I was of course sensitive to the political motives behind the information I got during the socialist period, such as the social capital of anti-fascist pedigrees, so I guess I was just disappointed that the transition had perpetuated this politicization rather than reducing it.

The new context allowed me to return to mysteries I had not yet solved from my earliest fieldwork. One of those was the choice of Zamfi - rovo for my research. I gathered a new collection of explanations but no definitive answer. The first obvious factor was the local support for the

Communist Party in both the village and the region. I knew this from the history of the village, but it was much more evident after 1989 as the village and county voted heavily for the Socialist successor to the Communist Party. Most rural areas did, but this area of the country was especially red (the color associated with the Socialist Party). So, it is obvious that authorities thought my chances for meeting dissidents, or even political critics, was less in Zamfirovo than other places. This confirmed my earlier conclusions about the primacy of politics. A second factor I learned about was that a native of Zamfirovo who had risen to significant positions in both the Communist Party and NAPS advocated for the village as my location. I never verified this claim, but it confirmed my own observations about the importance of individuals. A final factor was the involvement of the state security apparatus and their insistence that I should not be in a region with Turks. This made perfect sense as Turks were at the time suffering under a severe and sometime brutal assimilation campaign. There were no Turkish villages or communities in the area around Zamfirovo, but it turns out there was a Turk interned in Zamfirovo for refusing to change his name, and he was quartered just a few houses away from where I lived. This irony helped me appreciate the limits to the totalitarian state and its apparent inability to know everything or coordinate all its objectives.

I never got much insight into why I was allowed to relocate to a village in the first place. But my effort to make sense of it benefited from another quality I learned about socialism, the consequential role of serendipity and timing. My proposal reached Bulgaria in 1986, a year into the era of glasnost and perestroika. Bulgaria was lagging in these reforms and its tardiness had attracted the criticism of Gorbachev. State and Party leaders needed to look more open exactly when I was petitioning for access. The ability of my advocates to prevail and secure approvals may have hinged on this new openness, and their success was then an omen of the bigger changes that were coming. In this explanation my appearance in Zamfirovo was indeed related to the subsequent transition and the timing was not coincidental, just as my former friend suspected.

© Gerald Creed. Celebrating my 30th birthday in the field with my landlords and two neighbor couples who I also considered good friends.

© Gerald Creed. The view from the room where I lived looking down
the cobblestone road that leads to the village center. The cornstalks and wood
stacked beside the road, along with the still green leaves, indicate it is
late summer or early fall.

© Gerald Creed. A banquet for the wedding party.

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Fieldwork as Initiation in Anthropological Knowing. Aspects of Personal Experience [1]

Gheorghifa Geana

Introduction: The Rhetoric of Anthropology in Four Paradigms

A close examination of the title of the present *book -Recalling Fieldwork* -brings forth two meani ngs of the term 'recall ing'. The keyword is obviously 'fieldwork ', yet 'recalling ' proves to be contextually momentous as well: this term is fully operational only if its signification is integrally perceived.

The first meaning of 'recalling' projects an arch of memory into the past in order to revive facts and happenings of considerable consequence – either to the researcher's professional career or to clarify certain aspects of anthropologica l theory.

Less evidently, the second meaning appears to be a reminder, or, more exactly, a call to a certain order, in a time when the connection between anthropology and the field seems to become less important. This distancing from the field occurs in circumstances in which the specu lative[2] rhetoric (i.e. *discourse* about) of anthropology is facilitated in several ways.

Philosophical anthropology is one such way. The genesis of scientific anthropology is theoretically based on the phi losophical ideas of the Enl ightenment (cf. Hon igsheim 1942, Harri s 1968: ch. 2, etc.). Moreover, some outstanding anthropologi sts have particularly been influenced by one or another among the consecrated philosophers: Adolf Bastian by Johann Friedrich Herbart (stages of consciousness), Alfred Kroeber by the neo-Kantian epi stemology, Robert Lowie by Ernst Mach 's empiriocriticism,

[2] The term 'speculative' as used in the present text docs not entail any pejorative connotation. It simply suggests an approach according to which the upholder of ideas at stake has not passed through any laboratory or field experience.

Ruth Benedict by Friedrich Nietzsche's typology of Apollonian and Dionysian. Yet, apart from this influential trend, philosophical anthropology as such has a status of its own. From an anthroposcopic[3] point of view, philosophy itself appears as a set of systematic answers to what Malinowski (1944) called the 'integrative needs' of human beings. Nevertheless, the core of these answers stands for itself as anthropology, namely as philosophical anthropology. The ancient adage 'rvcb0t crsmn6v'/'Know yourself' became the basic principle of philosophical anthropology from Socrates and Plato to the philosophers of the modem epoch. Among the thinkers who have accredited this discipline with their prestige one can mention: Hume (1739), Kant (1798), Scheler (1921 and 1947 (1928]), Cassirer (1944), Groethuysen (1953), Plessner (1975), Fischer (2008), Gehlen (2009 (1983]), Ricoeur (2013) etc.

Another way to approach the human topic with speculative means is *theological anthropology*. Like philosophy, theology -as interpretation of religion, i.e. of the answer to another 'integrative need '– may also be seen as an anthroposcopic system of ideas. In the same Malinowskian view, the 'function' of religion is to guide people toward Salvation, so that its trustfulness depends upon the faith in God. As in the case of philosophy, one can easily discover in theology a general anthropological dimension, as well as some special elements that may be directly labelled overall as 'theological anthropology '. The patrologists identified such elements in the writings of the Fathers of the Christian Church like Origen, Clement of Alexandria, John Chrysostom, Gregory Palamas, Augustine, Gregory of Nyssa, Maximus the Confessor etc. Essentially, in the perspective of theological anthropology, the human being lives in a permanent reference to the double ontological status of the Saviour: as a theandric creature – having both a divine and a human nature –, Christ became the ideal paradigm for any human person (cf. Cortez 2010, Farris & Taliaferro 2015, Scrima 2016 (1952]).

A third way to speak about anthropology in the framework of the speculative rhetoric consists in the invocation of the term within the areas of *literature* and *art criticism.* Particularly, an anthropological mirror is put in the face of Renaissance creations to reveal their humanistic message. Otherwise, critics generally resort to a kind of literary anthropology in order to demonstrate the concretization of the fundamental aspects of human

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[3] I want to say: with a special view on human being.

nature in literature and arts. In this sense, for example, Riedel (1996) sum-marized how Schopenhauer's ideas about the 'world as will and represen-tation' led (at the turn of the XXth century) to a new literary discourse, in which writers like Arthur Schnitzler, Rainer Maria Rilke, Frank Wede-kind, Hugo von Hofmannsthal emphasized the vital -instinctual -compo-nent of the human being. The same author (Riedel 1985) applied his 'anthropological ' view to the personality of Friedrich Schiller. Not less interesting is the tentative to interpret anthropologists' texts as stylistic products imbued with the talent of their authors (Geertz 1988, Rapport 1994).

Besides these three paradigm s of 'speaking' in the name of anthro-pology4, the 'profession' of anthropology receives validation from field-work. In other words, the *practice* of anthropology is possible but only in a single way – and this way or manner is based *onfi eldwork*. This para-d igm of the professional anthropology found its expression even at the an-ecdotical level. As Clammer observed (1984: 63), a genuine mood is spread among the practitioners in the domain, according to which:

> the act of havi ng done fieldwork is a *sine qua non* for ad-mission to full profesional standing and to the recognition by one's peers of the validity of a claim to be an anthropol-ogist. (The first question that is so frequently asked by one anthropologist meeting another who he does not know, is 'where did you do your fieldwork?' -a question incidently institutionali zed in the criteria for membership of at least one major professional body, the Association of Social An-thropologists5).

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[4] Other labels attached to the term 'anthropol ogy' and producin g formulae l ike 'economic anthropology', 'political anthropology', or 'linguistic anthropology' are of a different kind; they do not offer essentialist but phenomenalist perspectives over the human being and, thus, they are available as behavioural branches within the field of the concrete an-thropology.

[5] This rem inds me of a detail from the anthropological symposium 'Culture and Popula-tion Change' that preceded the World Population Conference of Bucharest, in September 1974. Margaret Mead, Sol Tax, and Moni Nag were the most prominent figures of the symposium, but the detail that would return obsessively in my mind was the conversation Professor Vasile Caramclea and I had with Or. Samuel Stanley -an American anthropol-ogist, attendant at the Conference as representative of the Smithsonian Institution of Washi ngton D.C. As the promoter of the new – sociocu ltural – orientation in Romanian

How I Discovered Fieldwork and Participant Observation

These were the circumstances in which my own scientific itinerary began. For me, the three speculative ways of anthropology required different efforts of adaptation. Under the communist rule, the social life of religion was aggressively controlled and officially isolated to restrict its public manifestation s to its strictly specific sphere; words like 'church', 'God', 'Christian/ity' etc. were usually censored, and even Christmas and Easter celebrations took place under the total silence of mass media.[6] Consequently, the theologica l way of anthropology did not count in my confrontations.

In its turn, the literary and artistic mil ieu did not associate a disciplinary meaning with the term 'anthropology', but rather one of an ornamental or expressive kind. Representative for such a category of authors were, for example, Vianu[7] (1934; 1946; 1966), and Dumitrescu-Buulenga8 (1975), or, in the arts, Ghitescu[9] (1979 / 1981).

In contrast with these two spheres of activity (theological and artistic), my confrontation with phi losophical anthropology was definitely real. Still haloed by its etymologic meaning ('love of wisdom'), philos-

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anthropology , Vasile Caramclea insisted upon this change of perspective he had ju st achieved. After listen ing to him , Dr. Stanley addressed the question to us: 'Well, and where do you carry out fieldwork?'. Momenta rily, I did not understand the meaning of the question. As a matter of fact, it was for him then, as it is for us now – in the present context – more than a simple question: it was, namely, the *key prob lem.*

[6] Because of its religious connotation, even the name of *'Mo§· Craciun'* (Romanian idiom for Santa Claus / Father Christmas) was replaced with the secular *'Mo Geri/a'* (Jack Frosty).

[7] Tudor Vianu (1898-1964): Romanian encyclopcdist scholar. He taught at the University of Bucharest as Professor of l iterary criticism, aesthetics, and philosophy of culture. On this line, he approached such topics as: literature as knowledge of the human, the portrait in literature, humanity in the work of Shakespeare, the history of the idea of 'genius', the functions of symbols, the system of values – and many others, interpretable in the perspective of philosophical anthropology.

[8] Zoe Dumitrescu -Buulenga (1920-2006): Professor ofunivcrsal literature at the Uni versity of Bucharest. An extensive erudition allowed her to comment and jud ge intellectual produ cts not only in the sphere of literature, but also in music, art, architecture, and theology. The humanism of Renaissance counted among her preferred topics of interest, as well as the literary and artistic anthropol ogy.

[9] Gheorghe Ghitescu (1915-1978): Professor of artistic anatomy at the Institute of Fine Arts in Bucharest. His concern for understanding stylistic changes in the hi story of art led him to coin the term 'disanatomization ' as an essential trait of modern art. His interest in artistic anatomy can adequately be considered a hypostasis of 'artistic anthropology'.

ophy was highly appreciated withi n the Romanian intellectual milieu. So much the more the affinity toward s the problems of the hum an being encouraged philosophers to adopt the label of 'philosophical anthropology' for some of their work.

My access to anthropology was a turning point in my life. I had studied phi losophy at the University of Bucharest. During my five-year undergraduate stud ies at the Faculty (between 1960 and 1965), we – the students -were indoctrinated with the perspective of becomin g communist 'ideological militants'. Against this trend, I was firmly incl ined towards the ideologically neutral matters of study such as logic and epistemology. An important encouragement in this direction came in my fourth year of study, when a paper of mine about the relationship between 'whole and parts '[10] was awarded 1st prize at the 1964 national conference of the students' scientific circles held in Jassy. This motivated me at the end of my university studies, when my chief aspiration was to work as researcher at the Centre of Logic of the Romanian Academy. Contrary to my expectations, this did not happen. As a frequently used Romanian saying goes: 'It was not meant to be!' (*'N-a Jost sa fie!'*). Exceedingly disappointed, I avoided working as an ideological pawn for the commun i st regime and went to Soveja, my native village in the district of Vrancea. My plan was to return to Bucharest as a student in mathematics and to try again to attain the position oflogician. During two years ofliving there, I worked successively at a forest office and at the local school (as music teacher – one of my hobbies).

After this unforeseeable auto-exile, an equally unexpected phone call from Bucharest changed the course of my life: 1was informed in this way about an avai lable position as researcher at the Romanian Academy, but not in the speciality of logic; the position at stake was for a sociologist at the Centre of Anthropological Research (today 'Francisc I. Rainer' Institute of Anthropological Research). The choice came ind i rectly to me from with in the Faculty of Philosophy, where my case was sympatheti cally commented. My first reaction was to decline the opportunity: 'To tell you the truth – I replied to the unknown voice representing the Anthropology Centre -, my training is not in sociology, but in philosophy! ...' Although the story itself is spectacular, I shall spare the reader all the details. Suffice to say that eventually l accepted the job offer and, after passi ng the legal

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[10] Today th is topic has come to be assimilated into the metaphysical realm of mereology.

examination, I became an assistant researcher at Centre of Anthropologica l Research by the midd le of October 1967.

From the very beginning I set for myself the goal to practice this discipl ine from the altitude of its critical consciousness. No wonder that for the first four years I would wake up in the middle of the night asking myself: 'What am I doing here?'. The question itself did not merely refer to different types of know ledge – philosophical vs. anthropological – it especially referred to different methodologie s. Since then the acute prob-lem became to me the*fi eld,* or, more precisely, the*fieldwork!*

An important aspect that is frequently approached now in anthro-pological discussions and which I was confronted with at that time, is the profile of anthropology. InRomania, a con sistent tradition of anthropol og-ical research had been established since the third decade of the twentieth century, under the leadership of Francisc I. Rainer. A prestigious anato-mist, Professor Rainer covered topics of human biology in the framework of the social monographic research initiated in the inter-war period by the eminent sociologist Dimitrie Gusti. For all that, in Romania, anthropology followed the coordinates proper to the discipline on the continental Eu-rope, where the term 'anthropology' was used mainly with the restrained meaning of 'physical anthropology'.

On other occasions I have described in detail how the institution-alization of cultural anthropology took place in Romania (see Geana 1990; 1999; 2002; 2014b).*Hie et nunc* I want to underline three aspects of inter-est in the present context:

(1) The Tension of Institutionalization. The implementation of cultural anthropology in the organizational scheme of the Centre was a dramatic process. In fact, the challenge came from the inside: Vasile Car-amelea, a sociologist trained at Gusti's school, had been co-opted in the Centre in the mid 1950s to study family and demographic phenomena . In the general research plan these compl ementary aspects were considered marginal. On the contrary, in Caramelea's opinion, such facts of social milieu could be interpreted, independently, as data of social and cultural anthropol ogy. [11] The physical anthropologists of the institution -biologists and physicians -put up a harsh resistance to the new trend. According to him self, Caramelea was told threateningly: 'You are not an anthropologist,

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[11] With his personal humour he used to say: 'llow long should we be garnishing potatoes to the roast?!...'

you are a demographer!'. Nevertheless, on July 1st, 1964, a Section of Social and Cultural Anthropology in the organizational scheme of the Centre has officially been accepted by the Romanian Academy. In retrospect, this process of institutionalization seems to be a genuine illustration of Thomas Kuhn's theory of paradigm shifts in science (see Geana 2014b).

(2) **Fieldwork** as a **(Pre)condition of Professionalization.** My integration into the research body of the Centre was answering an imperative need of consolidating the newly created Section. However, before the act of acceptance, Caramelea found necessary to subject me to a preliminary test: he wanted to verify my availability for fieldwork.

Accordingly, in July and August 1967, based on a convention between the academic Centre of Anthropological Research of Bucharest and the mayoral office of Soveja, I spent forty days in the field. At first, I was guided towards Berevoeti, Caramelea's native village, an old settlement situated within the hilly Arge region. The place was not too adequate for initiating a young philosopher in the secrets and, especially, in the difficulties of the fieldwork. The inhabitants earned their living by practicing agriculture and coal mining. What was I to do? Apparently simple, my task was to approach them at mine, at the end of their eight-hour shift, and interview them individually about their social problems: family, leisure, relations with co-villagers, etc. However, the mining work being very hard, they were not in the mood of talking to me at all. In short, my first encounter with the fieldwork was a considerable stress for me.

For the second part of the testing period I traveled to Campulung Muscel, about 20 km away from Berevoeti. Apart from its historical tradition, this little urban locality (a Romanian illustration of the classical Middletown!) was famous for its field car factory Dacia ARO. This time I had to fill in a questionnaire of no less than seventy points regarding people's adaptability to their work (including the hierarchical relations). I could not say that it has been a pleasure. Nevertheless, my activity at the car factory intersected with that of a team of ergonomists from the National Institute of Hygiene (Bucharest), who were studying the variability of some workers' physiological and psychological parameters in correlation with their work in day/night shifts. We were all lodged in a boarding school and, during our evening discussions, the senior team leader – Professor Iacob Mihaila, who had studied human physiology in Germany – appreciated my work style; in the ergonomists' team there was a young lady who was mainly investigating the same kind of problems as I was, but our methods were dissimilar: while she was discussing with the workers by calling

them into the headquarters of the communist political committee of the company, I chose to work in the big hal l to have the questionnaires filled in by talking with the workers in their work milieu , near their lathes. Professor Mihaila was so impressed with the data I had collected that, one day, he walked into the hall of the factory and watched me from a distance to see how I was talking with the workers. The ergonomists, therefore, encouraged me and later put in a good word for me to Vasi le Caramelea.

Finally, I became assistant researcher at Centre of Anthropological Research, but, as I have already said, my anxiety lasted. I had no theoretical knowledge in the discipline (no courses in cultural anthropology were taught at that time in Romania) and, as far as the concrete research was concerned, I had no idea about the requirements of fieldwork. (I was imagining, for example, that a field interview must be a perfectly fluent dialogue between the researcher and the informant, without any break or hesitation!...). Soon, however, 1 understood what I had to do; in general, there are two conditions to be satisfied for getting a professiona l status in a certain science: to *assimilate the history of the discipline,* and to *get acquainted with the main theories up to date* in that discipline. Consequent ly, in a short time, I assimilated an essential bibliography consisting in writings by Franz Boas, Ruth Benedict, Margaret Mead, A. R. Radcliffe-Brown, B. Malinowski, E. E. Evans-Pritchard, R. Firth, C. Levi-Strau ss, etc. [12]

In the case of anthropology, a supplementary condition of professionalization m ust be added to the two just mentioned: I refer, obviously, to*fie ldwork.* As I have mentioned earlier, this was *myfirst true encounter with anthropology.* Given my previous speculative philosophica l training, I lived through this encounter in shock, but I took it openly as a promising

[12] I found such books in the Centre's library, as well as in the Academy Library, and at the University Central Library. In addition, the Centre's l ibrary hosted a large collection of the journals *American Anthropologist* and *Current Anthropology.* Also, I have never forgotten that some of the books were sent to me by a few professors from the U.S.A. (Ralph Beals and Jan Vansina among them). Mostly, however, I benefitted from my personal contact with Andreas Argyres, an anthropologist from the University of California, Davis. Between 1972 and 1974, he came to Romania on a scholarship awarded by IREX (Internationa l Research and Exchange) Foundation and carried out fieldwork in the Banat county (western Romania). (In September 1974, at his suggestion, we went together for a two-week field research to the village of Hitia – in the mentioned region.) Long time after his going back to U.S.A. an intense reciprocal exchange of printed material s kept our relation ali ve.

moment. I quickly recovered after that experience, all the more as it happened during a preliminary stage in my becoming an anthropologist. As to Berevoe$ti and Campulung, taken either separately or together, they would remain for me a kind of a formal target: I would go there twice or three times a year, yet rather on the grounds of an official duty than to discover new social or cultural facts.[13]

Thus, the actual place that served as field laboratory for my initiation into the hidden aspects of the fieldwork would prove to be the microregion of Bran, a mountain side of the Bra$OV district. Unlike the experience in Berevoe$ti and Campulung Muscel, in Bran I worked independently. Paradoxical ly, this way of action proved to be more efficient for me than that resulting from my work in Caramelea's companionship (see the reason in note 13).Traditionally, Bran is a pastoral zone consisting of eleven small vi llages; I carried out intensive researches in three of them: Sirnea, Magura, and Sohodol. In Sirnea and Sohodol I was attracted by the survival (in classical anthropological sense) of a traditional form of economic activity in *'ob$fe'* (local organization based on common ownership ofland and produce), while in Magura I detected a cultural mod el of family vertically extended over three generations.

In 1974 (from 7 to 20 Septem ber), I went through a referential experience when I accompanied Andreas Argyres in the village of Hitia$ (see also note 12). Andreas was interested in the economic behaviour of the inhabitants of that village in the plain of the Banat county, but in his approach, he was looking for the impact of economic value on the whole value system of the locals. As for the working style, I was impressed by his tenacity and effort intensity. At the end of the day we would run through the information written down in the notebook trying to understand the subtlest significations of the words. I particularly appreciated his easiness in putting empirical facts into an interpretative scheme; I told myself:

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[13] The lack of 'science productivity' (so to say) in my trips to Berevoeti and Campulung may be explained by a state of saturation.Vasile Caramelea had already acquired a quasi-tota l knowledge over that socio-cultural area about which he had published solid studies early in his life. Ifso, why did he choose Berevoeti as an almost exclusive place for his many trips to the field? The answer lies in the saying he used to repeat frequently: 'Keep on grazing, horse, the grass that you already know!' [orig. Romanian : *'Pa$fe, murgule, iarba pe care o cunoti!* ']. In other words, i n his strategy of promotin g cultural anthropology against the biological oponents, Caramelea felt the need to walk on a safe ground.

'Herein lies the importance of having been trained in a good school of sociology' !...

It goes without saying that these moments lived in the field were appropriated as individual experiences. In time, however, I participated in team field researches as well. [14] In the 1980s I conducted team researches in the districts of Vrancea, Alba, and Arge . The researchers in my team (no more than eight members) would slightly vary around a basic core made up of biologists and culturalists. So, while Dimitrie Gusti's big student teams of the inter-war period used to be metaphorically compared to an orchestra, I used to think of my team as a 'chamber formation '. Comparatively, I appreciate more the individual research as being more efficient in the tentative to capturing the subtleties of a culture.[15]

(3) The Key to the Field Research: Participant Observation. It was in this same phase of my first contacts with the field that I discovered the key of this kind of scientific investigation, namely *participant observation.* Considering that my research was developing within my own culture (i.e. I moved in the framework of the so called 'native anthropology' – see Geana 1999), I was exempted from the requirement of *learning the language* of the people under study. Other requirements, however, proved to be more difficult. Let us refer, for example, to the need of *sharing the insiders ' way of life* by assuming some roles in their social behaviour. In a totalitarian state the suspicion around the researcher as intruder feeds itself on the facts of life commonly shared by both the researcher and the researched. Once, while being with my team in Barseti (the district of Vrancea), a peasant told me frankly: 'Hey, Sir, don't put us off with fine words as if you came from the Academy -you are actually from the *Securitate* and nowhere el se!' *(Securitate* being the dreaded secret police during the communist rule). Fortunately, time brings about a peaceful way out of such unpleasant situations. How does it do it? By *a long stay within the*

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[14] The field research performed in a team was an influence from the Sociological School of Bucharest of the inter-war period. Margaret Mead gave weight to this ascertained fact writing on a photograph offered to Vasile Caramelea: 'In great appreciation of the integralistic approach of anthropology in Romania, where the first team work on shared material – studies of particular villages by Prof. Gusti – was inaugurated and spread to the rest of the world. Margaret Mead, Bucharest, June 28, 1971' (Ploeteanu 1979: 83).

[15] I have in mind the case of the six-member team -Sam Beck, John W. Cole (the leader), David A. Kideckel, Marilyn McArthur, Steven Randal l, and Steven Sampson – self-proclaimed 'Romanian Research Group', which even wh ile working on a group project, were settling in separate villages (see Romanian Research Group 1984).

community under study, this being another requirement for the researcher. Generally accepted, a culture is structured on two main cycles: the yearly cycle (comprising the alternative behaviours – in each season – of work time and feast time) and the life cycle (the behaviour related to the crucial events in any human lifetime – birth, marriage, and death –, all of them determining the corresponding rites of passage). It is expected that, during one year, an anthropologist manages to 'attend ' and acquire knowledge about a culture as structured into the two cycles. According ly, an anthropological research is defined by an intensive approach and consequently is planned for at least one year. With reference to my own experience: nobody taught the neophyte I was such an elementary lesson, but while studying the villages of Bran, I had the intuition that I must go there each season of the year, even during the difficult winter time. So, I would often have lunch with my hosts, take care of the household in their absence from home, attend the hay harvesting, help the children with their school tasks, etc. I have never forgotten the most spectacular among such participatory moments: the night of Christmas 1969; then, together with a group of local young men, I skied caroling 'with the star' through the village of Sirnea.[16]

Indeed, I often felt that strange state of sympathy expressed by Evans-Pritchard in this reflection (1954 [1951]: 79):

> An anthropologist has failed un less, when he says goodbye
> to the natives, there is on both sides the sorrow of parting.
> It is evident that he can only establish this intimacy if he
> makes himself in some degree a member of their society
> and lives, thinks and feels in their culture...

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[16] Literally 'going with the star' is a special ki nd of caroling performed by a group of three children, symbolizing the three biblical shepherds and singing carols about the birth of Christ. One of them shakes with his hands an ornamental star – a representation of the heavenly body which announced the world about the Saviour's birth. Our group had four members (numerically, a simple coincidence with 'the three musketeers' who also were four!) and we moved on skis because Sirnea is a village whose houses are scattered on hills. In Romania, caroling with the star is a traditional custom with a rel igious message. Under the communist ru le it was suppressed , excepting some isolated mountain communities like the villages of Bran. Our action had not any political connotation -we simply enjoyed the beauty of the custom.

Revelations from the Field

Beyond appearances, Vasile Caramelea's decision to check up from the outset my ability to cope with the trials of fieldwork was not just self-respect for his own training (and, obviously, for his strategy); it was a spontaneous decision in consensus with one of Evans-Pritchard's reflections (1954 [1951]: 81):

> Anthropological field work therefore requires in addition to theoretical knowledge and technical training a certain kind of character and temperament. Some men cannot stand the strain of isolation, especially in what are often uncomfortable and unhealthy conditions. Others cannot make the intellectual and emotional transference required ...

One cannot understand such remarks until one has to deal with the reality behind them. Nevertheless, a question arises at this point: is the fieldwork a type of activity for which one should have a natural endowment, or is it something that can be learned? I think that irrespective of the answer, the availability (and even goodwill) of the object for being studied is necessary. We should not ignore that *in socio-human sciences the epistemic mind and the object of study share the same ontological condition: both are human beings.* Frorn this point of view, after a long experience with field research, I had two revelations, as follows:

(1) Participant Observation as a Solution to Aggressiveness in Anthropological Knowing. Undoubtedly, from the natives' point of view, the presence of an anthropologist in the middle of a community to be studied represents an intrusion. Most people in the field looked upon me or my team colleagues as honourable persons; sometimes we were invited by a householder to his home and, at the end of the visit, the host would tell us solemnly: 'Don't forget that you were the guests of Mr. This/That!'. Nevertheless, the reticence (even hostility) shown by others proves that they took my/our research for *an act of aggression.* I felt this on the spot, in the preliminary phase, and the impression was so strong that I noted it a few times in my diary (Geana 2017: 150):

Thursday, May 23, 1985 [before the Vrancea expedition]:

Mary Vladescu [17] accuses me of excessive delicacy in mobilizing people in order to be measured, in the field. This makes me think about the subject-obj ect relationship in the process of knowing. Knowi ng is an aggression on the part of the first term of the relationship. The whole dignity of the knowing subject consists in converti ng aggressiveness into love; an anthropo logist has a duty to engage him/herself in this transfiguring act. Actually, we offer nothing to those people, but we expect them to smile while letting themselves be measured . Do they have any idea why we approach them? None. We try to explain to them , but how much do they understand from our explanations? (...) We should understand their cares and troubles of the day and keep having sympathy for them, even when they refuse to cooperate.

And even more precisely, during one of the expeditions in the Western Carpathians, between 28 September and 11 October 1987:

In Salciua -great difficulties with the filling in of my paremiological questionnaires of value orientations. One day, when in the village shop, I asked the people in the queue, so insistently and with so little result, to answer the questions, that Nad ia [18] couldn 't stand it anymore and came out of the hall totally disappointed. I said to myself that I ought to write a text entitled 'Scientific Knowi ng, from Aggression to Humility' (Geana 20 17: 160).

The idea, which came to me while in the field, would become focal in my reflect ions; I discussed it in my paper at the 3rd EASA Conference of Oslo, in 1994, as well as on other occasions (among them: Geana 2014a).

As to the incidents like that from Salciua, they lead to the conclusion that a time of mutual adaptation is necessary, and that, in any case,

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[17] Maria Vladescu (1930-2013): physical anthropologist , specialist in anthropometries, having notable contribution s to some volumes of *Atlasul antropologic al Romaniei* [Anthropological Atlas of Romania]. She was a member of the research team I conducted in the regions of Vrancea, Alba, and Arge .
[18] Nadia Stahovski, a woman-colleague, member of the team.

participant observation is the best solution to attenuate and finally to an-
nihilate the aggressiveness in the process of anthropological knowledge
production.

Certain peculiar solutions must not be excluded either. As far as I
am concerned, I play the shepherd's flute and I made use of this instrument
in order to gain natives' sympathy; after a sequence of playing, people felt
as if I was 'one of them' and my chance to communicate better with them
increased.

(2) **Genius at Home.** Perhaps the greatest satisfaction I found out
in doing fieldwork has been the vivid contact with a profound layer of cre-
ativeness that I called *phreatic genius.* [19] The phrase currently used to des-
ignate this enigmatic zone of reality is 'anonymous genius'. Why do I
prefer to say 'phreatic' instead of 'anonymous'? Anonymity dissolves ge-
nius into a diffuse – void of impetus – state of things; on the contrary, the
idea of 'phreatic' confers reason, continuity, the chance of a leap, the pos-
sibility of overtopping by the agency of personalization to the notion of
genius. Thus, Homer sprang from the ethos of myths and legends of oral
tradition in ancient Greece, Goethe from the Germans' need of action after
the detouring effect of Kantian speculative philosophy, Wagner from the
German medieval epos, Tchaikovsky from the Slavic nostalgia, Rodin
from admiration for the stone cutters by whose efforts the cathedrals were
built up, Brancusi from Romanian archaic mythology and beliefs (Geana
2012).

Foremost, let us note that the oldest epic creations of humanity –
The Epic of Gilgamesh, Panchatantra, One Thousand and One Nights, Po-
pol Vuh, The Song of Nibelungs, and even *Ramayana* and *Mahabharata,*
or *Kalevala* - are fruits of this inexhaustible phreatic genius.

Let us extend the idea and observe that those human beings called
'primitives' by the evolutionists of the nineteenth century have been capa-
ble of a reflexivity not at all inferior to our own. No wonder, therefore, that
we can recognize in their thoughts ideas or principles stated in great phil-
osophical doctrines. From an anthology of Amerindian thought (Aaron &
Borgenicht 1993) one could extract examples regarding the essence of the
dualist ethics (p. 24), the outlook on the world as creation of a Great Heav-
enly Father (p. 26), a conception on human nature (p. 103), a world view
about death as a change of worlds (p. 122), etc. Let us quote a short passage

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[19] I developed the content of this notion – as exemplified in the domains of technical
inventiveness, literature, music, philosophy, and morals - in Geanii 2012.

in which the famous *principium individuationis* (the principle of individu-
ation) is clearly fonnulated; it is a theme debated by Thomas Aquinas,
Duns Scotus, Leibniz in the Middle Ages and at the dawn of modern times,
but also by Carl Gustav Jung and Bertrand Russell in the twentieth century.
Related to our discussion, it appears in the genuine formulation of Shooter
(Teton Sioux), as follows:

> All birds, even those of the same species, are not alike, and
> it is the same with animals and with human beings. The rea-
> son Wakantanka does not make two birds, or animals, or
> human beings exactly alike is because each is placed here
> by Wakantanka to be an independent individuality and to
> rely upon itself (Aaron & Borgenicht 1993: 52).

In short, phreatic genius is an inexhaustible fund of wisdom. The
above examples serve as cross-cultural comparisons, in the classical spirit
of anthropology. Whenever, while in the field, *I* learned about a new idiom
(word or phrase) charged with genuine poetical beauty or a new sample of
profound primordial thinking, I was seized with jubilation. From the many
moments of this kind, I will extract two reflections from Hitia where, as
I have mentioned above, I did fieldwork in the company of Andreas Ar-
gyres. I shall hereby let the primordial wisdom speak for itself, as recorded
in my diary notes from 1974:

> To be wise means to do a good deed and not to show off
> that what you did is such a great thing (Pavel Spariosu, 70
> years old, see photo 3).

and:

> One takes a handful of earth and turns it into apiece of gold,
> while another takes a piece of gold and turns it into a hand-
> ful of earth (Simion Unguru, 73 years old).

Both authors of these reflections were peasants with minimal pri-
mary school education. The words of *'mo' I* 'old' Pavel reminded me of
Socrates' gnoseological definition of wisdom : 'Not pretending to know
what you don't know '. Mo Pavel stated the *moral* definition of wisdom.
As to Simion Unguru's words, while not stated as part of a definition, they
suggested an *applied* understanding of wisdom. The two characters belong
to the category of phreatic genius together with Ogotemmeli (Griaule 1966

[1948]) and Baldambe (Strecker 1998). One day I lectured to my students
at the Faculty of Philosophy on a field recording with *mo* Pavel and told
them: 'In other circumstances, this man could have stood at this desk'.

It is not an exaggeration to include the children 's imagination in
the same pattern of thinking . I could not say that children 's games have
ever been of a particular interest to me, but as an anthropologist with ho-
listic concerns I did not avoid this theme in the field. Currently, the prac-
ticing of games is a part of the process of enculturation and, in their games,
children reflect the value system of the culture to which they belong. Here
is a transcription of such a game (the note is abstracted from my diary,
namely from the pages dedicated to the fieldwork I carried out in the vil-
lage of Poaga, Western Carpathians, between 23 July and 6 August
1986)[20]:

> The game is called 'The flowers and the woman-florist'. A
> girl is the florist. The others (boys and girls alike) choose a
> flower name each, the florist attending as arbiter. When
> everybody has chosen a flower name, a bell clings -cling!
> cling! It is the Angel.
>
> 'Who are you '? asks the florist.
>
> 'The Angel', comes the answer.
>
> 'And what are you looking for'?
>
> 'A flower'.
>
> 'What flower'?
>
> Now the Angel says a flower name: 'A lily', or 'A lily-of-
> the-valley'; etc.
>
> If there is a child bearing that name, he/she enters the An-
> gel 's team. If nobody bears that flower name, the Angel
> waits for his turn because now the One-Over-the-Mountain
> (i.e. the Devil) comes with a thundering noise: 'boom!

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[20] I was an onlooker at such children's games, but I supplemented the information by
interviews with Florentina Urs, then a 12-year-old girl.

boom!'. He has the same dialogue with the florist. The Angel and the Devil take turns until all the 'flowers' are chosen. Then the players draw a line on the ground. The Angel 's team and the Devil's team stand in a row, with their leader at the front, on either side of the line, facing each other, arms around the waist of the player ahead. At the florist's signal, the Angel and the One-Over-the-Mountain hold hands and, supported by their team members, pull, trying to make the other cross the line and enter the opposite team. The winner is the one who manages to do it.

Here lies, in a children 's game, the whole meaning of biblical history. When I was a child, my grandparents described to me, in a similar manner, the becoming of the world as a fight between God and Devil for winning the human souls.

These considerations on the primordial thinking patterns go in consensus with Claude Levi-Strauss, who praised the 'Indians of the tropics, and others like them throughout th e world, who taught me their humble knowledge', in the lecture he delivered on January 5, 1960, at the inauguration of the chair of social anthropology at College de France:

To them I have incurred a debt which I can never repay even if, in the play in which you have put me, I couldju stify the tenderness I feel for them, and the gratitude I owe them, by continuing to be as I was among them, and as among you, I would never want to cease from being: their *pupil ,* and their *witness* (Levi-Strauss 1977: 32 – my italics, G. G.).

Synopsis

In the end, I shall sum up what has been said before in the following conclusions:

The anthropological fieldwork is a school of initiation, similar to the one a student in medicine attends in a dissection ward.

At the beginning of my scientific career in anthropology I was insistently told that originality is quasi-exclusively guaranteed by the empirical facts; hence the fieldwork as a necessity for a promising original ity. Later on I discovered that fieldwork is not only a ground for originality,

but equally a stimulus for interpretative thinki ng. In the meantime, my in-
itial passion for philosophy has not been abolished; it is still present in my
bias towards epistemological approaches related to anthropology (e.g.:
Geana 1995, 1997, 2005, 2014a, etc.).

Finally, the fieldwork does not bring about only ephemeral scien-
tific experiences, but memorabl e l ife stories, referential sequences in the
biography of any researcher devoted to anthropology.

© Gheorghi\a Geana. Participant observation in the Carpa-
thian Mountains: getting ready for the 'star caroling' on
skis. From left to right: Gheorghita Geana, Lica Benga,
Radu Frunte, and Naica Martoiu (the vi llage of irnea,
microregion of Bran, 1969).

© Gheorghita Geana. The author while filling in parem iological questionnaires in the village of Coza (Vrancea district, 1984).

© Gheorghita Geana. Pavel Spariosu, in front of the artistic gate he built at his dwelling, in Hitia (1974). In spite of a limited official education, this simple peasant possessed a refined sense of reflection that made of him an embodiment of the phreatic genius.

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In Search of the Lost Interwar Sociology

Zoltan Rostcis

Introduction

Inevitably, this study has a personal character; it is an endeavor to map the social history of one's own research experience in the 1980s. Around 1980 I was not concerned with the past of Romanian sociology yet, I was simply interested in the idea and the need to conduct research on my own. There were individual non-official projects covering important themes which were tolerated as they were not directly threatening the communist regime.

My searches were related to approachable themes of historical anthropology; I aimed at research ing in great details apparently ordinary topics. As I was not involved in the research plans of an institution, I was free to choose any research topic in my spare time. At that time, I was looking for research topics without thinking of publishing, which was impossible anyway without the ideological concessions that I obviously would not accept. Under those circumstances I was thinking – maybe romantically – that what matters is the journey, not the destination.

Relatively soon I identified my research direction -the topic of the present chapter: an unconventional approach to the history of the Sociological School in Bucharest. The idea that I managed to implement consisted of interviewing aged intellectuals who had worked with professor Dimitrie Gusti. [1] I did not focus on the significance of the works published by this school, but on the research er's lifestyle and the way in which one conducted the study of the Romanian village in the interwar period.

In order to understand this adventure unfolding in an all-pervasive communist rule, I concisely describe the sections of this paper as follows:

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[1] Gusti, Dimitrie (1880-1955) -sociologist, founder of the Sociological School in Bucharest. 1919- 1920, Professor at the University of la$i , then at the University of Bucharest unti l 1947. He conducted monographi c field researches from 1925 unti l 1939. Minister of Instruction, Arts and Cults, General Comm issioner of the Romanian Pavi lions at the World Exhibitions i n Paris (1937) and New York (1939), Member of the Romanian Academy and president between 1944 - 1946. In 1948 he was fired from both this forum and the university. From 1949, he lived in a marginal ized way until his death.

in *A defining intermezzo,* I evoke the circumstances of a 1980 international history congress which convinced me of the possibility and legitimacy of an individual oral history research. This topic cannot be understood without the section *General context,* in which the more and more restrictive environment of Nicolae Ceau escu era is described. For the history of interwar sociology, *A second intermezzo* was needed, with a focus on the publication of the inciting volume of memories by professor Henri H. Stahl, *Memories and Thoughtsfrom the Old School of Sociological 'Monographs'. (Amintiri $i gd nduri din vechea $Coala a 'monografi ilor ' sociologice, 1981)* . The section *The Evolution of the Gusti School* is a historical synthesis of available data and information from oral history interviews. The section *From Research to Intervention* approaches the expansion of the knowledge strategy into that of social work . The section *Relationships in Teams* offers a deeper incursion into the understanding of the school 's configuration. The section *Between the Village and the Authorities* sketches the difficult integration of the researcher in the interwar village world. At the end of the chapter, in section *Instead of Ending,* I present a concise report on the school's end after World War II.

A Defining Intermezzo

In 1980, the announcement of the organization of the International History Congress in Bucharest i n August could have come as a surprise (the country being in complete intellectual isolation) had it not been known that such occasions were meant to reinforce Nicolae Ceauescu's personality cult. Thus, a unique possibility for Romanian historians to meet foreign colleagues came up. For me, the most surprising and at the same time inspiring section was the one of oral history. The papers and the d iscussions with the parti cipants in this special group made me realize that the oral history method is actually an extension of the non-structured anthropological interview towards history .

Since then, I consider this interviewin g method not only capable of completing written sources, but as an excellent way of discovering specific themes, which are then completed and endorsed by written sources. I was enthusiasti c about oral history also because of my experience in conducting face to face interviews, both sci entific and journalisti c, based on or without questionnaires.

Moreover, oral history had an anti-establishment ideology as well, as it addressed those grou ps, layers, categories that ended up in marginality

and which usually don't leave written testimonies, or if they do, these are elaborated by the authorities. At the beginning, and rather experimentally, I decided to focus my research on the multiculturali sm of the Capital, as this theme was not openly discussed for the simple reason that the party policy insistently aimed at creating a 'socialist homogenous nation'. On the other hand, I supposed that using the tape recorder to interview persons over 75-80 years would not draw the attention of the authorities. In fact, I was not concerned with the authorities, but with gaining the subjects' confidence. At the same time, I did not introduce myself as representing an instituti on, I was always saying that I wished to listen to one's life experience. Through this simple method I stmted discovering social worlds not only ethnically and linguistically diverse, but also belonging to some important social groups on the verge of disappearance.

General Context

I started this personal project under apparently unfavorable circumstances. After Nicolae Ceauescu's ideological theses of June 197 1, when the annihilation of the concessions made by the communist party in the second half of the 60s began, planned social and econom ic life slowly turned into a centralized 'leadership' of the state and party ruler. ltwas a slow process, even for a participant observer like me, as the party apparatus d i d not hurry to implement the president's instructions. Instead , a method rooted in the 60s on the d ifferentiated approach to the cultural sphere was practiced. Namely , a d ifference was made between the promotion, toleration and prohibition of certain literary or scientific work, new currents, individual or group actions of intellectuals. But in the end, the famous tool used by the communi st party, the 'rotation of cadres', had its inauspicious effect both in the cultural and economic environment, and in the citizen 's life.

Thus, in 1980, a month after the International History Congress on the 16-17[111] of October, the Great National Assembly, the legislative forum of the country, voted a series oflaws which, on one hand, consolidated the forced development of industry, and on the other, stroke the human rights: one law stipulated that one family could only own one house and the other, 'regulated ' the citizens' journeys abroad. The enforcement of this subsequently more restrictive law displeased mainly the intellectual ity, strength ening the feel ing of captivity. In December of the same year, the aforementi oned legislative forum enacted another set of laws recalling the

comm unist war epoch: the rationalization of food consumption, the ban-
ning of transportation and of the trading of products from other counties
without official approval. For the latter, one had the obligation of transfer-
ring the surplus to the state centralized fund to be re-distributed by the
authorities. As restrictions were progressively enforced, the population
learnt self-defense methods through the development of parallel or under-
ground economic mechani sms. In the 80s, the population of Bucharest had
to bear the demolition of some old districts, churches, monuments, in order
to leave place for the pharaonic constructions desired by Ceauescu. Under
these circumstances, a hilarious and annoying decree, such as the one on
declaring all private typewriters at the Militia, did not seem out of place
anymore. In order to prevent the writing and the multiplication of mani-
festos against the regime, each typewriter owner was bound by law to ap-
pear every January to the Militia headquarters and leave a sample of the
typewriter characters, from 1983 until the regime change in 1989.

The Second Decisive Intermezzo

I had already gathered considerable experience on the exploration of mul -
ticulturalism when, in 1981, Professor Henri H. Stahl's[2] volume *Memori es
and Thoughts from the Old School of Sociological 'Monographs '* ap-
peared. Professor Stahl was the most important and original collaborator
of Dimitrie Gusti, founder of the Sociological School of Bucharest. Thus,
I read Stahl 's volume through the lens of social history, aiming to plunge
into the Gusti school through oral history interviews beyond their socio-
logical feature. Professor Stahl, being perfectly fami liar with the utility of
this method, was open to having multiple discussions. I did the same with
the other members of the school or of the royal student teams.

        As in other situations, I confessed from the beginning my intention
to work on an oral history of the school, to transcribe all that that I recorded

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[2] Stahl, Henri I-I. (1901- 1991) sociologist and historian, participant in all the activities of
the Gusti school from 1926. From 1929 he is honorary assistant of sociology in Prof.
Gusti 's Department. From 1934 until 1939 he is director at the Royal Cultural Founda-
tion, from 1940 director at the external service of the Central Institute of Statistics. From
1941 until 1948 he is a lecturer at the Faculty of Letters, University of Bucharest. After a
ten-year long marginalization begins the publishing of his essential work *Contribuf ii la
studiului satului deviilma·* [Contributi on to the study of the Communal Village]. After
the rehabilitation of sociology in 1965 he taught and published in the fields of sociology
and social history. He was a Member of the Romani an Academy.

and to publish the materials, with their agreement, when I would be sure that no one would intervene in the text. Despite a lack of perspective regarding the publishing of the interviews, no one disregarded my undertaking, on the contrary.

Even if the urgent start of the campaign was motivated by the age and health condition of the former monographers, my interest was not limited to the attempt of salvaging the memory of sociology. I supposed that, through these interviews and by examining in depth their everyday life, career options and social relations, ways of solving difficulties and social conflicts, I would manage to reconstruct the functioning of this school in the Romanian interwar society. At the same time, I tried to discern the direct and indirect influence of the social environment upon the school and conversely, its influence on society.

The Evolution of the Gusti School

Even if, in 1936, some syntheses[3] on the history of the ten year long monographic sociological research initiated by Dimitire Gusti and Stahl's book on its whole activity were published, the interviews still filled with vibe this segment of scientific life.

After fifty years, Henri H. Stahl recounted the animating and creative style of the sociology seminar from the Faculty of Letters in the 20s: '[Gusti 's] seminar was a gathering place for the best that the youth had at that time. Not only Gusti 's students, but students from all faculties came, and it was a permanent place for discussions and problem-based debates'. (Rostas 2000:19)

Consequently, the Seminar[4] was the first station, the workshop where the monographic method was drafted, in terms of both theoretical and methodological grounding. In the 1924/25 academic year, Gusti assigned the Sociology Seminar members to build up 'the plan of monographic research'. There were one-year long discussions on the works, in order to correlate rural research plans with Gusti's texts. Then, in 1925, the first ten-day long fieldtrip to Goicea Mare[5] followed. The only witness

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[3] The Archive for Social Science and Reform published in 1936 a homage volume to Prof. D. Gusti at 25 years of university activity in which the school leaders critically examined aspects of this activity.

[4] The Seminar was introduced by Dimitric Gusti in Iai and was re-launched after 1920 in Bucharest as a student creative workshop.

[5] Goicea Mare, Dolj county, south of Romania.

of this monographic trip that I could approach was Ion Costin.[6] According to him, the choice of the first village was made very simply: 'Among Gusti's students there were some from that region as well, I can't remember their names... Popescu Goicea'. (Rostas 2003: 90) This is what Ion Costin remembers after 60 years: 'Everybody was pleased. It was a region with good wines. We were very welcome, they took us places. In the evening, we stayed up very late. There was this method – in the evening, after supper, at about 6 o'clock, we all discussed what we brought, what we gathered, what was needed, and everyone scheduled their turn to talk during one of the next meetings'. (Rostas 2003: 92) This initial experience revealed all the things that had to be clarified within a group-based scientific work. Consequently, the seminars during the academic year were held yearly, as well as the one-month long field researches during the summer.

After the trip to Goicea Mare, the methodological input of the first campaign was analyzed in detail. An initial questionnaire (a kind of research topics list) was conceived, and based on it, two more campaigns were undertaken, one in *Ru§efu*[7] in 1926 and the other in Nereju[8] in 1927.

After gathering experience from three village-based monographs, a qualitatively different second stop followed. In Fundul Moldovei[9], in 1928, the monographers did huge steps in finalizing the research method but also the social and professional identity. The team system was used for the first time then, based on common sheets and files.[10] Based on the Gustian theory of 'cadres' and 'manifestations' the following teams were formed: cosmological, biological, historical, psychological, economic, judicial, of musical and literary folklore, esthetics, philology, studying the sheepfold, family, household, gender, home-based industry, cultural problems, administrative politics, criminology, war, religious manifestations and the tavern.

This Bukovina-based campaign of 1928, the fourth one in a row, coincided with the institutionalization of sociological monographic research. Then, the decision for founding an *Association of Monographers*

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[6] Costin, Ion (1903- 1991), Licentiate of Philosophy, participant in the first three sociological monographs. Following this, he was a journalist with a press agency.

[7] Ru etu, commune in Buzau country, in the Baragan Plain.

[8] Ncreju, commune in Vrancea county, in the Carpathians Arch.

[9] Fundul Moldovei, commune from Campul Lung county (today, Suceava) from the old Bukovina, North-East Romania.

[10] Based on his field experience, in 1929, H.H. Stahl started to draft a methodology album, published in 1934 with the title 'The Technique of Sociological Monograph'.

was taken, the purpose being the creation of an organizational framework to include both the specialists and the students interested in monography. The resonance of this campaign was higher than the preceding ones. Based on the objects collected by monographers, the first sociological exhibiti on was set up in the rooms of the Seminar. It was a successful exhibition not only among the Bucharest-based intelligentsia, but also withi n the Romanian pavilion at the World Exhibition in Barcelona in 1929.

With ninety participants and a well-defined methodology, the Dragu campaign (1929) was the thi rd step. It was the best known, it produced the most numerous studi es and it is the most 'publicized' up to thi s day. The school was also consolidated due to Gusti 's election as dean of the Faculty of Letters. No less important was the fact that three collaborators, close to the professors and 'raised' in countryside monographic campaigns - Traian Herseni [11],Henri H. Stal and Mircea Vulcanescu -became assistant professors in Gusti's department.

No matter how interesting and special the campaigns of Runcu [12] (1930) and Cornova [13] (193 1) were, they did not bring any theoretical or methodological novelties. They were more difficult to coordinate, since Gusti was not directly and perm anently in charge, nor did he appoint a replacer with a solid enough authority. Despite this, for the young ones and those with individual interests, these campaigns were also rewarding and conduci ve to scientific papers.

Even if the monographers' world was more colorful and peopl e's motivation and participation unequal, everyone respected Gusti's program and his assignments of tasks to sections. Especially the ritual participation at the evening reunion in *Sala luminoasa* [The Glowing Room], where discussions on the preced ing day took pl ace. The d iscipline of these applicative 'seminars' in the countryside ran by Gusti had a huge role in the development of monographic sociology.

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[11] Hersen i, Traian (1907- l980) sociol ogi st, psychosociol ogist, promi nent mem ber of the Gusti school, participating since 1927 in all the research activities of the school. He was the most prolific collaborator of Prof. Gusti. Since 1929 he is assistant at the Prof. Gusti's sociology department, then lecturer at Cluj-Sibiu Un iversity. Because of his legionary activity, since 1940, he is marginalized during the war, after 1945 he only wrote under a pseudonym, and was arrested in the 50s. With the sociology's rehabil itati on, he takes up research and publishing work again, but will not be included in the sociological teach ing field.

[12] Runcu, commune in Gorj county, Oltenia, south of Roman ia.

[13] Cornova, Bessarabian commune of Orhei county, today in the Republic of Moldova.

In the recollections of Marcela Foca, after a few sum mer campaigns, fieldwork appears as well-structured: 'During the day everyone did their own work and, in the evening, we went to school. .. Gusti didn't have much to do during the day.... We all had lunch together and, in the afternoon, we bundled off to the river to swim, or the like. After supper, at the school, we had our meeting, and everyone came'. (Rostas 2003: 111)

Starting with 1930, a division between the work in the seminar and the monographic activity can be noticed. Gusti's withdrawa l from the seminar occasioned H.H. Stahl and Traian Herseni to introduce the students to the theory and techn ique of the monograph. At the same time, here also started the selection of the future monographers.

This is how Nicolae Dunare[14],a student in the 30s, recalls the beginnings of the contact with the monograph: 'First we all received a copy, actually we bought it, but they had been recommended some time ago, *The Theory of the Sociological Monograph,* by Traian Herseni, and *The Method of the Sociological Monograph*, by H. H. Stahl. I already had them, I think every member of the seminar had them. Afterwards , Stahl too, and Herseni, who prepared me to go there, gave us a bibliography about Dragu, Fagara , i .e., Tara Oltului, so that we could read something before going there... I think the thing was that most of them were Greek Catholic. I recall this thing from over there, for he made me pay attention: "You are [Christian] Orthodox. Don't talk about being Orthodox over there, or that a Christian confession is better than another ..." Herseni was Greek Catholic too, and he was a free man, former boyar, in his family... So, he made it clear we shouldn 't do propaganda over there, nor politics. I wasn't about to, anyway, but basically we were told we shouldn't, either moral, or religious. "You don't say anything, they should teach you everything, and what they do, that's the right thing. You j ust take notes, and, in the end, we' ll see what 's good and what's bad ". Anyway, he stressed that *I* shouldn't impose my viewpoint on the people I'd talk to.He was very keen on that. [...] And the instruction Herseni gave me, not to be overtly [Christian] Orthodox when I'm talking to Greek Catholic peasants, not to bully ... and, generally, not to have opinions, to let them have the opinions. To give the peasant informant the feeling he is our teacher in all things. This was a fundamental element he instilled'. (Rostas 2006: 58-59)

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[14] Dunare, N icolae (1916-1987), sociologist, ethnologist trained in Gusti monograph school. ln I 939 he participated in the research i n the *plasa* of Dambovni c; later he was a researcher at the ethnography and folklore institutes in Cluj and Bucharest.

Despite this consolidation, in 1932 there begins the so-called monograph crisis, which can be considered the fourth stage of the Gusti school. Jn this period, an unforeseen problem appeared for those monograph ers who were university graduates, acquainted with Gusti monographic school and keen on succeeding as researchers and continuing work in the summer campaigns. Apparently, this was a management crisis: in the summer of 1932, Gusti was invited in the new government of the Nationa l Peasant Party as Minister of Education, Cults and the Arts and consequently could not directly lead another campaign. Then, the idea of a new research was postponed , and an option was mad e for an 'ed iting campaign' based in Fagara, due to its proximity to Dragu, for possible additional research.

The real problem did not consist in using the field research and in the editing and publishi ng of resu lting material s. The crisis was caused by the ardent search of the young monographers for a political identity, in addition to the professional one. Looking closer, one can see that behind the misunderstanding s among monographers, there was not only pride, but also differences of political orientations which, during the 30s, became more and more contrastive.

Within a mature school, doubts appear as well, while the sedimentary parad igm remain s. Even during these years of the monographic crisis, tens of important [15] studies appeared and volumes on the theory and technique of monography were in progress. H. H. Stahl outspoken ly concluded on the drama of the monograph: said that the sociological monograph... everybody knows, it was common knowledge... the monograph was a school for learning sociology. It's a school like "Bricul Mircea", the ship; it carries neither personnel, nor goods. [...] I used to say this very clearly. We had to teach sociology there.still think that it's impossible for someone to learn sociology if they don 't do any fieldwork. Until you have done field research yourself, you don't have that critical approach to the value of the information. However, collective research can also have a different purpose – that of attain ing a scholarly work. In this case, one doesn't use students one wants to teach, but peop le who know the trade'. (Rostas 2000: 93)

Despite the recognition of Gusti monograph 's utopianism, the school continued, but in different modes and ways. After the 1934 crisis,

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[15] Studies publish ed in no. 1-4 of the Arch ive for Science and Social Reform of 1932, of more than 1000 pages, was the first representative proof of the novelty of the Gusti monograph in publ ic space.

the monograph went on, even with the criticism of Anton Golopentia [16] who introduced the concept of *concise monograph.* But this was achieved after Dimitrie Gusti and his team of sociologists trained on the field of rural monographs took over the Royal Cultural Foundation 'Prince Carol '. This was, in fact, the fifth stage of the school.

From Research to Intervention

Without diminishing the importance of the Sociology Seminar at the Faculty of Letters, in December 1933, Gusti took over the leadership of this foundation aimed at the cultural emancipation of the village.

By accepting this appointment, Gusti radically changed the strategy of the Foundation 's cultural activities. Stahl remembers that 'in 1934, we entered like some kind of intruders, like some kind of Trojan horses. First, I was the only one, then Neamtu [17] , and later Golopentia and Foc;a[18] and a few more others. [...] And we went along neither well, nor bad ly; they used to do what they had to do... and we knocked ourselves out, if I may say so ... we went about our business'. (Rostas 2000: 198)

Instead of the old bureaucratic practices, Gusti suggested, on one hand, the organization of the student royal teams, on the other – in conformity with 'the four elements rule ' in his system -the directing of their work towards four fields: the culture of work, of health, of soul and of the m ind. Based on this conception, a royal student team had to include, according to Octavian Neamtu: 'a male medicine student, a female medicine

[16] Golopentia, Anton (1909-1950), sociologist and statistician, important member of the Gusti school, with a PhD completed in Germany. Cabinet chief between 1932-33 for Gusti ministry , assistant and director of the Royal Cultural Foundat ion. After the suspension of the Social Service he is appointed chief of the Studies office of the Central Institute of Statistics. In this position, he conducts the census of the Romanian s beyond the Bug river, behind the front line. In 1947 he becomes general director of the Central Institute of Statistics, wherefrom he is fired and then arrested. He dies in prison without condemnation.

[17] Neamiu, Octavian (1910-1976), sociologist, close collaborator of Professor Gusti, and his follower at the head of the Royal Cultural Foundation (194 I – 1948). He participated in the monographic research campaigns from Cornova and Dragu, lead the social action student teams (I 934-1939). Arrested and marginalized in the 50s, he is reactivated in the 60s and works on the critical edition of Dimitrie Gusti's work, together with Ovidiu Badina.

[18] Foca, Gheorghe (1903- 1995), ethnographer and participant in the monographic campaigns starting with 1929. Inspector at the Royal Cultural Foundation, director at the Village Museum in Bucharest between 1948-1978.

student, a student in veterinary medicine, an agronomy student, a sports teacher, a theologian, a sociology student, a woman skilled in home management. All these should be in their last year of study, so that their educational level allows for effective work. Their activity is strengthened by one of the Team's technicians, a group of specialists with work experience, attached to each Team of students'. (Neamtu 1935: 1033)

Just like the monographers who, at the beginning, did not have an adequate behavior in the rural environment, the royal student teams were astonishing as well. In this sense, the report of the former team leader, Gheorghe Macarie, is relevant. [19]: 'I reported to the Foundation that three of the team members did not measure up [...] And who do you think came to investigate whether it was true or not? Mister Stahl. When I met him, I said "Mister Stahl, I am not an ambitious man. I wrote the report, you came to make inquiries, we had a meeting with everybody at the school. And I said, I could leave, but it'd be a pity, the entire team ..." "Wait, don 't worry , let's see what everyone has to say." When everybody said their piece, mister Stahl said: "All three of you must pack your bags and go home."- *What was the reason?* – In my mind, the reason... they used to party at night. There were three boys and one girl. For the girl I intervened and then Stahl, too, to let her stay. Fifth year medical student, who, if not for her, maybe the boys would have quieted down. They stayed up and partied night after night, making a lot of noise in the village. The village has its own laws that mu st be observed. Especially if you go there to teach them something'.(Rostas 2009: 90-91)

From 1934, when the first twelve teams set out to experiment, their number grew every year as did the range of interests for Gusti 's collaborators.20 Beyond the growing extension and efficiency of the teams' cultural work, the special motivation generated by the rhetoric and

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[19] Macarie, Gheorghe (1912-1988), Licentiate of Law, since 1935 inspector at the Royal Cultural Foundation 'Prince Carol'.

[20] In 1935, 25 teams were set up: in 1936 -59; 1937 -77; 1938 - 68. In 1939 all University and Superior School Graduates were mobilized according to the Social Service Law, tens of thousands of students. To all these we must add other activities which took the time and energy of Gusti's collaborators: schools of team leaders, peasant schools, the conceiving and building of the Village Museum, assistance in the completion of the Romanian Pavilions for international exhibitions, participation in new monographic research and completing others, for completing the works for the International Congress of Sociology, organization of the Congress, conceiving and participation in the elaboration of the Romanian Encyclopedia, and the publication of the journals 'Romanian Sociology'

propaganda of the Royal Foundation must be underlined as well. Octavian Neamtu, the most devoted of Gusti's collaborators in terms of organizing the cultural work of the Foundation, was underlining in the mentioned article the distinction between a nationalism conceived as 'fight against the foreigners' and the 'positive nationalism of constructive work'. As Gusti foundation adopted the latter, the activity of the royal students' teams was conceived as an alternative for the 'voluntary work' of the legionary movement. The fact that the results of the teams were propagated without ostentation and without anti-legionary rhetoric succeeded in attracting the sympathy of many intellectual circles, even left ones.[21]

The involvement of the Gusti school in the new strategy of the Foundation did not mean the abandoning of the monograph. What could be presented as a contribution to achieving the Foundation's mission, was automatically supported by the state budget. Thus, in 1934, in addition to the 'Cultural House' (Caminul Cultural)[22] - a guide magazine of the Foundation, the 'Student Teams' Messenger' ('Curierul Echipelor Studenteti '), appeared as well.[23] In 1936 the 'Romanian Sociology'[24] appeared, a scientific journal still accessible to the youth. In the same year the Romanian Village Museum was also built with the Foundation 's budget.

With the beginning oft!ie royal dictatorship[25], in the spring of 1938 comes the occasion for Gusti to elaborate the Social Service Law which, after the promulgation, will transform the voluntary work of the student

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and 'Student Teams Messenger', in addition to their own works, some of them commissioned.

[21] See the articles of Stefan Yoicu, *Expozifia de lucru a echipelor studen/e·ti* (Work Exhibition of Student Teams) 'Era Noua', 1 februarie 1936, and Erno Gall, *A romaniai tarsadalmi munkaszolgalat* (The Social Service of Romania), 'Korunk' journal, nr. 3, 1939.

[22] *Ciiminul Cultural,* organ of the Royal Cultural Foundation appeared with interruptions between 1927‑1947.

[23] The *'Student Teams Messenger'* appeared between 1934 and 1939, but later it changed its name (and partially its profile also) to the *'Social Service Messenger'*.

[24] *'Romanian Sociology '* appeared between 1936 and 1943 under the leadership of Dimitrie Gusti with H.H. Stahl, Octavian Nemtu (1936-37), Anton Golopentia (1938-39) and Gheorghe Foca (l940-43) as editors.

[25] The royal dictatorship instituted by Carol II (1938- 1040) consisted in the abolition of the Constitution of 1923, the suspension of the political parties activity, the establishing of the single party The National Renaissance Front (joined by the majority of the political elite) and a harsh fight against the Legionary Movement supported by the Nazi regime in Germany.

teams in obligatory cultural work in the countryside involving all university and higher education graduates.

Relations in Teams

It was desirable and predictable that the oral history interviews bring supplementary clarifications not only on the visible activity of the Gusti school, but above all on its internal sphere. These internal relations are relevant as they show how Gusti was perceived as the leader of the school.

Based on the observations of H.H. Stahl, the Professor had the gift of decisions, both in theory and in the program, obviously, within the logic of the historical moment. 'When there was an important problem, a turning point, as we called it, his presence was of a totally different nature, and it was decisive. For instance, when we went from research-oriented teams to action-oriented teams, we needed a plan, a theoretical conceptualization, and Gusti did that. This theoretical conception, turning from basic research to the fourfold action of 'work culture, health culture, mind culture, and soul culture'. This was a theory, wasn't it? And he developed it, all on his own, no one else had anything to do with it'. (Rostas 2000: 127)

But Gusti was talented not only in situations involving a theoretical effort, but also in public administration. He was not hesitant in taking decisions. This quality was observed by professor Gheorghe Zane[26] as well. Stahl relates: 'The House of *Regie [Casa Monopolurilor]* [27] had some sort of a council, like any other such autonomous *regies,* and Zane was a member of it. And Zane says that this council had periodical meetings, but they only lasted for half an hour, during which time specific matters were decided. No talks. With only a few words and opinions, Gusti managed to make good decisions'. (Rostas 2000: 127)

Organization skills, the second element of leadership was Gusti 's great and recognized quality. Apparently, no one would convince him to deviate from his principles, not even his closest collaborators. Still, in 1934, the journal for the guidance of royal student teams was edited by Stahl and Neamtu in the absence of Gusti, which was a courageous act.

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[26] Zane, Gheorghe (1897-1978), economist and historian, university professor in Iai and Bucharest. He studied political economy, finance, economic history, social history. He studied and edited the work of Nicolae Balcescu.; Member of the Romanian Academy.
[27] Casa Monopoluri lor was founded in 1929. Its first president was Dimitrie Gusti, until 1932. It was in charge with the cultivation, processing and selling of tobacco.

Back from his holiday, Gusti congratulated them and ensured the continuous issuing of the journal. Not the same thing happened with a sociology journal for the young generation of monographers who had wanted for a long time their own publication. Perfectionism was, accord ing to Stahl, Gusti 's greatest fault.

All of the above shows that Gusti knew how to set up objectives, make plans, but he was not careful with the hierarchical structure of the organization. And this was not caused by absent organizational discipline, but by reasons related to the personal aspect of leadership, namely, communication and manner of leadership. In this regard, Gusti was a bunch of contradicti ons. On one hand, he had a matchless convincing force. When aiming at conquering a young student, he would overwhelm him / her with flattering tasks, giving green light to personal in iti ative. On the other hand, he would get lost in detai ls beyond his level.

<p style="text-align:center">*</p>

Not less rich is the information from the interviews regarding the attracting of youth or specialists in summer campaigns. If the first invitations were made by Gusti himself, the recruiting of the future monographers became soon enough the task of close collaborators. For talented students, Gusti had a special strategy. He viewed a professor's vocation – according to Stahl's testi mony – as a 'breeder of colleagues'. 'This was precisely his great quality as a professor, that he knew how to persuade his students that they were his collaborators, and to convince each and every one of them that they had a task, a personal mission which they were responsible for. Gusti was a great educator! He is the only professor – and in my career I saw many - he is the onl y one who had this q uality'. (Rostas 2000: 53)

The recruiting practice for monographic campaigns was more comprehensive and differentiated. After one or two years, as professionalization advanced, the campaign could become an aptitudes test. Stahl relates: 'I did the recruiting ... Let me tell you something. In the field, you immediately realize if someone is or is not. .. if it's worth engaging them or not. (...) And, moreover, [there were] those who gathered in groups. Mitu Georgescu, for instance, managed to gather around him all those concerned with demography and all those concern ed with medicine. And there were many male and female medical students. That one was a special group'. (Rostas 2000: 249)

Beyond Gusti 's convincing force, I tried to find more tangible reasons within the research. Starting from my own field experience[28] and remembering the various motivations for participation of my professors and colleagues, I supposed that it could not be otherwise for the monographic campaigns in the interwar period.

One monographer - Marcela Foca -daughter of an admiral, motivated her participation through changes between her generation and that of her parents: 'We were bourgeois and went on holidays like Techirghiol or somewhere like that ... it was so boring and monotonous. And here there was such a freedom from all prejudices and these social conventions, do you understand? Already my generation, compared with my mother 's, had taken a huge leap. [...] As a result, these research trips were a new and thrilling experience for us all'. (Rostas 2003: 123-124)

The Bessarabian Roman Cresin discovered the integration channel in Bucharest 's intellectual circles: 'If I was able to get in the Bucharest intellectual circles, it was all because of the monographic campaigns. I came from Bessarabia all alone; I didn't have any other opportunity to get in. And then, suddenly, I had new perspectives. So what Gusti did for me is immeasurable '. (Rostas 2003: 97-98)

But not only the freedom and the penetration in the intellectual elite were attractive for the youth. Some wanted from the very beginning to research the village and had personal projects in this respect. Gusti 'was content -Stahl said – if within the boundaries of his conception you would do your own thing. He knew, for instance, that, when I work in teams, I rigorously apply his conception. This was my task within the collective. But Gusti also knew very well that I pursue my own interests. And he didn't mind; on the contrary, he helped me as much as he could'. (Rostas 2000: 91-92)

After 1934, with Gusti's taking over the Foundation' s leadership, the recruiting method changed. The criteria of the student royal teams that Octavian Neamtu was in charge of were different, and so were those of monograph ic research, for the teams recruited by H. H. Stahl, Traian Herseni and Anton Golopentia. With the introduction of the Social Service Law in 1938 the selection of youth for the team commander schools was

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[28] During my studies at the Babe-Bolyai Un iversity I gathered (between 1967- 1969) field experience in sociological research in the Garbou village. (Salaj county) Jn the context of sociology's rehabilitation it was the first student team sent to the field.

necessary. As the Social Service was also invented for countering the le-
gionary movement, the selection aimed at the preparation of youth loyal to
the Foundation and to the king. For the monographic teams, the recruitin g
method did not change radically. Youngsters interested in knowledge gath-
ered around H. H. Stahl, A. Golopentia and T. Herseni.

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With the consolidation of the school, it is interesting to follow the struc-
turi ng, the emergence of small groups and interpersonal relationships. For
the first three monographic campaigns we can also talk about a formal
group, in which the connections were mainly professor – student or pro-
fessor – collaborator, but since Fundul Moldovei in 1929 the 'us', or the
monographer's conscience conspicuously appeared.

The repetition of these campaigns every summer, the ritualization
of some actions, the emerging of an organizational culture obviously led
to the crystallization of some group connections between monographers:
'Because at Fundul Moldovei we realized there were a few of us who had
more experience than all the others: Vulcanescu, Mitu Georgescu[29] , Xenia
Costa-Foru [30] and I. And the others called us "the elders". The Elders of the
Monograph' (Rostas 2000: 85), Stahl recalls.

But this 'amusement' covered a social reality: the transformation
process of a crowd into a structured collectivity. Beyond these relation-
ships there appears the perception of some interaction differences based
on social origin. Marcela Foca even launched the hypothesis that between
the two parts of the team in Fundul Moldovei there was a difference of
social origin: 'There, we divided ourselves into two camps: the "Upper
Fundu" and the "Lower Fundu." They published a magazine [31], those from

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[29] Georgescu, Dumitru (Mitu), after medical studies he is attached to the Sociological
School in Bucharest and studies the med ical and hygienic dimension of the village. In
paralle l he is interested in statistics and becom es deputy director of the Central Institute
of Statistics. In 1947 he is demised because of ideological reasons, after which he entirely
dedicates himself to medical statistics.

[30] Costaforu, Xen ia (1902- 1983), sociologist, collaborator of the Gusti school since 1937.
In the USA she specialized in social work. She was the co-founder (together with Veturia
Manu ila) of the Superior School of Social Work where she had been teaching since 1929
until its dissolution i n 1950. She is the author of the volume *Cercetare monograficii a
famili ei* (Monographi c Research on the Family) of 1945.

[31] Humoristic magazine.

"Lower Fundu" published one too. "Upper Fundu" were all those around Gusti, whom Gusti had friendlier and more professional relationships with: Stahl, Nel Costin, Mitu Georgescu, Vulcanescu. "Lower Fundu" were the younger ones, less hornogenous, the underprivileged and the half-witted , with no personal charm' (Rostas 2003: 129-130).

Even if Stahl did not subscribe to the social character of differentiations between the two strata in the campaign at Fundul Moldovei, it is obvious that after four years of research more circles were formed around Gusti.

Even in the so-called period of the 'monographic crisis', after 1931, the 'Upper Fundu ' group which was intersecting the intellectual bohemia of Bucharest, often met in the homes of Mac Constantinescu [32] and Floria Capsali[33] both monographers .

The need for meetings, contacts, orality, characterized this generation – obviously, larger than the monographers ' group. The informal discussions of this period gave birth to some unexpected cultural movements. [34]

<div align="center">*</div>

The yearly growth of the participants' number in the summer monographic campaigns did not produce only formal and informal groups, but also various conflicts. It seems that the *'gustieni* [35] were capable of observing the social conflicts, but not those in their own back garden. Just like the impact of the teams on the villages was not researched, these phenomena of internal collision did not concern the *'gustieni '*.

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[32] Constantinescu, Mac (1900-1979, sculptor, graphic artist, decorator, participant in the monographic researchers starting with 1928. University professor at the Institute of Fine Arts in Bucharest.

[33] Capsali-Dumitrescu, Floria (1900-1982) participated in two monographic campaigns and researched the folk dance. A distinguished dancer and choreographer, she had an important role in the evolution of the Romanian ballet through promoting the folklore-inspired dance.

[34] Criterion was an informal cultural association initiated by the critic Petru Comarnescu in 1932, for the debate of some actual themes in the political and cultural life of that time. The permanent members were Mircea Vulcanescu, Mircea Eliade, Henri H. Stahl, Constantin Noica, Cristian Tell and more collaborators from the young generation. Their symposiums continued intermittently until 1934.

[35] Name that the members of the Gusti school attributed to themselves.

Between the Village and the Authorities

Of course, a social history of a sociology school cannot be conceived without approaching the social environment of the group. In consequence, we have to see the village as a research object not only as described by the sociologists, but also as perceived by the village inhabitants in relation to the outsiders. It would be as important to know the opinions of the authorities regarding the activity of Gusti group. Obviously, in the circumstances of the 80s, I could not interview the actors of the two categories. But I sought to approximate the reaction of the environment through mediated information.

First of all, it should be reminded that Gusti, from the very beginning, involved the State, the political elite of the time, in the Association for the Study of the Social Reform founded in 1918, then in the Romanian Social Institute. Even if he was criticizing them, he did not want to study and reform the village, the culture and education of the Romanian village in parallel and against these entities. For this very reason he waited, asked support for his actions and organization from highly placed spheres. In the 20s, the sponsoring of Gusti's actions was weak and diffuse. As Stahl witnessed, the Professor managed to raise the necessary money for the campaigns from 'who knows where'. Only in 1930 a funding perspective for Gusti's plans showed up, as the Professor had been assigned positions in the leadership of some high-level state organizations. But the assistance given by the monarchy was more noteworthy. With the experience of seven monographic campaigns, as a minister (1932-1933) he could develop a law of Royal Foundations in consensus with the royal cultural doctrine and his own research and cultural reform plans.

Gusti's relation with Carol II was not impersonal, in the sense of codified Western bureaucratic relations, when someone opens some funding sources for some services. This relation was based on personal trust, not on rules and regulations. In this sense, Stahl's testimonial is significant: 'Gusti came with a new conception, but the route had already been laid down by King Carol. He had this ambition of being some sort of Brancoveanu of Romanian culture. (.....) I recall, when we worked at the Village Museum, he used to come and see how things went. And I would tell him: "We would like to extend here and there, but there's a fence here and Dombrovschi – the mayor – doesn't allow us to tear it down." King Carol lifted his foot, placed it on the fence and pushed it down. 'Tell him *I* tore it down!" [...] This was the kind of relationships you had with him. Very

human. Straightforward, with no attempt of making use of his prestige, he was -how should I put it? -you were his collaborator, that's how you felt ' (Rostas 2000: 110).

This portrait, this relation between a sovereign and the Gusti school has multiple meanings. The support in the building of the Village Museum with both theatrical and debatable acts from a legal point of view reveals a sympathetic attitude towards the *gusti§ti* '.

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The relationship between the village and the Gusti school was, from the very beginning asymmetrical. The researchers 'descend' deeply in the country, in the village – from the high summits of science in Bucharest. Their work is considered as a help given to the peasant *ob§te*[36] which must be grateful to them. Thus, even if, formally, the village was not a social unit subordinated to the School, symbolically, it was so to a great extent. Certainly, the monographers and later the royal teams did have an *ab ovo* sympathy for the fate of the village. But, the ideological pre-conception according to which the village is both a reservoir of 'national authentic culture' and a social space in need of emancipation stopped the *gusti§ti* from efficiently getting rid of the missionary attitude regarding research and cultural assistance.

Instead of Concluding

Two aspects deserve to be recalled. First, we have to mention that, after the postponing of the International Congress of Sociology, prepared and planned for end of August 1939 in Bucharest and the suspension of the Social Service because of the outbreak of the war, Gusti and his collaborators, adopting new survival strategies, continued to edit their works. They would not have abandoned their vocation not even after August 23, 1944, but in 1948, administratively and brutally, the school and the sociology as science were dissolved. Eight members were imprisoned, among which three died in prison. The others were forced to change their profession. After fifteen years the re-habilitation of the school followed.

Secondly, T h ave to m ention that the oral history project unfolded in the last decade of the communist regime, when shadow was cast again

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[36] Private property of the local community, dating back to the 15th century.

on the Gusti school. After the change of the politi cal regime it did not fare better, either. The Manicheism of the 90s did not leave place for nuanced descriptions. The revival of a balanced analysis, based on various sources, started only after the year 2000.

© Zoltan Rostas. Dragu (Fagara county), 1929.
Young monographer sociologists bathing at the village mill.
The first three: Xenia Costa-Foru, Henri H. Stahl and Mircea
Vu lcancscu.

© Zoltan Rostas, Dragu (Fagara county), 1929.
Work Reunion with Henri H. Stahl, Constatin Brailoiu, Di-
mitrie Gusti, Ernest Bernea (stand ing) and Traian Herseni
(standing).

© Zoltan Rostas. Cornova (Orhei county) 193 1.
Peasant party after the weddi ng.

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Conclusion: Spheres of Intimacy within the Field

Raluca Mateoc

The volume proposed a reflection on ethnographic fieldwork as initial experience and ways in which it impacted subsequent research paths. Ten anthropologists who studied everyday socialism and post-socialism in Eastern and Central Europe look at these societies as *Gesellschaft* and examine the changes in social structures, roles, positions as well as ruptures, confrontations, tensions, latent conflicts and negotiation practices between collective and individual agents. Some chapters are bound to fields based in one country (Romania, Bulgaria), while others look at more (Sicily, Bulgaria, Malaysia), revealing what informs their choice and what unites and separates them. All chapters address universal field-related challenges:entering the field, approaching 'informants', living in the everyday field atmosphere or institutional constraints and possibilities. Thus, fieldwork is represented as a physical and mental space of being, professionally and existentially, of learning and unlearning. Certainly, the volume mirrors the choice of privileged topics for a generation of researchers starting fieldwork in the 70s and 80s in Eastern and Central Europe: systematization plans, music, collectivization of agriculture, everyday social relations, kinship or oral history. In my conclusion, *I* propose to examine spheres of intimacy coming out from everyday interactions, while depicting two discursive realms in the field narratives: references to everyday socialism and post-socialism, and methodology.

Literature Review

Fieldwork in socialist and post-social i st societies was approached in relation to specific ethical and methodological challenges. Giordano, Rilegg and Boscoboi nik (2014) examine whether post-socialism as a concept can adequately apply to the current situation in Eastern Europe and show that specific elements derived from postcolonial studies may prove most useful to analyze Eastern Europe's post-soc ialist countri es. The contributions in

De Soto and Dudwick (2000) deal with difficult ethical and methodologi-cal issues when examining some aspects that the people in the countries under study (from East Germany to Uzbekistan) experience: poverty, mis-trust, domination by the West, ethnic conflicts or nostalgia for the past. The authors discuss two approaches for doing research – the anthropolo-gist as advocate and as researcher – and consider that the dilemma seems to be finding the right balance between advocacy and deconstruction, dis-tance and friendship. Schauble (2006) illustrates the significance of imag-ination and emotion for post-socialist identity and their role within current political processes, in a selection of communities from Poland, Romania, Hungary, Georgia, Serbia and Croatia. Hann, Humphrey and Verdery (2002) point to the rupture and continuity in the topics for anthropologists and other disciplines in the post-socialist period all over the world. Land privatization, privatization in general and moral justice, markets and con-sumption, policies based on the transfer of Western models, development of local administrative and political institutions, ethnicity, nationalism and 'minority rights', ritual and religion, engagement with history are some of the topics privileged for the study of post-socialist societies. The authors also believe that the post-socialist setting invites a re-consideration of con-cepts such as *civil society* or *embeddedness.* At the same time, they show that anthropologists are professionally obliged to recognize the moral com-plexities, to respect as well as critique the world that has now (almost eve-rywhere) disappeared. Kilrti and Skalnik (2009) focus on political and economic issues, transnationalism and the alternative music scene in postsocialist Europe, and collect for this purpose local ethnographies, or anthropological perspectives 'from home'. Hann (2015) investigates three periods reflecting macro-societal turning points of Hungarian history and their consequences at local level. By doing so, he connects the materialities of political economy with the subjective experiences of inhabitants, which he calls the 'social imagery'.

While narrowing the focus to the area approached by the contribu-tors to this volume, I outline studies on everyday fieldwork challenges. Verdery (2018), by investigating one slippery but powerful apparatus of the Romanian socialist state, the Securitate, provides a fascinating ethnog-raphy of this state. Kideckel and Sampson (1984) reflect on political, prac-tical and ethical aspects of their fieldwork in Romania which unfold through a mixture of learning and unlearning. They discuss the acceptabil-ity of their topics - domestic economy, mountain peasants, urban planning,

agricultural collectivization, and ethnic minoriti es -and their possi bly sensitive nature. [1] Beck (1986) addresses the prob lems related to the study of ethnic groups in Romania. Silverman (1986) explores the adaptations of contemporary Bulgarian Gypsies in terms of government policy, economic niche, and cultural roles while in the 70s, the ethnic category 'Gypsy' was abolished. Later reflections of fieldworkers from the 'Romanian Research Group' look back at their ethnographies. Kideckel (2018) looks at how transportation and mobility model the character of Romanian - American interaction during fieldwork from the mid - 1970s to the mid - 1980s. Beck (2018) .gives a biographical account of his work in Romania and the influence it had on his subsequent research and suggests that anthropologists have a moral imperative they must carry out when they choose to conduct research among the most vu lnerable in society. Sampson (2018) discusses the challenges of researchers studying small , insignificant places, particularly when our specific knowledge pu shes us to become generalists.

Fieldwork and Intimacy

My conclusion proposes a conceph1alization of fieldwork in relation to intimacy. Anthropologist Niko Besnier sees the strength of intimacy in its elusiveness. Intimacy, he suggests, means nothing independent of a context, but once thi s context has been established, it serves to help people classify, characterize and understand human activity. It is th is semiotic complexity that bestows its slippery quality, but it is preci sely this indeterminacy that makes intimacy fascinating as an ethnographic object of enquiry. (2015: 109) Stoler (2006) shows that intimacy is not so much something that can be measured by physical distance as it is the degree of involvement; engagement, concern, and attention ones gives to it. At the same time, intimacy forges distan ces and proximiti es as well as bonds and attachments and creates new meanings (Stoler 2008). Fieldwork recollection s allow an exploration of inti maci es as they account for representation s of emotional and social relationships. First, I look at how reflections on everyday socialism and post-socialism contain spaces of intimacy at the level of the studied societies. Then, accounts on methodolo gy can mirror the unfoldin g of human relation ships between fieldworkers and studied

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[1] D. Kideckel and S. Sampson refer to topics of fieldworkers from the UMASS 'Romanian Research Group'.

subjects and related multi-level impacts. Prior to this exploration, I provide an overview of the chapters.

Some chapters focus on fieldwork in a single country. Gail Kligman reflects on her experiences in Maramure, a region in the far north of Romania, from the end of 1977 to the collapse of Ceau escu regime in 1989, by exploring three key aspects of the social construction of her identity: an American, a single woman and a secular Jew. Katherine Verdery recalls how she gradually learnt to do fieldwork in Aurel Vlaicu, Romania, where she continuously conducted research from 1974 until the 1990s. She gradually learned that ethnography demands a continuous desire to listen to people, and at the same time to use herself-her reactions, her sentiment s-as an instrument for knowing. Gerald Creed focuses on how communist agricultural policies affected village household economies in the late 80s in Bulgaria.His fieldwork revealed something about the nature of socialism and its susceptibility to social relations. The participation in shepherding, hoeing, pruning or harvesting helped him appreciate the link between the socialist state sector of the village economy and the personal and household economies of villagers. Carol Silverman describes how Balkan music became contingent to the state regulations in Bulgaria in the 70s -change of names, forbidding of performances and of clothing. Thus, the state became an important_ and unavoidable element in her research. She formulated her research so as to explore what the state endorsed and what it censured and why and analyzed what became official folklore, for example what was presented at folk festivals, and what was omitted. Steven Sampson studied urban systematization in Feldioara, Romania and showed how his first field experiences in the early 80s, both in the village and at the party school impacted his professional experiences. He names these experiences 'ankle bracelets' or 'tattoos' – as they were a lifetime lesson on the meaning of informal networks as a way oflife. Zoltan Rostas maps the social history of his own research experience in the 80s in Romania, following the idea and need to conduct research on his own. He takes an unconventional approach of the history of the Sociological School in Bucharest, by interviewing aged intellectuals that collaborated with professor Dimitrie Gusti. He did not focus on the meanings of the works published by this school, but on the researcher's lifestyle and examination manner when studying the interwar village. Gheorghita Geana looks back on his professional initiation in anthropology as a philosopher, when the problem concerned not only the different types of knowledge but espe-

cially the specific methodologie s. He presents three aspects of the institu-
tionalization of cultural anthropology in Romania: the tension of institu-
tional ization, fieldwork as a (pre)condition of professionalization , and the
key of field research : participant observation.

Other chapters focus on fieldwork conducted in multiple places.
Christian Giordano'!' conducted long-term empirical research in three d if-
ferent locations, i.e. Sicily, Bulgaria and Malaysia over half a century,
dealing with highly different subjects: Sicily -the social representation of
the state, Bulgaria -the re-privatization of agriculture in the post-sociali st
scenario and Malaysia – the local political elites as skilled managers of
unity i n separation of three ethnic communities -Chinese, Malay, Indians.
However, the three fields are linked by a political anthropology of the
elites. Franyois ROegg's contribution is a story of travelling spaces while
following his interest on the habitat and vernacular architecture. He made
his first field trip in Romania as a student in the 70s and landmarked the
habitat styles, guided by two ways of being – mobi lity and spontaneity.
After similar 'mobile fieldwork' in Yugoslavia and Poland, he eventually
turned to history and travel literature about the Austrian colonization of
Western Romania in the 18th century, which had exported its own models
of colonial architecture. In the 90s he turned his interest to Roman ia where
various minoriti es were affirming themselves in the EU pre-adhesion con-
text. Overall, his research was guided by the comparative and interpreta-
tive methods. Peter Skalnik reflects on his 'first proper anthropological
fieldwork' in Sunava, Northern Slovakia, 1970-1975, upon his passage
from fieldwork in West Africa. Nevertheless, starting with the Slovakia
experience he has developed *a life is fi eldwork* phi losophy or attitude
which means that he perceives his life as a continuous fieldwork experi-
ence.

Spheres of Intimacy in Representations of Socialism and Post-socialism

In representation s of socialism and postsoci alism, fieldworkers depict con-
cerns of everyday life, roles of relationships or ways of accommodating
the state. At the same time, they show how these realities impacted the
unfold ing of fieldwork. I discuss these representations in relation to three
countries where fieldwork unfold ed: Roman ia, Bulgaria and Slovakia.

While recalling her fieldwork in Romania, Katherine Verdery shows how, particularly under socialism, relationships were crucial to getting anything done: to lower-level Party officials needing patronage, to higher-level Party officials needing clients, to city-dwellers needing reliable sources of food, to villagers needing help with weddings or funerals or with schooling their children in town, and so on. In a similar vein, Steven Sampson writes that socialism was a system that requi red people to spend their time and energy trying to figure out how to cope, how to influence an official confronted with vague or contrad ictory regulations, and how to find means around restrictions. His struggle to obtain access to various documents and to attend meetings began to tattoo itself on his entire approach to how he understood life in Feldioara, how he understood Romanian planning, and eventually how he understood East European *'real socialism '.* Gail Kligman refers to realities such as deliberate unavailabil - ity of contraceptives in Romania and how she was sought for assistance in thi s respect. She draws on how being an American, a single woman and a secular Jew impacted everyday life situations. For example, in Ieud, 'American ' encompassed the class and status distinctions associated with 'domni', whether local , from urban cities, or foreign countries. Franc;ois Rilegg, in parallel with studying the country's habitat, discovered the real socialism in Grozaveti campus, Bucharest, and beyond. Namely, the dormitories full of Romanian students, while the foreigners had an individu al room , the queues for buying food, the voluntary work *(munca pa triotica)* of students in autumn, the propaganda banners on public edifices, the Party's shops, censorship etc. Zoltan Rostas describes the 80s as a time when a series of laws were enforced which , on one hand, consolidated the forced development of industry, and on the other, stroke the human rights: one law stipul ated that one family could only owe one house, and the other, 'regulated' the citizens' j ourneys abroad. Gheorghi ta Geana shows that under the communi st rule, the social life of religion was aggressively controlled and officially isolated in order to restrict its public manifestations to specific spheres; word s like 'ch urch ', 'God', 'Christian/ity' etc. were usually censored, and the Christmas and Easter feasts took place under a total silence of the mass media.

Fieldworkers in Bulgaria recall at their turn the social life of the studied subjects and their adaptation s to socialism, processes and procedures for getting to the field and the persistence of social relations from the old system through post-socialism. Carol Silverman shows how the relatives of one informan t, in addition to having state job s, were involved

in the sale of building materials, clothing, horses, brooms, and foodstuffs at various times during a decade of socialism. At the same time, state policy in the 1970s and 1980s supported mono-ethnism and Bulgarization and severely regulated the display of Muslim ethnicity. By the mid-1980s wedding musicians faced a coordinated program of prohibitions, fines, and imprisonment. The author points at the cracks in the state dogma - police officers arrested musicians but secretly loved kyuchek; wedding musicians not only resisted but also accommodated to the state. Gerald Creed shows that the research he conducted in a Bulgarian village provided the basis for his understanding of life under late socialism, but in some ways the effort to get to the village, and his early struggles once he got there, also taught him a lot about the socialist system. For example, when recalling the access to the field, he reveals a typical pattern of socialism in keeping processes or procedures opaque so that citizens could never be certain that particular actions would produce desired results. Christian Giordano·j· shows that the re-privatization of agriculture in Bulgaria was without peasants because the existing social relations mirrored the socialist ones but looked like capitalist ones. In sociological terms, the social actors of the old system were, rather surprisingly for them researchers at least, the same ones of the new system.

As for the research in Slovakia, Peter Skalnik shows that in the initial phases of the research, one had to be very careful. The topic of his research was officially known as 'social relations' or 'social transformations during socialism ', and even so it was suspicious. He recalls the most interesting parts along the research , for example the tension between cooperative members and private holders.

Post-socialism is approached in relation to specific transformations for the studied people and places. The authors reveal the decline in the economic situation, the increase in violence and the possibility of migrating. Then, in terms of the land reform, the post-socialist policy was a sort of *back to thefitture,* i.e. the new elite in power having a rather populist and unlikely vision of a return to the land since the land reform in the early 1990s provided for the restoration of property boundaries to what they had been before the socialist era. At the same time, post-socialism is a time when the studied subjects become more vocal about the problems of socialism, the researcher being able to collect additional information on previously sensitive topics, such as resistance to collectivization . The anthropologist comes to the village not as a researcher but as an old acquaintance or friend, or a visitor coming for vacation.

In narratives of socialism and post-social ism the intimate is shaped by everyday adaptations of the individual taking place on different scales and sites of power (villages, institutions, temporary networks). At the same time, what takes place on a large scale, such as that of the state, reflects what takes place on a more modest scale, such as that of the household or the solving of everyday needs (e.g. Narotsky and Besnier 2014). Michael Herzfeld's (2005) concept of intimacy refers to aspects of identity making processes that are reproduced at the national, regional and local levels and that mutually create one another on these different scales, even though they may have d ifferent implications i n these various manifestations. At a micro-level, we see the crucial importance of relations, the figuring out of ways for solving the needs. Intimacy stands as a space of human bonds, as well as a site of power in a specific setting of uncertainties.

Methodologies and Spheres of Intimacy

In accounts on methodology , fieldworkers relate to the impact of their work on the studied subjects, perceptions of long-tenn fieldwork, or the total role of the self in the capacity to form relations with people. I discuss the methods recalled by the authors in relation to the studied countries.

Fieldworkers in Bulgaria (Silverman, Creed) reveal that long-term residence and partici pant observation was not common for any research in the 70s and 80s. At the same time, requesting to live with a village family was an added difficulty since it was illegal for a foreigner to spend the night with a Bulgarian citizen. (Gerald Creed) The greatest inconvenience was the suffering caused for the collaborators: their surveillance, fines, interrogations, and emotional stress. (Carol Silverman) The author further shows that the access to the music of Roma, the rare record ing she could make, the hidden practice of Roma wedding musicians, and the state coming out in its intimate forms are precious research outcomes. Christian Giordano"!, in his fieldwork on the new relations of property in post-socialist Bu lgaria used field free forms interviews, i.e., without questionnaires, which were very thorough and detailed, especially concern ing his theme. In the end, by deciding to study a smaller number of new agricultural entrepreneurs, a particularly forthcoming one who later became primary informant was selected.

Fieldworkers in Romania first point to the power of the participant's observation in the field. Fieldwork involves observing how Romanians negotiated an economy of shortage and bureaucratic regulations, as

well as interactions and confrontations with bureaucratic actors, e o s
obtain access to documents or attending a meeting or making a visit. It is
a way of understanding how Romanians use informal networks to negotiate their world. (Steve Sampson) Gail Kligman's thirteen months
ethnographic research in Ieud consisted of intensive participant observation, semi-structured in-depth interviews, and daily informal discussion
She recorded ritual events and interviews unless asked not to and points
out that it is important to keep in mind that her research was done in the
context of a surveillance state. Gheorghita Geana, when speaking of conducting participant observation, shows that in a totalitarian state the suspicion around the researcher as intruder nourishes itself from the life fact
commonly shared by the researcher and the researched. At the same time
he views participant observation as a solution to the aggression in anthropological knowledge.

Second, fieldwork in Romania was conducted by using the *comparative and interpretative method*. Franvois Rliegg's study on the 'Gypsy
palaces ' *(palais tsiganes)* brought him back to his work on the habitat
through the lens of social representations. The method remained the same
as in his previous work, i.e. an attempt at interpreting the social transformations of Gypsies and of their habitat, while criticizing the usual ethicizing, denigrating, or emphatic but always pitiful discourse on the Roma.
The comparative and interpretative method were his main resources for
rendering the research interesting, i.e. significant and stimulating, beyond
any pretention to objectivity or exclusivity.

Third, Katherine Verdery shows that, in ethnographic fieldwork,
our principal work instrument becomes ourselves and our capacity to form
relations with people. As a method of work, it is at the same time both
enjoyable and very difficult. Essential to its success is the receptivity of
the people we work with. Because in her experience, Romanians have a
true genius for creating social relations with others, including foreigners,
she was lucky that she went there. Vlaicenii enabled her to make use of
her principal work instrument: herself.

Fourth, Zoltan Rostas reveals the power of the oral history method
when conducting research in Romania in the 80s. This method had an anti-establishment ideology as well, as it aimed at those groups, layers, categories that ended up in marginality and that usually don't leave written traces,
or otherwise their written traces had been elaborated by the authorities.

Peter Skalnik's work in Slovakia reveals at its turn the importance of participant observation. His research was conceived as social anthropological, that is, it followed the principle of holistic knowledge of social relations gained during the time of research. Gaining authentic sets of data through a synchronic approach presupposes as much participant observation as possible. He concentrated therefore on events that happened spontaneously in front of his eyes or those about which he knew in advance because they were included into the ritual or agricultural calendar or otherwise. He also shows that trust was the most important principle, for example, never referring to one informant what is learnt from another.

Finally, I note the endorsement of a distinctly empirical pluralism by Christian Giordano·l, which, however, is also a legitimate justification for his personal choices since, in fact, he has not circumscribed his researches to a single field.

In recollections of methodology, intimacy stands as an intersubjective circulation of emotional essences between researcher and informants. It is a source of comfort and safety, of lifelong friendships, and a realm where the self of the fieldworker becomes the main working instrument. Fieldworkers become entangled in ties of intimacy through gossip, friendship, or the discovery of selves, and it is particularly these ties which give them meaning. Equally, intimacy can be a source of danger, unpredictability and violence, viewing the risks that the researcher might put his / her interlocutors into. When looking at intimacy in this double way, we are also able to depict the things which matter in the everyday fieldwork.

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Steven Sampson, Professor (emerit.) at Lund University, received his Ph.D. in Anthropology from the University of Massachusetts, Amherst in 1980. Following his doctoral research on socialist planning in Romania, and he has researched several aspects of the informal sector in Eastern Europe.Beginning in 1992, he also began to work as a project consultant for programs in civil society, administrative reform, human rights, NGO development and democracy export. This work took back to Romania, as well as Albania, Bosnia and Kosovo. His most recent research is on rise of the anti-corruption industry and the role of ethics and compliance in private fitms. Among his publication s are the articles 'Rumours in Socialist Romania ' (1984), 'The Informal Sector in Eastern Europe' (1986), 'The Social Life of Projects: Importing Civil Society to Albania' (1996), 'Trouble Spots: Projects, Bandits and State Fragmentation' (2003), 'The Anti-Corruption Industry: From Movement to Institution ' (2010), 'The Right Way: Moral Capitalism and the Emergence of the Corporate Ethics and Complian ce Officer' (2016) and a co-edited volume *Cultures of Doing Good: Anthropologists and NGOs* (2017).

Carol Silverman, Professor of Cultural Anthropo logy and Folklore / Public Culture at the Un ivers ity of Oregon has done research with Roma for over 30 years in Bulgaria, Macedonia, Western Europe and the US. She explores pol i tics, music, human rights, gender, migration and state policy with a focus on representation . Her 2012 book *Romani Routes: Cultural Politics and Balkan Music in Diaspora* (Oxford), wh ich won the Merriam Prize from the Society for Ethnomusico logy, analyzes how Romani music is both an exotic commodity in the world music market and a trope of multiculturalism i n cosmopolitan contexts. Her recent research , supported by the Guggenheim foundation, examines the issues of migration , representation, labor, and appropriation in the globalizat ion of Romani culture. She is now preparing Balkanology (Global 33 1⁄3 Seri es, Bloomsbury Press) on the history and politics of Bulgarian wedd ing music. She has also written numerous atiicles, incl uding : Promises and Prospects of the Romani

Women 's Movement in Central and Eastern Europe, in *The Romani Women's Movement: Struggles and Debates in Central and Eastern Europe* (2018); From Reflexivity to Collaboration: Changing Roles of a non-Romani Scholar/activist/performer , *Critical Romani Studies* (2018); Community Beyond Locality : Circuits of Transnational Macedonian Romani Music, in *Routledge Companion to the Study of Local Musicking* (2018), Oregon Roma (Gypsies): A Hidden History, *Oregon Historical Quarterly* (2017); Gypsy/K1ezmer Dialectics: Jewish and Romani Traces and Erasures in Contemporary European World Music, *Ethnomusico logy Forum* (2015); Macedonia, UNESCO, and Intangible Cultural Heritage: The Challenging Fate of Teskoto, *Journal of Folklore Research* (2015). For many years she has worked with the US NGO Voice of Roma on activist projects, includi ng educational music festivals. She was recently appointed co-curator of the music division of the new international digital RomArchive. She is also a professiona l vocalist and teacher of Balkan Romani music.

Gerald Creed is Professor of Anthropolo gy at the City University of New York, with a joint appointment at Hunter College and the Graduate Center. He is a speciali st on agrarian political economy, ritual and identity in Eastern Europe, specifically Bulgaria, and his fieldwork spans both socialist and post-socialist eras. His first major project exami ned the impact of collectivization, socialist agrarian reforms and subsequent privatization efforts on village and household economies. Thi s long-term research is synthesized in his book *Domesticating Revolution: From Socialist Reform to Ambival ent Transition in a Bulgarian Village* (Pennsylvania State University Press, 1998), which won the 1998 Book Award from the Bulgarian Studies Association. He subsequently completed another long-tenn project published as *Masquerade and Postsocialism: Ritual and Cultural Dispos - sess ion in Bulgaria* (Indiana University Press, 2011), which uses ancient fertility rites still popular in Bulgaria to challenge standard orthodoxies of postsocia list studi es, especially those regardin g gender, civil society, community and nationalism. This book won awards from both the Society for the Anthropo logy of Europe and the Bulgarian Stud ies Associat ion. He has also edited two volumes. The first, *Knowing Your Place: Rural Identity and Cultural Hierarchy* (Routledge, 1997) is a collaborative project with English Professor Barbara Ching on rural identity and the politics of place cross-culturally. He subsequently organized an Advanced Seminar for the

School of Advanced Research (SAR) examining the concept of community, again from interdisciplinary and cross-cultural perspectives , the results of wh ich were publish ed as *The Seductions of Community: Emancipations, Oppressions, Quandaries* (SAR Press, 2006). His current research builds upon an earlier interest in socialist nostalgia to address how cultural commitments influence experience and practice after the associated social forms have transformed or disappeared. He is President-elect of the Society for the Anthropology of Europe.

Gheorghita Geana, PhD, is Senior Researcher (between 1984-2015: head of the Department of Cultural Anthropology) at the "F. I. Rainer" Institute of Anthropology of the Romanian Academy. Since 1991 he is also Professor of anthropology at the Univers ity of Bucharest (faculties of Philosophy and Sociology). His contributions range both in the domain of field research and in theory. As a fieldworker he conducted researches in several regions of Romania (Vrancea, Arge, Alba, etc.) on such themes as community stud ies, micro-demography , kinship, rites, values, world view. As to the theoretical approach, he is interested especia lly in ethnicity and globalization, history and epistemology of anthropology, anthropology of arts. He is the author of *Antropologia culturala: Unprofil epistemologic* [Cultural Anthropology. An Epistemologica l Profile], Bucureti, Criterion Publishing, 2005. He also contributed to several books and journals printed by Romanian and international pu blishing houses (Editura Academiei Romane, Routledge, Cambridge University Press, Wiley, LIT Verlag, Slidosteuropa-Gesellschaft). Among such contributions are: "Discovering the whole of humankind : The genesis of anthropology throu gh the Hegelian looking-glass" (in: Han F. Vermeu len & Arturo Alvarez Roldan, eds, 1995, *Fieldwork and Footnotes: Studies in the History of European Anthropology,* London & New York, Routledge , pp. 60-74); "Ethnicity and globalisation. Outline of a complementarist conceptualisation" *(Social Anthropology,* 5 [2], 1997: 197-209); "Enlarging the classical paradigm : Romanian experience In doing anthropology at home "-includin g the idea of "circles of otherness" *(Anthropological Journal on European Cultures,* 8 [2], 1999: 61-78); "Remembering ancestors: Commemorative rituals and the foundation of historicity "-an attempt to ground philosophy of history on ancestors' cult *(History and An thropology,* 16 [3], 2005: 349-361); "The phenomenological programme and anthropological research. A mutual mi rroring ", in: Tomasz Rakowski and Helena Patzer, eds, 2018, *Pre-*

textual Ethnographi es. Challenging the Phenomenological Level of An- thropological Knowledge-Making, Canon Pyon, Sean Kingston Publish- ing, pp. 40-57.

Zoltan Rostas is an emeritus professor of sociology at the University of Bucharest, having also taught at Sapientia University (Miercurea Ciuc, Ro). Since 1981, he is conducting research in the field of oral history; he has an interest in the hidden multicultu ral everyday life of people, with a special focus on social history and the history of sociology during the Ro- manian interwar era. After 1989, he developed an interest in the study of everyday life under the socialist regime and in the rebuilding of the Roma- nian sociology after 1965. His books include: *Monografia ca utopie,* In- terviu cu Henri H. Stahl [Monography as Utopia. Interviews with Henri H. Stahl], (2000), *O istorie orala a Seo/ii Sociologice de la Bucure$ti* [An Oral History of the Bucharest Sociological School], (2001), *Chipurile ora§ului. Istorii de via/a din Bucure$ti. Secolul XX* [The Faces of the City. Bucharest Life Histories. Twentieth Century] (2002), *Sala luminoasa. Primii monografi$ti ai $CO!ii gustiene* [The Bright Room. First Mo- nographers of the Gustian School] (2003) *Atelierul gustian. O abordare organizationala* [The Gustian Workshop. An Organizational Approach] (2005), *Parcurs ſntrerupt. Discipolii §Colii gustiene din anii 30* [Inter- rupted Journey . Disciples of the Gustian School in the 1930s] (2006), *Strada Latina nr. 8. Monografi$ti §i echipieri gustieni la Fundafi a Cultur- ala Regala ,,Principele Charles"* [Latin Street no. 8. Monographists and Gustians teammates at the Royal Cultural Foundation ,,Prince Charles"], (2009). He also published a series of anthologies with the neglected writ- ings of the Gustian School. Together with his younger collaborators, he published several volumes of oral history. He has been the head of the Cultural Anthropology and Communication Department at the University of Bucharest and founder and dean of the Social Sciences Department of the Sapientia University. He founded and is currently leading the Gusti Cooperative, a group of independent researchers who are invested in stud- ying the history of the Romanian and East-European sociology from a so- cial history perspective. He is a member of several Romanian anthropo- logical and sociological associations.

Marc De Santis
State Legitimacy in a Fragile Context
An Anthropological Analysis of the Colombian Society
During the course of the last decades, the state experienced a revival on the scene of international development as there has been a growing acknowledgment amongst the international development community that the state plays a key role in enabling development in a specific society. Therefore, the role of the state and especially the concept of state-building have occupied a central place in the development discourse. In that respect, a growing interest has manifested itself in the discussion and analysis around so-called "fragile states". The author discussed the development discourse around that state-building paradigm in general and focuses through its field research in Colombia specifically on the question of the state legitimacy in so-called fragile contexts.

Barbara Waldis
Styles of Multiculturalism in Mauritius
A case study in education Policy
What does multiculturalism mean in Mauritius? This question was the starting point of an ethnographic study on an island state in the Indian Ocean that had always been part of a global project and always been (post)colonial. The introduction of citizenship education at school in this Republic with its ethnically, religiously and linguistically diverse population serves as an example for the analysis of how different approaches to multicultural policy-making collide. The negotiations on the school subject illustrate the organisation of cultural difference by the state mainly through Indo-Mauritian and Creole nationalism.

Barbara Waldis
Das cthnische Kalcidoskop Mauritius
Multikulturalismus beim politischen EinH.ihrungsprozess des Schulfaches Citizenship Education
Wie ist Multikulturalismus in Mauritius definiert? Diese Frage leitet die ethnografische Studie Uber den lnselstaat im lndischen Ozean, der seit Beginn der Kolonialzeit global ausgerichtet ist. Anhand der Einflihrung der ,,Citizenship Education" wird gczeigt, wie in dcr postkolonialen Republik mit ihrer ethnisch, religios und sprachlich gemischten Bevolkerung verschiedene Anstltze multi kulturel-ler Politik aufeinandertreffen. Die Analyse der Yerhandlungen Uber das Schulfach thematisiert die staatliche Organisation kultureller Di!Terenz <lurch den indo-mauritischen und den kreolischen Natio-nalismus.

Nicole Gallina; Katerina Gehl (Hg.)
Kultur der politischen Eliten in Ostcuropa
Neue Zugange zum Forschungsfeld

Andrea Friedl
Ethnizitlit als Kapital
lden ti Uitsmanagement tatarischer Jugendlicher in Kazan, Tatarstan

Andrea Friedli; Aline Gohard-Radenkovic; Franc,:ois Ruegg (Eds.)
Nation-building and Identities in Post-Soviet Societies
New Challenges for Social Sciences

Hana Horakova; Andrea Boscoboinik; Robin Smith (Eds.)
Utopia and Ncolibcralism
Ethnographies of rural spaces

Sonja Schi.iler (Ed.)
Exchange, Dialogue, New Divisions?
Ethnic Groups and Political Cultures in Eastern Europe

Marcello Mollica (Ed.)
Fundamentalism
Ethnographies on Minorities, Discrimination and Transnationalisrn

Christian Giordano
Powc l;Legitimacy, Historical Legacies
A Disenchanted Political Anthropology

Julien Bondaz; Florence Graezer Bideau; Cyril lsnart; Anais Leblon (Eds.)
Les vocabu laires locaux du « patrimoin c »
Traductions, negociations et transformations

Andrea Boscoboinik; Hana Horakova (Eds.)
The Anthropology of Fear
Cultures beyond Emotions

Sabine Haupt (Hg.)
Tcrtium Datur!
Formen und Facetten interkultureller Hybriditat. Formes et facettes d'hybridite interculturelle

Christian Giordano; Jean-Luc Patry (Hg.)
Fragcn! Antwortcn?
lnterdisziplinare Perspektiven

Christian Giordano; Franryois Ruegg; Andrea Boscoboinik (Eds.)
Docs East Go West?
Anthropological Pathways Through Postsocial ism

Flavia Cangia
Performing the Buraku
Narratives on Cultures and Everyday Life in Contemporary Japan

Waltraud Kokot; Christian Giordano; Mijal Gandclsman-Trier (Eds.)
Diaspora as a Resource
Comparative Studies in Strategies, Networks and Urban Space

Hana 1-lorakova; Andrea Boscoboinik (Eds.)
From Production to Consumption
Transformation of Rural Communities