Paideuma 56:189–204 (2010)

WHATEVER HAPPENED TO THE SPIRIT OF ADVENTURE?

Signe Howell

Are we witnessing the end of anthropology? To judge by the ever increasing membership of the European Association of Social Anthropologists – especially from universities in countries where there previously was little academic interest in the discipline, such as Germany and Eastern Europe – the answer would be a resounding 'no'. But, to judge from the kind of research projects engaged in by PhD students and academic staff in countries where the discipline has been established for many years, such as the United Kingdom, France and the Scandinavian countries and where the basic disciplinary issues and principles were developed, I feel more uncertain about my answer. Certainly, the practice of social anthropology as we have known it since the time of Malinowski – and which set it apart from the other social sciences, characterised as it was by long-term fieldwork and participant observation in remote and unknown parts of the world and informed by the inductive approach – is changing. This may, indeed, be contributing to the end of anthropology as the discipline used to be understood. What we are witnessing today is a trend marked by an increase in research projects that deal with clearly defined topics for investigation, that increasingly are located in the anthropologist's own country of residence, and that are multidisciplinary.

Does this matter? I think it does, not least because there are clear signs that the trademarks of anthropology that underscored all ethnographic fieldwork are by many no longer perceived as essential. Our particular method – open-ended participant observation – the sole purpose of which was (and is) to achieve understanding of local knowledge 'from the native's point of view' in unknown parts of the world and to contextualise it in wider local significations, is losing its theoretical centrality. The alien gaze, once held to be highly important, is no longer emphasised. This quest, which was (and is) epistemologically linked to the comparative study of human social and cultural life, is undergoing serious redefinitions. While the old methodological terminology is still in use, the actual practice of many ethnographers is giving it new meanings. Among the more striking changes I have observed are the following: actual time spent in the field is shorter than it was a few decades ago, often no more than twelve months; the local language is used less; interviews and questionnaires are used more; topics for investigations are more sharply delineated; more projects are undertaken in the anthropologist's own country and are multidisciplinary; and the holistic ambition seems to be on the wane.

I wish to argue that this demise may be attributed to both internal and external factors – factors that reinforce each other. These I wish to characterise as a loss of

the spirit of adventure among graduate students combined with new demands from universities and the institutions that fund research. In what follows, I will deal with each of these. My presentation will be rather personal and guided by my worries about the changes that I perceive to be taking place. I must emphasise that I do not paint a nuanced picture of the current situation. I ignore the many exceptions to my critical statements. I disregard examples of exciting and thriving research projects carried out according to all the cherished ethnographic criteria, whether 'at home' or elsewhere. I am looking for patterns of what I regard as decline.

When I was a post-graduate student at the University of Oxford in the mid- and late 1970s, the common understanding among the students – and our teaching staff – was that we would undertake sustained fieldwork in some distant and unknown part of the world. Here we would seek to acquire an understanding of local ideas, values and practices, primarily through the use of the local language (however imperfectly mastered) and by hanging around twenty-four hours a day. Our projects were open-ended, holistic in their ambition, and, whatever 'social facts' we uncovered, our unquestioned ambition was to interpret them contextually. It was not a question of s h o u l d we do this, but w h e r e we would most like to settle for eighteen months of participant observation. The choice of w h e r e was often dictated by two considerations: a place that we felt would be congenial to our taste, perhaps a place we had heard or read about and which appealed to our imagination and sense of adventure and discovery, and a place that we thought might help us answer some theoretical quandaries that, through our readings and the lectures of our teachers, had aroused our intellectual curiosity. Together these two concerns added up to a general desire to explore the unknown: geographical, social, cultural or intellectual. Through rigorous and persistent study of the various social values and practices that we encountered, we would seek to provide a study of the community that was both informed and anthropologically relevant, as well as contribute to fundamental intellectual questions inherent in the discipline of anthropology about the nature of social institutions and social life. Perhaps I have an unrealistic and rather romantic notion of the anthropological ambition, but it was one that I – and most of my contemporaries – believed in and that we tried to live up to.

It is a notion that I still cherish today, but one that I observe is in the process of being undermined for a number of different reasons. In what follows, I want to examine what I mean by 'undermine' and explore some of the reasons for this. They range from external factors attributable to current political thinking about what constitutes 'useful' knowledge, linked to recent trends in the understating of the nature of universities and how this effects research funding, as well as to internal factors within the discipline of social anthropology itself. Certainly in the UK and Scandinavia, where I know the situation best, all these factors threaten to undermine the practice of social anthropology as it used to be practiced. I believe the time has come for senior anthropologists everywhere to examine the current situation and ask some tough questions. Are we allowing our unique contribution to the understanding of human social life to be undermined? If

we do not like what we see, what can we do to prevent the discipline from slowly losing its identity and being merged with a number of related disciplines, such as sociology, cultural geography, media studies, ethnology, social psychology, cultural studies and education – several of whose members increasingly proclaim to be 'sort of anthropologists' and to 'really be doing ethnography because we do qualitative studies'. The question is, do we agree that they are 'really doing ethnography'? If not, why not? The topic of this collection is, therefore, an important first step in helping to clarify what some senior anthropologists from different countries actually think about the situation.

Internal factors of change

Small places, large issues

In order to rescue what many of us agree is the heart of the anthropological enterprise, namely to immerse oneself in unknown 'small places' and thereby address the 'large issues', as my colleague Thomas Hylland Eriksen has aptly called his introductory book, we anthropologists must become more proactive in the defense of our methods and the insights and results we claim they give rise to. On the home front we can best do this in the syllabuses we offer our students – what we teach and how we label and organise our courses; in how we work in grant-giving and selection committees at different levels; and what kind of projects we demand from our graduate students. At the same time, we have to confront the aftermath of the work by the fifth column within our own ranks, the so-called postmodernists who, through their 'critique of anthropology' – more precisely, of ethnographic practice – knocked away the foundations from beneath the discipline. Their criticism had the effect in some influential circles of rendering fieldwork in the world outside Euro-America politically incorrect, indeed illegitimate. The spirit of adventure itself was made suspicious. Although their influence is abating, it had an effect from which many anthropology departments are struggling to recover, namely the loss of prospective PhD students' desire to explore the unknown in distant and unknown places.

In order to highlight some of the changes that seem to me to be the most serious to have occurred during the past twenty years or so, I will run quickly through those I have observed first hand in British and Scandinavian universities. I will probably emerge as old-fashioned and conservative, refusing to face the 'realities of contemporary life'. But I am willing not only to face such accusations but also to argue against them because I feel that, if we do not fight these current trends, it could easily mean the end of anthropology as I and my generation learnt and loved it – anthropology as it developed from Malinowski and Boas to Firth, Evans-Pritchard, Mead, Leach, Douglas, Needham, Lévi-Strauss, Dumont, Geertz, Sahlins, Strathern and many, many others, who, despite

important theoretical differences, had one thing in common: a commitment to exploring social, cultural, mental and moral forms of life in places far from home and to use that knowledge to address overarching theoretical questions concerning the meaning and role of human life as this is manifested through kinship, religion, classification, economic and political life. Where are their future successors? Where are the daring grand theories that we once could engage with? Nowhere. Rather, anthropologists, including those who engage in the old-style fieldwork, have become timid, fearful of grand theories of all descriptions, tending to stick to their ethnographic or topical expertise and avoiding the big questions.

It is true that the important journals such as the "Journal of the Royal Anthropological Institute" (JRAI), "Ethnos", "Social Anthropology", "American Ethnology" and "American Anthropologist" continue to publish articles of high quality, articles in which authors use their carefully amassed ethnographic knowledge to consider and critique the 'eternal perplexities' of theoretical issues. Indeed, for example, the 2008 special issue of the JRAI was devoted to a consideration of what constitutes 'evidence' in our research and how anthropologists construct the objects of their knowledge. These notable efforts notwithstanding, the broader picture suggests that it is precisely the survival of the anthropological approach to knowledge that is at stake. And it is this approach that, ultimately, is most 'useful' in a broad sense of *Bildung* – not researching narrowly defined topics of more or less policy relevance, however well this may be done. In fact, I want to suggest that the latter will not be well done if we abandon our understanding of the former.

We cannot afford to relax. In his epilogue, "Notes on the future of anthropology", to the volume of the same title edited by Akbar Ahmed and Cris Shore (1995), Anthony Giddens argues that anthropology has nothing unique to offer, that with the 'disappearance of the exotic' and the fall of colonialism the distinctiveness of anthropology is under threat. He goes on to state that

> [a] discipline which deals with an evaporating subject matter, staking claim to a method which it shares with the rest of the social sciences anyway, and deficient in theoretical traditions [...] does not exactly add up to defensible identity of anthropology today (Giddens 1995:274).

I disagree with Giddens in most respects and feel enraged by his lack of understanding of the anthropological aims and methods, but I also read the warning signs in his statement. His critique gives legitimacy to all those others who claim to 'be really doing ethnographic fieldwork'. It is only by adhering to the unique features of our methods that we may be able to contribute 'relevant' and 'useful' knowledge that is different from that of other social sciences. Moreover – contra Ahmed and Shore – I maintain that it is after the experience of long-term fieldwork outside our own country that we can best engage with the study of current issues at home (I return to this at the end). If we continue on the path that is discernible today – of avoiding geographically distant

and unknown social settings for our research in favour of demarcated research projects at home, of dropping participant observation in favour of 'qualitative research' – then Giddens may be proved right in his assertion that we a r e indistinguishable from the other social sciences. In this regard, let me remind you of what participant observation entails beyond the purely academic and intellectual. I will quote Jonathan Spencer from an article criticising some of the arguments emanating from the writing culture critique:

> Anthropologists wade into paddy fields, get sick and read bad novels rather than confront another day of mounting misapprehensions; they also take photographs, make films and tape recordings [...] the fact that they mainly do it by themselves in strange places is another oddity that passes unremarked upon in Writing Culture (Spencer 1989:160).

However much sociologists, ethnologists, cultural studies students and others insist that they 'do ethnography', I will bet my bottom dollar that this is not what they mean by it, expect or experience. What about the new generations of aspiring anthropologists?

The spirit of adventure

The teaching and degree structure at British and Norwegian universities used to be organised in a manner that supported the classical aims. Having passed various tests that satisfied a department that a PhD student would be able to complete eighteen to twenty-four months of fieldwork in a disciplinarily responsible manner, students set off for all parts of the globe. Not everybody went to the jungles of South America or Southeast Asia, the villages of sub-Saharan Africa, India or the Middle East, the islands of the Pacific or the far-flung Arctic settlements. Some went to urban areas on the same continents, or in their own country or another European country. But unlike much research undertaken there today, earlier anthropologists usually undertook a local micro-study of some kind. However, I want to argue that from the time of Malinowski and his students until the late 1980s and early 1990s, regardless of the chosen field sites, the majority of British anthropologists (and my Norwegian colleagues) were driven by a sense of personal and intellectual adventure along the lines I have outlined. Although most had some kind of formulated research aims, these were often vague, like a desire to investigate religious practices, or the kinship system, or to learn about the dynamics of political institutions, shamanistic practices, etc. What they (we) all had in common was that the fieldwork task was open-ended and inductive: the anthropologist allowed him- or herself to be guided by the preoccupations of the people he or she studied. They (we) wanted to be amazed, to be stretched to our physical and intellectual limits, to experience the unexpected and to make anthropological sense of it. Is the situation like this today? I think not.

Two major changes amongst graduate students can, broadly speaking, be observed to have taken place. Firstly, students' motivations seem to be more pragmatic and goal-

oriented. They want a PhD; they want a job. Many seem to think that this is best achieved by exploring some relatively narrowly defined topic arising out of contemporary life in their own country. They are less interested in going to uncomfortable places to see what presents itself; they prefer urban areas if they go outside their own national boundaries; and their projects are not open-ended, but have clearly enunciated research aims. If this trend continues, what will easily be lost is the experience of total immersion, the realisation that 'the field' can never be just a physical site, but is a social and a moral one too. The experience of the field as a total social fact where, to paraphrase Mauss, all kinds of events and factors promote simultaneous impressions in the head, the heart, the body, and linking the religious, the moral, the economic, the political and the aesthetic will be lost by a narrowly defined and narrowly pursued 'research proposal'. Having read and evaluated a number of research proposals over the past years, I am struck by precisely the absence of a desire for this kind of experience. Rather, I often ask myself why this particular person wants to investigate his or her particular stated problem, since they already seem to know most of the answers they expect to find. This is not entirely their fault, as the format for submitting research applications streamlines the proposals in such a direction, but this does not excuse, to my mind, the lack of genuine curiosity about the venture – the sense of excitement and of a spirit of adventure that I think all anthropological research proposals ought to demonstrate.

External factors of change

The Economic and Social Research Council benchmarking exercise

This brings me to some external constraints upon the anthropology of the future. They are not insignificant. Two years ago, I was invited by the UK Economic and Social Research Council to be part of an international panel of anthropologists to consider the state of social anthropology in Britain. It was not an evaluation – something British academic departments have been regularly subjected to for the past couple of decades – but a 'benchmarking exercise'. It involved travelling to twelve major departments throughout the country in order to ascertain the state of affairs there. We were not interested in numbers (numbers of publications, research grants, etc., which increasingly have become the markers of quality in the eyes of the authorities), but in how the academics in those departments perceived their current situations and the prospects for the future. Was social anthropology in a good state of health? And if not, why not? These were the main questions to which we sought answers. The result was mixed. In our report we stressed the consistently high quality of disciplinary engagement by British academic anthropologists, as well as their expressed desire to continue the traditions of holistic fieldwork and inductive research. So far so good.

However, it emerged that the British anthropologists were also experiencing a profound disquiet with regard to the future, largely due to the changes in funding. Increasingly, grant-giving bodies discourage self-initiated, long-term individual research in favour of team-work, preferably inter-disciplinary, the research aims of which are largely dictated by the grant-givers. Frequently the overarching need for 'useful' research is stressed. More often than not useful research is thought of as useful to the grant-giving country itself, and there is a tendency to identify policy areas within fields that are currently perceived as 'problematic' in some way or other, such as health, education, urban development or the multicultural population. Although such topics may, of course, be studied anthropologically with benefit, it is unfortunate that these have become the main topics of anthropological research. Furthermore, grant-givers in several countries fail to appreciate that a comparative dimension will enhance the understanding of the local situation and are reluctant to fund research abroad, even if it is on the same topic.

European Union research-funding policy is similarly focused upon 'relevant and useful' projects undertaken within the EU. This kind of research policy has several serious consequences. It limits the geographical region to the home domain, defines the research focus, presupposes a multidisciplinary approach, limits the time available for fieldwork and demands much reporting along the way. British anthropologists were fully aware of these constraints, but they chose not to elaborate upon them in our meetings. In the words of our report, 'the colleagues seemed at times focused on presenting a brave and unified face rather than addressing broader issues about the future of the discipline rather than the department' (Economic and Social Research Council 2006:7). That in itself I regard as disturbing. Is the 'audit culture' (Strathern 2000) rendering academics fearful of criticising its effects? If so, they are contributing to the strangulation of anthropology.

Furthermore, in the UK today, most PhD grants for British students (and there are very few) come from these sources, making self-initiated, inductive ethnographic doctoral research that is carried out beyond the home country and deals with central disciplinary issues almost a thing of the past. An added fact is that, with today's high university fees, British students without a grant are unable to embark upon a PhD project. In so far as more traditional doctoral research is undertaken, this is done almost exclusively by non-British students with grants from their home countries. Due to the constant pressure to generate income, departments do not exercise the earlier stringent demands on qualifications for acceptance to post-graduate studies. Those from outside the EU, many of whom do fieldwork in their own country, are particularly sought after, as they have to pay even higher fees. Moreover, in order to make money, most British anthropology departments have abandoned a previous disinclination against so-called hyphenated anthropology. Master's courses – mainly directed at foreign students – in such fields as medical anthropology, or the anthropology of migration, childhood, obesity, development, refugees, gender, etc. are offered. Many students who take these twelve months courses have little or no previous training in anthropology.

Those who continue to the PhD level tend to continue with the hyphened specialisation. Pressure to complete a PhD in a maximum of four years further discourages classical fieldwork. In response to the direct question whether their current students undertook fieldwork of the same duration and quality as those which the staff themselves had done ten or thirty years previously, the answer, with a couple of exceptions, was 'no'. This is disturbing. Perhaps more disturbing is that teaching staff tend to identify themselves more and more with hyphenated anthropology and, just like their PhD students, tend to take an interest only in research and writing which corresponds with their own. One effect is that the weekly departmental seminar – previously the high point of the week in all British anthropology departments – is no longer attended by all staff and research students. This, I argue, will have a debilitating effect on an anthropology which used to pride itself – in distinction to the other social sciences – on being one inclusive discipline, in which all research questions were, in principle, of equal interest and relevance to all active researchers.

Our report further stressed the 'British tradition of a strong commitment to grounded, analytical, investigation in interaction with an eclectic range of interpretative resources', and went on to say that this notwithstanding, 'British social anthropologists are amongst those theorists within the discipline most frequently cited worldwide' (Economic and Social Research Council 2006:15). The report (whose authors were mostly from American universities) stressed that this particular tradition differed somewhat from the American one, where 'theory' is more in evidence. The point about the intertwined nature of theory and fieldwork in the British tradition is one that Giddens failed to appreciate.

The situation in the Scandinavian departments, and also I think in Germany, is not quite as dismal as in the UK. First, there are minimal tuition fees, so that the cost of embarking upon a MA and PhD degree is limited to living and fieldwork expenses. Secondly, the grant situation is not quite as bleak. Although in Norway too there is an increasing tendency to prioritise large interdisciplinary research teams whose focus is on some topic of direct interest to the Norwegian state, with one or more PhD studentships included, it is still possible to obtain individual PhD grants to undertake the kind of research that I outlined at the beginning, in which the classical concerns and methods can prove their worth. However, two other factors have emerged that threaten the continuation of the old ambitions, namely students' reluctance to do that kind of research, and the fact that the same requirements for stringent fieldwork are not applied in many cases when the students engage in research at home. A prime example of this is that none of the anthropologists who have studied immigration and ethnicity in Norway have troubled themselves to learn a relevant immigrant language. In contrast, an anthropologist who failed to study the language of a people researched in a distant place would have little or no credibility. Yet, different criteria appear to be applied to anthropological research at home. What is going on?

More internal factors: the heritage of post-modernism

Anthropologists have always engaged in soul-searching regarding their disciplinary practices. In this regard they differ from their colleagues in the other social sciences. Debates about methods, the status of findings, the profoundly personal and idiosyncratic nature of fieldwork have all been hotly discussed – in and out of print – since the famous LSE seminars under Malinowski (Firth, personal communication). In light of this, I find it surprising that the critique launched by post-modernism of the social sciences and some of the humanities in the late 1980s and early 1990s for their lack of reflexivity regarding the research process hit anthropology very hard. The two volumes that appeared in America in 1986 – "Writing culture", edited by James Clifford and George Marcus, and "Anthropology as a cultural critique", edited by George Marcus and Michael M.J. Fischer – marked the start of raging debates about the practice of anthropology (participant observation in distant and exotic places, usually places that were or had been subject to colonial rule) and the way the research findings were presented – mainly in ethnographic monographs. The debate went in two directions, both of which hit the identity of the discipline hard. Firstly, the critique concerned the validity of our findings, criticising much ethnographic writing as being positivistic, expressed in what was called ethnographic realism. John Borneman and Abedellah Hammoudi (2009) have characterised this apects of the postmodernist critique as an accusation of three denials: that ethnography is a literary genre which denies itself as such; that reliance on observation leads to a denial of the role of the ethnographer in shaping the object or subject studied; that ethnographers tend to deny the constructed character of their objects and of the knowledge they produce.

While no doubt this was a valid criticism of some publications from the pre- and postwar periods, it was far from relevant – or fair – as regards many of our most influential predecessors. Let us not condemn a whole profession because of Radcliffe-Brown! Evans-Pritchard, for example, inspired to a large extent by Collingwood and Marcel Mauss – no propagators of empiricism – argued that we should not apply scientific criteria to our investigations, that anthropology had more in common with history and the moral sciences. His own ethnographic writings bear this out. I have observed how many advanced students (and colleagues) who read "The Nuer" (1940) for the first time are amazed at Evans-Pritchard's relentless questioning of his methods, findings and interpretations. In his "Witchcraft, oracles and magic among the Azande" (1937), one of the most influential studies on indigenous epistemology inside and outside the discipline, he openly acknowledges his confusion regarding Azande explanations of causality and places himself in the middle of his text. The same can be said of numerous so-called 'realistic ethnographies' (Marcus and Fisher 1986). Nevertheless, the discussion in the wake of the writing culture debates did alert anthropologists to the demand that they be open in their texts about the actual nature of their fieldwork, their analysis and interpretations. It was helpful to take a critical look at the use of the notion of culture (or

society or community), at a-historical presentations, at a tendency (perhaps) to exoticise, etc. However, this does not mean that ethnographic fieldwork is an impossible task and had better be avoided – only that we take care to more deliberately integrate reflexivity in our interpretations.

The other thrust of the postmodernist critique concerned anthropology as a colonial practice: it became politically incorrect and morally unacceptable to study supposedly powerless small communities in former colonial domains, to make them, the argument went, into the reified 'Other'. As a result of these two aspects of the p o – m o critique, many went in for historical archival studies or studies at home (Borneman and Hammoudi 2009). Borneman and Hammoudi are also critical of the solution offered by Marcus, called 'putting things together' – an approach that relied heavily on vignettes, travelogues, media images, texts and literature of the most diverse origins (2009). In addition, a rather obscure notion of dialogue was promoted, giving fieldwork a veneer of morally acceptable interaction, especially when carried out in one's own country. However, similar criticism might be levied against both scenarious; as methods they can lead to superficial insights, quick analyses informed by the latest trendy concepts. More recently, discourse analyses performed on the media, such as interaction on the internet and television, have in many cases become a popular substitute for engaging with immediate face-to-face social life.

Policy-oriented research may seem more ideologically correct today, more 'useful' and relevant in a rapidly changing world than setting off for the Highlands of New Guinea. However, it is worth bearing in mind that much innovative theoretical insight has been gained in recent years that emanates precisely from high-quality ethnographic fieldwork carried out in New Guinea and the Pacific, not least inspired by the work of Marilyn Strathern. Gender studies have been revitalised, a new-found interest in indigenous ontologies and concepts of personhood has inspired much exciting theorising generally, and novel interpretations of exchange and classification owe their sources to ethnographic fieldwork from these parts. Let me make two more points in connection with the postmodern critique of ethnographic practice. Most of us who have carried out fieldwork in rural areas of Asia, Africa or Latin America do not agree that we study down, or that the relationship is an unequal power relationship. More often anthropologists are totally at the mercy of the communities they study, struggling to gain acceptance and coping with what goes on around them, and rarely being in a position to influence anything of importance, even if they should wish to do so. To suggest otherwise demonstrates a high degree of lack of understanding on the part of the critics. At the same time, as the people we study are being educated, they increasingly become acknowledged partners in the anthropological enterprise, thereby enhancing the understanding and knowledge of the fieldworker. Secondly, to claim, as Giddens did, that there are no more exotic places to study is equally uninformed. Anyone who has travelled in Central or Southeast Asia or Melanesia knows that there is no shortage of fascinating localities in which to settle in to conduct in-depth anthropological fieldwork.

More external factors: 'pop ethnography'

Much more can be said about the writing culture movement, but I am convinced that it was in danger of throwing out the baby with the bathwater. It certainly contributed to a perceived epistemological vacuum that has been difficult to fill. It is, I think, an ironic fact that, as television documentaries about travel to distant 'exotic' places are increasing in popularity, students of anthropology are less and less interested in these places. A number of television series (often British) in which one or more individuals set out to explore places that are unfamiliar to them and to Western audiences are currently being produced. They are very popular and appear to appeal to the general public's sense of adventure. During a recent sabbatical spent in Oxford I watched with interest many such programmes, most of which, it seems, are more interested in the character of the traveller than in trying to achieve a serious understanding of the societies visited. In this respect they differ markedly from the ethnographic films that were produced during the 1970s, 1980s and 1990s, such as the "Disappearing world" and the "World about us" series, neither of which would receive funding today. Instead, we are treated to the travels of Michael Palin, who, with great charm, takes us to a range of places from the Himalayas, to South America and Eastern Europe. He presents the viewer with a number of rather strange people and quaint habits that he encounters on the way, which he valiantly tries to understand, always with a smile that can easily be construed as kindly condescending.

The series entitled "Tribe" is perhaps the one that comes closest to the anthropological endeavour in its stated aims and, as such, is the most provoking. A former army officer, Bruce Parry, travels to extremely remote places (in Borneo, Amazonia, Siberia, Melanesia, etc), where he lives with 'tribal people'. He stays for one month, and the stated aim is 'to live like one of them' in order to experience how their society works. Parry is the main character, who bravely undergoes a range of horrific ritual practices. He grins and bears it; the people he lives amongst grin and enjoy his discomfiture. But we learn nothing of their social, political or religious organisation beyond whatever catches Parry's attention, which is then presented totally without context. Another popular series, which has also been produced in Norway and other European countries, is one in which a family is transported to a 'primitive' society somewhere, where they live 'like the natives' for three weeks. Again, the purpose is to chart how t h e y cope, not how the 'natives' live – what makes them tick from their own point of view. These programmes could with good reason be subjected to a devastating critique of 'othering', neo-colonialism and gender blindness. This would be much more appropriate than the writing culture criticism levied at ethnographic texts.

Whatever one may say about these and similar programmes, the main characters display a terrific sense of adventure, reminiscent of former generations of anthropologists, and are willing to succumb to much physical and emotional deprivation and hardship – albeit with the presence of an invisible (to the viewers) crew of cameramen,

producers and others. The pay-off, of course, is fame and, probably, in some cases fortune. There is a seemingly insatiable demand for such programmes. However, none of the central characters are anthropologists: they are celebrities, they are photogenic, and they are adventurous. So, I ask myself, where are the anthropologists? If the general public has become so interested in seeing how people live in distant exotic places, one would assume that anthropologists would also be so, that university departments would be inundated by young people wanting to rush off to carry on the tradition of their forefathers and mothers and that they would wish to do a better job than the Bruce Parrys of this world. The reality today is different. To be sardonic, few PhD students in the UK and Norway seem willing to subject themselves to the challenges of living alone among people in faraway places, where discomfort must be expected, where they are far from internet cafés and where there is no mobile telephone reception. Rather, in so far as they travel to Asia, Africa or Latin America, the vast majority settle in an urban area, studying topics such as domestic migration, syncretism of religious or healing practices, diaspora communities, urban elites, youth and pop-music, fashion and so on.

What characterised the endeavours of my generation was that, by and large, we were on our own, there was very little institutional assistance (this was not always positive), and the whole thing took on the aura of a personal quest. While it probably led to much personal distress resulting from feelings of inadequacy during fieldwork, it also resulted in much good ethnography.

I want to end on personal note, and try to draw some lessons from it. I did my obligatory eighteen months of fieldwork for my doctorate among the Chewong, a small and hitherto unstudied group of hunters, gatherers and shifting cultivators who lived deep inside the tropical rain forest of Malaysia. Thirty-one years ago I began what was to become a protracted engagement with these people. I have visited them many times since, most recently in April 2008. I have observed them having to face a number of externally initiated changes that pull them into the modern industrialised world of contemporary Malaysia. In fact, my current research project concerns precisely my involvement with them over such a long period of time. Together with a group of international colleagues with similar experiences, I am exploring the methodological and epistemological implications of what I call multi-temporal fieldwork.

However, having completed my doctoral thesis and published it and written a few articles, I felt that I did not have much more to say about the Chewong for the time being. As a student I had been particularly excited by structuralism. As typical hunter-gatherers, Chewong social organisation was extremely loosely structured and did not provide me with the kinds of 'pegs' upon which I might perform some kind of structural analysis. I turned therefore to Eastern Indonesia. From the anthropological literature, it seemed likely that the kind of social and cultural organisation to be found there – complicated kinship system, elaborate ritual life and highly structured socio-political organisation – would enable me to think in terms of more classical interpretations. I therefore started fieldwork with the Lio on the island of Flores in the mid-1980s. Although I was

unable to undertake uninterrupted fieldwork with them for more than five months at a time, I returned several times and discovered that *Übung macht den Meister* (literally 'practice makes the master', or more loosely translated, practice makes perfect). I was, if not a better, at least a more efficient fieldworker the second time round, more confident about my anthropological identity, and less intimidated by the people I studied.

My third research project, begun in 2000, was a typical project of the kind that I have been criticising here, thematically delineated and based in my own country. Tired of physically difficult fieldwork, I started to investigate the recent and fast-growing practice of the transnational adoption of infants from the poor South to involuntarily childless people of the rich North mainly Norwegians. It was meant to be a short interlude, but turned out to be so interesting in anthropological terms that I continued with it until last year. However, the completion of this last project can to a large extent be brought back to the fact that I had the experience of traditional participant observation. I was used to looking for insights in unlikely places, and to follow leads as they emerged. Undoubtedly, my ability to identify and take advantage of serendipitous events was enhanced by this experience. It stood me in good stead in this latest project. Because I was able to undertake only a semblance of ethnographic fieldwork in my research on adoption, I had to compensate by exploring many less obvious paths that would enable me to achieve some kind of thick description and to give my interpretations a holistic as well as a comparative dimension. But I found it challenging to distance myself from the known social and cultural world of contemporary Norway, to look at it as if everything was unknown. In many ways this was my toughest project. For these reasons, I agree with those of my colleagues who argue that anthropology at home is best done after anthropology far away from home.

To conclude

What is the future of anthropology in today's world? With more and more anthropological research undertaken on the anthropologist's own geographical turf and guided as much by grant-givers' needs and understandings as by the researchers' own inclinations, I have severe misgivings. Clearly, present-day external factors are not conducive to the continuation of the old Malinowskian ideals. But internal factors that have similar effects are also observable. New generations of anthropologists – whether for political, intellectual or private reasons – increasingly choose 'safe' research projects, driven as much by pragmatic reasons of future employment, family demands or financial constraints. As anthropologists increasingly involve themselves with contemporary problems, defined as much by Western values and concerns as springing out of local ones, will they cease to find the classical literature of relevance? Indications are that undergraduate students are beginning to find much of it irrelevant, a few iconic clas-

sics excepting, such as Marcel Mauss's "The gift" (1954), Mary Douglas's "Purity and danger" (1960) and Edward E. Evans-Pritchard's "Oracles, witchcraft and magic among the Azande" (1937). Increasingly, they demand 'up-to-date' literature, what in their view is relevant for the contemporary world. They certainly try to avoid reading whole ethnographic monographs, especially if they are from parts of the world that they are not interested in. I ask myself to what extent today's students are driven by a spirit of adventure, and whether they have a commitment to the holistic ambition and to the rigorous ethnographic fieldwork that this necessitates. If not, then I fear for the future.

If I am right in positing that there is a trend towards thematically deliminated and narrowly focused studies – in the sense that broad and deep knowledge about social intuitions, values and practices anchored in one particular social world are not produced – then what can anthropologists contribute to an enhanced understanding of the complexities of human life? With narrowly problem-focused fieldwork carried out with the help of interpreters, what insights can anthropologists produce that a clever journalist cannot, or someone from cultural studies armed with exciting theoretical concepts (Howell 1997)? I think this is becoming an increasingly relevant question. As far as I am concerned, anthropology is empirical philosophy. This suggests that we can only provide a unique contribution to knowledge about other life-worlds and our own by insisting on long-term participant observation carried out in fieldwork. By all means let us acknowledge that many myths have grown up around this disciplinary holy cow, but, at the same time let us seek to improve upon the practice and openly acknowledge the many pitfalls and the ultimately very personal nature of such a scientific enterprise. Cultural relativism is not a philosophical stance, but a methodological one necessary for exploring the deeper meanings of practices and for drawing comparisons. Moreover, we must retain our broad interest within the discipline. I for one am an enemy of hyphenated anthropology. We are social anthropologists, first and foremost, whose research interests may range at different times in our career from, *inter alia*, indigenous medical systems, to mythology, to power relations and socio-political change, to principles of classification, to development aid, to new kinds of human reproduction, to the morality of trade and barter. But in order to say anything interesting about these and all the other topics that anthropologists have written about, our information and our interpretations must spring out of solid, rigorous fieldwork, the ultimate aim of which continues to be to interpret the natives' point of view, and to relate this to human social life generally and to overarching theoretical debates.

References

AHMED, Akbar S., and Cris SHORE (eds.)
1995 *The future of anthropology: its relevance to the contemporary world.* London: Athlone

BORNEMAN, John, and Abedellah HAMMOUDI (eds.)
2009 *Being there: the fieldwork encounter and the making of truth.* Berkeley: University of California Press

CLIFFORD, James, and George E. MARCUS (eds.)
1986 *Writing culture: the poetics and politics of ethnography.* Berkeley: University of California Press

DOUGLAS, Mary
1960 *Purity and danger: an analysis of concepts of pollution and taboo.* London: Routledge & Kegan Paul

ECONOMIC AND SOCIAL RESEARCH COUNCIL (UK)
2006 *International benchmarking review of UK social anthropology.* London: ESRC

ERIKSEN, Thomas Hylland
1995 *Small places, large issues: an introduction to social and cultural anthropology.* London: Pluto Press

EVANS-PRITCHARD, Edward E.
1937 *Witchcraft, oracles and magic among the Azande.* Oxford: Clarendon
1940 *The Nuer: a description of the modes of livelihood and political institutions of a Nilotic people.* Oxford: Clarendon.

GIDDENS, Anthony
1995 "Epilogue: notes on the future of anthropology", in: Akbar S. Ahmed, and Cris Shore (eds.), *The future of anthropology: its relevance to the contemporary world*, 272–277. London: Athlone

HOWELL, Signe
1997 "Cultural studies and anthropology: contesting or complementary discourses?", in: Stephen Nugent, and Cris Shore (eds.), *Anthropology and cultural studies*, 103–125. London: Pluto Press

MARCUS, George E., and Michael M.J. FISHER
1986 *Anthropology as cultural critique: an experimental moment in the human sciences.* Chicago: The University of Chicago Press

MAUSS, Marcel
1954 *The gift: forms and functions of exchange in archaic societies.* London: Cohen & West

SPENCER, Jonathan
1989 "Anthropology as a kind of writing", *Man* (N.S.) 24:145–164

STRATHERN, Marlyn
2000 *Audit cultures: anthropological studies in accountability, ethics and the academy.* London: Routledge