The repatriation of anthropology: some observations on endo-ethnography

ROB VAN GINKEL

Department of Anthropology, University of Amsterdam, The Netherlands

ABSTRACT   *For a few decades, anthropologists have increasingly turned to the study of their own society and culture. This article addresses the problems and the relative advantages and disadvantages of endogenous ethnography. It devotes special attention to the experiences of medical anthropologists who have conducted fieldwork at home. It is concluded that anthropology at home has no special epistemological position compared with anthropological research abroad, though there may be differences at the practical level of fieldwork and publishing.*

The days when anthropology was automatically associated with accounts of so-called 'primitive peoples' in faraway lands are gone. Although the confrontation and dialogue with others deemed 'exotic' constituted anthropology's initial *raison d'être*, today it is not in the least exceptional that anthropologists study an aspect or a segment of the society of which they themselves are members. Thus, instead of studying ourselves through the detour of studying others—occasionally with the justification that perceiving others as 'exotic' will ultimately lead to the recognition of our own peculiarities (cf. Leach, 1982; p. 127)—many ethnographers nowadays tend to take a short cut.

This fact notwithstanding, participant observation has remained the much-heralded trademark of ethnographic research. In this respect, anthropology differs from all other disciplines in the humanities and social sciences. It is probably the distinct method of participant observation which is the reason why anthropologists doing fieldwork at home reflect upon the implications of their position as natives for their research and its results. At least, historians or sociologists conducting research in their own society do not seem to be as reflexive on this methodological matter.

When anthropologists predominantly went to foreign countries far afield, they considered themselves 'strangers and friends', 'marginal natives' or 'professional strangers'. These terms are an indication of their role as outsiders. Now that anthropologists carry out fieldwork at home, have they become insiders? What are the relative advantages and disadvantages of endogenous ethnography, or

Correspondence to: Rob van Ginkel, Department of Anthropology, University of Amsterdam, O.Z. Achter-burgwal 185, 1012 DK Amsterdam, The Netherlands. Tel: + 31 20 5252614. Fax: + 31 20 5253010. E-mail: vanginkel@pscw.uva.nl

endo-ethnography for short?[a] Does the native anthropologist's view differ from that of his foreign counterpart? What are the particular problems of endo-ethnographers? Does speaking of anthropology at home (Here) and abroad (There) create a meaningful distinction or a false dichotomy? Edmund Leach states that "fieldwork in a cultural context of which you already have first-hand experience seems to be more difficult than fieldwork which is approached from the naïve viewpoint of a total stranger" (1982, p. 24; cf. also Myerhoff, 1980, p. 18). To what extent does his observation hold true? Does intimate knowledge of and identification with one's research group yield a deeper understanding and a more thorough ethnography? Or does an inside view inhibit the perception of familiar socio-cultural patterns, cultural translation and a reflexive stance? These are some of the questions underlying this article, which is based on a review of the relevant literature.

Although I devote some attention to medical anthropology at home, it should be made clear that I am not a medical anthropologist. My interest in endogenous anthropology stems from a dozen or so years of research experience in The Netherlands, my native country. Comparing my own experiences with what is written in anthropological work concerning research at home has made me aware of some of the more general problems anthropologists face when conducting fieldwork in familiar settings. In the case of medical anthropology at home, these problems may be similar to some extent, while its practicioners may also encounter problems that are specific to their subdiscipline. I will attempt to reflect on some of these particular issues, but the present article is intended first and foremost as a piece which medical anthropologists working at home can use to compare notes.[b] Doing so would enable them to discover similarities and dissimilarities in their own experiences and those of endogenous anthropologists conducting research in other thematic fields. In this regard, I can merely make a first move. But first I will discuss the matter of why anthropology came home at all.

The partial repatriation of anthropology

The fact that social and cultural anthropologists are increasingly turning to the study of their own society and culture is a relatively recent phenomenon. To be sure, early armchair anthropologists used piecemeal evidence from non-Western as well as European societies—including their native countries—to test their evolutionary hypotheses. And it is true that in the 19th century in countries without colonies (such as Sweden and Russia) the interest of ethnologists was directed towards the 'primitive within'. But most of those investigating domestic issues in the 19th and early 20th centuries were involved in 'rescue' ethnography. That is, they recorded aspects of native culture which were under threat of extinction. In that sense, their work was more akin to that of folklorists than to that of anthropologists working in the tropics.

Although there are several early examples, it was largely after the Second World War that anthropologists started to undertake fieldwork at home. In

Europe and North America, they began to understand that anthropology is the study of all peoples and cultures and following this realization they discovered many previously neglected research subjects. Almost simultaneously, Western-trained African and Asian anthropologists went back home to do research. To some extent, this brought about a reshuffling of fieldwork locations. However, it was only in the 1960s that endo-ethnography really took off. After a hesitating start it has now achieved a well-established position.

The 'repatriation of anthropology', as Marcus & Fischer (1986, p. 113) dub the partial homecoming of the discipline, was stimulated by a number of political, economic, and academic developments (cf., for example, Boissevain, 1975, pp. 10–12; Aguilar, 1981, p. 15; Messerschmidt, 1981b, pp. 9–13, 1981c, p. 198; Jackson, 1987a, pp. 8–9; Altorki & El-Solh, 1988, pp. 3–4). Firstly, the decolonization process has had a major impact. The authorities of many newly independent states objected to research by foreign anthropologists who had earned themselves a bad reputation during the era of colonialism and in some cases were no longer welcome. The governments of the developing nations were suspicious of neo-colonial intellectual imperialism and authorized fewer research permits. To some extent, the vacancies were filled by native—usually Western-trained—anthropologists. Several governments encouraged or urged them to conduct domestic development-oriented research.

Secondly, with access to some traditional fieldwork locations having become more difficult, many Western anthropologists had to stake out new concerns and new areas close to home. Some preferred the relative freedom of research there to the bureaucratic restrictions of fieldwork in the newly independent states (if they could obtain a permit to do research there at all). In the wake of these developments, it dawned upon them that the study of their own society and culture was both compelling and legitimate. As Anthony Cohen forcefully argues: "If anthropology cannot enlighten the complexities of its own national contexts, then it is impotent and trivial" (1982, p. 17). Anthropologists could also find cultural variation in these national contexts, and it was literally brought home to them that they were ignorant about many aspects of their own society. Some have deliberately sought the exotic and unfamiliar in their own country.

Thirdly, in Europe and North America less funding became available for ethnography in the tropics, especially for expensive large-scale expeditions. At the same time, governments and NGOs increasingly financed applied and policy research at home, for example, regarding ethnic minorities, marginal groupings, crime, and so forth. As regards medical anthropology, Young observes that "anthropologists have been increasingly invited into clinical settings, particularly in connection with programs in primary care and family medicine" (1982, p. 258). However, entry to medical institutions has not been so easy everywhere (see below). The redistribution of research funds has partly been a consequence of the growing number of anthropologists applying for grants. This in turn was caused by the democratization of academic education and the concomitant rise of student numbers. To train these students, many anthropology departments

have organized fieldwork at home or in neighboring countries. Once they discovered that this research could be interesting and rewarding, several graduates pursuing an academic career decided to become endogenous ethnographers. However, the shortage of academic jobs forced many to get involved in applied research for state or private institutions. This kind of endo-ethnography has produced results that are relevant and useful in a practical way.

Fourthly, the so-called 'crisis' in anthropology and the subsequent questioning and rethinking of its theories and methodologies forged the recognition that anthropology is the study of *all* humankind and not some special segments of it. In the Euro-American domain, ethnographers confronted a particularly challenging task, since they could no longer mystify communities as static and isolated social units, a mystification typical of much of their previous ethnographic work in foreign countries. Anthropology's traditional conceptual and analytical apparatus insufficiently equipped these ethnographers for their new task. However, several pioneers successfully tried to bridge this void by rethinking methods and concepts which could be applied in so-called complex societies. Some encouraged young anthropologists to do fieldwork in their backyards. At the same time, there was an increasing self-awareness on the part of many Third World anthropologists and anthropologists in Western countries who belonged to specific ethnic minorities. They felt they had a special obligation in doing research which could be of practical use to 'their' people. Some of them also preferred the view from within to the view from without, because the latter might harbor bias or even ethnocentrism.

At home in the field

Among many other things, the ethnicity, gender, class, religion, residence, age, marital status, education, speech, appearance, ability, and personality of the interlocutors affect the ethnographer's fieldwork, including his or her relations with informants, the information solicited and gained, and, as a corollary, the final ethnographic product. This may seem a truism, but the majority of anthropologists hardly account for the impact of autobiography on ethnography. If we accept that autobiography and the presentation of self have an effect on the nature and results of fieldwork, then it follows that being a native anthropologist has consequences for the ethnographic encounter and the ethnographer's writing. In other words, all ethnographers are 'positioned subjects' (Okely, 1992, p. 14). In the case of medical anthropologists, overlapping experiences in the biographies of researcher and interlocutor are more likely when fieldwork is done at home rather than abroad (van Dongen, 1996, p. 343) and this may have a strong impact on the research (Ria Reis, this issue).

According to Emiko Ohnuki-Tierney, "[t]he effect of the presence of an anthropologist differs greatly between native and nonnative anthropologists" (1984b, p. 585). On the basis of her research concerning illness and culture in contemporary urban Japan, she claims that this has profound theoretical and even epistemological implications (1984a, p. 14). Ohnuki-Tierney explains that

informants 'perform' for outsider anthropologists, who at best document a 'negotiated reality'. Insiders, on the other hand, have an *a priori* intimate knowledge of their own culture but face the problem of 'distancing'. This may be true in some cases, but the aforementioned aspects of the interlocutors' personas influencing their dialogue are of course to a large extent also of consequence in research at home. Ohnuki-Tierney clearly generalizes and exaggerates the discrepancy between anthropologists from within and from without. For example, Indonesian anthropologist Koentjaraningrat writes that he experienced his research among fellow Javanese "almost as if studying an unfamiliar environment" and that he "could not even take the basic cultural elements and value orientation for granted" (1982, p. 178). The class difference between the anthropologist and his informants prevented him from becoming an insider and he felt he could not penetrate their subculture.

It is sometimes claimed that in other cultures anthropologists are "initially treated as outsider[s], rebuked for rule breaking and by varying degrees incorporated and rejected" (Okely, 1992, p. 14). Apparently, this may also hold true when they do research in their own societies. The Dutch anthropologist Lodewijk Brunt (1975) makes a point of stressing that it is fundamentally wrong to assume that ethnographers doing fieldwork in their own societies only have to deal with people who adhere to closely corresponding values: "Not only may there be a difference between the anthropologist and his informants, but it is very likely that there are differences between various groups of informants as well" (p. 36). Brunt observes that he was not at all familiar with all kinds of conventional rules in the village where he did research, nor could or would he call himself an insider *vis-à-vis* certain local groupings.

These examples and remarks serve as a warning not to overemphasize the differences between anthropology at home and abroad. We should not take the relative advantages and disadvantages of both kinds of research in an absolute way. Obviously, as a matter of degree there are pros and cons, but judging by the literature the experiences of anthropologists differ widely in this respect. Consider, for example, the aspect of linguistic competence. Sharing a language with informants is an asset because it facilitates communication, saves time, and enables avoiding distortion by interpreters. Many endo-ethnographers mention these as important advantages for those conducting domestic research (see, for example, Jones, 1970, p. 252; Hayano, 1979, p. 101). However, using one's mother tongue does not necessarily mean that communication is unequivocal or that anthropologists can take the words of their informants at face value (Brunt, 1979, p. 88; Hastrup, 1993a, p. 151; Gefou-Madianou, 1993, pp. 167–168). In addition, dialect or occupational lingo may distort communication considerably. Okely states that to do research among Traveller–Gypsies in her native country, she had "to learn another language in the words of [her] mother tongue" (1984, p. 5). Even if a shared language facilitates communication, this does not mean that an ethnographer will automatically elicit information by asking questions, since these questions are unsolicited. Being 'familiar' with the culture studied can even be deceptive (cf. Greenhouse, 1985, p. 261).

Informants will not be very permissive when native anthropologists break their cultural rules. They are supposed to know, and the margins for blundering are quite small. If they challenge certain norms, they risk estrangement or ostracism. Outsiders may be granted much more room in this respect. They are excused for their misunderstandings, improprieties, insensitivities—in short, their ignorance—precisely because they are outsiders. In this respect, anthropologists from abroad are 'privileged strangers' (Graham, 1981, p. 119). And in redressing the foreign anthropologist's behavior, informants unconsciously 'betray' their cultural codes to him or her. Moreover, it may be more convenient to deal with an outsider because—in private at least—it offers an opportunity to relax conformity to cultural conventions or behavioral standards precisely because he or she is a non-interested party. In contrast, being a true insider (that is, being a member of a subculture under study) is never "a neutral, uninvolved position" (Nakleh, 1979, p. 344). He or she is known, and there is no escape from control by the researched group, while at the same time its members cannot risk non-conformism: "indigenous anthropologists find themselves in a great variety of positions vis-à-vis the local groups that affect their self-perceived roles and the expectations of the local community" (Fahim *et al.*, 1980, p. 647). The danger of being so enmeshed and understanding so much as an ethnographer is that it may become impossible to retain the role of detached observer in certain situations "because one is caught up in the demands of the various other roles he or she occupies" (Stephenson & Greer, 1981, p. 128).

It is often pointed out that endo-ethnographers are in a privileged position because of their *a priori* intimate knowledge and comprehensive view of their own culture and society. Being familiar with many of each other's idiosyncrasies facilitates the interlocutors' dialogues. In this connection, a host of more or less related advantages is mentioned in the literature. Endo-ethnographers are supposed to have: a good understanding of the macro-society and its daily routines, symbols and value systems; no culture shock; feelings of empathy; and easy access to the intellectual, emotive, and sensory dimensions of behavior.[c] In his article on anthropology at home in America, Michael Moffat summarizes these claims as follows: "Studying subjects relatively 'like themselves,' local ethnographers may be more attuned to cultural nuance than far-from-home anthropologists, better able to draw on experiential understanding. They can often 'blend in' more completely—verbally, behaviorally, physically—possibly making for better *rapport*, possibly affecting who and what they are studying less by their presence" (1992, p. 206). Others, however, are far less optimistic and mainly emphasize the difficulties involved in doing fieldwork in the culture in which one is raised, precisely because of the matter of intimate knowledge and familiarity. Given the fact that there is much controversy regarding this aspect of ethnography at home, it merits further discussion.

Familiarity, distancing and detachment

It was Malinowski's dictum that to understand other people's cultures required

grasping the natives' point of view. But some anthropologists claim that studying their own culture involves a mental *tour de force*. Some deem it not unlike trying to push a car while being inside it, observing a parade whilst marching along, or being a fish attempting to see the water. Though we could probably come up with a dozen more metaphors, the problem boils down to the question of how to study one's culture when one lives it. How is it possible to prevent overlooking important matters and patterns that one sees, hears, and smells every day? It involves a process of making the familiar and the taken-for-granted seem strange to oneself so that one can record what in the first instance seemed insignificant (Hayano, 1979, p. 102; Feldman, 1981, p. 237; Stephenson & Greer, 1981, pp. 124–125; Weston, 1991, p. 14).

Of course, the problem is particularly acute for those anthropologists who conduct fieldwork in settings of which they have intimate knowledge. Kath Weston, for example, did research among lesbians in the San Francisco Bay Area where she (herself a lesbian American) spent years in university. Being so much a part of the subculture, "[p]resumptions of a common frame of reference and shared identity can ... complicate the anthropologist's task by leaving cultural notions implicit, making her work to get people to state, explain, and situate the obvious" (1991, p. 14). Even so, the lesbian subculture has its own subcultures, which may mean that a lesbian researcher would most likely be an outsider *vis-à-vis* several of these sub-subcultures. And, as Weston cautions, "every situation carries its exoticisms, insofar as the exotic is always defined in relation to a set of assumptions held by the observer" (Weston, 1991, p. 224, note 12). Endo-ethnographers may express surprise or shock "which can only be explained with reference to perceptions or experiences that contradict a researcher's preconceived expectations" (Weston, 1991).

Though it is generally assumed that it is advantageous to have a fund of inside knowledge, this clearly creates problems of its own. Some anthropologists have for some time failed to go beyond the self-evident. Edmund Leach (1982, pp. 124–126) attributes this hazard to the ethnographer's initial preconceptions and prejudices, which derive from private rather than public experience. They are liable to distort the vision of ethnographers studying their own society in a way that does not affect the *naïve* ethnographers from without.[d] What informants say may seem like common sense to the ethnographer-cum-insider. Emily Martin, who conducted research in her native United States on women's perceptions of human reproduction, faced this problem. It was only after she realized that the commonsensical 'obvious facts' her interviewees told her were also in need of explanation that she could re-approach her material from a new perspective. The length of time which passed before this *Gestalt switch* occurred, writes Martin, "stands as vivid testimony to how solidly entrenched our own cultural presuppositions are and how difficult it is to dig them up for introspection. The one I stumbled over was my acceptance of scientific, medical statements as truth, despite many warnings I had made to myself and heard from others about precisely this kind of danger when one tries to do fieldwork in one's own society" (1987, pp. 10–11).

Thus, whereas for anthropologists doing fieldwork abroad the problem is how to get *into* a culture, those conducting research at home may face the problem of how to get *out* in order to enable them to have an ethnographic gaze at familiar social environments. The process of distancing oneself from the research subject may prove difficult, since the situations studied can be almost identical to those confronting the ethnographers in daily life. In this connection, Ohnuki-Tierney writes: "If studying cultures other than our own represents a journey out from and back to our collective self, as embodied in our own culture, and if 'distancing' is critical for this endeavor, then it follows that native anthropologists face an even more difficult task in creating enough distance between themselves and their own culture" (1984b, p. 584; see also 1984a, p. 16). How is it possible to reach a deeper understanding of phenomena that may seem self-evident? How does one perceive cultural assumptions shared with the subjects of research? How can we get at tacit culture without experiencing the contrast and difference inherent in cross-cultural research which can attune the investigator to this tacit culture? Many anthropologists propose taking a comparative perspective, whether implicit or explicit (cf., for instance, Madan, 1982, p. 9; Sörbö, 1982, p. 156; Moffat, 1992, pp. 206–207; Kanaaneh, 1997, p. 3).

An example of explicit comparison is American anthropologist Loring Danforth's study of religious healing in Greece and the United States. He juxtaposes the cases of the American Firewalking movement and the Greek Anastenaria, which he sees as two similar yet different therapeutic systems, to gain new insights into aspects of both Greek and American culture. The American Firewalking movement seemed just as foreign and exotic to him as the Anastenaria, and he felt the need to distance himself from the former because it was more troubling and threatening to him than the latter. As Danforth explains: "It strikes too close to home. It forces me to abandon the safety and comfort that my relativism has provided. I can no longer bracket the crucial questions I have always refused to address. My work with the American Firewalking movement has made the Anastenaria seem much less distant and foreign, but it has also estranged me even more from my own culture, a culture I thought I knew" (1989, p. 291).

For sure, knowledge of other cultures, of the cultural variety in human societies, will help to see the taken-for-granted as well as the not-so-obvious. According to Marcus & Fischer, "Cross-cultural perspectives still have an important role to play in carrying out projects of repatriated ethnography, in defining novel approaches to taken-for-granted domestic phenomena, in framing questions, and in suggesting alternatives or possibilities among domestic subjects that are only revealed by comparative contrast with other cultural material" (1986, pp. 135–136). In order to defamiliarize oneself, they suggest disrupting common sense, for example, by doing the unexpected or placing familiar objects in unfamiliar contexts (pp. 137–138). But this may be ill advice. It could mean intentionally transgressing cultural codes, with all the risks this implies.

Several authors point to time economy as an advantage of endo-ethnography.

However, in contrast, others emphasize that anthropologists at home should not economize with time, but should conduct their research slowly so that the relations between the interlocutors can develop (Segalen & Zonabend, 1987, p. 117). Some endogenous researchers even go so far as to spend time abroad in order to defamiliarize or distance themselves before entering the field in their 'own' culture (Perin, 1988). There are anthropologists who maintain that before doing research at home, anthropologists should have done cross-cultural research to enable 'objective' observation (Nakane, 1982, p. 58).

Home blindness and heterogeneity

However, though detachment may avoid home blindness, 'distance' should not be confused with 'objectivity'. Many anthropologists who have rejected anthropology at home in the past have done so because they deemed it inherently subjective as opposed to the 'objective' approach of traditional anthropology.[e] This assertion is much too simplistic. Today, it is widely accepted that objectivity in ethnographic research is an illusion. At best, it is inter-subjective. To quote the French ethnologists Martine Segalen and Françoise Zonabend at some length:

> Whether one is familiar with or a stranger to the culture one is working on, there are no absolute grounds for considering the degree of cultural difference between object and observer as either an obstacle or an advantage with regard to its objective description. Certainly it can happen that, to the observer who is close to the culture which he or she is studying, the object can seem at first to be profoundly familiar, forming part of his immediate universe. In that case facts, attitudes, behaviour patterns seem hopelessly self-evident and so indescribable, because [they are] colourless, insipid, without precise contours, as if bathed in the implicit. In these extreme situations, the first imperative, which is only the obverse of that which applies to the ethnologist of the exotic, is to defamiliarize himself with the object, to re-create artificially that distance and perspective without which any perception is impossible. This distancing, this externality to the object, can be achieved when observer and observed come to know and take into account their respective positions ... Exoticism certainly offers data which are immediately and easily descriptive because they are new, whereas familiarity blurs the object to be described. *Both can prove to be deceptive.* If one rejects the complicity of the strange and the illusion of the known, then ethnographic 'fields', distant or near, are revealed as on an equal footing (Segalen & Zonabend, 1987, p. 111, emphasis added).

Again, a caution is in order. The problem of being an insider, who in order to gain a good vantage point has to pull himself up by the boot straps like a Baron von Münchhausen, is often exaggerated. We should not conceive of societies and cultures as homogeneous monoliths. Nor do anthropologists

possess a complete fund of knowledge pertaining to their own society: The endo-ethnographer "does not come into the field with all the knowledge and experiences generated by the various and complex structures of societies" (Shami, 1988, p. 135).

The degree to which investigators are genuine 'insiders' can differ widely, even when they are in their own country and do research among people with the same ethnic background (Narayan, 1993). Ethnographers may not suffer a 'culture shock' in confronting informants at home, but there is certainly a chance that they will encounter a 'subculture shock'. Zdzislaw Mach points out that he experienced a curious mixture of both familiarity and estrangement when doing fieldwork in a community in his native Poland (Mach, 1994, p. 44). Even in a small country like The Netherlands, an anthropologist can discover that something familiar like a morning coffee drinking ritual is performed and is lent significance in a different way within a rural community than in a city (Brunt, 1979). Urban settings are usually made up of so many subcultures that anthropologists will surely not be familiar with all of them. But even in general, "there is little likelihood of true insider research ever becoming common: the ethnographer will always be somewhere on the continuum between empathy and repulsion, home and strangeness, and seeing and not seeing" (Sarsby, 1984, p. 132).

Besides, it is not only significant where and among whom investigators conduct fieldwork; the research subject and goal are equally important. If anthropologists working in their own society try to elicit information which is supposed to be known, they surely face the difficulty of interpreting 'obvious facts'—if they get answers to their questions at all. However, if they seek to get an inside view of, say, the experiences and thought worlds of schizophrenics or epilepsy sufferers, they are bound to be overwhelmed with information which is not at all familiar. In medical anthropology, there are several examples of fieldworkers conducting research at home who still faced the problem of getting *inside* what is supposed to be a familiar culture. Some have gone at great lengths to do precisely that. For example, Barbara Myerhoff (1980), who did fieldwork in a Jewish Senior Citizens' Center, mimicked physical impairments so as to simulate the problems of functioning when very old: "At various times, I consciously tried to heighten my awareness of the physical feeling state of the elderly by wearing stiff garden gloves to perform ordinary tasks, taking off my glasses and plugging my ears, slowing down my movements and sometimes by wearing the heaviest shoes I could find to the Center" (p. 18). Sue Estroff (1981) went even further in this mode of 'imaginative identification'. She took antipsychotic medication for several weeks in order to undergo and share its strong side-effects with the psychiatric outpatients she studied. But a researcher's identification with patients may create problems of its own, as in the case of dying children (Bluebond-Langner, 1978). Getting inside a medical institution to do research is not easy. There are bound to be many gatekeepers, for instance, in the form of ethical committees. It took Robert Pool (1996), an anthropologist who studied voluntary euthanasia practices in a Dutch hospital,

a year before he gained access. In his ethnography, Pool details the mutual communication patterns, negotiations and interactions between patients and physicians in view of the patients' euthanasia requests. A physician had warned him in advance that it would be easier to enter secret societies in Africa than to conduct research among physicians. Even when one succeeds in beginning fieldwork, one has to deal with a "large and fluctuating cast of characters" (Anspach, 1993, p. 187). Though the medical staff may be available as interlocutors, they are often busy going about their business, while patients come and go. In the case of extramural health care, there is no concrete 'field' where anthropologists can conduct participant observation (cf. van Dongen, 1996, p. 342).

Fieldwork settings in hospitals in one's own society may appear familiar, but appearances can be deceptive. In an interview with a newspaper reporter, Robert Pool says that he expected to see familiar things in the hospital where he did research but that he entered a completely unfamiliar world with different codes of conduct and curious hierarchical structures (*de Volkskrant*, 11 May 1996). Anne-Mei The (1997), who also conducted research into voluntary euthanasia practices in a Dutch hospital (focusing on the role of nurses in the decision-making process), similarly states that she suffered a 'culture shock'. In medical institutions at home, anthropologists are outsiders, strangers who have to learn the staff's biomedical language, thought and practices and the patients' representations of and mental coping responses to illness and affliction. Fieldwork in such 'familiar yet unfamiliar' institutions may even lead to estrangement. After almost 10 years of anthropological research experience in a Dutch psychiatric hospital, Els van Dongen (1996, p. 341) contends that she feels less at home in her 'own culture' than previously.

More generally, though native anthropologists will share some cultural codes with members of subcultures, other conventions, rituals, and taboos of the latter will strike them as idiosyncratic. Thus, "the extent of relative insidedness and 'identity' between researcher and subjects is best conceived of as a continuum from virtual oneness to a marginal nearness" (Messerschmidt, 1981a, p. 8). Furthermore, the special problems of ethnography at home seem closely linked to the method of participant observation. For instance, those anthropologists using historical data, although facing numerous other difficulties, in many cases do not have to deal with the problem of familiarity in their inquiries. The longer the span of time which has elapsed between the lives of their research subjects and the investigation, the greater the distance between them and their 'informants'. This is probably the reason why historians hardly ever reflect on the problem of doing 'historiography at home'.

Practical problems

Once data are collected, categorized, and interpreted, the next problem of ethnographic writing is what or what not to publish. According to Hussein Fahim, "People who are subjected to anthropological research have a right to

see its finding" (1977, p. 83). I agree, though publishing one's ethnographic results is fraught with problems. This is particularly true when doing anthropology at home. If one carries out research abroad, there is often a barrier between the language used to communicate in the field and the language used to publish the results of fieldwork. In this case, ethnographers are also separated from the field and have a great deal of leeway in what they write, without having to worry that sponsors or informants may be reading over their shoulders. To circumvent this situation, endogenous ethnographers may opt to avoid using their native language and publish in English, the international academic *lingua franca*. (Of course, if English is their native language this is of no use.) On the other hand, at home, geographical proximity makes it easier to maintain relationships with informants: to keep up to date, to check data, and to ask for their opinion about reports in preparation or draft papers. It is also possible to get feedback from other anthropologists. The 'inside' view may correct culturally biased projections of foreign ethnographers in a similar way that the latter's perspective can lead to adjusting insider bias. The dialogue between native and foreign anthropologists doing fieldwork in each other's vicinity would mean an enrichment of perspective. Though their views are potentially mutually beneficial, so far most anthropologists from within and from without who share a fieldwork setting seem more often than not to disagree. Debates on the basis of published material often boil down to claims over ethnographic authority. In such cases, exchange of information and dialogue is rare.

But let me return to the problems in publishing the results of endogenous ethnography. In numerous countries, national bureaucracies will not only dictate what may be researched and which methods can be used, but also what may be published and what not (El-Solh, 1988, p. 113; Okely, 1987). This is not just the case in societies ruled by authoritarian regimes. With respect to contract research in particular, government representatives and state officials in Europe or North America often want to maintain control over what is stated in the final ethnographic product. Sometimes they will even try to prevent publication of unwelcome results or decree in advance what those results should be. Needless to say that this puts ethnographers in an awkward position as to their intellectual freedom. However, it is not just bureaucracies or other institutions commissioning research which may be trying to influence its outcome. In several cases, in particular when 'studying up', informants may demand more than a say in what is said about them. I would not be surprised at all if this would apply to fieldwork in medical institutions.

As Judith Okely observes, "When publication is in the same country as fieldwork, the anthropologist cannot escape being read or misread by a wide range of interested parties beyond the usual academic constituency. The text will therefore bear the marks of such future scrutiny" (1984, p. 5). Many ethnographers refrain from writing things which are potentially harmful to their informants' interests. This is true in general, but it is much easier to conceal their identity when working abroad. If a third party, for example, a news reporter, reveals the informants' identities, this would disrupt the relations

between the interlocutors and would perhaps lead to strife in the community where research was conducted (Nakleh, 1979, p. 349; Brunt, 1975). In the case of endo-ethnography, auto-censorship may also serve to protect one's self-interests. Of course, it is easy to be called to account by sponsors, respondents or fellow scholars in one's own society. Therefore, it is probably far from exceptional that ethnographers will be extremely careful—perhaps even overly careful—in deciding what to make public and what not. The anticipated continued relationship between investigators and their informants may affect what the former write. They face more direct dilemmas in decisions concerning whether or not to withhold certain information than their counterparts working abroad (Jones, 1970, p. 255). Ethnographers must also decide on what to report and what they can consider common knowledge by their potential readership.

Conclusions

It should be clear by now that the debate pertaining to the advantages and disadvantages of endo-ethnography remains unresolved and is to be continued. Yet, some preliminary conclusions are in order. Given the heterogeneous character of all cultures, it is not easy to find the locus where any anthropologist would genuinely be 'at home'. Even if such a setting exists, there will be many unfamiliar situations and moments in which researchers will either assume or will be ascribed the role of outsiders (Altorki & El-Solh, 1988, p. 16). Moreover, like all people they may "have many strands of identification available" (Narayan, 1993, p. 673). As Sonia Ryang rightly observes, anthropology at home "is infested with difference, diversity and division to the same extent as anthropology of other cultures and societies" (1997, p. 34). She claims that the difference between endogenous and exogenous anthropology lies in the epistemological terrain (p. 25).

But in the final analysis, attributing a special epistemological position to endo-ethnography may prove yet another anthropological mystification. It would rigorously dichotomize anthropology Here and There or Own and Other culture, and would ultimately bring about the fragmentation of anthropology into a plethora of ethnographies more or less written from within. Both the native and the foreign anthropologist operate as interpreters who are involved in cultural translation. Moreover, we should be careful with boundary marking and compartmentalizing the world. Boundaries are ambiguous. As Judith Okely succinctly states: "The division between 'known' or 'other' culture can be defined neither by national nor geographical territory. The exotic should be displaced" (1996; p. 1). It is only at the practical level of fieldwork and publishing, and not at the analytical level, that differences between anthropology at home and abroad exist—or should exist, for in a theoretical respect there should be no differences in the common endeavor of understanding humanity in all its heterogeneous aspects (Hastrup, 1987, p. 105). In this connection, it is important that endo-ethnographers reconsider the problem of involvement and detachment and of distancing processes. The project of defamiliarizing the

familiar may lead to deliberate 'exoticization' and 'otherization' or objectifying the other (Brown, 1994, p. 422ff.). Studying the kaleidoscope of cultures does not imply that we need another anthropological specialism for each and every social and cultural or subcultural reality (Sörbö, 1982, p. 155). The crucial point is not where anthropologists hail from, but how they perceive and interpret the reality they confront (Fahim, 1977, p. 86, note 1). Wherever they conduct research, to do their jobs properly anthropologists must remain 'professional strangers' with a keen sense of involvement and distance: "The tension between the need for both empathy and detachment is a problem facing *all* anthropologists" (Sarsby, 1984, p. 129), not just those working either at home or abroad. This very same tension enables the writing of ethnography and lends the anthropological perspective its special flavor. As Kirin Narayan contends: "Writing texts that mix lively narrative and rigorous analysis involves enacting hybridity, *regardless of our origins*" (1993, p. 682, emphasis added).

Notes

(a) By endogenous anthropology or ethnography, I simply mean research in the anthropologist's own national setting and ethnic group. I will use the term interchangeably with anthropology or ethnography at home. There is no consensus on what to call anthropology one does in one's own society. Besides 'anthropology at home' and 'endogenous ethnography', there is a plethora of names used to indicate this kind of research. Among the terms used are: 'native', 'domestic', 'indigenous', 'auto-', 'local', and 'insider' anthropology or ethnography (Messerschmidt, 1981b, p. 13, 1981c, p. 197, note 1). Although these concepts are often used as synonyms, some anthropologists distinguish between insider anthropology or anthropology at home (preferred by North American and European anthropologists who conduct fieldwork in their own society), native anthropology (ethnic and minority anthropologists doing research in their own ethnic group), and indigenous anthropology (used by Third World anthropologists working in their own countries, often taken to be synonymous with 'Third World perspective') (Fahim, 1977; Fahim *et al.*, 1980; Messerschmidt, 1981b, p. 13). Colson argues that the last concept is a misnomer when understood as 'Third World perspective', since we are all indigenous somewhere (in Fahim *et al.*, 1980, p. 650; Cernea, 1982, pp. 122–124). For various reasons, some anthropologists object to the term native anthropology (Jones, 1970, pp. 257–258; Cernea, 1982, pp. 122–124) or native anthropologists (Knowlton, 1992; Hastrup, 1993a, 1993b; Narayan, 1993). Kirsten Hastrup even considers native anthropology to be "a contradiction in terms" (1995, p. 159). The addition 'at home' in 'anthropology at home' is not very specific and sometimes refers to a vague category of European societies. Thus, a Danish anthropologist seems to be doing anthropology at home in Iceland (cf. e.g. several contributions in Jackson, 1987b).

(b) The original version of this paper was presented at the conference 'Medical Anthropology at Home', Zeist (The Netherlands), 15–18 April 1998, where it served a similar purpose.

(c) See, for example, Hayano, 1979, p. 101; Fahim *et al.*, 1980, p. 646; Aguilar, 1981, p. 17ff.; Altorki, 1982; Cernea, 1982, pp. 129–131; Koentjaraningrat, 1982, p. 176; Ohnuki-Tierney, 1984a, p. 18, 1984b, p. 585; Strathern, 1987, p. 17; Altorki & El-Solh, 1988, pp. 7–8; Gefou-Madianou, 1993, p. 162; Hastrup, 1993a, pp. 155–156. Compare also the special issues on 'Endo-Ethnography' of *Etnofoor* [1994, 7(1)] and ' "Indigenous" Anthropology: Paradox and Praxis' of *Dialectical Anthropology* [1997, 22(1)].

(d) Several anthropologists claim that subjectivity and cultural bias are inherent in endogenous ethnography. However, both insider and outsider perspectives "are subject to misconceptions based on different *a priori* assumptions" (Nakleh, 1979, p. 345).

(e) On various forms of bias mentioned by critics of anthropology at home see John Aguilar (1981, p. 22ff.).

References

AGUILAR, J. L. 1981. Insider research: an ethnography of a debate. In MESSERSCHMIDT, D. A. ed. *Anthropologists at Home in North America. Methods and Issues in the Study of One's Own Society*, pp. 15–26. Cambridge: Cambridge University Press.

ALTORKI, S. 1982. The anthropologist in the field: a case of 'indigenous anthropology' from Saudi Arabia. In FAHIM, H. ed. *Indigenous Anthropology in Non-Western Countries*, pp. 167–175. Durham, NC: Carolina Academic Press.

ALTORKI, S. & EL-SOLH, C. F. 1988. Introduction. In ALTORKI, S. & EL-SOLH, C. F. eds. *Arab Women in the Field. Studying Your Own Society*, pp. 1–23. Syracuse, NY: Syracuse University Press.

ANSPACH, R. R. 1993. *Deciding Who Lives. Fateful Choices in the Intensive-Care Nursery*. Berkeley: University of California Press.

BLUEBOND-LANGNER, M. 1978. *The Private Worlds of Dying Children*, Princeton: Princeton University Press.

BOISSEVAIN, J. 1975. Introduction: towards a social anthropology of Europe. In BOISSEVAIN, J. & FRIEDL, J. eds. *Beyond the Community: Social Process in Europe*, pp. 9–17. The Hague: Department of Educational Science of the Netherlands.

BROWN, B. A. 1994. Born in the USA: American anthropologists come home. *Dialectical Anthropology*, 19, 419–438.

BRUNT, L. 1975. On rule and reality of publishing ethnographic research data. In KLOOS, P. & VAN DER VEEN, K. W. eds. *Rule and Reality. Essays in Honour of André J.F. Köbben*, pp. 17–28. Amsterdam: Universiteit van Amsterdam, Afdeling Culturele Antropologie, Publicatie No. 8.

BRUNT, L. 1979. Veldonderzoek in de 'eigen cultuur'. Antropologische ervaringen in een Nederlands dorp. *Amsterdams Sociologisch Tijdschrift*, 6, 79–99.

CERNEA, M. 1982. Indigenous anthropologists and development-oriented research. In FAHIM, H. ed. *Indigenous Anthropology in Non-Western Countries*, pp. 121–137. Durham, NC: Carolina Academic Press.

COHEN, A. P. 1982. Belonging: the experience of culture. In COHEN, A. P. ed. *Belonging, Identity and Social Organisation in British Rural Cultures*, pp. 1–17. St. John's: ISER, Memorial University of Newfoundland.

DANFORTH, L. M. 1989. *Firewalking and Religious Healing. The Anastenaria of Greece and the American Firewalking Movement*. Princeton: Princeton University Press.

EL-SOLH, C. F. 1988. Gender, class, and origin. Aspects of role during fieldwork in Arab society. In ALTORKI, S. & EL-SOLH, C. F. eds. *Arab Women in the Field. Studying Your Own Society*, pp. 91–114. Syracuse, NY: Syracuse University Press.

ESTROFF, S. E. 1981. *Making it Crazy. An Ethnography of Psychiatric Clients in an American Community*. Berkeley: University of California Press.

FAHIM, H. M. 1977. Foreign and indigenous anthropology: the perspectives of an Egyptian anthropologist. *Human Organization*, 36, 80–86.

FAHIM, H. M. *et al.* 1980. Indigenous anthropology in non-Western countries: a further elaboration. *Current Anthropology*, 21, 644–663.

FELDMAN, K. D. 1981. Anthropology under contract: two examples from Alaska. In MESSERSCHMIDT, D. A. ed. *Anthropologists at Home in North America. Methods and Issues in the Study of One's Own Society*, pp. 223–237. Cambridge: Cambridge University Press.

GEFOU-MADIANOU, D. 1993. Mirroring ourselves through Western texts: the limits of an indigenous anthropology. In DRIESSEN, H. ed. *The Politics of Ethnographic Reading and Writing. Confrontations of Western and Indigenous Views*, pp. 160–181. Saarbrücken: Verlag Breitenbach.

GRAHAM, S. B. 1981. Social networks and community administration: a comparative study of two mining towns. In MESSERSCHMIDT, D. A. ed. *Anthropologists at Home in North America. Methods and Issues in the Study of One's Own Society*, pp. 106–120. Cambridge: Cambridge University Press.

GREENHOUSE, C. J. 1985. Anthropology at home: whose home? *Human Organization*, 44, 261–264.

HASTRUP, K. 1987. Fieldwork among friends: ethnographic exchange within the Northern civilization. In JACKSON, A. ed. *Anthropology at Home*, pp. 94–108. London: Tavistock.

HASTRUP, K. 1993a. Native anthropology: a contradiction in terms? *Folk*, 35, 147–161.

HASTRUP, K. 1993b. The native voice—and the anthropological vision. *Social Anthropology*, 1, 173–186.

HASTRUP, K. *A Passage to Anthropology. Between Experience and Theory*. London: Routledge.

HAYANO, D. M. 1979. Auto-ethnography: paradigms, problems, and prospects. *Human Organization*, 38, 99–104.

JACKSON, A. 1987a. Reflections on ethnography at home and the ASA. In JACKSON, A. ed. *Anthropology at Home*, pp. 1–15. London: Tavistock.

JACKSON, A. ed. 1987b *Anthropology at Home*. London: Tavistock.

JONES, D. J. 1970. Towards a native anthropology. *Human Organization*, 29, 251–259.

KANAANCH, M. 1997. The 'anthropologicality' of indigenous anthropology. *Dialectical Anthropology*, 22, 1–21.

KNOWLTON, D. 1992. No one can serve two masters, or native anthropologist as oxymoron. *International Journal of Moral and Social Sciences*, 7, 72–88.

KOENTJARANINGRAT 1982. Anthropology in developing countries. In FAHIM, H. ed. *Indigenous Anthropology in Non-Western Countries*, pp. 176–192. Durham, NC: Carolina Academic Press.

LEACH, E. 1982. *Social Anthropology*. London: Fontana Paperbacks.

MACH, Z. 1994. 'Anthropology at home' in a migrant community. *Ethnofoor*, 7, 41–52.

MADAN, T. N. 1982. Anthropology as the mutual interpretation of cultures: Indian perspectives. In FAHIM, H. ed. *Indigenous Anthropology in Non-Western Countries*, pp. 4–18. Durham, NC: Carolina Academic Press.

MARCUS, G. E. & FISCHER, M. M. J. 1986. *Anthropology as Cultural Critique. An Experimental Moment in the Human Sciences*. Chicago: University of Chicago Press.

MARTIN, E. 1987. *The Woman in the Body. A Cultural Analysis of Reproduction*. Milton Keynes: Open University Press.

MESSERSCHMIDT, D. A. 1981a. Constraints in government research: the anthropologist in a rural school district. In MESSERSCHMIDT, D. A. ed. *Anthropologists at Home in North America. Methods and Issues in the Study of One's Own Society*, pp. 185–201. Cambridge: Cambridge University Press.

MESSERSCHMIDT, D. A. 1981b. On anthropology 'at home'. In MESSERSCHMIDT, D. A. ed., *Anthropologists at Home in North America. Methods and Issues in the Study of One's Own Society*, pp. 3–14. Cambridge: Cambridge University Press.

MESSERSCHMIDT, D. A. 1981c. On indigenous anthropology: some observations. *Current Anthropology*, 22, 197–198.

MOFFAT, M., Ethnographic writing about American culture. *Annual Review of Anthropology*, 21, 205–229.

MYERHOFF, B. 1980. *Number Our Days*. New York: Simon and Schuster.

NAKANE, C. 1982. The effect of cultural tradition on anthropology. In FAHIM, H. ed. *Indigenous Anthropology in Non-Western Countries*, pp. 52–60. Durham, NC: Carolina Academic Press.

NAKLEH, K. 1979. On being a native anthropologist. In HUIZER, G. & MANNHEIM, B. eds. *The Politics of Anthropology*, pp. 343–352. The Hague: Mouton.

NARAYAN, K. 1993. How native is a 'native' anthropologist? *American Anthropologist*, 95, 671–686.

OHNUKI-TIERNEY, E. 1984. *Illness and Culture in Contemporary Japan. An Anthropological View*. Cambridge: Cambridge University Press.

OHNUKI-TIERNEY, E. 1984. 'Native' anthropologists. *American Ethnologist*, 11, 584–586.

OKELY, J. 1984. Fieldwork in the home counties. *RAIN*, 61, 4–6.

OKELY, J. 1987. Fieldwork up the M1. Policy and political aspects. In JACKSON, A. ed. *Anthropology at Home*, pp. 74–94. London: Tavistock.

OKELY, J. 1992. Anthropology and autobiography: participatory experience and embodied knowledge. In OKELY, J. & CALLAWAY, H. eds. *Anthropology and Autobiography*, pp. 1–28. London: Routledge.

OKELY, J. 1996. *Own or Other Culture*. London: Routledge.

PERIN, C. 1988. *Belonging in America: Reading Between the Lines*. Madison, WI: University of Wisconsin Press.

POOL, R. 1996. *Vragen om te sterven. Euthanasie in een Nederlands ziekenhuis*. Rotterdam: WYT Uitgeefgroep.

RYANG, S. 1997. Native anthropology and other problems. *Dialectical Anthropology*, **22**, 23–49.

SARSBY, J. G. 1984. Special problems of fieldwork in familiar settings. In ELLEN, R. F. ed. *Ethnographic Research*, pp. 129–132. London: Academic Press.

SEGALEN, M. & ZONABEND, F. 1987. Social anthropology and the ethnology of France. In JACKSON, A. ed. *Anthropology at Home*, pp. 109–119. London: Tavistock.

SHAMI, S. 1988. Studying your own: the complexities of a shared culture. In ALTORKI, S. & EL-SOLH, C. F. eds. *Arab Women in the Field. Studying Your Own Society*, pp. 115–138. Syracuse, NY: Syracuse University Press.

SÖRBÖ, G. M. 1982. Anthropologists at home and abroad: a discussion of epistemological and ethical issues. In FAHIM, H. ed. *Indigenous Anthropology in Non-Western Countries*, pp. 152–163. Durham, NC: Carolina Academic Press.

STEPHENSON, J. B. & GREER, L. S. 1981. Ethnographers in their own cultures: two Appalachian cases. *Human Organization*, **40**, 123–130.

STRATHERN, M. 1987. The limits of auto-anthropology. In JACKSON, A. ed. *Anthropology at Home*, pp. 16–37. London: Tavistock.

THE, A.-M. 1997. '*Vanavond om 8 uur ...*' *Verpleegkundige dilemma's bij euthanasie en andere beslissingen rond het levenseinde*, Houten: Bohn Stafleu Van Loghum.

VAN DONGEN, E. 1996. De 'native' als vreemdeling. Medisch-antropologisch onderzoek in Nederland. *Medische antropologie*, **8**, 340–349.

WESTON, K. 1991. *Families We Choose: Lesbians, Gays, Kinship*. New York: Columbia University Press.

YOUNG, A. 1982. The anthropologies of illness and sickness. *Annual Review of Anthropology*, **11**, 257–285.