THE STATE AND THE ANTHROPOLOGIES
OF THE STATE
(A POLITICAL ANTHROPOLOGIST'S TESTIMONY)

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Prologue

Why do we, social anthropologists, study the state? I presume that we want to know as much as possible about the origin, structure, function, the role in society, possibly also decline of the organization which at present (still) seems to be hegemonic in all what is human, if not natural and supernatural. At the same time we often forget that our work, thinking, fieldwork, writing up, teaching and public engagements are embedded in the existence of the state. While we would like to objectify the state by our scientific inquiry, we tend to forget that simultaneously we are objects of the state. In other words, the state dominates over our destinies as both humans and investigators.

Anthropologists, as other students of the state, face a potentially precarious situation. They are parts of the state and at the same time try to behave as if they are outside of it. In this piece I would like to point out the conditioning by the state of the work of political anthropologists such as myself but at the same time give thought to a possible import to theory of the state as a result of subject/object interplay. There are, perhaps, two extremes in this dilemmatic situation. In highly oppressive states hardly any political theory can emerge except apologetic. Critical thought is impossible. Seemingly paradoxically, in the most democratic states the degree of voluntary identification with the state could be so high that the research on the state produces laudatory, self-praising or narcissistic texts devoid of critical approach as well. Thus the best conditions for the study of the state are in countries which do not suppress independent research but where there are enough contrasts between the interests of the state and the society. I think that Simone Weil was right when she said, and was noted down by one of her students in the pre-war years, that

the secret of every state is its will to power and domination: "All power tends in the direction of making itself greater. The state has natural tendency to be totalitarian. That is seen everywhere" (Weil 1978: 158).

Anthropology was born as one of the products of the existence of the state. Specifically it was born in capitalist states that strove to extend its power by occupying and colonizing less militarily equipped societies, whether state-ruled ones or not. Although anthropology, similarly to other scientific disciplines, tries to present pure knowledge and be independent of the state and critical of it, anthropology cannot escape its dependence on the state as funder and censor, indeed a customer buying knowledge generated by anthropologists. In most countries where anthropology exists it is part and parcel of public institutions which are partly or fully funded by the state and increasingly the funder wants to know for what the money was used.[1] (Of course, the state as such has no money but it has power to collect and redistribute taxes that are then reaching anthropologists in the form of salaries, conference funding, travel allowances and research grants.) In the new neoliberal audit conditions there seems to be no place for "pure research" that used to be financed and evaluated without much more than academic peer reviewing although the demand of "relevance" for praxis or development is not new to anthropological projects. The state even more than before coerces the academy to be productive, to show results which can be useful to the state.

Of course the state is no *deus ex machina*, but, quite a human product. We may discuss how, where and why the state emerged, but the fascinating discovery that states did not exist during most of human history is due to the inquisitiveness of those who enjoyed research leisure because of the functioning of the state. Even those of us who would like to prove that the state should wither in the future do it in the framework of the existence of the state at present.

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1   In most European countries whether they had colonies or not, whether capitalist or so-called socialist, ethnography and folklore studies emerged hand in hand with nationalism. These disciplines such as Czech and Slovak *národopis*, German *Volkskunde*, Polish *ludoznawstwo*, Hungarian *néprajz*, Romanian *folclor*, Bulgarian *narodouka*, English *folk life studies*, Swedish *folklivforskning*, Dutch and Belgian Flemish *volkskunde*, etc. mostly studying their own nations and minorities survived till our times. They were directly expected nay requested to produce knowledge celebrating the nations and states which paid them to do so.

This paper does not constitute another attempt at resolving age-old disputes about origins, present role and future of the state but instead tries to attract attention on the state as a 'constant variable' of anthropological knowledge. In other words while the state as a principle stays hegemonic for several millennia there are different kinds of state to which anthropologists relate and react differently. In my experience, life has been a continuous field-work and this praxis has been evolving within the conditioning regulation by the state, or better, states. I shall not entirely limit myself to my own experience but will also reflect more broadly about the fate and status of anthropologies in different historical settings dominated by the state.

African States

My decision to study the state in Africa was informed by two important ramifications. On the one hand it was the almost sudden acquisition of political independence in many African colonies and dependent territories which took place when I was deciding about the direction of my university studies back in the early 1960s. Then, as a young man, I witnessed a virtual avalanche of newly formed states. Expectations were huge but often the most apparent change was that of the name, not so much of substance. Anglo-Egyptian Sudan became Sudan, French Sudan became Mali, Gold Coast turned into Ghana, Oubangi-Chari became Central African Republic, Moyen Congo changed into République du Congo. But many colonial state names did not change upon independence (Kenya, Uganda, Nigeria, Niger, Chad, Sierra Leone, Guinea, etc.) Without exception, the newly independent African states emerged on the same territories where colonies were previously established. The freshly elected political representatives of these states agreed that they did not wish to revise received (colonial) borders. At the same time ethnic groups only very exceptionally overlapped with the territory of new states (Rwanda, Burundi, Swaziland, Lesotho, Somalia, and Madagascar). If nations were to self-determine themselves, then in Africa, they would have to emerge within the colonial/postco-lonial borders disregarding (pre-state tribal) ethnic divisions. But that was hardly to happen overnight. Eminent Polish historian Michal Tymowski, in his penetrating essays, remarked that while in early mediaeval European history tribes were soon absorbed by the newly formed states/kingdoms, the African situation dramatically differs by simultaneous existence of tribes within states. Tribes do not disappear in Africa, they accompany modern state formation: "African states were organizations built *over* the

tribes rather than in *place of*" (Tymowski 2007: 155, 2008: 180).

On the other hand, I was living in a society (communist-ruled Czechoslovakia) which placed enormous emphasis on the all-encompassing power of the state. To do anything against the state - especially in favour of another, namely hostile, state - would equal to treason and was often punishable by death penalty. However, Soviet and eastern European totalitarian and post-totalitarian states have ostensibly followed prescriptions of 'leninized' Marxist ideology which contained, perhaps rather surprisingly, the idea that the state will, by virtue of historical logic, eventually wither because people would manage their affairs themselves (after all the eschatological aim of Marxism was "communism" which did not require any central power).

For me, the fascination with Africa consisted in the coexistence of modern imported western type of state and various traditional or neo-traditional forms of politics, especially chiefdoms or chieftaincies on the one hand and acephalous/anarchic 'tribes' on the other. By studying the emergence of the state as a theoretical problem and the state in Africa in particular, I was hoping to find out more about the modern state and its current hegemony, especially in state-centred nationalist and communist societies.

When I started my research I had to negotiate a twofold adversary: the data on Africa were mostly available from the works of social anthropologists who in turn were almost all non-Africans employed by the colonial or metropolitan states in order to optimize governance in the colonies. At the same time, in some countries, which opposed western imperialism, social anthropology was branded as a bourgeois science at the service of imperialism and was thus not acceptable. Moreover, Marxism-Leninism as a ruling ideology did not allow much space for the research on the state by using data other than those already supplied and canonized in the communist doctrine. Evidently my study of the state was bumping into the 'really' existing states that did not allow any other research on the state that would not confirm their historical teleological paramountcy.

Luckily, in the 1960s the research into the state problematic had taken place in the period of relative loosening of the communist regime which also reflected itself in the relaxation of the historical materialist Marxist dogmas. The contradictory data (Godelier called it rebellions of evidence) coming from the newly independent Africa and other non-European areas had to be processed even by the Marxists. Thus, for example, a new

wave of discussions on the concepts such as the Asiatic mode of production (Marx) and Oriental despotism (Wittfogel) gave impetus to my African state research.

In 1963, I was sent with a Czechoslovak state scholarship to the Soviet Union in order to continue my African studies at the Leningrad *State* University. Once there, however, my 1966 annual student essay on "State and community among the Mossi" was rejected because I introduced in it data about immigrant or conquering elements in West African state formation. The examiner (historian V.M. Misyugin) deemed me liable for several sins but most importantly he accused me of "drivel in the spirit of Great Germany" [*bred v dukhe Velikoy Germanii*] as if I approved of the *German Drang nach Osten* policy of conquest of Eastern Europe and Russia (Skalnik 2002: 46).[2] The question on the origins of African states suddenly became a highly sensitive topic touching upon the existential questions of European statehood to the east of Germany. Luckily again, these were no more times of sending ideological dissenters to Gulag camps and I only had to expand my paper into an MA thesis and find a new examiner. I did not need to repent or change the topic; it was enough to write the thesis in English because as a foreign student at Leningrad I was allowed such a language switch. The original examiner did not know English well and the new one (culturologist S.N. Artanovskiy) proposed the best mark. I returned to Czechoslovakia with the 'red' diploma reserved for eminent students. Back in Prague my thesis entitled "The political systems of five Voltaic societies. An attempt to make a comparative analysis" was welcomed, further expanded, revised, and defended as a PhD thesis in 1968.

The communist state had again intervened soon after PhD diploma was handed to me. The Czechoslovak attempt to liberalize communist system was 'rewarded' by an invasion of the Soviet army and armies of some Soviet satellites. I was, at that very moment, a conscript in the Czechoslovak army because I wanted to fulfil my citizen's duty before continuing my studies at Northwestern University in Evanston, near Chicago. I had hoped that from Evanston, which boasted the first African studies centre in the USA, founded by Melville Herskovits back in 1948, I would be able to do fieldwork in West Africa. However, the Czechoslovak state, now under direct Soviet tutelage, prevented me from using the American scholarship and I had to write my next major scholarly thesis without fieldwork. This was "The dynamics of early state development in the Voltaic area (West

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2   I keep the original hand-written annual essay among my papers.

Africa)" (Skalník 1973) that earned me the postdoctoral scientific degree of Candidate of Science (CSc.).[3] Frustrated as I was, I nevertheless used the period of 1970-1976 for a fieldwork in a sub-mountain Slovak village of Nižná Šuňava. The decision to carry out the research there was made when I coached Bratislava students during a winter research practice. It appeared that the villagers suffered a violent police razzia in 1950 when they resisted unrealistically high 'contingents' (forced supplies) and gave support to their parish priest. The research revealed contradictions in the state's treatment of this village and the nearest neighbour, Vyšná Šuňava, which was almost fully cooperativized. My Slovak village politics research lasted intermittently for six years but the appointment in Bratislava ended, officially because Slovakia could not afford an Africanist.

Early states research and the modern state

Here I wish to make a terminological remark. While 'political system' was a term coming from the western anthropological and political science tradition (see Skalník 1990), the term 'early state' was my modest revision of the late Marxist 'early class state' (see Skalník 2004: 79). As I see it today the naiveté of the research on early states which was set into momentum by the publication of *The Early State* (Claessen and Skalník 1978) resulted in what I would call 'state hegemony' in the then theory of political anthropology. Practically any kind of political centralization was a state in our understanding of those years. Service, whose neo-evolutionist sequence contained 'chiefdom' as a precursor to the state (Service 1962, cf. Carneiro 1981), was ignored by the then champions of 'early state' theorization. Interestingly enough and perhaps because of this insensitive all-embracing conceptualization of 'early state', the concept never achieved the popularity it initially hoped to gain (see Skalník 2009a). When writing the editors' chapters for *The Early State* we also unintentionally ignored Pierre Clastres' pioneering research which was in print in its original French since 1974. Thus our anthropological theory of the state appeared 'insularized', in effect without bridges to chiefdom on the one hand and the historical, archaeological and

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3   The prevailing political conditions in post-invasion Czechoslovakia were so hostile to non-supporters like myself that the CSc. thesis could only be defended in 1990, i.e. 17 years after it was submitted. I still keep a letter that came with the three returned copies where I am told that the thesis cannot be allowed for defence because I am politically unacceptable.

political science research on the state on the other. After *The Early State,* the small group of early state theorists has produced an impressive series of international volumes (Claessen and Skalník 1981; Claessen, van de Velde and Smith 1985, Claessen and van de Velde 1987, 1991; Claessen and Oosten 1996, and later also Kradin and Lynsha 1995; Feinman and Marcus 1998, Grinin et al. 2004) which were well received, for example, in Russia and Germany but did not spark the imagination of a new generation of political anthropologists. To them, this vast literature seems to have appeared less than relevant as testified by the most recent worldwide anthropological research on the state (cf. Vincent 1990, Abélès 1990, Gledhill 1994, Nugent and Vincent 2004, Krohn-Hansen and Nustad 2005, Sharma and Gupta 2006, cf. Skalník 2009a). The exception is the textbook by Lewellen that pays considerable attention to the early state concept (1983 and subsequent editions).

Research into African post-colonial states has been a response to Weber and Eisenstadt who respectively coined 'patrimonialism' and 'neo-patrimonialism' as concepts meant as impetuses for a fresh look at the state outside the orbit of western liberal democratic model of governance. That research travelled from a pragmatic understanding of the state as machines for personal aggrandizement of rulers (Bayart 1989, Bayart, Hibou and Ellis 1997) to more anthropologically sensitive analyses (Chabal and Daloz 1999, 2006; Chabal 2009, for the latest position different from Chabal, see Nugent 2010; cf. Skalník 2001). Meanwhile, following Clastres (1977, orig. 1974) a new wave of research on chiefdoms as alternatives to states has emerged and indicated that the alleged state hegemony might be less hegemonic than previously thought (Earle 1991, White and Lindstrom 1997, van Rouveroy and van Dijk 1999; Skalník 1983, 1989, 2004; Ray et al. 2011, a special issue of *Social Evolution and History,* 2011).

My own fieldwork in northern Ghana, made possible only after I escaped the embrace of the communist state and settled in the Netherlands in 1976, began as an attempt to capture a neo-traditional encapsulated 'state' situated in a belt of presumably least acculturated societies between the forest and Sahel zones of West Africa. However, the then politically correct respect for the interest in 'relevant research' of both Dutch state (whose Tropical research foundation or WOTRO was financing the initial stages) and the modern state of Ghana (whose University of Cape Coast's Centre for Development Studies was receiving me) led me to enlist it as primarily an inquiry into political aspects of development issues in the underdeveloping Ghanaian North.

Soon, once my field research progressed I was to be given a lesson that had to do with a case of coexistence of a chiefdom (Nanuŋ) with a state (Ghana), not one state within the other. [4] During armed clashes of 1981 the Nanumba 'state' was unable to defend itself vis-à-vis Konkomba 'acephalous tribesmen'. At the same time the modern Ghanaian state even when weakened by economic breakdown of the late 1970s and early 1980s survived the crisis and today serves as an example of a functioning African democracy (cf. Skalník 1981, 1986, 2011a). This does not take away the potential usefulness of a 'new indirect rule' model of governance in which chiefs would play a role of watchdogs of democracy in Africa (Skalník 1996, 2011b).[5]

In the early 1980s the state of the Netherlands, as if pre-empting the present debt crises facing Greece and many other states in Europe and elsewhere in the world, began to limit its expenditures for education, among other 'soft' spheres. Temporary jobs were phased away, among them also my part-time senior lectureship at the University of Leiden. The unemployment benefits were, however, generous at that time and in the initial periods (so-called *uitkering*) I even did not need to be present in the Netherlands. Thus, paradoxically loss of the formal job and therefore no teaching obligations created for me conditions for an extended fieldwork stint in Ghana. This also helped me psychologically because I happened to be in a challenging environment of a fieldwork site in Northern Ghana which, at the time, suffered unprecedented economic malaise. I realised that I, as a retrenched Dutch academic, am still much better than average Ghanaians. The research in Ghana coincided with

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4   'Monarchies within Republics' was the title of my first article on Ghana printed three years before the start of my fieldwork in Nanuŋ (Skalník 1975).

5   A thrilling reconstruction of attitudes and actions during the encounters between Westerners and Oceanians was offered by Sahlins (1981, 1985). His analysis is another strong evidence for the existence of a different logic in polities, deemed by the Europeans as governed by state organization, but nevertheless proving that they were not commensurable. At the same time the history of Hawaii in the 19th century shows the possibility of modernization of chiefdom and the formation of the modern state. Similar developments could be traced during the 19th/20th in countries as varied as Siam/Thailand, Kuwait, Saudi Arabia, Bali, Japan or Egypt. Today only few Asian and Oceanic states combine the qualities of chiefdoms with those of modern states. In Africa some states toy with the introduction of chiefs into modern politics but thus far with no credible results (cf. Ray *et al.* 2011).

dramatic developments on the political scene involving the second coming to power of Jerry John Rawlings, dubbed as "Junior Jesus", and in Nanuŋ, armed clashes between the Nanumba and the Konkomba. The clashes took place on the background of the economic and political weakness of the civilian democratically elected government of the Third Republic of Ghana. They no doubt contributed to the coup of 31st December 1981 because it was apparent that Limann regime was unable to keep peace in the country. This eventually led me to a theorization that during the civilian-democratic regime in weak states such as those in Africa the likelihood of open local, ethnic or regional political conflicts is higher than during the authoritarian, military regimes (Skalník 1986, 2011a). And indeed, soon after Rawlings' Provisional National Defence Council took power, the commission of inquiry appointed by Limann's civilian government was suspended indefinitely and Nanuŋ almost instantly became peaceful, at least superficially, because the power of the Ghanaian state was again felt in the far-away regions. My thesis about the weakness of civilian regimes in Africa and the likelihood of revival of old or suppressed local enmities was confirmed when Ghana, now with Rawlings as an elected president, became democracy under the Fourth Republic constitution in 1993. A much bloodier violent clashes involving several ethnic groups, chiefly and 'acephalous', broke out in seven districts of Northern Region, early in 1994 (Skalník 2002, 2003, 2011a).

The state and jobs

The state has influenced my anthropological life even deeper when Adam Kuper, while I was in Ghana, involved in the above mentioned unemployment fieldwork, urged me to apply for a job in South Africa, specifically at the University of Cape Town. Originally I never contemplated searching for job in South Africa (at that time the country was in final, quite violent, stages of the apartheid regime) but the relative autonomy and defiance of liberal universities such as Cape Town vis-à-vis South African state persuaded me to accept the offer of a Senior Lectureship in Social Anthropology there. Again I happened to be in the 'care' of an authoritarian state that took three years before granting me a permanent resident status. I arrived in South Africa as a Dutch national but for three years I was unable to obtain permanent residence and thus, was also unable to buy a house and use university subsidy for that. When I inquired at the Ministry of Justice they told me that either my wife joins me in Cape Town or I divorce. Separation while each partner lives in a different

country is not tolerated. As my wife did not want to go to South Africa we divorced. Otherwise, academically the conditions at the department and at the Cape Town University were demanding, but generally fair. In spite of my heavy teaching load I could begin to publish the results of my Ghana research (Skalník 1983, 1986, 1987, 1989), attend international meetings and even go to Ghana for a short fieldtrip. I tried to begin a study of a reform movement in a vineyard village of Franschhoek but that research floundered because a 'black bomb' exploded there: Vincent Crapanzano's book Waiting (Skalník 1993). However, the conditions in South Africa deteriorated in the late 1980s and I decided to apply for a grant to study the socio-cultural effects of a gold mine project in the Lihir Archipelago in Papua New Guinea (I spent a three-month leave in PNG in 1988 at the encouragement of my Swiss friends Florence Weiss and Milan Stanek with whom I also visited Lihir). When I was planning the Lihir fieldwork, the news of the fall of Berlin Wall and eventually overall collapse of communist regimes in central and south-eastern Europe reached me in South Africa. I nevertheless went to Lihir in 1990 because the Human Science Research Council of South Africa accorded me a generous research grant.[6] Prior to my departure for Lihir, I spent June and July 1990 in Prague preparing my 1973 CSc. thesis for defence. Meanwhile I was rehabilitated both by Bratislava and Prague universities and offered a post at Prague's Charles University, my alma mater. I accepted what was initially a visiting position that eventually became a permanent teaching post. This logically heralded the end of my Cape Town job. But following a year of sabbatical leave I had to return to Cape Town for at least one semester of teaching. This I did in the second half of 1991 when I also resigned my permanent position there. Ironically, soon after I returned for good to Prague and Charles University, I learned that all permanent positions in universities were turned into temporary. So it has been with all my positions since - they have been invariably temporary.

I tried to use my Prague job for the promotion of social and cultural anthropology. What was taught until then was physical anthropology and ethnography with folklore, in sepa-rate departments of separate faculties (natural sciences and arts respectively). My appointment was in the Department of Near East, India and Africa where I started teaching courses of overtly anthropological nature. Ethnology students who were dissatisfied

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6   I spent four months in Lihir (August-November 1990) but fell ill
     there. In spite of few attempts to resume this promising fieldwork
     I never managed to return.

with old fashioned subjects taught in their department flocked to my courses. I also contacted the Dean of the newly established Faculty of Social Sciences and he created a lectureship in social anthropology in their department of sociology. This was a great leap forward but much short of my original plan to either create a new department or reshape ethnography into anthropology. To say the truth, I was basically disappointed by very little institutional support for social and/or cultural anthropology at Charles University.

That is why I was not opposed to the opportunity of working part-time for the Czechoslovak Ministry of Foreign Affairs in their department of analysis and planning. The ministry looked for people with the past untarnished by collaboration with the communists. Pretty soon I was asked to assume the post of ambassador to Lebanon. That country emerged recently out of civil war which lasted for 15 years. Czechoslovakia's diplomatic mission in Beirut was barely functioning, but without an Ambassador. I accepted the offer because of the very special position of Lebanon among the Arab countries. The only democracy in the Arab world, the country was now in search of a new identity in between two major opponents, Israel and Syria. The latter, the eastern neighbour, had her army deployed in Lebanon. The billionaire entrepreneur Rafiq Hariri was considering helping reconstruction of his country by entering politics. At that time he did not suspect that his assassination thirteen years later would trigger off popular movement leading to the departure of the Syrian Army from Lebanon. My anthropological encounters with the state had become suddenly enriched by close contact with top representatives of two or rather three states. Soon after my credentials were handed to the Lebanese president Elias Hrawi, Czechoslovakia fell apart and I became by default Czech ambassador to Lebanon. One of my tasks was to promote one of two successor states in a country which was used to the existence of Czechoslovakia as a permanent fact. The period of more than four years of my ambassadorship were filled with observations, meetings with ministers, political party leaders, diplomats, Lebanese businessmen and other public. It was an exciting period of my life which still awaits deeper analysis. I have published, though, several articles (Skalník 2004c, 2005, 2006, 2007, 2011) reflecting the relative importance of the state in Lebanon.

After my return from diplomatic summits I fell almost instantly into the ditch of mundane academic existence in the Czech Republic. The state has shown its back to me or rather its normal Janus face. All my efforts at establishing socio-cultural

anthropology in the Faculty of Arts, Charles University were in vain (cf. Skalník 2002b). So I left that faculty for another, namely Department of Anthropology in the Faculty of Natural Sciences. Ostensibly the biological anthropologists there wanted to add socio-cultural anthropology to the portfolio of subjects taught there but in fact these people had no understanding for non-biological data. When in 2001, I presented my selected published works for a special evaluation leading to the so-called habilitation.[7] I was told that there is not enough biology in my writings. When I explained that socio-cultural anthropology is not biological anthropology my documents for habilitation were returned to me, this time not for political reasons like in 1973, but for reasons of disciplinary boundaries which internally structure anthropology. The would-be chairperson of my habilitation committee, a political geographer, openly told me that he was sure that, even if he would recommend me for habilitation, I would not get enough votes. Therefore he withdrew even before the start of the procedure.[8]

In the meantime I was lucky enough to enjoy ten months in the Netherlands Institute of Advanced Study in Humanities and Social Sciences (NIAS) which offered me a full fellowship for the 2001-2002 academic year. The state of the Netherlands that finances NIAS has shown a friendly face. First time in my life, I had full ten months just for research. It contrasted sharply with the unanimous vote in my home department against granting me at least a half year of sabbatical leave for which I was in

7   Habilitation in the Czech Republic and a number of central and east European countries, which were historically under German academic influence, is a kind of bench marking. When a candidate who has taught as assistant professor at university level for some years, presents a book-length text or a selection of her/his published works, gives a specialised lecture, and must be approved in a secret ballot of professors to be qualified for a title of docent. According to law on higher education only docents and professors are fully qualified university teachers. In practice, though, many assistant professors teach independently as well.

8   Interestingly enough, I repeated the attempt in 2007 at the Science Faculty of the Brno Masaryk University, but there I was told that even a habilitation committee could not be assembled due to the unusual subject. In another, social studies faculty of the same university a committee gathered once but found that my selected published writing did not have enough integrity and recommended that I concentrate on only one topic out of five or six. The committee also mentioned that the approach is not sociological enough. So I withdrew my application.

principle entitled because I have worked more than seven years since returning to Charles University in 1990. This time it was not a hostile state but just hostile colleagues! I was at least granted an unpaid leave of absence but my university internet account and Czech health insurance was discontinued for the period of absence. While enjoying the freedom of research in congenious conditions of NIAS, I was told by the head of department back at home that my post will be re-advertised (the contract was for three years) and that I may apply. Even though I knew that the department wanted to get rid of me I dutifully applied and stressed that I wished to work in the department the last seven years before retirement. And so it was that I was returning from the Netherlands back to Prague with a letter telling me that the selection committee did not select me. Later I learned that they appointed a less qualified biological anthropologist. The department apparently did not want to be disturbed by a "stranger" from socio-cultural anthropology.

I was lucky enough that already in the year 2000, I was approached by Bohuslav Šalanda from Prague's Institute of Ethnology at Charles University who invited me to join him in the newly established department of social sciences at a regional University of Pardubice (a city situated some 100 kilometres east of Prague). Šalanda used to be a head of the then Department of Ethnography and Folklore Studies at Charles University during the last two years of communist rule. A folklorist who was a former communist now came with the idea of establishing social anthropology in Pardubice, close to his native town of Sezemice! Obviously, as a former escapee from communist rule, I had my hesitations. But I decided to join him because it looked as if at long last I could help establish social anthropology in a Czech university, however marginal. First I was appointed to a 25% job as from 1st October 2000. I proposed to teach a course on modern anthropology of Africa which was attended by quite a number of students. Most importantly though, I suggested to apply for a grant enabling a re-study of the commune of Dolní Roveň located some 15 kilometres eastwards of Pardubice. The village was studied in the second half of 1930s by a rural sociologist Galla and of course, many things had changed there since that research. I went to visit the village in March 2001, collected basic data and impressions, and soon afterwards applied for a three-year grant for the re-study of Dolní Roveň at the beginning of the third millennium (Skalník 2004b, 2008). Before the end of the year (while in the Netherlands) I learned that the grant was approved by the Grant Agency of the Czech Republic (GAČR)

and we could start the research in 2002. Beside myself as the leading researcher two other Pardubice colleagues (Šalanda and the sociologist Šubrt) would complement me as co-researchers. The idea was that students of social anthropology (who started their study in October 2001) would assist us in the research starting in July 2002. My intention was that throughout their study each and every student would have to do fieldwork.

The re-study of Dolní Roveň proved to be a great opportunity to realize this maximalist criterion and cohorts of our students went through the fire of fieldwork in Dolní Roveň. A number of their bachelor and master theses were written about various aspects of that commune. The state, represented by GAČR, showed its positive face because social anthropology established itself in Pardubice vigorously and the Department of Social Studies, especially after it also obtained accreditation for sociology, was soon recognized as probably the best in the country. These successes were however not well received by the leadership of the Faculty of Humanities, later renamed into Faculty of Arts and Philosophy. The problem was partly due to the fact that the faculty was dominated by historians, apparently envious of a more scientific discipline of social anthropology. Social anthropology demanded money for field research which was consequently denied to us even though fieldwork was included in the state approved accreditation. Our department was most successful in international exchanges; we had excellent publication record, organized yearly conferences in which students and staff reported about their research. But we were also defiant whenever scholarship was compromised in favour of bureaucracy and xenophobic provincialism. In 2010, the frictions came to a pitch after the new Dean did not appoint any of the carefully selected candidates for professorship probably because they were foreigners. So after ten years of service I left Pardubice unceremoniously and the doctoral programme in social anthropology which we wanted to apply for remained a pipe dream...

Back in 2005, I was approached by the Department of Ethnology and Cultural Anthropology at University of Wroclaw in Poland, with which Pardubice had an exchange agreement, about helping that department with teaching their master students. I accepted this challenge and used an extraordinary professorship for launching another community re-study in Dobrzeń Wielki, a 'gmina' (commune) composed of nine villages near Opole in Silesia. This gave me an opportunity to look into the functioning

and problems of local autonomy in a neighbouring country, a fellow member of the European Union (both countries joined the Union in 2004 together with eight other countries). What was intriguing in Dobrzeń was the triple ethnicity composition. A number of locals were Silesian Germans who since 1990 were allowed to carry two citizenships: Polish and German. Thus they were able to work in the united Germany long before the accession of Poland into the European Union. This had expression in economic advancement of the commune that displayed a great number of enterprises and comparatively high standard of living. The other two ethnicities were Polish and Silesian. Polish were mostly those who came to western Poland from former eastern Poland annexed by the Soviet Union with the end of the World War II. Silesians were autochthons who either carried only Polish citizenship or those mentioned above who were also German by nationality. Unfortunately this promising fieldwork was interrupted by the end of the Wroclaw appointment in 2007. As I am now back in Wroclaw as a visiting professor I am hopeful that I can bring the Dobrzeń research to an end by combining it with the findings of the re-study of Dolní Roveň. The completion of this research project will provide insight into the functioning of at the lowest echelons of two post-communist states.

Before closing, I should mention another of my encounters with the state. For some time I have been spontaneously interested in political culture. My work on northern Ghana and South Africa can be cited as an evidence of it (Skalník 1989b, 1999). Since 1989, I have been also gathering data on post-communist Czechoslovakia and Czech Republic, their political scandals, and a very arduous march towards more direct democracy. While in Lebanon, as Ambassador, I also became fascinated by the complex political culture of that exceptional country. This all has drawn me into a serious study of the theory of political culture which obviously has lots to do with the state but also citizenship and ordinary daily attitudes to politics. I won a research grant, have developed a theoretical framework (Skalník 2000) and worked several years on both post-communist and post-colonial political culture in Europe and Africa (Skalník 2004b, 2004d, 2006d, 2009b). Interestingly enough, whereas initially there was little interest in political culture and this direction of research seemed to be underestimated. With time, however, a realization grew that politics has different features even in Europe - the continent that believed in its joint cultural heritage - each country and even region has drawn on the past and thus displayed a wide variety of political cultures. The more it was true of Africa with

its complex ethnic, economic and political structures, pre-colonial, colonial and post-colonial. I am still hoping to draft a synthesis on political culture in the era of restructuring of relations both within and between continents.

I think at this juncture that one of the conclusions which could be drawn from the above is that the direction of future research on the state leads away from the present exclusivist disciplinary solipsism towards more coordination among anthropologists, archaeologists, sociologists, political scientists and various area specialists. If the state really is to wither, there will be new thus far unknown candidates to fill the vacuum and anthropologists will find themselves both cornered by the successor(s) and challenged for studying those future Leviathans (cf. Hannerz 2010). Quite naturally the emerging research paradigm within anthropology viewed as a pluralism of *anthropologies* will contribute to this shift.

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