

Chapter 9:

A Critique of Anthropological Self-Contemplation (1988)

Useful to a degree, fieldwork introspection endlessly replayed can become a sub-genre that loses both its novelty and payoff for developing a knowledge of other cultures. (Marcus and Fischer 1986: 42)

1. Self and others in a village in Poland

This paper is a no doubt exaggerated protest against some recent tendencies in the discipline. Whilst acknowledging the importance of the disparate subjects raised by participants in this symposium,¹ I feel that the pursuit of an 'anthropology of anthropology' is beguiling but dangerous. I am far from denying the validity of the sociology of knowledge or the history of ideas as fields of academic enquiry, and I think that applying anthropological techniques to the history and method of anthropology, as well as to other branches of science, can be fruitful and stimulating. But I also think that a danger arises when a focus upon the production of anthropological/ethnographical texts comes to stifle the production of new ethnographies. Recent concerns with the 'genre' of ethnographic writing, and with the complex triad of relations which exist between fieldworker/author, people/informants, and audience (both lay and academic), have led to a situation in which anthropologists are invited to write 'reflexively'. I find that this is easily interpreted as an incitement to be autobiographical, to meditate upon the full extent to which the culture under focus is being assessed, for better or for worse, in relation to the culture of the observer, and to speculate upon the extent to which what is being reported about the 'other' is influenced by the fieldworker's presence, by his/her 'supremacist' ideology, by histories of imperialist relations, and so on. Alongside this authorial subjectivity is the increasing expectation that the anthropologist should explore in similar subjective style the particular character and 'world view' of his or her key informants. Some view the present period as an 'axial moment', one of exciting radical experimentation throughout the human sciences (Marcus and Fischer 1986). I tend to see a crisis of confidence, with autobiography as a most unsatisfactory refuge. As a reader, when I pick up an ethnographic representation of some aspect of contemporary East European socialist societies, it is Eastern Europe that I want to read about, and not the preconceptions and values of middle class English or American anthropologists.

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It is important to emphasize what is *not* being argued. I do not deny that the form and rhetoric used in anthropological writing are extremely interesting and worthy of study. If only because of the special political resonances which must enter any writing about Eastern Europe in our time, it is important for a westerner working in this area to be extremely careful with the vocabulary, metaphors and other literary devices used. Avoiding ethnocentricity may impose exceptional demands upon fieldworkers in a region so prominently stereotyped by the leaders of one's own society. But this does not require significant reference to the life-history of the scholar.

Nor do I wish to argue that there can be any such thing as a literal, 'naturalistic' account of another society, to be achieved through describing 'plain facts', meticulously assembled during fieldwork. Many choices are made by every author, and in my view it is a mistake even to try to suppress one's own opinions and values. I make no pretence to theoretical originality here, but find the framework of W. G. Runciman (1983) to be useful for anthropologists as well as other social scientists. Pursuing a fundamentally positivist approach, Runciman is fully sensitive to the factors that demarcate the social from the natural sciences. He distinguishes several levels of understanding in the activity of the social scientist, and ends up with a more refined version of the Weberian distinction between fact and value. Briefly, Runciman argues that as far as *reporting* observations is concerned, the standards in a social science must be no different from those in natural science: accuracy is essential, and the possibility of rival fieldworkers commenting on the truth or falsehood of ethnographic data is a realistic one at this level of understanding. Secondly there is the level of *explanation*, which Runciman handles basically (and ethnocentrically?) in terms of cause and effect; here, *validity* is the over-riding criterion, and Popperian philosophy lurks ominously. The third level of understanding is what Runciman calls *description* (many anthropologists might prefer the term 'interpretation'). It refers to an understanding not of falsifiable facts, but to feelings and the states of mind of human agents. Much sociological theory in the twentieth century has been preoccupied with this kind of understanding, and it is to this level that many anthropologists would give priority. At least since the time of Malinowski, it has been our explicit aim to present the 'native point of view'. More recently, in part stimulated by sociological theory, some anthropologists have begun to explore the non-western, non-individualist conceptions of 'agency' they have discovered in other cultures. (See e.g. Strathern 1987.) They thereby hope to move closer to the ideal of *authenticity*, which is Runciman's main criterion for adequate scholarly description.

My argument is simply that anthropological monographs do not necessarily gain in terms of authenticity by virtue of introducing the author prominently and explicitly in the text. The work may even suffer as a result, because the very self-consciousness of such an observer may render him less alert as an observer of the other society with which he is ostensibly concerned. It is one thing to historicize anthropology, to re-read Evans-Pritchard and Radcliffe-Brown and find all kinds of cultural baggage in their ethnographic texts to which they were not consciously owning up, proper recognition of which can modify our understanding of what they had to say in those books. It is quite another to maintain that better ethnographies will result if every ethnographer is encouraged to wrestle with his/her own values, political beliefs and even personality before presenting other voices in his/her text. It is better to have an author who does aspire to be like some 'recording angel' (a device much utilized by Runciman). This can never be fully achieved. There will be cases when the presence of the ethnographer has an important effect upon the social action observed, and these must be discussed. So, too, must broader issues concerning how much the people being studied have already taken from western culture, sometimes from academic culture specifically. But at the end of the day the reader still has to be left some freedom to infer naivety, political bias, and so on. I am not convinced that the reader's chances of 'getting it right' are greater with more 'reflexive' authors. I actually feel I learn more from the traditional, impersonal styles of authorship.

In terms of Runciman's framework, leaving aside the dimension of evaluation, which poses no special problems for anthropology that it does not pose for the other social sciences, it is the tertiary level of understanding, the level of authentic descriptions, that raises the critical distinctions between the natural and social sciences. Many anthropologists would not be satisfied with Runciman's discussion, mainly because he devotes little attention to problems of inter-cultural translation. He recognizes that the different levels of understanding are almost bound to become tangled up together in any actual social science project. The authenticity of descriptions, in his sense, is closely related to the quality and quantity of observation (level one in his schema) and to the validity of postulated explanations (level two). But I wish to introduce another key criterion for satisfactory description, that of *representativeness*. I am particularly concerned to argue that the persona of the anthropologist, in heroic or in exhibitionist mode, is no substitute for this representativeness.

Rather than pursue the origins of recent trends in anthropology (they seem to me to have more to do with the impact of philosophical currents and literary theories outside the discipline than with changes in the types of societies

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studied by anthropology and the institutional circumstances in which it is pursued) I shall now turn to an illustration from the area in which most of my own fieldwork has taken place. Eastern Europe is an interesting laboratory for anthropological research, as several scholars have pointed out (e.g. Cole 1977, 1985; Halpern and Kideckel 1983). Two aspects make the area useful for my purposes here. Firstly, it is close enough to Western European society for me to feel reasonably confident in a lot of practical as well as intellectual judgements. Thus, East European peasant villages, where a fieldworker who is baptized a Catholic in Britain can join in religious services as a fully-fledged co-religionist, clearly present a different situation from that faced by many anthropologists working in more remote fields. Secondly, however, the grass roots of East European society are clearly unfamiliar to westerners, in ways that we are not dissociated from the many disparate elements of our own western societies of which we lack first-hand knowledge. In other words, there is an unfamiliar world which the anthropologist has to find ways of representing. This is 'the culture of socialism', and I shall touch on only brief aspects of it here.

It would be unfair to single out examples of the personal style of which I disapprove, and in any case few of the published books and papers by western anthropologists about the socialist societies of Eastern Europe have used this style. Instead I shall illustrate my argument by composing (but not I assure you inventing) a cautionary tale from my own fieldwork experiences. In this paper I shall indulge in the personal, confessional, autobiographical mode which I have up to now refrained from utilizing in published work, and I shall try to show why this material is redundant in the 'ethnographic record'.

My work in a Polish village is largely summed up in the monograph *A Village without Solidarity: Polish peasants in years of crisis* (1985). You will find in this account some attempt to relate what it was like to be living in Poland in the years 1978-81, which included the economic and political dislocation of the *Solidarity* period. However, such contextualization apart, there is nothing about how I came to choose the village in which I did fieldwork, my entry into the village, the circumstances in which data were collected, and so on. Let me now fill in some of these lacunae.

The choice of village took place as follows. Soon after arriving in Poland and commencing language study in Cracow it became clear that, although a number of western anthropologists were currently working in different regions of southern Poland, nobody had made any study of the southeastern corner.

This region had a complex ethnic composition, and a tragic history in the twentieth century which was a direct consequence of the ethnic tensions. The choice of village within this region was more accidental. A French friend (originally encountered in Hungary) had provided me with the addresses of some intellectuals in Warsaw. One of these had an acquaintance whose erratic career had taken him from a Warsaw upbringing to the acquisition of his own farm in a small village within the area in which I was becoming interested. Let us call this man Jacek.

I drove to meet Jacek on a fine cold day in early spring, and some village schoolchildren were able to guide me to his farm. He was alone there, because conditions through the winter were not suitable for his wife and small children. They spent the winter with her parents in Silesia, for her family too was urban; she had graduated from a teacher training college. On this occasion I stayed just a couple of days, but it was a vivid introduction to the life Jacek led. We lived by opening cans. On the second day we took his horse and cart to the next village, ostensibly to acquire coal, though vodka turned out to be a more important focus of the journey. After queuing to obtain half a litre of vodka at the store we took this alcohol to the offices of the president of the community cooperative, which was the organization in charge of coal supplies. The president was a chum of Jacek's, and the sharing of the vodka was obviously an old routine. A 'Do not disturb' sign was hung on the door of the inner office, for the half hour or so that it took the two of them to dispose of the vodka. There was more drinking afterwards in the village restaurant. The journey back, by now with coal, took place after dark and was highly dangerous. Jacek's drunkenness was reflected initially in high speeds and abuse of his horses; this was followed by drowsiness, and we slipped off the road on several occasions; but after stops for hot tea at convenient intervals we kept going, and Jacek was singing merrily as we arrived back in the village.

Later, when I had collected Jacek's wife and family from Silesia and moved in with them on the farm, I experienced other aspects of my host's personality. As I expected, the life-style of the winter months, when he earned money through wage-labour in the forests, was necessarily transformed by the discipline of the agricultural year. Under his wife's patient influence, animals were acquired for the farm where only horses had been kept in the winter, and we worked hard to sow the spring crops. I learned all about farming in tough upland conditions, and I made rapid progress with the language. But after a couple of months I had still learned very little about the other inhabitants of the village. It was obvious that most of them were on bad terms with Jacek, whilst

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he for his part despised them all. I had numerous further encounters with socialist officials, most of them similar to the above, as I drove Jacek up and down the region in search of items of equipment, or a more satisfactory livestock contract. But I felt I had to find out more about the other inhabitants of the village, and no doubt this contributed to tensions which led to my leaving Jacek's house, though we remained on cordial terms for the duration of my fieldwork.

Rereading these paragraphs I find this account all rather mundane. But I am being coy as well as brief. I have not begun to do justice to all my feelings and impressions in that first spell of fieldwork. On the negative side, I can vividly recall the disasters that struck on the farm, when calves died with infections, cows gave birth untended in the field, and the potato harvest disappeared under weeds. Jacek's wife worked consistently hard, but was constrained by the presence of two small children. Jacek himself worked in fits and starts, but would hit the bottle whenever an opportunity presented itself. This happened frequently, largely because visitors, old friends and family, mostly city dwellers who had a standing invitation to supplement the labour force in the summer months, all brought vodka as gifts. Some of the parties at Jacek's farm were very wild and violent. I remember on a later visit waking up one New Year's Day with the delirious complaints of a battered wife ringing in my ears, her blood on the sheets, and her ex-convict husband fuming morosely to himself as he sobered up. Jacek himself was only violent with words, and he was very good with words; others chose blunter instruments, such as chairs. I could quote a lot more of this kind of material, much of which readers might find entertaining.

There is also much I could say of a more positive nature about the period I spent with Jacek. He was highly intelligent, and even now as a farmer he read voraciously. He had very well informed views and strong opinions about politics, both within Poland and internationally (though his foreign travel had been limited to brief excursions within the socialist bloc). At bottom he was a kind and humane person. He also appreciated art and poetry, and experimented frequently in the latter medium. What struck me most, for I was living in the mountains for the first time myself, was his sensitivity to the beauties, but also the desolation and harshness, of that zone of the Carpathians which the Poles call Bieszczady. He had breathed their air over many years, as tourist official, state farm worker, and now as independent farmer. His contempt for the politics and public morality of his country, which certainly included contempt for many of his own public performances, was balanced within him by a

profound identification with the natural beauty of the mountain environment. He reserved his greatest bitterness for the many evident signs of contamination by the contemptible forces of the lowlands, such as houses made from concrete blocks and the building of new sawmills in the mountains, leading to excessive rates of tree destruction as well as human casualties through the proliferation of wage-labour employment. In short, I saw some real nobility in his character: honesty, imagination, and poetry.²

But how could I have stayed? I could not have written an ethnography of that village from such a special vantage point. I have already said that Jacek's relations with other villagers were almost uniformly bad. He was very strongly anticlerical, yet all the other inhabitants of the village received the Roman Catholic priest regularly into their homes, in addition to attending Mass in the village church. He shared the prejudice of most immigrant Poles in this region against the Ukrainian ethnic group which constituted the indigenous population; yet several indigenous families had returned to this village in the recent past, and I wanted to establish contact with them. Jacek also scorned those farming independently like himself who thought that they could expand and modernize their farms in the prevailing conditions. Far from seeking to cooperate with such farmers, he ridiculed them as his intellectual inferiors. This seemed unfair, and I felt that I needed to talk to these farmers myself.

In the later phases of fieldwork I therefore set about exploring these other vantage points more rigorously. I lived for some months with one of the Ukrainian families, who had clear memories of the period before the old community was destroyed and the village opened to Polish colonization. I spent a further five months in rooms in the modern house of a dynamic, well-to-do farmer who later became the village headman, and who was himself a highly intelligent commentator on social life in the village, though in a radically different way from Jacek. In these later residential bases I was able to pay a rent in cash. This gave me greater freedom to move about the village, eventually visiting all sixty households. People accepted that I had entered the village via Jacek purely as the result of a chance introduction. They seemed to grasp the nature of my interest in them, and were invariably hospitable and cooperative. The monograph I eventually wrote devoted significant space to the position of those farmers who were trying to break out of the old peasant mould, to the position of the Ukrainian minority, and to the enormous importance of the role played by the Catholic Church, to note just three of the themes which I could scarcely have documented at all had I remained with Jacek and his family.

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Of course, he played a prominent part in the monograph too. The validity of what I learned in his company about the culture of socialism (particularly concerning the use of personal ties and vodka to resolve grass roots allocation problems in economies of chronic shortage) was later confirmed by many others (though not all were as good as he was in participating in that culture - see Hann 1985: 89). It was partly because I felt that I had not let enough of his spirit penetrate my main text that I decided to allow his voice to speak in an appendix: I translated a text which he had helped to prepare for a Polish newspaper. One passage, which I thought particularly effective, described the frustrating experience which independent farmers feel when they travel to the neighbouring larger centre to order some job or other from the Agricultural Circle (the state controlled organization which is supposed to help them with equipment and services). Jacek referred to 'Dantesque scenes' as people jostled for position when the officials arrived. Though other farmers would not be familiar with Dante, his general description of this scene is certainly authentic for them too. In other words, though I refrained from spelling out any of the details of my personal relationship with Jacek in the published monograph, much of what I learned and observed during those months is reflected and distilled in my text; where his experience was not representative generally, I sometimes included it anyway, whilst making the specificity clear; and I judged his distinctive voice to be of sufficient interest to warrant a separate platform in an Appendix.

But in many areas Jacek's opinions would have carried only personal authenticity (he seldom lapsed into affectation or pontification, so that almost everything I heard from him was authentic in this sense). For example, his uncompromising atheism and anticlericalism were entirely incompatible with the evident popularity of the local priests, and the religiosity of the local society. I eventually concluded that, for reasons partly to do with its peculiar history of ethnic conflict and resettlement by diverse groups of colonists, accentuated by socialist institutions of political and economic management, there was very little sense of belonging to a common community in this village. Jacek was an extreme case of isolation, but other families and groups were also isolated to greater or lesser degrees, and the local social structure could not be represented as a harmonious whole. Nevertheless, for almost all inhabitants religion did provide a significant focus of identity, personal, national, and, to some limited extent, at the level of the parish community.

I have now summarized some of my conclusions in the monograph. The main point I wish to make here is that, whatever the imperfections of my

ethnography (and there are undoubtedly many), this would not have been a better work of anthropology if I had stayed closer to my own story as it unfolded during the fieldwork. Should I have managed the fieldwork any differently? Several alternatives were available to me. It might have been possible to swallow the sense that I was being exploited by Jacek, and the sense that I was missing out on other kinds of data elsewhere in the village. I could have stayed with him, carefully documenting all our interaction, and published an account which (let us be optimistic) might have been able to capture some of the man's artistic vision, as well as narrate more of his lunacies when drunk. Such a book might have been planned as a sort of allegory of the of the People's Republic, for Jacek was born in 1944 at its inception, in a camp in Holland for those who had survived the war in Warsaw and had hopes of a glorious future. He himself felt that those hopes were irreparably dashed in 1968 and 1970; thereafter he sought refuge in the purer environment of Bieszczady where, partly due to his own deficiencies no doubt, but also due to those of the socialist state, his vision and intelligence were not sufficient to produce good results as a farmer. I could have tried to let this voice speak for itself, or sought his collaboration and used a dialogue form, and the project might have been an elegant one.

Jacek's voice could stand for some others in this part of Poland, indeed the type is not unknown in some other socialist states too (cf. Széleányi 1988). But I felt the nagging obligation to introduce other voices, and to come up with some kind of analytic framework that would help to make sense of all of these subjectivities. Even if I had never left Jacek's house I would have had to find room for the sane, moderating voice of his wife, lamenting his inability to get up early in the morning as a farmer must, even as she sympathized with all his frustration in dealing with the outside administration. But I wanted also to write about other villagers, about the large majority without advanced education, who read only the farming papers not the Warsaw political weeklies - that is, if they could read at all.

It might be argued that I still have an obligation to supply more detail about exactly how I came to hear these other voices myself during fieldwork. I should have outlined the circumstances in which I knocked on the old Ukrainian's door and begged him to take me in, before recounting our conversation, over vodka, about how his house was burnt down and his daughter killed by the Polish army in 1945. I might have detailed all my interaction with the headman in the style of Dumont (1978).³ It could have been done. But the book would have grown longer, and I don't think the benefits would have been substantial. The reader would soon have grown weary, since a lot of the account would

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inevitably come back to pressures of hospitality which are not especially peculiar to this culture (though I do discuss them a little in the monograph, and in particular the centrality of alcohol).

I sometimes worried that my occasional squeamishness over consuming vodka at the expected rate seriously limited my data collection. I cannot absolutely exclude the possibility that this behaviour on my part may have caused some people to *increase* their drinking in my presence, or to dissemble or lie when talking to or observed by the odd puritanical *Anglik*. There is much more I could say to raise doubts about my fieldwork. The very fact that I owned an (old and socialist-produced) car may have given me a certain status in villagers' eyes, at a time when there was only one other private car in the village. The fact that I was joined by my wife during the last months of fieldwork may have influenced perceptions of me in that phase, and the fact that she was of Hungarian nationality may also have had certain resonances. Many villagers saw me regularly running along the road and up and down the mountain slopes, since I was training for a marathon during one period of fieldwork. I am sure that they found it odd, and that this too must have coloured their perceptions of me to some degree. A few would also have known that every so often a police snooper was sent out from the county town to make discreet enquiries about what the Englishman had been up to. They, especially, might have had reason for caution in all contacts with me.

Nevertheless, I did not consider it necessary or profitable to spell out these factors in my book. Perhaps for a more sensitive kind of enquiry into the *Weltanschauung* of the contemporary Polish peasant I would need to consider the position more carefully and provide more detail about myself, about how the work was done, and how particular conversations unfolded. (The principal theme of the monograph is the distorted survival of peasant economy, documented with facts and figures; concomitant distortions of traditional peasant beliefs and values are treated more superficially, though not ignored.) Perhaps had I worked in a different culture more remote from my own Western European one I, too, would feel that the author should be more visible. But I remain convinced that the ethnographer's main job is to represent as well as he or she can the people being studied, and that this does *not* require the ethnographer to be constantly parading himself/herself before the reader, as I have in this paper. It is enough to attempt to set out what goes on, or has been going on in the past (facts); to try to account for these occurrences (explanations);

and, the hardest task of all, to gauge the meanings of social action for actors/agents (description). None of these levels of understanding requires autobiography on the part of the ethnographer.⁴

I am not attempting to defend a simple positivism, because I admit that such formulations do not adequately grasp the special descriptive problems faced in the social sciences, and in an extreme form by anthropology. Like Runciman, I want in the end to steer a cautious course '... between the Scylla of positivistic empiricism and the Charybdis of phenomenological hermeneutics' (1983: 144). My main point has been to criticize excessive introspection on the part of the anthropologist, obsessions with the extent to which his or her presence is affecting the data to be reported, and with the literary form of the ethnography. After a point (admittedly vague, to be negotiated in each instance) these concerns can in my view exert a negative influence over the ethnographical information conveyed (both the type of information presented, and the quantity of information, given normal publishing constraints).

I am not denying that for the reader it may be very helpful indeed to have some background information about the anthropologist and the circumstances in which fieldwork was carried out in order to understand the work. For example, leaving aside my class background and university training, it may also be relevant so far as my own work is concerned that I worked in Hungary first, and had far fewer practical difficulties in Hungary than I encountered whilst carrying through the project in Poland some years later.⁵ Up to a point I would agree with Edmund Leach:

'Unless we pay much closer attention than has been customary to the personal background of the authors of anthropological works, we shall miss out on most of what these texts are capable of telling us about the history of anthropology.' (1984: 22)

But anthropologists are not only, or even primarily, interested in the history of their discipline. Their main goal is to shed light on the societies they study, and so far as this goal is concerned I find it impossible to endorse the Leach of 1987 who argues that all ethnography is fiction. As Abrahams has pointed out in a rejoinder to this, it needs to be much more than that if we are to retain confidence in its future. In this paper I have tried to show that it also needs to be much more than allegory and (auto)biography.⁶

2. The headman and I in a village in Hungary

Some anthropologists seem in recent years to find rigorous analysis of the fieldwork predicament to be more important than rigorous analysis of the community in question. Some of the results of this work have been revealing and should help to improve research practices in the future. However there is a danger that the subject could become too introspective and lose sight of its ultimate goals, to increase understanding of human societies. I find it curious, for example, that 'reflexivity' is increasingly referred to as a theoretical orientation in its own right.⁷

It is clearly no accident that post-positivist critiques have been felt with special force in anthropology, given the added difficulties associated with inter-cultural understanding. Yet the consequences of anthropology's so-called 'literary turn' may actually diminish the prospects for entering the subjective worlds of the people anthropologists study. I am thinking in particular of metaphors of textuality, and the tendency to construct homogenized images of culture on the basis of only limited observation and heavy reliance upon key informants. There is a risk that the text, far from aiding our understanding, becomes a barrier between the anthropologist and the social realities under scrutiny.

In this paper I do not address these general issues but focus on one particular problem concerning the representation of 'the other' in socialist societies. Representation is always a political issue, but superpower conflict makes for a particularly salient backdrop whenever a western anthropologist conducts field research in Eastern Europe. How can this anthropologist hope to present unbiased accounts of the eastern reality? On the one hand there will be plenty of people who use this anthropologist as a sounding board for their complaints about life under socialism. On the other hand there will be others whose loyalty to communist dogmas, imbibed and diligently studied, will lead them to the opposite distortions. These are real problems, but the best response is to hope that through lengthy periods of participant observation one can at least succeed in seeing through the grosser distortions. In the case of Eastern Europe, a researcher from Britain is not visibly different from the locals, and if you are competent linguistically you will be able to validate many propositions in highly objective ways.

Each researcher has to decide how much space to devote in publications to details of how he or she was received at various stages of the project. I chose not to use up space in my Hungarian village monograph with descriptions of

the time wasted at the beginning of fieldwork by tedious trips to police stations for registration formalities. However, I did outline some of these practical hindrances in my dissertation, thinking that this information might be of benefit to other researchers (and also that it was relevant for my examiners in their assessment of my work). I shall return to publication dilemmas below.

Including all the outlying farms within its boundaries the village of Tázlár had over two thousand inhabitants. Like other fieldworkers I inevitably formed closer relationships with a relatively small number of these. Although there is a sense in which all interpersonal relations are nowadays classified as political, because this was socialist Hungary in 1976, some of my village contacts were political in a narrower sense: in other words, I had to form certain relationships to the local authorities, both in the village council and in the agricultural cooperative. Although they in no way prevented me from interacting with others (my landlady had no contacts at all with local administrative élites), these political relationships were always central to the conduct of the research.

One of the most fruitful of these relationships was initially fortuitous. While travelling on foot around a number of villages in the region I hitched a lift one evening on the road between Tázlár and Kiskőrös. The driver who picked me up was a young agronomist at the Tázlár cooperative whose main job was to liaise with members about their private farming activities (*háztáji agrónómus*). He liked to have company because his car, a very old Trabant, frequently needed bump-starting and the extra pair of hands was essential. It was soon obvious that he was just the sort of intelligent, well-informed, good-humoured patron that every fieldworker dreams of. Since he was also able, because of his job, to offer all the facilities I needed for my research at the cooperative, I was delighted to be taken under his wing. This was the decisive factor in my decision to choose Tázlár as the village in which I would settle for ten months in 1976-7.

It was also necessary to explain my reasons for being in the village at its other main public institution, the council offices. Here my reception was entirely different. The chairman was a veteran communist of poor peasant origin in Kiskőrös. He had been appointed by the higher authorities soon after the 1956 'counter-revolution', and had no popular base in the village. He lived in a state-owned house a short distance away from his office in the village centre. Unwilling initially to accept my residence in the village, the chairman was consistently obstructive throughout my fieldwork. He would not agree to an interview, nor even to a photograph. When other local officials agreed to help me in compiling census data from council records, he forbade them from

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doing so. I perceived the need to proceed cautiously if I was not to land other people in trouble. The chairman was the only individual in Tázlár with whom I had an awkward relationship of this kind. Other state employees and party officials at all levels were unfailingly helpful; they were either genuinely interested in the subject of my work or polite enough to feign such an interest. But the chairman seemed constantly suspicious of my intentions.

I soon realized that he was very unpopular, both among ordinary villagers and among other white-collar employees. Some villagers complained about favouritism in the allocation of jobs at the council. A more common complaint was that he demanded payments to facilitate the extension of public services to farmers, notably the electrification of the community's outlying farms. I gradually became convinced that these allegations were not being made just for my benefit: they were widely discussed and sincerely believed by villagers. Of course I had no firm evidence that the chairman was corrupt, but there was a good deal of circumstantial evidence. The chairman's official salary was modest and his wife, unlike almost all other women, did not work at all. Yet they had recently bought houses and cars for their sons as well as a private home for themselves in Kiskőrös.

In my monograph I chose not to elaborate on the personal sense of frustration that I felt over the way I was treated by the council chairman. I think my reasoning was as follows. Had I entered into the personal details of my frosty reception and the later obstructiveness, this might have weakened rather than added to my general analysis of the workings of the local political system. Briefly, this was a rather straightforward and highly unflattering picture. Unlike some other anthropological analysts of local government (Sampson 1984a) I came close to arguing that local responses were immaterial to the outside authorities, that party dominance was complete and left no room for local autonomy in the realm of politics. Some readers might have been inclined to attribute such an apparent vindication of 'totalitarian' models of socialist society to the 'cultural baggage' that I had brought with me from the west. I therefore wanted to support the analysis with specific examples of the council's anti-democratic behaviour (e.g. in relation to the women's group in the upper hamlet). I did not want to give readers any grounds for suspicion that personal inconveniences created by the chairman might lie behind the overall picture I presented (see 1980a: Ch.5).

It is of course difficult to prove that my analysis in this chapter does not reflect western bias. Ostensibly, I had been arguing *against* the intellectual coherence of totalitarian models since my undergraduate days, but had I

nonetheless absorbed so much from earlier studies of communism that I was incapable of seeing political activity in Tázlár in any other terms? Be that as it may, I think I was right to try to set out as much evidence as I could in an impersonal fashion to support the diagnosis I wished to make. I set out data about economic production, and about the social inequalities consequent upon the new policies to stimulate the small-farm sector, in a similar way. The main thrust of my analysis here was that more state intervention and regulation was called for, in order to overcome some of the social problems associated with the 'specialist cooperatives'. My whole approach may have been conditioned by ideological debates in the west about socialism, but given the contrast between my portrayals of the political and the economic domains, I don't think I could be accused of distorting all the evidence to suit a critique from either the bourgeois right or the Stalinist left. In this sense the project was scientific and empirical. I documented facts as I found them, inconvenient as well as convenient: their basic outline could have been agreed by any other observer. Of course the use made of such data was specifically mine. I drew on theories deriving from Chayanov, but also on values deriving from the 1970s western left, to advance arguments that readers would not have to agree with. In particular, anthropological audiences would be rather unlikely to agree with a diagnosis that called for action in the economic sphere that very few local people would have endorsed. Readers should always have this room to manoeuvre and to discriminate. My point is that more personalized accounts are no help in this process, and they may well be a distraction and a hindrance.

Although I did not want the readers of my monograph to speculate about my personal feelings toward the council chairman, I did want them to know about the mixture of fear and contempt in which he seemed to be held generally in the village. I was surprised when the editor at Cambridge University Press expressed her concern that certain passages might be libellous, and insisted on taking legal advice. The lawyers suggested a careful redrafting and my text was toned down. For example, where I had written that there were many in the village who 'scorn his intellectual abilities', scorn had to be altered to 'criticize'. I wrote that 'his personal entrenchment ... has been responsible for the failure of the Council to mobilise or express the views of the population'. This was modified to 'He maintains firm control ... and has restricted the Council's ability to mobilise or express the views of the population in general'. They did not object to leaving an indirect suggestion, in the context of a general discussion of corruption, that the chairman had his price for certain services.⁸ Nevertheless, although I could set out an analysis of the local political system

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as I chose, I feel that I did not succeed in presenting the 'true' standing of the chairman – a serious diminution of the authenticity of my account, but one imposed on me in part by publishing conventions.

In spite of 'post-modern' contentions, I might even dare to use the word true without the quotation marks, and for two reasons. First, I heard from enough people I trusted in 1976-7 to be able to say with confidence that the chairman was indeed widely *reputed* to take bribes. Second, in 1986 corroboration came from friends in the form of a newspaper cutting from the regional newspaper. This reported that the chairman had been found guilty of taking bribes, in kind as well as in cash, over many years. He had been sentenced to sixteen months imprisonment, but the sentence had been suspended owing to his poor health. (Friends in Hungary tell me that this is a fairly typical outcome of the *glasnost* experienced in Hungary in recent years: a good many corrupt officials have been brought to book and disgraced, but they have often managed to evade punishment on health grounds, or because of 'long years of public service'.)

My visits to the village since the main spell of fieldwork was completed in 1977 have been frequent but brief. Even the chairman was more cordial on some of these later visits. I normally visit both local peasant families and members of the élites, and points of view continue to differ much as they did in the 1970s. People know me a little better, and they know that no adverse consequences have flowed from my research; but greater warmth and friendship has not substantially altered the picture they present to me. My agronomist patron was for some years the communist party secretary in the village, on a full time basis. The post was then axed as an economy measure, and he now supervises an ancillary production unit for the cooperative. Many villagers still condemn the public sector, including all those who work at the cooperative and the council offices, as wasteful and liable to corruption. They are sceptical that anything fundamental has changed in the culture of socialism. However, no one alleges that the new council chairman is personally corrupt. Though a communist and not a native of the village, the new chairman has married here and also holds the respected office of primary school headmaster. He sees the former chairman as exemplifying a type of leadership that flourished in the past, but which has no place in the more flexible and open society that has been built in recent decades. I can chat to this man very easily, either in one of his offices or over a meal in his home. But if I attempt to update my earlier study, to investigate whether there have been any changes in the local political system in recent years, I should need to make longer visits and talk to a wider range of people; but I would still choose not to highlight the personal relationships I have forged with the new leaders.

On the basis of this case material from Hungary I therefore reach rather different conclusions from those that seem to emerge from most recent discussions of reflexivity and 'the politics of the fieldwork encounter'. I do not deny reflexivity, but I feel I can live with it – even in the rather special circumstances that necessarily affect the conduct of fieldwork by a western anthropologist in socialist Eastern Europe. I do not feel that the personal element needs to be made more central in my work, and I suggest that, in some respects, to make the analysis more personal is to risk producing a weaker account. The news, many years after my main fieldwork, that the council chairman had been found guilty by the county court was particularly gratifying. Not only did it confirm all those unverifiable stories and rumours, it also indicated to me that the system as a whole, i.e. Hungarian socialist society in the mid-1980s, was not so corrupt as to be incapable of renewal from within. This raises many important further questions for anthropological investigation. What general conclusions can be drawn from the Tázlár data? Is it possible that the power lines were drawn differently here, precisely because villages with 'specialist cooperatives' enjoyed so much more freedom in the economic domain. Is the appointment of a respected schoolteacher to head the local administration a sign that party and state machinery are becoming more responsive to 'civil society'? Anthropologists can play a key role in answering questions like this about changes currently taking place throughout the socialist world; but I suggest that their answers will not impress either colleagues in other disciplines or wider audiences if they become preoccupied with their personal narratives.

Notes

- 1 An anthropology of anthropology*, organized by David Kideckel at the XIIth ICAES Congress, Zagreb, July 1988.
- 2 Jacek also confessed – it sometimes seemed more like a boast – to a pride in his family's former noble status. This was, he told me, quite evident to Poles from his family name, though he could not elaborate with any details of family history. This pride is still common among Poles descended from the country's formerly large gentry classes.
- 3 Risky though it is for a non-specialist to make the suggestion, it seems to me possible that, had Dumont been less concerned with his own ego during his fieldwork among the Panare, he might have committed fewer of the ethnographic errors detected by a later ethnographer (see Henley 1982).
- 4 The reader can, of course, infer a certain amount for himself/herself. For example, when the ethnographer is male, it is obviously open to the critical

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reader to suggest that certain materials distort or suppress from consideration the female point of view. Or a reader might feel that an author's material reveals a political prejudice, to be explained in relation to the ethnographer's class origins, or to some other source of bias. But I would contend that it is up to readers, other ethnographers, and, in the case of classics, later historians of the discipline, to probe these points; and for the ethnographer himself to attempt to preempt these reactions is likely to be counterproductive, to diminish the value of the material being presented.

- ⁵ See Heider (1988: 78) for the suggestion that anthropologists are inclined to exaggerate negative features in a second society when they have become much attached to another after doing fieldwork there first.
- ⁶ ... by defining ourselves as writers of our own brand of creative fiction, we may also write ourselves out of an involvement in the real world with potentially damaging results not only for ourselves but also, more importantly, for those we study. There are already enough politicians, economists, civil engineers and agricultural experts who are prepared to think that they know all that matters about these people; and self-styled "fiction", however we define it, may not be the best way to persuade them that they are wrong.' (Abrahams 1987: 20)
- ⁷ For example, Pat Caplan has recently explained that '... one of reflexivity's basic tenets ... is that the self ... helps shape the ethnographic encounter' (1988: 16).
- ⁸ Although a few names were altered, my general policy in this monograph was not to disguise individual or village identities. It would in any case it would have been fatuous to use a pseudonym in the case of the council chairman.