An ethnographer in the global arena: 
globography perhaps?

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Abstract In this article Hendry addresses the difficulties and apparent contradictions of applying the qualitative rigour of the ethnographic research method to fieldwork carried out in a global context. While pursuing a discourse evidently shared by people indigenous to many different parts of the world, the author reflects on why she feels the work she is doing still draws on elements of the qualitative strength of the method first developed by her own discipline of social anthropology. This subject is now somewhat unfashionable for reasons precisely associated with the discourse she is following, namely a status inequality seen as implicit in the representation of ‘other’ peoples. In the article she argues against throwing the baby out with the bathwater, however, and seeks to demonstrate how the value the ethnographic method gleaned from social anthropology offers an important contribution to understanding local aspects of global issues.

In the opening paragraphs of the Introduction to his collaborative volume Global ethnography, Michael Burawoy (2000: 1) challenges the very idea that ethnography can be global: ‘How can the study of everyday life grasp lofty processes that transcend national boundaries?’ he asks. His book, which he admits flies in the face of such convention, answers his question by documenting the work of a team of self-styled ‘global ethnographers’ who address, in a variety of grounded research locations, issues such as ‘global forces’ and ‘global connections’. They continue to keep in touch with each other, indeed regularly ‘to conference’ together, and by working in this collaborative fashion they devise a method of research that they entitle ‘Global ethnography’.

This team effort is one attempt to address the twofold challenge Guarnizo and Smith neatly identified ‘to integrate macro- and micro-determinants into analysis, and to develop an appropriate research strategy capable of capturing the complexity of transnational processes’ (Guarnizo and Smith 1998: 24). Their focus is ‘transnationalism from below’ and these authors see themselves drawing on the pioneering though theoretically different work of scholars of ‘cultural studies’ and ‘social sciences’ where they nevertheless notice a convergence arising in a common tendency to celebrate subversive popular resistance ‘from below’ (Guarnizo and Smith 1998: 4–5).

Research in the global arena has indeed come from many different disciplines, each with its own special reasons for interest, and each with its own characteristic methodology. On the one hand, there are scholars in areas such as international relations and political science, whose approach has for long been drawn from the
arena that is now termed global, and they are clearly well placed to contribute. On the
other, there are those whose interests, while criss-crossing the globe in scope, have
individually preferred an approach more closely focused in specific ‘local’ situations.
As our two examples show, they too have an important input, and in this article I
would like to offer another contribution from the ‘local’ end of the spectrum towards
refining a method for ‘global’ research.

My proposal arises from empirical research in my own discipline of social anthro-
pology – a subject associated, if anything, with the most local end of the spectrum.
Indeed, Burawoy describes ‘classical anthropology’ and its interests as ‘irredeemably
local’. However, I would like to suggest, seemingly paradoxically, that the very
detailed local focus of social anthropology has in fact a contribution to offer beyond
the simply ‘grounded’ ‘everyday’ nature of ethnographic work. I make this proposal
despite the somewhat disconcerting fact that anthropology is often sidelined in
general books and courses on research methods in the social sciences – or reclassified
as ‘cultural studies’, as in the case of Smith and Guarnizo – so that the contribution it
can make to a global arena is not usually discussed at all.

Anthropology has in fact recently been making a considerable impact in the field
of global networks, notably in the growing body of research on transnational com-
munities (for example, see Hannerz 1996, 1998; Vertovec 1999), but there are also
two major exceptions to my assertion that anthropology is not discussed in the context
of global research. These are the well-cited work of George Marcus (1995) on multi-
sited ethnography, and an article by Ulf Hannerz (1998) on the contributions anthro-
pologists have made to transnational research, which raises several pertinent issues
about the methods they use. In this article I seek to push further with both sets of
ideas, but in particular to report on a case that follows Hannerz’s suggestion that ‘we
need to experiment with the distribution of attention’ (Hannerz 1998: 248).

Hannerz’s proposal relates to the question of whether one can maintain what he
refers to as ‘normal ethnographic standards and … the expectation of deep involve-
ment’ when ‘one has to scatter one’s attention over many sites’ (Hannerz 1998: 248).
Anthropologists have long distinguished themselves from others in related fields, and
beyond the wider method of ethnography, by referring to the long-term nature of the
fieldwork they do, usually in one place.’ They also point out the greater depth of
understanding they achieve by returning again and again to the same site (Foster et al.
1979; Kenna 1992). These factors might seem to exclude at least a single practitioner
from global research, and another of Hannerz’s suggestions is indeed a form of team-
work. This is one solution, illustrated by Burawoy et al., and my proposal could
perhaps be described as a version of the same, but I would also like to examine some
of the reasons why the field methods associated with anthropology might also actually
offer a particular strength, perhaps ideally to be combined with the efforts of those
working in other disciplines.

To examine global networks and transnational communities it has become
important to be able to follow the people or the issues concerned, and carrying out
research in different areas has become a vital part of the project. Schein (1998: 294)
called the endeavour ‘itinerant ethnography’ and the editors of the book in which she
uses this phrase refer to her as an ‘ethnographic nomad’ (Guarnizo and Smith 1998:
26). In my own recent research, I discussed not only Japan, where I have been
working for a quarter of a century so can feel justified in having had a long-term
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experience. But also examples of my subject matter found in China, Europe, Indonesia, Nepal, Thailand, the United States and Uzbekistan (Hendry 2000). On another recent research trip, I travelled through great swathes of North America and the South Pacific, returning to my base in the UK via Malaysia, where I have to admit that I began to feel overwhelmed by the enormity of the task I had taken on (Hendry 2002). Curiously undaunted, however, I went on to pursue the latest topic of interest in an African context, and I am now engaged in trying to bring it all together within aboriginal communities in Canada.

Somehow I feel that the work I am doing is still the subject I trained for, the one I have spent years practising. The reason for my extensive travel is based on a conviction that the people I am visiting – despite their various languages – share a discourse, a kind of global discourse, which I am following. The extent to which they form a network is one of the questions I am seeking to address. In this article, I would like to reflect upon the methods I have been using, examine their validity, and explain why I think that though they are drawn from rather orthodox social anthropology they also have a contribution to make in the global arena. My initial concern – that I had strayed beyond the bounds of acceptability within my own subject – has been replaced with a conviction that this subject actually offers a new, possibly ethnographic strength within the broader context in which we all find ourselves. Indeed, I hope that this article might dispel the idea that anthropology is a parochial subject, and that instead it offers a dimension of depth in studies located in the global arena.

Multi-sited ethnography?

It is important to concede immediately, however, that, within the anthropological community, it is not new either to travel around in pursuit of one’s subject matter or even to devise a methodology to describe the endeavour. The earlier statements about long-term fieldwork still stand, but there have been studies of nomadic people, travellers and transhumant populations for almost as long as there have been anthropologists to carry them out. Indeed, some of the most influential anthropology has treated just such cases. Among these are Campbell’s (1964) study of the Sarakatsani shepherds of the Greek mountains who travelled to find pasture; Malinowski’s (1922) work with the Argonauts of the Western Pacific; which, despite his extensive travel with the Trobriand Islanders, was picked out as an example of ‘seclusion’ and ‘confinement’ (Burawoy 2000: 6); and Evans-Pritchard’s (1940) study of the Nuer of southern Sudan who move seasonally towards and away from a flood plain. These classic works all deal with particular groups of people, and the anthropologists very often travelled with them. They also all touch on relations with members of the wider society in which they were found.

A more recent trend has been to follow objects rather than people, and works already becoming classics in this field include the aptly named Social life of things (Appadurai 1986), Entangled objects (Thomas 1991) and The traffic in art and culture (Marcus and Myers 1996). In all these cases, the authors identify material links that cut across boundaries between peoples, and their work illustrates different meanings that the same objects may hold in different social contexts. The movement of objects takes place within a much larger overall ‘global’ community, but these studies show that it is important to recognize distinct attitudes within that community,
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and ‘local’ expertise on the ground provides evidence of these differences and thus justifies the need to work in multiple sites.

There are in fact several other reasons why anthropologists move around and George Marcus’s (1995) initial work on multi-sited ethnography was an attempt to draw them into a single methodological framework. He cites all sorts of possibilities, including Malinowski’s *Argonauts*, Appadurai’s *Things*, and Levi Strauss’s myth analysis that identified interesting local modifications to stories shared widely in different parts of the world. However, his chief focus of interest is what he sees as a fundamental shift in anthropological research that now cuts through the capitalist world system rather than using it as a backdrop. An old context has become the very focus of investigation, and the ‘site’ of fieldwork has become multiple and complex as everyday modes of communication allow easy contact between related peoples scattered throughout the world.

The UK Economic and Social Research Council’s Transnational Communities Programme would seem to fall squarely into this mode of research and, indeed, Vertovec’s introductory article on the subject cites Marcus’s notion of multi-sited ethnography as ‘essential to the study’ (Vertovec 1999: 457). Hannerz (1998: 243) has further advocated that the research be ‘not merely multilocational but also translocal’, and he has specifically discussed how new technologies have become incorporated into the project. With the possibility of almost instantaneous communication across the globe, anthropologists may even carry out some of this multi-sited research without leaving their computer screens. They could thus be said to have created an old Frazerian-like armchair method for examining communities separated geographically, but in their case, sharing some common interest or background and regularly in touch by electronic means. This regular communication forms part of the new translocal field.

These scholars have identified various types of research that cross boundaries that were formerly easier to observe than they are now, and Marcus’s article classifies these types into a series of pathways: follow the people, follow the thing, follow the metaphor, follow the conflict, and so on. Each pathway offers a mode in which to construct a multi-sited ethnographic research project, and examples are cited for each type, including the work of scholars in subjects such as cultural studies and media studies. Marcus briefly addresses the question that these mobile ethnographers are ‘testing the limits’ of ethnographic research: ‘predicated upon attention to the everyday, an intimate knowledge of face-to-face communities and groups’ (Marcus 1995: 99). Clearly, a piece of fieldwork that involves a number of sites visited for a short period of time will offer less opportunity to build up this intimate knowledge. However, as Hannerz makes clear, an in-depth study in a local site marked by its translocal connections would also not result in ‘satisfactorily complete, deep ethnography’ (Hannerz 1998: 248).

My first ‘global’ research project encountered this dilemma, and one more as well. Concerned with a group of parks I had found in Japan, locally known as *tēma piku* (or ‘theme parks’), I clearly needed to examine the Disney influence that had inspired them. The word ‘theme’ has influenced builders and marketeers of parks in various parts of the world, and it seemed imperative to place the Japanese ones in a global context. I was ‘following’ the theme, and a local in-depth study would have missed a great deal. However, although the name ‘theme park’ came from the English
language, in British English it sparks off quite a different idea from that now translated into Japanese as *tēma pāku*. In its country of probable origin, the United States, there is still usually a definite ‘theme’ attached to the parks thus named. Hence, an early one, Knott’s Berry Farm, still serves a pie made from the strain of berry discovered by its founder, Walter Knott, and it has ‘themed’ areas such as Ghost Town and Indian Trails. Like Disneyland, however, it is known for its exciting rides.

In Japan, the careful reproductions of foreign countries that are called *tēma pāku* follow the initial model, sometimes neglecting the rides altogether, while in Britain a theme park tends to remember little more than the ‘thrills and spills’.

In this case, then, I started by examining a phenomenon I had found in my area of existing long-term fieldwork and observation, namely Japan, and I visited several examples for relatively short periods in order to gain a broad overview of the subject in question. I then set about placing my findings in a ‘global’ anthropological context, through an examination of studies of apparently similar locations in other parts of the world. In each case, I sought the interpretation of scholars with local expertise, and I then tried to establish an historical context for the development of the phenomenon, which I came to call ‘cultural display’. Eventually, I returned to the original Japanese examples to seek signs of forbears and influences in their own cultural context, and tried to disentangle Japanese versions of the ‘theme’ from the wider theories I had found. At this point I decided that these wider theories were actually inadequate to explain the Japanese material, though they could rather easily, and in my view dangerously, be applied for just such an explanation. As Guarnizo and Smith have pointed out, starting from the macro-structural vantage point may lead to the kind of ‘overgeneralization that produces the self-fulfilling great theories’ (Guarnizo and Smith 1998: 25). Terms such as ‘theme park’, ‘pastiche’, ‘kitsch’ and ‘post-modern’ have acquired a global currency, but they have a range of differential values often quite unrecognized by some of those who use them.

My project was not, however, an exercise in extreme cultural relativism, for the global influence in Japan was by no means denied. Indeed, there could be no wider context if it were not acknowledged, and Japanese academics use the above terms in much the same way as their Western counterparts, to condemn their parks without even looking at them. One aspect of the book (Hendry 2000) was to demonstrate how apparently ‘global’ phenomena exhibit interesting local differences despite overt similarities (see Miller 1995 among others). It draws on a much wider range of material than any participant observer could muster by focusing on one or two of the parks and gaining a deeper understanding of the motivations and intentions of the planners, producers and visitors, but it has the advantage of being able to place local discourse (whether of informants or observers) in a broader context.

For the very multiplicity of sites visited, and the working through of a global currency, then, this piece of work would seem to be ‘multi-sited ethnography’. It was certainly ‘multi-sited’, but could it still be called ‘ethnography’? Like the notion of a ‘theme park’ this term ‘ethnography’ has also acquired a global currency, and it again has different meanings in different contexts. In fact I am not sure that the research I carried out could be called ethnography at all. Before proposing a new term for this type of work, however, I would like to examine some of these meanings, and scavenge the old concept for its evident strengths.
Ethnography: its meanings and its strengths

In the strict meaning of the term, ethnography, as far as social anthropologists are concerned, is something we write. We do fieldwork, usually what we call participant observation, though it very often includes other ‘methods’ such as interviews, surveys and even questionnaires (Davies, 1999: 67). Then, we come back and write up our findings in such a way that the descriptive part (somewhat artificially opposed to the theoretical part) is called ethnography. The word is derived from Greek, of course, and graphy refers to the writing. Kearney addresses this aspect of ‘ethnography’ in his reconceptualization of the notion of peasantry (Kearney 1996: 3).

More broadly, however, the term ethnography is used to describe a kind of research method, albeit drawn from the work of social anthropologists (Denscombe 1998). Characteristics mentioned include: sharing in the lives of those under study, finding out how such people understand things, a holistic approach, and the observation of everyday life. Time in the field is another of this author’s characteristics, but ‘ethnographic research’ may be much shorter than the year or two at least a neophyte social anthropologist would be expected to do. Clearly there is a parallel value in studies carried out behind the bike sheds by educational researchers, or by health care researchers mingling with the nurses, but such studies might involve only a few weeks of contact. Either way, the researcher is in his or her field for much longer than I was in any of my multiple sites. So what is the strength I claim?

Another aspect of this ‘ethnographic research’ is that it is often carried out by one individual (or sometimes a small team), whose insight into understanding a group of people, their language and symbolism requires a good degree of interpretation in the exegesis of their work. Anthropologists very often learn what they know in a language different from the one in which they write, so that much of what they do could be compared with the work of a translator. Like those relying on interpreters for communication or negotiation, then, readers must to some extent trust the ethnographer to do their best to explain what they know. Moreover, much of an ethnographer’s work involves interpreting communication that goes beyond spoken language. This may be as simple as understanding non-spoken gestures, or reading deeper meaning into an apparently innocuous, but actually quite barbed statement. It may seek signals in forms of dress, modes of gift presentation, or ways of preparing a room for a meeting (Hendry 1993; Hendry and Watson 2001).

In complex analyses of symbolic communication, the interpretation of an anthropologist may go beyond, even contest, the interpretation of native speakers of that same language (Turner 1967: 20–1). This is because trained anthropologists acquire specialist knowledge of various types of communication in a wide range of languages and cultures. This range of possibility informs their interpretations, then, and they can place what they observe in the broader general context of human communication. What for local actors is simply seen as ‘the way we have always done things’, or ‘the proper way to proceed’ may fit very nicely into a scheme of behaviour noted much more widely, but with regional variation. An understanding of some of the broader meaning of like activity helps an anthropologist to interpret the specific behaviour of one particular group, and their findings may corroborate or conflict with theory made elsewhere. My own research on different understandings of the various meanings of ‘wrapping’ is an example of such an approach (Hendry
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1993). This moves beyond the ‘ethnographic method’, then, but it is an advantage operating in the global sphere.

Many anthropologists, too, operate as outsiders to the people under study, and this gives them a view uncluttered with internal preconceptions and prejudice. Thus, while different members of the same society may hold quite diverse views about particular issues, and may find some more easy to talk about than others, the anthropologist can achieve an overall picture they have not been brought up to evaluate, to criticize or to suppress. Religious and ritual ideas and activities are good examples of areas where discussion may be limited, and in many societies certain political subjects are also positively taboo. An anthropologist has a fund of theory through which to examine such ideas, though an insider may disagree with the interpretation.

It is true that this kind of situation has led other social scientists, and even some anthropologists themselves, to wonder about the validity of their work. Clifford and Marcus (1986) describe ethnography (in its written form) as little more than a construction of the ethnographer, and Geertz (1988) seeks to demonstrate the importance of the writing skills of the best-known exponents of the trade. The personality of a lone researcher who spends a long time in the field can hardly be ignored, and one reaction has been to insist that ethnographers provide information about themselves so that readers can try to disentangle the subjective researcher from the information provided about the people under study. This has come to be known as reflexivity. Pure objectivity is recognized as impossible in the analysis of the behaviour of other human beings so it is a means to limit the extent of the subjectivity. It is difficult without entering fully into it to measure some of the facts of social life, such as the difference between what people do, what they say they do, and what they say they ought to do. A long-term participant observer – or ethnographer – can gather information of this sort, but must not forget that their very participation may have affected the result.

Davies (1999) has analysed this process in some detail in a book entitled Reflexive ethnography in which she summarizes the contemporary arguments. She points out that reflexivity can recognize both the positivistic view that social arrangements exist outside the individual and may be observed and recorded, and the various hermeneutic approaches that appear to deny the first by relying on individual interpretations. Drawing on the work of Bhasker and G. H. Mead, she shows that individuals are both shaped by the society in which they are formed – exemplified in the language and other symbolic social interaction they use – and at the same time have the ability continually and creatively to engage with the social worlds in which they live.

Social scientists study these social worlds, analyse them and interpret them, and the long-term fieldwork of anthropologists involves deliberately trying to absorb a specific type of ‘social forming’ in order better to understand the world view of the people under study. Bloch (1991), examining this process from a cognitive point of view, likened the ‘data’ collected to those acquired by the body in learning to drive, or to play the piano. Once adjusted, many daily activities are carried out without conscious thought, and an anthropologist can learn to behave almost as a native, given enough interaction. Indeed, many anthropologists talk of the faux pas they commit on returning ‘home’ to their own societies in greetings, involuntary movements and other small elements of interaction because they have become so accustomed to different ways of behaving. Bloch called the knowledge thus acquired ‘chunked mental
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models’, and he argues that anthropologists who have once absorbed the ways of a people by living with them on a long-term basis must have confidence in using that incorporated knowledge in their explanations of them.

In a parallel fashion, anthropological training requires the study of the work of anthropologists and therefore allows the absorption of an understanding of the world that contains a whole gamut of possibilities for social behaviour that may go beyond normal experience in one social milieu. This understanding might be compared with that of an unqualified individual with extensive experience in different social milieu – indeed many of the subjects of transnational studies – but might equally be beyond the imagination of colleagues who practise ethnographic research methodology without knowledge of more than one language or culture (Marcus 1995). I believe that it is this dual collection of anthropological ‘chunked mental models’, acquired both first hand in one’s own fieldwork and second hand from the work of others, that distinguishes social anthropological research from other types of ‘ethnographic’ enterprise. It is this, too, that gives the subject a particular value in a global context where long-standing differences in systems of thought can be easily overlooked.

Finally, let us turn briefly to the other part of the Greek origin of this term, ethnography. Ethno is more difficult literally to translate than graphy, as although its root has been adopted in many European languages, including English, its meanings are quite various. It is of course used to describe the broader study of anthropologists, in the term ethnology. This is part of the problem, for the subject matter of anthropologists has been variously described in English as ‘races’, ‘tribes’, ‘savages’, ‘peoples’ and several other terms, usually but not always implying some kind of ‘other’ to the self of the anthropologist. This is quite in keeping then with more recent associations of the adjective ethnic, which may mean ‘foreign’, ‘exotic’, even ‘trendy’, as well as still having the unfortunate hangover of ‘race’.

The term ethnos, on the other hand, from which it derives, in contemporary Greek means ‘nation’. This is a term with a lot of philosophical history, but ultimately it serves the same purpose as the other translations above, namely to draw boundaries around groups of people. Transnational studies, by definition, cross these very same boundaries, and although some of the studies focus on people whose sources of identity derive from the original national project, others do not, and yet others are even opposed to it (Hannerz 1998). Like them, my previous and my present research cross such boundaries, but at the same time focus on expressions of them. The Japanese theme parks use the nation as an object of display, but they are directly comparable with places that use quite different markers, such as historical periods, or regional groups, so the use of the term ethnography, or even ethnology, could be misleading. In the next section I examine some of the methods used in my present project where the focus has an even more confusing relationship with the term ethnos.

Around the world in forty days: Experimenting with method?

Encouraged by the apparent success of my first, somewhat experimental venture into new pastures of research methodology to investigate theme parks, I then proceeded rather blithely into even less well-charted fields of study. Or, to be more precise, I am actually working in areas of territory that could be described as overly well-charted, though by investigators using more traditional research methodology. Indeed, I find
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myself in locations so well studied by orthodox anthropologists and ethnologists that the target populations are rebelling. They are tired of being represented by others, however well qualified, and they want instead to represent themselves. The network these people have established, if there is one, has perhaps partly been created – albeit inadvertently – by the anthropologists themselves. And in the terms of Hannerz (1998: 248–50), they might be said to constitute a ‘transnational’, or ‘global’ group.

The common feature they share is that they echo the sentiment expressed in the title of my book about theme parks, *The Orient strikes back*, and the resistance noted by Guarnizo and Smith (1998). Japan and other countries of the area we in the ‘West’ called ‘The Far East’ have for long been represented by Europeans and our colonial offspring in museums and exhibitions. The Japanese parks that were built to represent foreign countries are so sophisticated that I suggested they be compared with museums, but at the same time I point out some of the less sophisticated elements of museums around the world. The Japanese theme parks of course present only a partial view of the countries they represent, but they are positively comprehensive compared with the random selections of objects we can view about particular peoples in ethnographic museums. Many of the peoples represented in those establishments are now also striking back by setting up their own forms of cultural record or display. It was to examine some examples of these new forms that I set out on my recent project.

The first port-of-call was unexpectedly inspired by the talk in Oxford of a young Ojibwe woman named Nokomis, who came from ‘sovereign’ native American territory in the north of Minnesota. She had not travelled beyond the Twin Cities before, and her encounter with the objects in the Pitt Rivers Museum (and possibly also the British Museum) made her physically sick. She spoke movingly about her reactions to the collections, for as well as being nauseated by the remnants of human remains, and shocked by the sacred objects on display, she also appreciated that if these and other items of material culture had not been preserved in museums, there would be little evidence of the past of her people. The Ojibwe, like many other Native Americans, believed in allowing the natural process of rotting to consume objects beyond their best, and instead, used to replace them by making new ones.

Now, aware of the value placed on their material culture by the wider world, and with ample evidence of the advantages of recording local history in the political negotiations between ‘Indians’ and their American overseers, the people of the Red Lake Nation have set up their own Tribal Information Center. This centre was described by Nokomis as a place to reclaim their past and to stamp it with the personal memories and recollections of the people who had lived it, rather than seeing objects and photographs as data for study, as it still seems to be in the Minnesota Historical Society. This latter houses a splendid ‘museum’, where many of the Ojibwe objects are currently stored. It is a place in the city with enormous resources and apparent goodwill, but it is a good five or six hours drive from Red Lake, so some Ojibwe are even unaware of its existence.

Nokomis’s mother is in charge of the Tribal Information Center, and some literature about it was distributed at the talk, including the e-mail address. It so happens that I have an old friend in Minnesota, a novelist who seems to enjoy visiting and being visited, so I fired off a couple of enquiries. Both responded most encouragingly and my plans were then constrained only by the timing of my impending term of leave, the weather and, eventually, the invitation of Jodi, Nokomis’s mother, to
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attend a special event up at Red Lake. My friend agreed to drive me up, and when I arrived in Minnesota, she also took me to a number of other local examples of cultural display such as museums, institutes (for example the Swedish Institute records her own cultural story) and sites of historical re-enactment.

As it happened, then, I set off on this trip rather like a neophyte anthropologist going off to do a first bout of fieldwork (Bestor, Steinhoff and Bestor 2003). I had done some background reading; indeed, I had a wholly completed previous study on which to build (Hendry 2000). I also had a broad topic of interest, namely that people around the world who were tired of being represented by others wanted to devise their own means of recording their cultural particularity. I also had a limited grant to carry out an initial study. I had several groups of people in mind to visit, but as I began to make my plans I had only the barest of information about how to find them, about the places where they lived and about whether they would even talk to me. The trip that I planned would be a real voyage of discovery, just like any piece of anthropological fieldwork (Denscombe 1998).

The other visits I made were arranged through a similarly haphazard combination of inspiration, casually acquired information and fortuitous invitations. To make economic use of the grant and at the same time to retain a modicum of flexibility I purchased a round-the-world ticket. There were some promising places I had discussed but not visited in *The Orient strikes back* that I wanted to see first hand, and I decided on this first exploratory trip otherwise to pick a couple of broad areas of resistance to national, usually colonial, representation and seek examples of it. Partly for geographical reasons and partly thanks to some good advice I received from anthropologists before I left, I settled on a combination of North America and the South Pacific.

I had previously worked in Mexico, where I had a friend who promised support, so my second port-of-call was a Zapotec community in Oaxaca. The chain of introductions here led through several offices of research and administration of indigenous affairs to an anthropologist whose work was precisely to help indigenous people set up their own community museums. I was only in Mexico for a week, but it was long enough to unearth considerable complexity in forms of resistance to the Mexican national project, including anthropological endeavour, and in some areas (such as Chiapas) this was so explosive that I was advised not to travel. However, I already had considerable local knowledge and a command of Spanish, and I believe I made some progress.

The trip then proceeded with a visit to the above-mentioned Knott’s Berry Farm, in Anaheim, California. I had been attracted to see this place because of the ‘Indian Trails’ and ‘Mexican village’, and I wanted to see who was interpreting them for the visiting public. I had no local contacts here, so I felt a little uncertain about my conclusions, but the displays were open enough that one could speak to the people involved, and I did have a chance to discuss one show later with an anthropologist who knew the people presenting it. This site, like other theme parks around the world, is a commercial venture but, if ‘culture’ is to be displayed, there are usually political implications and these are subject to global critique.

My next few ports of call took me to places again entirely new to me, but which together illustrated a whole range of types of display, some more controlled by the people involved than others. My local backup was different in each place, and this
undoubtedly affected the findings and the outcome. In Hawai‘i, for example, I ben-efited from the introduction of an anthropologist to participants in the Polynesian Cultural Centre who gave me a great deal of time and attention; in the local museum I had to rely on labels and professional explanation. In Fiji I was invited to stay with a prominent, though British, citizen and my visits to cultural centres were again as a tourist. In Vanuatu I had the benefit of two or three valuable introductions, one to the anthropologically trained but native head of the ‘museum’, another to a member of the University of the South Pacific. In New Caledonia I was again able to interview the influential head of the Culture Centre, but a local contact I had been given was unfortunately unavailable at the time I called.

In New Zealand my visit was organized entirely by the family of a colleague in Japanese Studies who not only kindly took me to museums and culture centres but also introduced me to some anthropologists and a Maori professor in the university. Unfortunately, perhaps because I therefore became too dependent and relaxed, I missed one of the most innovative examples of cultural display in the museum world at Tepapa in Wellington. In Australia, however, where I had planned only to drop in on friends for a break, I discovered a new Aboriginal Interpretation Centre (in the process of being built) simply by picking up a brochure at the airport. In New Zealand, too, I met by chance in the Department of Japanese Studies a person whose relatives had been involved in an early example of Polynesian cultural display that informed my understanding of several other issues.

A colleague, this time an anthropologist who invited me to speak about my research, also arranged my final port of call, in Malaysia, and put me up in a splendid hotel. He offered me a highly intelligent and original thinking postgraduate student to take me to the places I wanted to visit and, for a modest daily sum, I acquired not only a guide with a car and local knowledge but also a top quality informant. My guide was of a different ethnic origin to my anthropological colleague, but the serendipitous breakthrough in my thinking during this period of the research was, perhaps not surprisingly, of the third major ethnic group living in Malaysia. This is not the place to go into all the details (see Hendry 2002), but the excellent support I received in Malaysia made the difference between being totally overwhelmed by the complexity of the place and gaining a completely new insight in the global discourse I was following.

What –ology can this be?

Now, clearly this series of visits could not constitute participant observation, except in some very brief bouts, nor does it even qualify to be ‘ethnography’ in the sense of ‘sharing in the lives of those under study’, or even much in the way of ‘observation of everyday life’ (Denscombe 1998). The fieldwork, however, could certainly be described as a ‘journey of discovery’, as mentioned above, and there was plenty of opportunity for serendipity, which, as anthropologists argue, is often a valuable part of their work (see, for example, Bestor, Steinhoff and Bestor 2003). There was one more crucial feature that places the study firmly in an anthropological mode, and this is to be found in the determined efforts I made to discern the ‘ways of thinking’ of the local people under study. They were also ‘other’ people, from my point of view, but the new feature of this project was that several such groups of ‘other’ people seemed to share ways of thinking based precisely on their own perceptions of that otherness.
The people I visited (and am currently still visiting), wherever they happened to reside, were categorized by outsiders like myself and represented by other outsiders in projects (like museums) largely of outside making. Some of these projects were colonial, some nation building, some commercial and some merely artistic or amusing, but they had all been projects initiated outside the boundaries of the group as now perceived. Academics such as anthropologists have played a large part in making possible these representations, even if they did not themselves do them, and it is hardly surprising that they are not always popular. The people I visited have chosen a great variety of ways in which to take over their own representation and a description of these might constitute some kind of -graphy in the old sense. Together, they hardly constitute an *ethnos*, however, though each may claim his or her own membership of such a group. What they share is simply the common aim of trying to reclaim their own cultural identity.

Membership in this sharing of ways of thinking actually does not constitute belonging to a new community as far as I can tell at this stage in the research. The people I visit usually do not think of themselves as part of a ‘global network’, though some may, and this is something to be pursued and evaluated. Nevertheless, I am certain that I have identified a common discourse, a common way of thinking about the globe that the people I meet inhabit, though I need a bit more time before I am ready to put forward a view on exactly how it was disseminated. It is not enough to call it ‘postcolonial’, though this word has very wide currency, because people who were neither colonizers nor colonized form part of my research field, and the term ‘postcolonial’ has itself been appropriated by the academic community in a way that clouds some of the issues.

In the meantime, however, an important question remains to be answered. How can I claim that I gained any understanding at all of the ‘ways of thinking’ of the people I visited when I spent so little time with them, and when I moved so rapidly from one group to another? First, and very basically, I certainly could not have done this without considerable help from local contacts who were clearly crucial in any success I achieved. It became clear, too, that different types of local contact offered benefits of different orders and, while some helped me to gain depth in my understanding of the people in question, others were more able to fill in details of the broader political situations in which they found themselves. In some cases, I ended up with a more partial view than in others, and in these cases I am dissatisfied with the results.

In order to plan for future trips, then, and for my own peace of mind to back up my assertion that this work does indeed qualify to be called serious research, I have attempted to categorize the local support I received into various types (see Table 1). These ‘types’, I argue, correspond to the ‘types’ of knowledge gained by a social or cultural anthropologist living for long periods in the field. In this case, however, my field is much larger than usual, while my time span is much shorter. My knowledge is second hand, but we always did rely to a great extent on our informants, and if I am careful, my informants can be very well prepared to help me out. They can provide essential elements of my aim to identify a way of thinking that spans the globe, but also to distinguish it from other ‘global’ ways of thinking that may be less aware of their limitations.
Table 1: Classification of support for fieldwork in a global arena

<table>
<thead>
<tr>
<th>Support</th>
<th>Need</th>
<th>Type of help</th>
</tr>
</thead>
<tbody>
<tr>
<td>Preparatory</td>
<td>Anthropologist or historian of the area</td>
<td>Expert preliminary advice about approach</td>
</tr>
<tr>
<td>Practical</td>
<td>Local resident</td>
<td>Local advice, practical help cf. neighbour</td>
</tr>
<tr>
<td>Conceptual</td>
<td>One or more natives of group represented</td>
<td>To express views, problems, discourse cf. chief informants or ritual experts</td>
</tr>
<tr>
<td>Material</td>
<td>Site of cultural display/archive</td>
<td>Material evidence of the issues</td>
</tr>
<tr>
<td>Analytical</td>
<td>Anthropological chunked mental models (training and experience)</td>
<td>Awareness of possibilities, openness to issues and ability to see beyond words</td>
</tr>
</tbody>
</table>

Some of these informants are already anthropologists, so it is hoped that we share those chunked mental models we learned as we trained; it is to these informants that I owe my decisions about where to go and how to approach people. Others simply know their way around the district on the ground; they save much time and the inconvenience of finding people and places; and they often also help interpret a local political situation. Most crucial informants, of course, are the ‘natives’ of the ‘global village’ I have created for them, and I rely on them to express the ‘ways of thinking’ that form my subject matter, preferably in the context of the material objects that represent the new approach I believe they are advocating.

Finally, according to the table I have drawn up, there is one other crucial category of support required, and that is to be found inside the body of the researcher. As always in an ethnographic project, the person carrying out the research is the main tool of the research and quite a lot depends on the ability of that person to fit as sensitively and as unobtrusively as possible into the situations of investigation. The ability to do this and then to make meaningful sense of the findings is not just an aspect of personality, as some reflexive work suggests. It is based, for anthropologists, on their anthropological training and experience and it seems likely that the longer and more detailed the prior participative research the more skilled and finely tuned will be the professional ‘chunked mental models’. This then is the element of global research that I contend can be the special contribution of anthropologists.

This is not a project to be undertaken lightly, however, and it is hardly an appropriate project for a neophyte anthropology student, even if he or she has already spent time travelling around the world. It seems to me that this kind of global research needs to be built on solid research of a more conventional kind. In knowing one or more ‘other(s)’ extremely well, preferably with a completely different language to one’s own, anthropologists build up a fund of possible ways of thinking in the context of one or more examples of great depth. They also acquire an ability to enter into and understand another’s point of view in a way that goes beyond the imagination of a person who has only read and digested the literature – or ethnography.
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Can this new research also be written up as ethnography, then, even if it seems better not to call it ethnographic research? In my case, since some of the people I have visited actually use the terms ‘nation’ or ‘sovereign’ to express their local identity, and as they are only one part of the wider field of study, I think it would be positively confusing to keep using this old term. These ‘ethno-nations’, as prior ‘tribes’, ‘minorities’ and ‘ethnic groups’ have been dubbed in the field of globalization (Scholte 2000: 166), have often taken an old model for their collective identity, but their representatives have met in various global gatherings since the mid-1970s. Because of such gatherings, it might be appropriate to call their new shared identity a global network, then, even if the characteristics they share are largely only to be found in their common discourse. Some members of the various different local communities I have visited know and meet some of the others, and these links are probably much better described literally as a global network than as any kind of community.

The people themselves can hardly be classed an *ethnos* and they do not even qualify to be a transnational community, so I wonder if it might be the time to create a neologism for the methodology we use to examine such a network as it develops. Anthropologists have always emphasized the importance of the context and, as well as the local one here, there is the new global context so vital to understanding issues that are discussed across all the old boundaries.

Perhaps what I write up here, then, should be called *globography*? This is not a very satisfactory term, in that it combines the Greek and Latin roots, but it could equally be applied to other such global communication and it is quite an accurate way to describe the reactions of members of a particular ‘global network’, in a minority again, to the larger, more powerful, but different global world from which they are trying to differentiate themselves. A word with more strictly Greek origins, like *ecumenography*, is another possibility, but the ecumenical, unlike the global, has become particularly associated with religious branches of social interaction.

Trends described as global, on the other hand, as Scholte (2000: 166) clearly points out, have ‘promoted the growth of alternative frameworks of community’. Studies such as mine, identifying and describing discourses held by people with different ways of defining themselves but who communicate through new, global forms of technology and exist only because of these forms of technology, could then be called *globology*. There are of course numerous other communities – or networks – of this sort and each has created and continues to create its own discourse for intercourse and interaction across the cyberspaces that separate, but also link them. Already their characteristics are studied for their economic, political and cultural elements (Axford 1995), but as far as I can discern no others have proposed the use of this word to describe their work.

This pair of terms reflects the original anthropological distinction between ethnography and ethnology and, while it will be quite impossible to prevent the adoption of such words by researchers in other fields, it strikes me that their introduction into this apparently unlikely context might again mark the kind of high quality study that the first pair characterized within ethnic groups. We may still be known for our detailed study of small-scale groups, but we have also gained good ground in a cross-cultural understanding of broad global issues like business and finance. Now let us celebrate the way our collective understanding of widely differing systems of thought is
informing burgeoning global theory and let us cooperate with those in other
disciplines to make this knowledge and understanding as widely available as possible.

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Notes
1. See Amit (2000: 1–2) for a short discussion of references to this characteristic as a feature
   distinguishing anthropology from sociology.
2. Sir James Frazer’s approach may have been more eclectic, but, as Hannerz (1996: 33) has
   pointed out, his work demonstrated not only great difference but also many similarities.
   Dealing with this apparent conundrum is one of anthropology’s strengths, to which I shall
   return.
3. Nowadays, of course, anthropologists work in societies full of researchers from other
   disciplines, many of them natives of the country themselves, so their work is easily subject
   to the checks of a range of other social scientists. Long ago, Edmund Leach pointed out the
   advantages for the understanding of people he worked with in Sri Lanka for anthropo-
   logists, very often from the outside world, to cooperate with sociologists trained locally
   (Leach 1961). In Japan, Japanese anthropologists themselves have commented on the
   advantages of cooperation with those who bring an outside view (Yoshida 1987), and it is
   generally encouraging to find that Japanese researchers from any discipline agree with
   one’s outside analysis (Hendry 1999: 141–7).

References
York: Cambridge University Press.
of Hawaii Press.
ethnography: forces, connections, and imaginations in a postmodern world, Berkeley, CA:
Clifford, J. and G. Marcus (eds) (1986) Writing culture: the poetics and politics of
Davies, C. A. (1999) Reflexive anthropology: a guide to researching selves and others,
London: Routledge.
Denscombe, M. (1998) The good research guide for small-scale social research projects,
Buckingham: Open University Press.
Foster, G. M., T. Scudder, E. Colson and R. V. Kemper (eds) (1979) Long-term field research
University Press.
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