MULTI-SITED ETHNOGRAPHY
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Editor’s Acknowledgements

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Introduction
Multi-sited Ethnography: Theory, Praxis and Locality in Contemporary Research
Mark-Anthony Falzon

Definitions and Propositions

Ethnography is an eclectic methodological choice which privileges an engaged, contextually rich and nuanced type of qualitative social research, in which fine grained daily interactions constitute the lifeblood of the data produced. With respect to method, it entails the situational combination of field techniques (note taking, audio-/visual recording, interviews, examination of indigenous literature, observation, and such) rooted in the ideal of participant observation (to live, to some extent, as the ‘natives’ themselves do), itself based on relations of trust and a belief that data are produced in and of ‘thick’ interaction between researcher/s and researched. Ethnographers typically think of data as a gift from their informants, with all the implications of reciprocity that gift exchange implies.

Conventionally, ethnography has involved the idea – if not necessarily the practice – of a relatively long term (typically several months upwards) stay in a field site of choice. The site was understood – contingently, although a significant chunk of monographs seem to imply the opposite\(^1\) – to be the container of a particular set of social relations, which could be studied and possibly compared with the contents of other containers elsewhere. To some extent, the contents might also be generalized into area, regional, or, most optimistically, universal knowledge.

‘Multi-sited ethnography’ purports to break with this convention. The standard reformative thesis was nailed by George E. Marcus to the door of the 1995 *Annual Review of Anthropology*. This deceptively short piece has been so widely bandied about that a lengthy discussion is hardly warranted. Telegraphically, Marcus argued that multi-sited ethnography defines as its objective the study of social phenomena that cannot be accounted for by focusing on a single site. Previously, the ‘world system’ was seen as a framework within which the local was contextualized or compared; it now becomes integral to and embedded in multi-sited objects of study. The essence of multi-sited research is to follow people, connections, associations,

\(^1\) There is, however, a marked tendency among the more starry-eyed advocates of the ‘cutting edge’ to exaggerate the extent to which conventional ethnographers saw their object as bounded (see Falzon 2005, 10).
and relationships across space (because they are substantially continuous but spatially non-contiguous). Research design proceeds by a series of juxtapositions in which the global is collapsed into and made an integral part of parallel, related local situations, rather than something monolithic or external to them. In terms of method, multi-sited ethnography involves a spatially dispersed field through which the ethnographer moves – actually, via sojourns in two or more places, or conceptually, by means of techniques of juxtaposition of data.

Since 1995, Marcus – sometimes in collaboration with other scholars – has pursued an ongoing project of ‘refunctioning ethnography’ (Holmes and Marcus 2004; 2005). The small number of sophisticated articulations of what multi-sited ethnography might actually mean, in theory and practice (see for instance Hannerz 2003; Gustavson and Cytrynbaum 2003), can be read as variations on a theme. I should also add at this stage that I take multi-sited ethnography necessarily to imply some form of (geographical) spatial de-centredness. I say this because, under pressure, the advocates of multi-sitedness sometimes defend themselves by saying that ‘site’ does not necessarily mean ‘location’ or ‘place’, but also ‘perspective’. As I see it, however, multi-sitedness is not synonymous with perspectivism. That would be a sleight, too easy and in any case counter-productive.

Predictably, reactions to Marcus’s programme have been mixed. On the one hand, there are those in the ‘nothing new under the sun’ camp, who – usually privately – tend to shrug it off as much ado about nothing. On the other, it has fired the spatial imagination of a generation of social scientists and, in the language of our times, greatly enlarged the discipline’s carbon footprint. In the last few years, we have also seen a number of sustained and serious critiques of Marcus’s original formulation and its elaborations. Hage (2005), for instance, who mischievously suggests that multi-sited imaginings may well be a symptom of delusions of innovativeness if not grandeur, argues that, with respect to studying, say, migration, the concept of a single geographically discontinuous site is much more useful than that of multi-sitedness. He also suggests that multi-sited research may imply a tacit holism, and proposes that a ‘certain reflexivity concerning the social relations that one is opting not to cover in depth’ (ibid., 466) makes for a better definition of one’s partiality. His conclusion is startling: ‘I simply do not think that there can be such a thing as a multi-sited ethnography’ (ibid., 465). For Hage, multi-sited ethnography is a buzzword, since ‘its signification and ramifications are (not) explored by many of its users ... (who) use it mechanically’ (ibid., 464).

I tend to agree with Hage that the worldliness of Marcus’s programme proved too seductive to allow the first generation of multi-sited ethnographers much room for self-critical reflection. Which is why there is hope yet for this book, the contents of which are anything but ‘mechanical’.

2 Although there is at least one interesting junction at which perspectivism and geographical multi-sitedness converge – that of looking at anthropology in terms of a multiple space where ‘other anthropologies’ and ‘anthropology otherwise’ (different epistemologies and methodologies, that is) develop (Restrepo and Escobar 2005).
A second significant recent critique of the multi-sited imaginary is that by Matei Candea (2007, reprinted in shortened version in the present volume). Like Hage, Candea targets in particular what he sees as a latter-day holism implicit (and sometimes explicit) in Marcus’s and subsequent formulations. He argues instead for an acceptance that ethnography is really about setting up ‘arbitrary locations’ – which in any case it invariably does, in the sense that a work like for example Garsten’s *Apple World* (1994) is actually Apple ‘places’, since her ethnography of the multinational company was based on fieldwork in a limited number of locations chosen by herself. Candea grants that the multi-sited programme has probably served to broaden the range of topics considered suitable for ethnographic study; however, he posits that Marcus’s device of ‘following’, for instance, could be applied equally well in a local, arbitrary setting – which is what ethnographers have pretty much always done. What the more naïve advocates of multi-sitedness see as ‘incompleteness’, Candea sees as a self-critical methodological decision (‘making the cut’) which one ‘reflects upon and takes responsibility for’ (ibid., 174). Ethnographers need to be more cautious of the seductions of ‘limitless narrative possibilities’ – which are deceptive in any case – opting instead for ‘sensibilities based on self-imposed restrictions’ (ibid., 168). To my mind, both Hage’s and Candea’s critiques are worth taking seriously by those of us who wish to develop the idea of multi-sitedness further and flesh it out both in theory and in practice.

With this in mind, the aims of this volume are fourfold:

- To present the theoretical and practical facets of multi-sited ethnography, and to take stock of these ideas and chart their development to date;
- To represent the main thrusts of multi-sited research via a number of empirical ethnographic case studies;
- To identify directions of research (including, very importantly, collaboration) using multi-sited ethnography as a means of studying contemporary social phenomena;
- To outline a programme for a ‘second generation’ multi-sited ethnography as a legitimate proposition for contemporary research.

**Why Multi-sited, Why Now?**

In order to try to take stock of multi-sited ethnography, it makes sense to outline some of the reasons why the idea emerged when it did, and why it is thought to be so apt (according to some, necessary) an approach to contemporary research. The question may be phrased as: Why did the localizing strategies of ethnography come into question, and why in the late twentieth century? Briefly, I think there are three main reasons.

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3 For a recent and succinct outline of the process and its implications – many facets of which are mirrored in the present volume – see Coleman and Collins (2006).
The first has to do with the notion, and its ethnographic consequences, that space is socially produced. Lefebvre (1991 [1974]) was the first to flesh it out in a sustained way, the story goes, though Foucault was foresighted enough to sense, years earlier, that the ‘turn’ was round the corner (see Soja 1989). Definitely by the 1990s, space was all over the social sciences. As summed up by Massey (2005, 9), our contemporary sensitivity to issues of space rests on three propositions:

First, ‘that we recognise space as the product of interrelations; as constituted through interactions, from the immensity of the global to the intimately tiny ... Second, that we understand space as the sphere of the possibility of the existence of multiplicity in the sense of contemporaneous plurality; as the sphere in which distinct trajectories coexist; as the sphere therefore of coexisting heterogeneity ... Third, that we recognise space as always under construction.

There is of course, in ethnography as well as in other social scientific circles, a long tradition of representing the relation between people and places. What is more recent is the reflection on the methodological corollary of this relation; at some point, ethnographers were banished from the Garden of Eden of spatially bounded cultural delights. By 1986, Salzman (1986, 528) was asking ‘Is traditional fieldwork outmoded?’, and suggesting among other things team research models of ethnographic practice. Ten years later, Fog Olwig and Hastrup suggested that ‘the methodological implications of this insight [that space is socially produced] are still being worked out’ (1996, 1, my parenthesis). Even today, the model of stable and bounded islands of cultural distinctiveness afloat in a sea of transnationalism remains, as Bashkow puts it, the ‘Achilles’ heel – or at least a recurring inflamed tendon – of anthropology’ (2004, 443).

Interestingly, the ‘spatial turn’ has affected fields beyond the social sciences. In literary criticism for one, recent years have seen a spate of works dealing with the spatialization of the text. Davidson (2007), for example, links free verse, and the way that the shape on the page is produced by the poem, to the Lefebvrian concept that space is not prior to but produced by human activity; Huang (2006) looks at ‘spatial negotiation’ in Asian American fiction; and Michelucci (2002) draws on the works of D.H. Lawrence to discuss place as a culturally constructed category which exists in relation to space as a physical and philosophical one.

In sum, contemporary research has to come to terms with the idea that, logically, if space is produced, there is no reason why the space of ethnography should be exempt. Which puts the processes of this production, and the possibility of alternatives, on the agenda.

The Perceived Inadequacy of the Local

The second set of forces which inspired Marcus, and which lend allure to multi-sited ethnography, may analytically be separated into two types: first, the idea
that contemporary societies are invariably, inevitably, and self-evidently located within larger wholes (see Cook et al., this volume); second, the seemingly obvious corollary that within these wholes, people, information, goods, and ideas are in a constant state of displacement – that it is, indeed, the ease of displacement that makes the whole possible.

There is a sense in which a cautious analytical holism seems to be at the heart of the ethnographic approach. At the same time as ethnography is about the particular, it is also thought to give, as Gay y Blasco and Wardle (2007, 43) put it, ‘further contextual meaning to particular lives by demonstrating their integration within more inclusive social forms’. Applying this to space/place, the question of what to do with the local has a long pedigree in ethnographic methodology.  

I have elsewhere traced how, during the latter half of the twentieth century, anthropologists – in particular those studying seemingly-bounded peasant villages, or ‘immigrant communities’ – became increasingly uncomfortable with the conventional idea that the local was an adequate form of ethnographic space (Falzon 2005). Even so, as recently as 2007, Russell could complain that migrants are people one says ‘hello’ or ‘goodbye’ to in their destinations and places of origin respectively, ‘but rarely with knowledge ... of the travels and travails in between’ (2007, 362).

The grandest themes of late twentieth century and contemporary social science seem to revolve around a problematic relation to the local and the search for some larger scale of analysis, and the study of connections between places. As Mintz (1998, 117) puts it, ‘(t)he new anthropology is many things, among them the study of human groups in motion. That motion is thought to be more than international; it is transnational’. World systems theory, transnationalism, migration studies that go beyond classical push-pull and/or integration concerns, diasporas, cosmopolitanism, and so forth: all posit frameworks and scales that invite supra-local understanding and therefore methodology. (There is in this respect some similarity with earlier paradigms such as evolutionism, diffusionism, and the study of regional ‘civilisations’ [see Falzon 2005] – one can only hope that history will judge us to have been less speculative.) The most prominent interpretive framework is of course that of globalization, which as a paradigm patterns much of our contemporary thought about people and places. A host of ‘anthropological problems’ are nowadays seen as being formed and reformed in and of ‘global assemblages’, in Ong and Collier’s words (2005).

Recent formulations of globalization have moved well away from both the ‘global village’ model and its less banal if more insidious cousin, namely the idea

4 As Geertz (1973, 23–4) points out, in anthropology this problem was all too often solved in either or both of two ways: by assuming that villages were perfect microcosms of larger social and political units (the ‘Jonesville-is-America’ model) or that, given their pristine condition, remote islands and villages made perfect laboratories for anthropological study (the ‘Easter-Island-is-a-testing-case’ model).

5 He proceeded to do multi-sited fieldwork with Yakkha people in Tamaphok, Nepal, and various migrant destinations in India and elsewhere.
that global interconnectedness co-exists with local variability. As Massey (2005, 88) puts it, “‘spatialising globalisation’ means recognising crucial characteristics of the spatial: its multiplicity, its openness, the fact that it is not reducible to a “surface”, its integral relation with temporality’. Invariably, the contributors to this volume appear well aware of these characteristics; indeed their multi-sited ethnographies are presented as ways of researching them in practice.

The point is that the paradigms of globalization and its cousin, transnationalism, no doubt posed the major twentieth century challenge to ethnographic methods of inquiry and units of analysis by destabilizing the embeddedness of social relations in particular communities and places (Gille and Ó Riain 2002). As such, they were also behind the multi-sited model; indeed, for Hannerz (1998), for example, ‘transnational research’ is broadly interchangeable with Marcus’s own terminology. It is commonly thought that a refusal by ethnography to engage with a type of spatialization associated in the popular (and sometime the scholarly) imagination with modernity would limit practitioners to an ever-tighter circle of apparently-bounded locales – which, given that the mantra of social science has always been ‘I am human and nothing human is alien to me’, would not do.

Clearly, my earlier point about space and the present one on the space of modernity are directly linked. If, as summed up by Marshall Berman’s (1983) application of a famous sentence, modernity is about ‘All that is solid Melts into air’, that includes ethnographic space.

**Historical-pragmatic Reasons**

Another set of reasons behind the multi-sited programme is more logistical than methodological. First, the institutionalization of the social sciences into mainstream academia, coupled with the prescribed work practices of contemporary academic careers (in which teaching and administration are on a par with research), has made it increasingly difficult for ethnographers to stay put in the field for the long durations classically associated with ethnography. There is a tendency, in other words, towards shorter field stints – especially as one progresses from doctoral studies to a ‘position’. Second, perhaps there is a supplementary point to be made, that the conventional idea of a fieldwork site was a walking one – a temenos with the human body as its yardstick, a place (as in a village or urban neighbourhood) across which one could walk comfortably in a day’s work. As ethnographers moved into places that were not villages, small islands, or urban neighbourhoods (‘street corners’), this accepted practice became increasingly problematic. How does one use one’s body to plot the temenos of ethnographies that take, say, the city of Mumbai as their site – such as Mazzarella’s (2003) on advertising and Hansen’s (2001) on Hindu nationalist politics? The answer is that it cannot be done; the consequence seems to be that such a spatial shift necessitates at least a reformulation of conventional methodology.
Nothing to Lose but our Gains: The Charges, Prosecution, and Defence

The discussion on multi-sited ethnography revolves around the idea that it may well be a contradiction in terms. That is, there exists a preoccupation that, while there is much to be said for researching spatially dispersed objects, a programme that proposes to be more routes than roots (see Clifford 1997) could well end up throwing out the proverbial bathwater and robbing ethnography of its central tenets as presented earlier. This preoccupation is not necessarily born of a purist conservatism (although that element is at times present), but rather the belief that the ethnographic paradigm – traced to Malinowski by anthropology and to early twentieth century urban studies by sociologists – has produced some of the richest social scientific insights, and is as such worth preserving.\(^6\) The contributors to this volume, myself included, never once depart from this premise. We also need it to tease apart the main strands of the critique that has been levelled at multi-sited ethnography, in that depth, ethnographic authority, and holistic analysis are key attributes of the Malinowskian paradigm.

The ‘Lack of Depth’ Charge

‘Depth’ – or, as Geertz (1973) famously put it, ‘thick description’ – is unquestionably one of ethnography’s richest offerings. Its lack is also thought to be the major enemy of the multi-sited programme. Briefly put, given that this type of research implies moving around and ‘following’ horizontally, there is little time for staying put and ‘following’ vertically.

In order to address this issue, we must question the process of production of depth/thickness in conventional ethnography; only then can we decide whether or not multi-sited ethnography measures up. Participant observation is the obvious answer, but what about it? The methodological stance of ‘getting off the verandah’ is only part, albeit a necessary one, of the story. Crucially, the factor that enables ethnographers to achieve depth (and also to make informed decisions about the partiality of their work – possibly the main concern of the following fourteen chapters) is time. Participant observation has its own time order, which typically runs to several months. Initiates believe that the growth of ethnographic consciousness reveals itself to the fieldworker as water boils, that is gradually but also through a defining moment, at which one suddenly realizes that one ‘understands’. The moment of inspiration may be abrupt but is actually the product of a gradual process. Prospective fieldworkers are told that, provided they

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\(^6\) Hage epitomizes this belief: ‘(A)fter spending so many years examining all kinds of work in the areas of migration and diasporic studies, I have consistently found it to be the case that of all the disciplines deployed in studying globalization, migration and mobility, none are better equipped ... than an ethnographic analysis’ (2005, 474). Having myself undertaken an ‘ethnography of diaspora’, I will not argue.
stick it out long enough, their moment will come. In ethnography, therefore, time transforms and makes. This element is so factored in historically that it has become almost invisible. We might here take a leaf out of Bourdieu’s writing on the implicit (and therefore unquestioned, by researchers and researched alike) periodicity of gift exchange, to the effect that it is not just the nature of the gift which gives it meaning but also its timing – ‘to abolish the interval is to abolish strategy’ (1977, 6). By analogy, to abolish the interval – and multi-sited ethnography ostensibly threatens to do just that – is to abolish depth.

There are at least three ways out of this. The first, notably explored in the present volume by Horst and Leonard, is to substitute long term with very long term fieldwork, thus enabling one to ‘take in’ more sites. A very reasonable proposition which, however, has its own special requirements and limitations. The second, popularly – and probably unfairly – associated with first generation formulations, is that the multiplicity of multi-sitedness makes up for its inadequacies in any single site. That it, as ethnographers move around, it becomes a matter of adding short durations to make a long one. The argument is clearly flawed in that two or more shallownesses do not make a depth. The third solution is much more compelling. There is a sense – to some extent implicit in notions of ‘time-space compression’ and such – in which space and time are methodologically interchangeable. As Thomas Mann puts it in The Magic Mountain (1995 [1924], 4)

Space, as it rolls and tumbles away between him and his native soil, proves to have powers normally ascribed only to time ... Time, they say, is water from the river Lethe, but alien air is a similar drink; and if its effects are less profound, it works all the more quickly.

In other words, it is not just time that transforms and makes, but also space. To my mind, this shift has always been part of the ethnographic paradigm, not least since conventional ethnography posits a long stay in one place (hence the relevance of space). Historically, too, fieldwork ‘away from home’ (therefore, spatial displacement) was also seen as a prime route to what textbooks call ‘de-focusing’, an ethnographic state of mind which in turn enables the production of data.

Clifford’s writings on route-based research and the conventionally backgrounded spatial shift of ‘getting there’, are well known (1997). To go further, just as classical foundation myths have the oikists [founders] mark out a new city’s boundaries or sacred temenos by circumambulation, as in the story of Remus and Romulus in Rome (Rehm 2002), ethnographers have classically set up spatial routines which enabled them to know a people by knowing a place. Consider the following excerpt from Jeremy Boissevain’s A Village in Malta (1980, 116), which I choose partly because of familiarity with the venue, partly because the anthropologist describes in rare lucid detail his daily spatial routines:

About a month after arrival I began to establish a routine ... Normally I went a roundabout way which took me past the parish church, the parish priest’s house, the baker’s shop,
and Pietru Cardona’s. It also took me past the main bus stop. All along the way I talked to people ... This twenty- to thirty-minute swing through the village usually brought me up to date.

The ethnographer here appears as serial circumambulist, daily retracing their steps and in so doing producing the site and knowledge about it. Spatial routine becomes a route to ethnographic knowledge.

Which really brings us back to Malinowski, in the sense that the need for participant observation – as the main portal to the native’s point of view – perhaps constitutes the strongest case for multi-sited ethnography, and one made by a number of contributors to this volume. If our object is mobile and/or spatially dispersed, being likewise surely becomes a form of participant observation – as Clifford (1992) puts it, it is ‘fieldwork as travel practice’. And, if conventional depth is hard to come by in unsettled circumstances, that is probably as things should be, in the sense that it represents the way our people themselves experience the world. Let me illustrate. On one occasion while doing fieldwork in Mumbai, I was talking to an informant over drinks at the Royal Yacht Club in Colaba. ‘It’s not so easy with the Sindworkis’, he lamented, ‘they globe trot so much that they barely have time for dinner with friends when visiting Mumbai’. I realized that this was exactly the ‘problem’ I had with my fieldwork. Trying to spread my tentacles as far and wide as possible (my field sites were separated by thousands of miles) was proving an excellent means of getting at certain aspects of my people’s social relations, but I was also not managing to establish the type of field relations that my Cambridge doctoral colleagues who lived in villages of a hundred-odd souls in Siberia or the Lakshadweep islands enjoyed.

Depressingly, it has taken me the best part of ten years to realize that this is exactly what Maurice Bloch (1991) was referring to when he wrote about cognitive non-linguistic ethnographic understandings that are as crucial to our enterprise as they are difficult to produce using contrived linguistic (as in interviews) techniques. Bloch was making an argument for the importance of participant observation as the sole means of achieving these understandings, and in my case this involved moving around, as my people did, and experiencing a broader but possible ‘shallower’ world, as they did. Understanding the shallow may itself be a form of depth.

Russell, who cites Clifford’s work extensively, is himself worth quoting more fully: ‘(I have) done this (fieldwork as a form of travel practice) through the example of a thirty-six hour sojourn with a historically migrant Yakkha family met fortuitously while I was ‘on the road’ ... Such experiences help to expand, and challenge, the conventional geographical and socio-cultural boundaries of groups such as the ‘Yakkha’ (2007, 378, my parentheses).

Sindworkis are a loosely defined subgroup of Indian business people. They characteristically own transnational businesses (not necessarily big businesses, but spatially spread out) and lead relatively mobile lives.
This volume is rich in examples of the homology between the shallowness of fieldwork and that of the object. Consider for instance Hovland’s missionaries, trying – like her – to juggle family and work commitments in Norway and a host of far-flung places. Or Gatt’s activists, seeking – like her – to reconcile the somewhat utopian notion of a decentralized, cosmopolitan organization with locally (grass)rooted practice. Finally, Mazzucato’s undercurrent of indignation at being used as a courier of gifts and information by her migrant informants brought back memories of being asked to find a job for a Mumbai-based young man by using my contacts among the ‘big’ London Sindhis, or an ‘aunty’’s attempt to find out more about a girl of marriageable age living across the world (see Falzon 2005). In other words, getting off the verandah may involve a longer trip than Malinowski probably ever thought necessary.

Which brings us to writing. It was all very well for Bloch (1991) to convince us that, in time, he learned how to tell a patch of good agricultural land without having to ask (that is, by spontaneous non-linguistic comparison to a cognitive composite of clues generated through months of fieldwork), but the problem was always going to be conveying this knowledge to the reader. Ethnographic consciousness is useless unless it can find expression in ethnographic voice. The question how to write this spatial depth must be of central concern to the multi-sited programme; in the present volume the chapters by Fortun and the Matsutake Worlds Research Group address it in a sustained and innovative way.

The ‘Abdication of Ethnographic Responsibility’ Charge

One charge that is sometimes levelled at the multi-sited programme is that, in advocating ‘following’, it appears to assume a pre-existing field – a ‘given’ space or set of trajectories produced by the people, goods, information, and so on, that are being followed. Clearly, that would go against the basic principle that space is socially produced, in this case by both researcher and researched. In other words, the very logic of contemporary understandings of space (as discussed earlier) requires that ethnographers take responsibility for their production of their field sites.

Interestingly, a number of contributors to this volume propose methodological devices that serve to connect the native’s point of view (with respect to siting) with that of the ethnographer. Marcus’s ‘paraethnographies’, Nadai and Maeder’s symbolic interactionism-‘cooling the mark’ combination, and Mazzucato’s name-generator questionnaires all function as bottom-up mediating contrivances that guide the ethnographer’s multi-siting. In a sense, the old hermeneutic device of ‘co-production’ of data is here being extended from a first level of interpretation to a second one of spatialization:9 in ‘following’, the ethnographer also co-produces space.

9 Perhaps the notion of ‘complicity’ is as old – albeit in a different application – as Humboldt. Humboldt did away with older transportative ideas of the production of meaning,
At the same time, Marcus’s and the other devices are all born of situations contrived essentially by the ethnographer. Which means that ethnography is not robbed of ethnographic responsibility, and that practitioners at the very least have an important part to play in the dynamics of these devices – a shared responsibility, so to speak. Viewed this way, multi-sited ethnography is a form of, to use Marcus’s earlier notion (1998), ‘complicity’.¹⁰ As law teaches us, complicity is still a very real, if partial, responsibility.

Perhaps more importantly, the notion of partial ethnographic responsibility opens up the whole field of looking at ways in which it is produced; surely, this must be one of the more useful corollaries of Marcus’s programme. As Gille and Ó Riaín (2002, 289) put it, ‘(t)he extension of the ethnographic site in space and time sharpens one’s sensibilities to the political consequences of defining a site or sites’. The issue and/or its variants have been discussed by among others Candea (2007 and this volume).

As a small contribution to the study of how ethnographers ‘make the cut’, I suggest we might consider borrowing from economics the principle of ‘satisficing’, famously coined by Herbert A. Simon. Simon (1997, see also Byron 2004) held that in practice, businesspeople do not ‘maximize’ in the straightforward neoclassical sense. Rather, they usually ‘satisfice’, that is look for courses of action that both satisfy and suffice – that are, in other words, ‘good enough’:

Examples of satisficing criteria, familiar enough to business people, if unfamiliar to most economists, are ‘share of market’, ‘reasonable profit’, ‘fair price’ ... But how do we know that this is a correct description of administrative decision-making – more accurate, for example, than the model of economic man? The first test, and perhaps not the least important, is the test of common sense. It is not difficult to imagine the decision-making mechanisms that the administrator of bounded rationality would use. Our picture of decision-making fits pretty well our introspective knowledge of our own judgemental processes. But the theory also passes a more severe test: It fits the mass of observations of human decision processes that have been made by the psychologists and researchers on organization and management who have studied them (Simon 1997, 119–20).

To my mind, the device of satisficing applies particularly well to our case. First, it strikes a compromise between a grand holistic ambition (in our case, maximizing to study the whole ‘system’) and a nonchalant way of ‘making the cut’. It requires the

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¹⁰ Two caveats on ‘complicity’. First one must distinguish, as Marcus does, between moral and methodological-discursive forms. Second, it may in any case have been an unfortunate choice of term, given that British social anthropology especially has often been accused (largely unfairly I think) of complicity with the colonial project (see Macdonald 2007).
cut to be good enough, or, in Simon’s terms, satisficing. Second, it absolves us of an arbitrariness that would expose ethnography to the charge of lack of rigour and method. For, just as satisficing cannot be disembedded from historical and cultural economic notions of what is satisfactory and sufficient, ethnographic partiality (the ‘cut’) is not established by the ethnographer in an autocratic and arbitrary way. Rather, one is guided by the scholarly literature on a particular topic, the current state of methodology, and one’s unfolding ethnographic insights on the ground. It is, therefore, not the individual ethnographer in isolation who decides what is good enough/satisficing, but the whole methodological and epistemological complex which they are part of. In these terms, ethnographic responsibility is more a matter of discipline-embedded partiality than arbitrariness.

*The ‘Latter-day Holism’ Charge*

The multi-sited programme has produced its own little ‘road to Damascus’ storyline. It basically goes something like this: ‘As originally planned, my fieldwork was to be conventionally single-sited; after some time on site, however, an epiphanic moment revealed to me that this was inadequate; I therefore chose to move around’. This seems to be something of a paradox, since a single located site appears to be adequate enough to reveal its own inadequacy. But that is not the point here. Rather, it is that multi-sited ethnography seems to imply holistic ambitions, not least since it purports to study the ‘world system’ no less. Certainly the ‘world-systems’ model proposed by Frank and Wallerstein (for example Wallerstein 1979) verges on the universal if not the dogmatic – indeed two of the three propositions of the model (that social scientists should look at social wholes, and that in the modern world there is only one effective whole, and that is the world system) are reminiscent of a rather more famous set of rules inscribed in stone.

Note that we are talking about holism as comprehensiveness here, rather than as contextualization. It is fair to say that the latter form, which posits that ‘behaviours be considered within the larger framework of people’s lives and cosmoologies’ (Miller 1997, as cited in Macdonald 2007, 72) is largely unscathed. It is also a key and historically-inbuilt characteristic of the ethnographic approach; Thornton (1988 – see Rumsey 2004 for a more recent discussion of holism as an ‘ethnographic macro-trope’), for example, has traced the rise of the ethnographic monograph, in the late nineteenth century, as a genre which operates a sense of holism in the image of a social whole made up of a number of interlocking parts. The former type, that ethnography is about the comprehensive study of wholes, is less defensible.

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11 Consider for instance Strauss on her ‘multi-local’ study of yoga: ‘Despite best intentions, the “traditional” ethnography I had imagined myself completing did not materialize; instead I found myself following threads and trails of people ...’ (2000, 163).
The critics of multi-sited ethnography hold that, no matter how fluid and contiguous a research object, it is best studied by focusing on a limited slice of the action. The best expression of this idea I can think of comes from a treatise on architecture written forty years ago. Talking about the ‘study area’ in terms of a recinto [a contained space], Aldo Rossi (1982 [1966], 64) holds that

> With respect to urban intervention today one should operate on a limited part of the city, although this does not preclude an abstract plan of the city’s development and the possibility of an altogether different point of view. Such a self-imposed limitation is a more realistic approach from the standpoint of both knowledge and program.

How, then, does the multi-sited approach square up with self-imposed limitations? How realistic is it as a form of knowledge and programme?

It might help to consider for a moment what actually makes research multi-sited. Let us assume that we want to keep the spatial displacement requisite mentioned earlier. In that case, multi-sited means that one works in more than one site and that these sites are dispersed. The first component need not detain us – it is generally accepted that this means two or more. The second is more interesting. How dispersed must the sites be? They cannot be too close, since that would make say Gellner’s 1960s research multi-sited (1969, 303):

> Thus my knowledge of the main lodge is based on prolonged visits and frequent and prolonged visits subsequently. Amzrai, Taria and Tighanimin are all within two hours’ walking distance of the main lodge: I know them both from frequent visits and from staying in them ...

Should they therefore be in different countries? Not necessarily – most people who have written about multi-sitedness distinguish it methodologically from multi-country research, although it may be that in practice. It seems that multi-sitedness actually means not just sites, but spatialized (cultural) difference – it is not important how many and how distant sites are, what matters is that they are different. This must be a requisite, because without it there would be absolutely no point in moving around. But it also seems to bring us dangerously close to the supposedly-conventional model of bounded culture.

Perhaps the main difference between single- and multi-sited approaches is language. The former talk about containing, the latter about extending. Ultimately, both are partial, because both have their self-/imposed limits. Multi-sited ethnography is no more holistically inclined than its predecessor.

**An Outline of the Chapters**

I am confident that what follows amply achieves the aims of the book as proposed earlier. All the contributors – who come from a range of disciplinary backgrounds
have themselves conducted ethnographic research, in some form or another. The general theme is to use this field experience, itself described in rich and nuanced form, in order theoretically to contribute to a young methodological field. While the chapters seek to take stock of the first ten years in the life of an idea, they do so with a critical eye on a second generation multi-sited ethnography that, one hopes, stands to play a key part in future social scientific research. Which is why themes like holism and partiality, perspectivism, ethnographic authority and accountability, systemic viability, and, importantly, collaboration, constitute the leitmotifs of the book. A word on sequence. I had originally planned to organize the chapters into sections. However, I now find that there are so many cross-cutting themes, that no one sequence of content and/or methodology quite works. Thus the alphabetical.

Chapter 1 is a shortened version of Matei Candea’s 2007 paper in the Journal of the Royal Anthropological Institute. Its contents have been discussed above. The paper is both recent and seminal and as such constitutes too relevant a perspective to leave out on the basis of some fuzzy notion of recycling. Moreover, Candea has added a postscript which conveys his latest thoughts on the topic.

If multi-sited ethnography is all about ‘the elephant in the room, stupid’, in Chapter 2 three Cambridge anthropologists join forces to question precisely the necessary presence of the elephant, that being a ‘system’ located theoretically at a ‘higher level’ of which ethnographers may, with luck, engage with some local manifestation – which would mean that multi-sited ethnographers, engaging as they do with several locations, are better placed to comprehend the bigger picture. Joanna Cook, James Laidlaw and Jonathan Mair draw a parallel between multi-sited methodology and the study of ‘world religions’. These two raise a set of similar issues; the latter, however, began seriously to question their implicit holistic assumptions just as the former raised them to epistemological glory. The authors make it clear that they are not after a rejection of the multi-sited programme. On the contrary, they seek to reformulate it by proposing to conceptualize the field in such a way as to detach it from the concepts of space and place, thus opening up the possibilities of an ‘unsited’ field within which comparison across theoretically relevant spatial boundaries can take place. The theory of the chapter is illustrated in practice using the authors’ ongoing collaborative research into the new forms of Buddhism that are being adopted across Asia. Aside, one might playfully suggest that the parallel between the anthropology of religion and the questioning of ‘bigger systems out there’ works on more levels than one – but this is my divertissement.

In Chapter 3, Kim Fortun draws upon a number of systems of knowledge – geology, the life sciences, informatics, and so on – to generate a language of what she calls ‘meta-data’, in turn used to describe how data ‘lower down in a system are configured so that they can be found, talked about, and more easily interpreted, shared and compared’. In her own acclaimed research on the aftermath of the 1984 gas leak in Bhopal, Fortun chose to privilege the transnationally produced plurivivocity that characterized the response to the disaster. She holds that ‘Bhopal’ (in quotes partly to unsettle it as a bounded location, partly because
Bhopal came to represent an open discursive space) was constituted by a number of metaphorical, technological, discursive, legal ‘locations’ and articulated on a range of scales. Further, the act of constructing ‘Bhopal’ happened on two levels: that produced (historically and in contested ways) by the protagonists of the aftermath, as well as that (equally partial) framed by the ethnographer herself. Throughout, Fortun stresses that multi-sited ethnography should not be seen as an attempt to be comprehensive and/or construct the real scale of the real system, as it were. Rather, its value lies in its capacity analytically to set up self-critical perspectives. Relativity and the ability to ‘play’ with different models and scales become themselves intrinsic parts of the ethnographic project.

Ester Gallo’s chapter constitutes a critique of the state of the art on various levels. It takes to task contemporary ‘transnational’ migration theory for essentializing and de-problematizing locations, and for assuming that migrants simply proceed to move between and connect across them; by inference, multi-sited research risks becoming a uni-dimensional case of moving around instead of staying put. Gallo draws on her own field experience to argue that multi-sitedness can be useful in revealing just how disparate and power-charged competing histories of transnationalism can be, even within the same ‘community’. Rather than being swapped, locations are therefore actively made and contested, and this makes their history at least as important as their geography. In methodological parallel, Gallo resists the idea that multi-sitedness is some sort of especially-ambitious research design. Instead, it is more a matter of research process of selection, generated as one ‘follows’ (or is it ‘constructs’?) one’s migrants. The ‘site’ as analytical framework and relational practice is expanded and limited as one does fieldwork, and this is a dynamic that should be made explicit by the ethnographer. Gallo’s tasks include revisiting the classical notion of ethnographic responsiveness, seeking to overcome the single-/multi-sited dichotomy, and dismissing the notion of pre-existing ‘depth’.

Caroline Gatt’s work with Friends of the Earth International (FoEI), an environmentalist federation with 69 national member groups and two million activists worldwide, raises a number of methodological issues. Gatt argues that the ‘multi-sitedness’ lies not so much in the plurality of geographical locations of the object, as much as in the ways activists experience and engage with different types of emplacements. Accordingly, multi-sited ethnography was for her more of a heuristic device, an ‘enabling factor’ that opened up what she calls the ‘topological multiplicity’ of her activists’ world views. This in three senses. First, the spate of literature associated with Marcus’s original programme suggested ways how to go about studying one of the key characteristics of FoEI, its ‘in-betweeness’. Second, the question of scale raised by discussions of multi-sitedness – global/local, system/lifeworld, nation-state/world – happened to fit snugly with her own data and particularly with the apparently contradictory logics of scale of her informants. On the one hand they saw the world as a seamless system, on the other they defined their activities in terms of the uniqueness of nation-state circumstance. Single-/multi-sited binaries were not useful to understand this; rather, activists inhabited,
experienced, and co-produced a number of places simultaneously. Third, and with respect to the perennial ‘depth’ question, Gatt’s experience points towards an overemphasis by some anthropologists on face-to-face relations – this was not necessarily true of how her informants saw and did things.

Chapter 6 is one of the more practically and empirically inclined in this collection. Cindy Horst draws on her field experience with Somali refugees in several countries and localities to make a cautiously optimistic case for the feasibility of multi-sited research. She is honest about the compromise between resources at hand, specifics of a research question/s, and methodological bent, that any fieldworker must strike; in the case of multi-sited approaches this may also involve multiple context-bound competences. As such she underlines her belief that multi-sitedness is still very much about partial choices, never about a ‘fuller’ picture by stacking site upon site. It is, however, a compromise worth making, especially in the case of research with transnational groups or phenomena like refugee camps, which are only deceptively isolated. In such cases, ‘ethnographic depth’ should be defined in terms of thick description of a network rather than its individual nodes. One of Horst’s main points concerns time. For her, the time order of such large scale projects is that of years. Fieldwork thus emerges as process rather than event, a ‘spiralling’ cumulative progression which borrows on a number of empirical strands – collaboration, the appointment of field assistants, direct participant observation, Internet research, and so on. Interestingly, Horst actively engages Mazzucato’s insights on ‘simultaneous research’ and ‘matched sampling’ (this volume). She prefers to opt for step-wise individual research based on cumulative snowball sampling, but not without discussing its ethical and logistical benefits/drawbacks vis-à-vis Mazzucato’s programme.

Ingie Hovland takes us to a number of locations in Norway and Madagascar and argues that aspects of Marcus’s 1995 vision can be put into practice to handsome dividend. In ‘follow the idea’ mode, she traces the notion of ‘the missionary’ at its various (not necessarily geographically dispersed) sites of production within a transnational missionary society. Hovland is in agreement with a number of recent scholars who hold that disjunctures and communicative gaps are as important an aspect of transnationalism as flows and connections – though the broader literature tends to focus, somewhat doe eyed, on the latter. Although the chapter rejects triangulation, it makes a clear statement in favour of multi-sited ethnography as being particularly well placed to study this ambiguous complex of dis/connections. In the case of the Norwegian Missionary Society, this emerges through contested notions of historical and geographical ‘locations’, and is mediated particularly through the multi-faceted figure of ‘the missionary’. Hovland draws a parallel between missionary and fieldworker, both of whom are variously constructed in time and space. She also argues that disconnections are not hindrances to transnational phenomena; on the contrary, they provide them with the ‘organizational creativity’ which makes them so interesting to social science.
If there is one topic which appears beautifully to lend itself to multi-sited ethnography it must be climate change, with its imagery of one cauldron in which all nations simmer. In Chapter 8, Werner Krauss locates himself geographically in an especially ‘threatened’ zone, the North Sea coast of Germany. In terms of perspectives, however, he combines coastal fieldwork, participant observation at an institute for climate research, transitory sites such as conferences, and virtual space. For him, multi-sitedness is very much about a plurality of ‘settings’ – climate research science, anthropology, policy making, the press, and so on. The chapter floats two important ideas. First, it questions the ways in which (globalist models and images of) climate change are localized. Second, it looks at the process of recalibration of scale that goes with such a process. (In this his inquiry cross-cuts with a number of contributions in the volume.) With respect to climate change, the two shifts are related: localizing becomes the process of downscaling from global climate models. This is as complex for the anthropologist as it is for climate researchers – both have to commute between very different localities and scales. This, for Krauss, is the essence of multi-sited ethnography. Interestingly, in ‘following’ (à la Marcus) climate scientists and their debates, he also finds himself questioning, as the more reflexive of his informants do, the dichotomy between social and natural science.

Even by the jet-setting standards of this book, Karen Leonard emerges as the most intrepid traveller of all – her fieldwork spans no less than eight sites. However, hers is no sequential album of travel snapshots. On the contrary, Leonard argues against the collage model of multi-sitedness in favour of what we might call fieldwork as one’s life’s work, a long term cumulative research trajectory that takes the ethnographer ‘across sites over time’ (significantly, Leonard is trained as both anthropologist and historian). The emphasis on longitudinality engages the chapter with a number of others in this collection (notably Horst’s and Mazzucato’s). The usefulness of such an approach emerges very clearly in Leonard’s data, which show how contexts have played a pivotal role in shaping migrant identities – in her case Hyderabadi, caught between ‘Deccani synthesis’ and other, less unifying, models. ‘Hyderabadi’ is thus de-essentialized as an ‘ethnic’ type in favour of a more structural-contextual understanding – a perspective which, Leonard argues, is only possible through multi-sited framing. Moving around, the researcher realizes that flows are rather less free than one might think, and that the latter component of ‘trans-national’ is rarely fully overcome by the former.

We are fortunate to be able to include an introspective, retrospective, but also highly innovative piece by the originator of the idea himself, George E. Marcus. In Chapter 10, Marcus sketches a course for a reform (note, not a revolution) of the Malinowskian paradigm that would constitute the real ‘turn’ of multi-sited ethnography. To do so, he summons and combines two of his legacies: first, as expected, his 1990s work on the specific approach itself; second, his major contribution to the ‘writing culture’ reflexivity discourse of the late 1980s. Having first outlined the four pillars of ‘anxiety reaction formation’ to the first wave of writings about multi-sited ethnography, he then moves on to talk about the
doctoral dissertation as a strategic site of methodological innovation. For Marcus, the key device that will finally enable the multi-sited project to achieve intelligent articulation, is the ‘collaborative alliance’ between ethnographer and informants. In this scheme, the conceptual design and apparatus of a given research project is not simply a researcher-literature coupling that is introduced into the system, but rather a process which is generated through ethnography itself, with the researcher and researched in tandem. The missing link between ethnographer and informants is provided by a further mediative device, ‘paraethnography’; ethnography thus becomes a distributed knowledge system. Further, since any given research project will involve a plurality of paraethnographies (in addition to the ethnographer’s own standard issue), ethnography becomes profoundly multi-sited. And, one should add, profoundly Malinowskian – in that paraethnographies are another take on ‘the native’s point of view’. Marcus is no armchair methodologist, and he shows how his scheme has proved useful in his own research with central bankers and Portuguese aristocrats.

The next instalment strikes a savoury note. If social science can productively ‘follow’ sugar (Mintz 1985) and tuna (Bestor 2003), why not mushrooms? Indeed, in Chapter 11, written by various collaborators from the Matsutake Worlds Research Group, the matsutake mushroom becomes a venue for an innovative discussion of aspects of contemporary methodology. The key term is ‘strong collaboration’, a type of teamwork that eschews classical notions of triangulation or ‘piecing together’ (‘research reports from here and there’, as the authors put it), and therefore remains rooted in a reflexive belief in scholarly partiality. So much so, that the authors’ model of collaboration extends to the non-human world, and includes matsutake itself as a legitimate party. In practice, what this means is that dynamics like translation and group writing – implicit in any collaborative venture – do not seek to achieve some sort of composite or extrapolative truth, but rather become themselves part of ethnographic participant observation. The authors are also critical of large scale conceptions like capitalism or an integrated world system, and opt instead for a premise that localities are sui generis. This may seem out of place in a volume on multi-sitedness, but it is precisely the paradox that makes the chapter so insightful.

Chapter 12 reports on ‘simultaneous matched sample’ methodology, an approach used by collaborators on the Ghana TransNet study, which looked at Ghanaian migrants to The Netherlands. Valentina Mazzucato and her team were intrigued by the fact that the existing literature which studies transnationalism using a multi-sited approach tends towards sequential research in a number of sites (typically two to three) as well as an over-reliance on interviews (rather than ethnography proper). Which results in a major shortcoming: on the one hand, the hallmark of transnationalism is said to be cross-border relations that take place in real time; on the other, studies of transnationalism proceed in a step-wise fashion. The Ghana TransNet project sought to break with this approach by designing research that looked at matched samples of translocally linked informants (networks), simultaneously. In other words, it took the ‘time order’
factor seriously – not in homage to some hoary notion of ‘history effect source of error’, but because simultaneity\textsuperscript{12} is one of the defining features of transnational phenomena. The project was managed somewhat along the lines of a multinational business, with a centralized decision-making structure and collaborators based in Amsterdam, Accra, and rural villages and towns in the Ashanti region of Ghana. The key concerns, which Mazzucato discusses in detail, were working as part of a team and the considerable outlay of resources involved. Implicit in the chapter is a model of a two-tiered level of collaboration which includes, first, that between the team members themselves, and, second, that between the team and informants – for it was ultimately informants who supplied the names of network partners.

In Chapter 13, sociologists Nadai and Maeder tell us that, even though the small-scale isolate has never struck a chord with sociology – with the consequence that ethnographic siting was always going to be more self-conscious than in anthropology – there remains a dearth of methodological-theoretical reflection on the topic. They seek to redress the balance by discussing ‘fuzzy fields’, the locations that sociological ethnography must actively construct for itself. Empirically, the example chosen is that of the ‘entrepreneurial self’, which is said to inhabit spaces like ‘the contemporary labour market’. The chapter’s guiding question is: ‘Where is our field when we track a highly theoretical concept with supposedly near-unlimited applicability?’ The answer is not attempted a priori, but develops as fieldwork unfolds, drawing in part on the input of the ‘important going concerns’ (Marcus’s paraethnographies?) of the informants themselves. Nadai and Maeder use two time honoured devices to tease out these concerns in a non-arbitrary way – symbolic interactionism and ‘cooling the mark’. In their case, these led them to (and through) a multinational company, a bank, a large Swiss retail chain, employment welfare programmes, job interviews, performance appraisals, and so on. These many ‘social worlds’ represented their multiple sites. The normative model of the ubiquitous entrepreneurial self was borne out, but not without a struggle by a number of important contextual variants.

The final chapter reconstructs parts of the lives of Sudanese migrants. Theoretically, Cordula Weißköppel criticises the dichotomy between stationary and multi-sited research; in fact, she says, all fieldwork is to some extent a composite of styles. She also distinguishes between sites and fields, and draws upon German vocabulary and semantics to make the point that multi-sited and multi-local research are not coterminous. For her, the essence of the former is an intense methodological reflection, as part of the research process, that evokes a ‘process-related generation of knowledge’ and leads to partial truths (see especially Candea, this volume). Research in several places, therefore, becomes more an

\textsuperscript{12} Also known in the literature as ‘instantaneity’, defined by Massey (2005, 76) as ‘a single global present’). Massey is critical of ‘extreme formulations’ of this imaginary, which she sees as synonymous with ‘depthlessness’ and the debunking of history (ibid.). Mazzucato does not of course argue for a single global present, but simply for simultaneity as one historically-embedded facet of transnationalism.
effect of multi-sited strategy than a requirement of it. Empirically, Weißköppel moves – or ‘gropes her way forward’ – between a number of ‘sites’ in Germany, the Sudan, and cyberspace. These are the transitory spaces of production of Sudanese migration in the ethnographic imaginary, and include one-to-one interviews, religious and civil society events, ‘community’ clubs, and so on. The two major thrusts of the chapter are, first, the fact that multi-sitedness fosters a sense of and feeling for ‘tracks, paths of understanding visualized horizontally rather than vertically (Weißköppel’s reflections on airlines and the in/visibility of violence are fascinating in this respect); second, that it involves a compromise between a bottom-up actor-centred ethnography and top-down decisions made by the ethnographer.

One of the less savoury characters in Flaubert’s *Madame Bovary* must be Homais, the local pharmacist. Scheming, noxious and essentially ignorant, he is almost a caricature of provincial pettiness. And yet, the novel ends with him being awarded the cross of the Legion of Honour. Aspirations of renown and worldly sophistication come and go in the shape of various characters (who can forget Madame Bovary tracing wistful lines on maps with their fingertips as she daydreams places?), yet it is ultimately Homais, content with his ‘thick’ knowledge of and control over his patch, who triumphs. It would be both unkind and unfair to compare sedentary ethnographers to Homais; likewise, the hope is that multi-sitedness is not a case of cartographical lines of desire that will in the end trounce us.

**References**


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Chapter 1
Arbitrary Locations: In Defence of the Bounded Field-site

Matei Candea

Introduction: 1995

In 1995, the filmmaker Peter Jackson embarked upon an adaptation of Tolkien’s fantasy epic *Lord of the Rings*. In the many interviews which followed the international success of the ensuing trilogy, Jackson reminisced on the roots of his project. He had been encouraged, he often stated, by a realization about the level of complexity reached by computer animation and special effect technology. Following these advances, Jackson realized it was now possible to put anything on screen, or as the director once put it, to do anything. Suddenly, all technological limitations having been removed, Jackson felt he was in a position to create a believable *Lord of the Rings*. And indeed, the director’s explicit policy throughout filming was to shoot with the realism of an historical reconstruction, and he enjoined all those involved in the project to consider it as such.

Back in 1995, two other filmmakers reacted in a rather different way to similar understandings of their growing technological freedom. The Danish directors Lars Von Trier and Thomas Vinterberg together agreed upon a manifesto, entitled *Dogme 95*, laying down strict limits on what, to use Jackson’s terminology, a filmmaker should do. In order to qualify for the Dogme seal of approval, a director had to forsake the use not only of visual effects, but also of lighting, props, overlaid musical soundtracks or even professional make-up. All shooting had to be done on location, using a handheld camera, and the plot was to forsake superficial action and ‘temporal or geographic alienation’. Von Trier and Vinterberg entitled their charter ‘the Vow of Chastity’, and it closed with the following pledge: ‘My supreme goal is to force the truth out of my characters and settings. I swear to do so by all the means available and at the cost of any good taste and any aesthetic considerations’ (Von Trier and Vinterberg 1995).

[...] This is where I would like to open my account, in this space between *Lord of the Rings* and Dogme, between sensibilities based on limitless narrative possibilities and sensibilities based on self-imposed restriction. But this chapter is

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1 This chapter is a shortened version of an article published in the *Journal of the Royal Anthropological Institute (N.S.)* 13, 167–84. The postscript has been written specifically for the present volume.
not about film, it is about anthropological methodology, about the way in which it has been shaped by a sense that the world was increasingly connected and seamless. More specifically, I will examine calls for multi-sited research which have urged us to expand the possibilities and vistas of ethnography in order to deal with a complex world. Drawing on my own recent fieldwork on the French island of Corsica, this chapter asks what ethnography would look like if we took the other path, the path of self-limitation rather than the path of expansion?

In 1995, George Marcus coined a phrase which was to achieve resounding fame in and beyond anthropological circles, namely ‘multi-sited ethnography’ (Marcus 1995). Less a programmatic piece than a review of already existing research strategies, Marcus’s article nevertheless framed and concretized a methodological trend, by providing it with historical contextualization, a range of practical suggestions, and a defence against potential critiques and anxieties. This is a trend which the author and Michael Fischer had called for a decade earlier in their book *Anthropology as Cultural Critique*, under the designation of ‘multi-locale ethnography’ (Marcus and Fischer 1986, 90–5). Multi-locale/multi-sited ethnography was an attempt to adapt anthropology to the changing realities of what had been known since the 1970s as the ‘world-system’, and in the 1990s became increasingly glossed as ‘globalization’. This implied a reconfiguration of the ‘traditional’ anthropological method of intensive participant observation in a single bounded location, what Marcus and Fischer refer to as ‘the convention of restricting ethnographic description to a delimited fieldsite, or locale, and set of subjects’ (ibid., 90). This meant going further even than studies in which a local setting is related to a global system, since these ‘still very much frame their research and writing in terms of knowable communities, to use Raymond Williams’s phrase, the kind of setting in which, by definition, ethnographers have always worked’ (ibid.). Although this was perhaps debatable as a characterization of actual anthropological practice, it was at the time an accurate representation of what Marcus later termed the ‘research imaginary’ of the discipline, namely ‘a sense of the changing presuppositions, or sensibilities […] that informs the way research ideas are formulated and actual fieldwork projects are conceived’ (Marcus 1998, 10)

The single-sited methodology, its sensibility and epistemological presuppositions, were no longer felt to be adequate to the realities of an increasingly mobile, shifting and interconnected world. Even lay readers of anthropological texts, Marcus and Fischer pointed out, were increasingly aware of the fallacy of localized holistic studies of ‘a culture’ (Marcus and Fischer 1986, 95). If anthropology was to remain convincing and meaningful, it would have to adapt its

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2 The juxtaposition between ethnography and cinema is presented here rather in the spirit of a surrealist collage. This is not to assert any direct equivalence between the two fields, but rather to operationalize both the parallels and the incommensurabilities, both the familiar and the uncanny aspects of such a comparison.
methods to ‘cultures in fragments increasingly held together by their resistance and accommodation to penetrating impersonal systems of political economy’ (ibid.).

This involved freeing ethnographers from the conceptual boundaries of the delimited site, and allowing them to follow movements of people, ideas and objects, to trace and map complex networks. By the time ‘multi-sitedness’ was coined in print in 1995, the practice and the idea were in communication with a wider literature on the anthropology of globalization (see for instance Appadurai 1991; 1995; 1996) and networks (Latour 1991; Mol 1994), which signalled a broader dissatisfaction with the perceived rigidities of social scientific method.

The first part of this chapter attempts to characterize this new ‘research imaginary’ (to use George Marcus’s phrase), an imaginary which is centrally concerned with freedom, complexity and expansion. To return to my earlier analogy, in the past decade anthropological theorists of the field have been Peter Jacksons rather than Lars Von Triers: the main drive has been to transcend boundaries spatial, intellectual and disciplinary, to weave together accounts of ever-increasing complexity, in multiple spaces, times and languages. I will argue that there is, however, a problematic reconfiguration of holism implicit (and sometimes explicit) in the multi-sited research sensibility – a suggestion that bursting out of our fieldsites will enable us to provide an account of a totality ‘out there’.

This leads me to a reconsideration of the value of delimited field-sites as what I will call ‘arbitrary locations’, methodological instruments for deferring closure and challenging totality. The sub-title of this chapter is thus to be understood as anything but a call for a return to ‘traditional practice’: the bounded field-site I am suggesting here is a development on the same dissatisfactions with previous practice which gave birth to multi-sitedness itself. To illustrate the notion of an arbitrary location, I will give a brief account of my own ‘field-site’ in Corsica, and of the implications of considering it a ‘bounded’ unit of analysis.

This somewhat abstract theoretical discussion is flanked by a more tentative and experiential one. In the introduction to Ethnography Through Thick and Thin, George Marcus notes that his conception of multi-sited ethnography was developed partly in response to the predicament of his and other graduate students, ‘anthropologists-in-the-making’ struggling with the difficult interplay between convention and invention in the production of dissertation research projects (Marcus 1998, 11). This chapter emerges from a similar dynamic: it reflects the predicament of an anthropologist-in-the-making trained at a time when Marcus’s multi-sited ethnography has already to a great extent become part of conventional practice. Drawing on the ethnographic and theoretical problems I encountered in my work on inclusion and exclusion in Corsica (Candea 2005), I will suggest that whereas the strength of the multi-sited imaginary lies in its enabling anthropologists to expand their horizons in an unprecedented way, its weakness lies in its lack of attention to processes of bounding, selection and choice – processes which any ethnographer has to undergo to reduce the initial indeterminacy of field experience into a meaningful account.
It would of course be a serious mistake to conflate a ‘research imaginary’ with the actual research it produces, enables or inspires. Just as a so-called ‘traditional’ ethnographic imaginary gave rise in practice to works which were as mobile and, in some senses, ‘multi-sited’ as the Argonauts of the Western Pacific or those arising from the Manchester school’s ‘extended case method’ (Malinowski 1992; Gluckman 1958; cf. Burawoy 1998), recent ethnographies inspired by the ‘multi-sited imaginary’ necessarily deal, in practice, with the issues of bounding and limitation which theoretical proposals for multi-sitedness do not explicitly address. Drawing on various non-anthropological sources of potential inspiration, the chapter ends with a call to make such processes of bounding and self-limitation more intentional and explicit.

The Multi-sited Imaginary 1: Seamless Reality

[...] Since the early 1990s, two major tropes in anthropological musings on the field-site have been unboundedness and complexity. Pushing ever further the boundaries of so-called ‘proper anthropology’, theorists have suggested and ethnographers have proved that anthropology could talk about anything, anywhere and in any way. There are now anthropologies of the past, anthropologies of the future, anthropologies of the ‘world-system’ and anthropologies of the individual life history, anthropologies of the metro and of the pipe-line, of the bizarre and of the banal, of scallops and virtual reality (Fortun 2001; Linger 2001; Latour 1993; Callon 1986; Wilson and Peterson 2002).

This multiplicity of objects is indissociable from a multiplicity of method, and George Marcus provided a key trope for this new anthropological wave, when he wrote a decade ago now, of ‘multi-sited ethnography’ (Marcus 1998, 13–4, my emphasis)

For me the development of multi-sited strategies for doing ethnography so as to discover and define more complex and surprising objects of study is literally one important way at present to expand the significance and power, while at the same time changing the form, of ethnographic knowledge.

In other words, one might suggest somewhat mischievously, multi-sited ethnography is to anthropology what Computer Generated Imaging is to the Lord of the Rings. It is a methodological bonanza which removes limitations and allows us, like Peter Jackson, to ‘do’ anything (including for instance an ethnography of the Lord of the Rings in the many sites of its production, consumption and imagination – if we so wished).³

³ As has been done recently by Steven Caton (1999) with the film Lawrence of Arabia.
In his famous 1995 article, Marcus suggests a number of possible strategies for multi-sited ethnography, all but one of which are premised on ‘following’ (following the people, the thing, the metaphor, the conflict, and so on – Marcus 1995, 105ff). ‘Following’ takes us through various sites, each of which is understood to be, not a self-contained local instance in communication with a global system, but an ethnographic location for the direct study of this system itself.

This is because, as these authors and others have made clear, their model of reality is seamless. Moving away from the contrast between the local and the global, George Marcus, together with other theorists such as Arjun Appadurai and Bruno Latour, has emphasised the fact that any ‘global’ entity is – must be, can only be – local in all its points. It follows that each ‘localized’, sited study is – must be, can only be – simultaneously a study of the ‘world-system’ (Marcus 1995; Appadurai 1995; Latour 1991).

In describing their model of reality as ‘seamless’, I am not suggesting that these theorists necessarily disregard the blockages, confinements and boundaries evident in their material – although this accusation has indeed been levelled at some theorists of globalization (Navaro-Yashin 2003, 108). I will examine below the status which such boundaries hold in their accounts. Here, I am referring to the epistemological ground of their descriptions: contrary to previous notions of a world composed of discrete ‘cultural gardens’ (Fabian 1983, 44ff), naturally bounded and eminently comparable, the world of multi-sitedness is woven of a single, many stranded cloth (albeit with its knots, rips and tears). By implication therefore, a field-site is a contingent framing cut out of this seamless reality. This contingent nature of the field-site is expressed most clearly by Vered Amit (2000b, 6, my emphasis):

In a world of infinite interconnections and overlapping contexts, the ethnographic field cannot simply exist, awaiting discovery. It has to be laboriously constructed, prised apart from all the other possibilities for contextualisation to which its constituent relationships and connections could also be referred.

The Multi-sited Imaginary 2: The Site as Ethnographic Object

So far, so good. But if ‘the field’ is a framing cut out of a seamless reality, how does one make the cut? In his 1995 article, Marcus does not address this point, and the ‘site’ part of ‘multi-sited ethnography’ remains unelaborated. This is no mere oversight: it is indicative of the fact that the explicit delimitation of the field-site is

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4 It is clear that for all the occasional cross-borrowing, Latour, Appadurai and Marcus’s projects are profoundly different in scope, orientation and sensibility. I bring them together only insofar as all three posit versions of a ‘seamless world’ which challenge macro/micro distinctions. In this, of course, they are part of a wider and older intellectual tradition (see for instance McDonald 1989).
increasingly out of the ethnographer’s hands. Sites are understood as the products of often conflicting political and epistemological processes ‘on the ground’, processes which should themselves be the object of anthropological study.

In his work on locality, Arjun Appadurai goes so far as to read this back in time: the main object of ethnography is discovered to have always been, not in fact localities, but processes of ‘localization’ (Appadurai 1995, 207). Even when they thought they were studying geographically bounded sites, anthropologists were in fact studying and contributing to processes of siting. Appadurai argues that ethnography has always misrecognized the fact that it was dealing with locality as process, because it tended to take for granted the stories of permanence which localization tells about itself (ibid.).

In other words, locality, but also stoppages, blocks, confinements and divides, are not forgotten in this reconfiguration of the ethnographic method. But their status has changed: the locality, the site itself has become an object rather than a tool of ethnographic enquiry. That is to say, the ethnographer is increasingly understood to be working ‘in’ (and ‘on’) the sites which are meaningful to the people he works with. The relevant boundaries to the analysis are not fixed a priori, they are ‘discovered’ on the ground. Thus Marcus (1999c, 117) notes approvingly that

> the intellectual environment surrounding contemporary ethnographic study makes it seem incomplete or even trivial if it does not encompass within its own research design a full mapping of a cultural formation, the contours of which cannot be presumed but are themselves a key discovery of ethnographic enquiry.\(^5\)

Paradoxically therefore, the ‘research design’ must be a result of ‘the ethnographic enquiry’. The research – beyond the broadest initial orientations – is thus ‘designed’ a posteriori. This is a fairly accurate reflection of the actual state of the art in many cases, but it does suggest that ‘sites’ are no longer the preliminary limits set by the researcher, they are discovered in the course of an initially open-ended research. This accords well, both with a long-standing and justified valorization of anthropological open-endedness (see below), and with an emergent conception of sites as ‘found objects’, artefacts of the ‘informants’ making, rather than of the ‘ethnographer’’s.

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\(^5\) Although it is not necessarily evident from the quotation, this is something Marcus approves of. The passage continues: ‘The sense of the object of study being “here and there” has begun to wreak productive havoc on the “being there” of classic ethnographic authority’ (ibid.). More generally, the methodological literature examined here is poised between claims to represent an already existing and broadly accepted ‘new’ way of doing ethnography, and attacks on an ‘old’, fettering, academic consensus. Marcus weighs rather on the former side, while Gupta and Ferguson or Amit weigh more on the latter. My own position is closer to Marcus’s, and I consider that to a great extent, the battle against ‘proper anthropology’ has been won.
In this sense, Akhil Gupta and James Ferguson’s later deconstruction of the anthropological field could be seen as the culmination of both Appadurai and Marcus’s arguments (Gupta and Ferguson 1997b). These authors ask anthropologists to abandon ‘bounded fields’ altogether, while looking out for the ways in which locations are constituted politically and epistemologically (ibid., 38). In this way, they are drawing to its logical conclusion the methodological project of a ‘multi-sited’ fieldwork which was always more concerned with the multi (the connection, the movement, the ‘following’), than with the ‘site’. The bounded site, the locality itself becomes an object of study, and the ethnographer is now free to follow others as they do the bounding, the localization and the delimitation. The site, as an anthropological heuristic device, an analytical framework, is concomitantly dissolved.

There is something of a discrepancy, however, between this and the previous point: on the one hand, as we have seen, multi-sitedness highlights the construction and contingency of sites in a seamless world, on the other, the desire to leave ‘siting’ to others, and to study ‘their’ sites, seems to revive earlier notions of a site as a really existing entity out there, something to be discovered.

An example of this paradox can be found in the introductory pages of a recent self-definedly multi-sited ethnography, Kim Fortun’s stylistically innovative account of advocacy after the Bhopal disaster (Fortun 2001, 1):

I traveled to Bhopal to collect material illustrative of the background from which concern about chemical pollution had emerged. Immediately, it was clear that Bhopal could not be conceived as a ‘case study’, a bounded unit of analysis easily organised for comparative ends. To the contrary, Bhopal showed no evidence of boundaries of time, space or concept.

But boundedness is a methodological issue, as Fortun herself notes a few pages later; that is to say, Bhopal in and of itself is neither bounded nor unbound. Surely, one could very well ‘conceive’ of Bhopal as a case study (and then trace all its ‘connections to’ and ‘parallels with’ other cases); equally, one could – and Fortun does it masterfully – analyse it as ‘a whirlwind – a maelstrom produced by opposing currents, sucking everything into an upwards spiral- with gas victims at the storm center’ (ibid.). This is, however, an analytical (and in this case, political/ethical) decision.

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6 ‘The question is not whether interpretations exclude, but how and to what effect’ (Fortun 2001, 6). ‘My goal was to avoid isolating Bhopal in space and time by continually seeking new connections – connections that drew out the complex systems that continue to bind me to Bhopal’ (ibid., 5–6).

7 ‘The settlement of the Bhopal case invokes a need for accounts of the disaster that show how it continues. Across time. Across space. At the intersection of crosscutting forces’ (Fortun 2001, 9).
The problem is that when it presents (un)boundedness as a real feature of the world out there (Bhopal ‘showed no evidence of boundaries’), rather than a methodological issue, the multi-sited approach forgets the possibility, indeed the necessity of bounding as an anthropological practice. This raises issues both for the epistemology of the discipline and for the experience of the fieldworker. Let us begin with the latter.

The Multi-sited Imaginary 3: Freedom

The master metaphors of multi-sitedness in anthropology have been liberation and ‘the changing times’: in order to cope with and participate in a world which has either suddenly become, or has suddenly been identified as, seamless, ‘fluid’ and interconnected, the ethnographer needs to be freed from the limitations of the bounded field-site.\(^8\)

In this vein, Vered Amit’s collection *Constructing the Field* is dedicated to ‘opening up […] the scope of anthropological enquiry’ (Amit 2000b, 2), and the contributors, in various ways, probe the limits of ‘archetypal’ field-work. Amit’s plea for methodological freedom is based on her understanding of ethnography as a powerfully unfettered generative practice (ibid., 17):

> To overdetermine fieldwork practices is therefore to undermine the very strength of ethnography, the way in which it deliberately leaves openings for unanticipated discoveries and directions. If in cleaving to a methodological orthodoxy, anthropologists a priori limit rather than leave open the scope of circumstances to be studied, they will be operating at epistemological cross purposes with their own disciplinary objectives.

This is very much the spirit in which I approached my own first attempt at ethnography, which I have recently exorcised into a thesis on exclusion and inclusion in Corsica (Candea 2005). Although my work and residence centred on what may appear to be, on the face of it, a most ‘traditional’ context, namely a village in the north of Corsica, my own experience was a powerful embodiment

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8 This sense of ‘the field’ and ‘the world’ as objects brought out of joint by the forces of history, and which need to be re-aligned, was already suggested in the title of Marcus’s article (‘Ethnography in/of the World System’ – Marcus 1995); it reappears in the subtitle of Vered Amit’s collection on the same subject (*Constructing the Field: Ethnographic Fieldwork in the Contemporary World* – Amit, 2000a). In the introduction to this volume, Amit reiterates Gupta and Ferguson’s point about the increasing gap between the experience and archetype of fieldwork, and argues that the archetype ‘no longer suffices even as a serviceable fiction for many contemporary ethnographers’ (Amit 2000b, 2 – my emphasis; cf. Gupta and Ferguson 1997b). Similarly, Marcus presents multi-sited ethnography as a way of ‘adapting ethnographic practices of fieldwork and writing to new conditions of work’ (Marcus 1998, 3 – my emphasis).
of the point made by Marcus that any fieldwork is initially and potentially multi-sited, as the ethnographer is faced with the teeming multiplicity of an unfamiliar context. According to Marcus, however, this initial state does not last: ‘as research evolves, principles of selection operate to bound the effective field in line with long-standing disciplinary perceptions about what the object of study should be.’ (Marcus 1995, 100)

Marcus, prefiguring Amit, seems to deplore these fetters on the generative multiplicity of the field experience. But as far as I was concerned, ten years on, these ‘long-standing disciplinary perceptions’ were nowhere to be found. It is a tribute to the success of Marcus and others that I found myself in a village in Corsica without any sense of what the object of study should be. This is far from an ironic comment: the multi-sited imaginary has played a crucial role in expanding the possibilities of anthropology and the range of topics which could be considered suitable for ‘fieldwork’. [

But this success has contributed to erode the guidelines and rigidities against which critics of ‘traditional methods’ so productively strained. And the resulting freedom has its drawbacks. As a novice ethnographer and PhD student I had of course the detailed text of my research proposal, but I had been informed in no uncertain terms that this was anything but a binding document, and that ‘no one ends up working on what they set out to work on in the first place’. On the contrary, I was instructed to find everything interesting, and I did.

This initial indeterminacy persisted throughout fieldwork. The famous ‘principles of selection’ which were supposed to come in and shackle me to a boring single object of study, never materialized. In the spirit of multi-sited ethnography, I followed people, stories, metaphors and debates through multiple spaces both within ‘the village’ and without, with a constant attention to the way such spaces were constituted. But this in practice led to a constant indeterminacy: how many leads to follow? how much context to seek? how much information is enough information?

Not being a ubiquitous being, I had to make choices: should I go to the sheep shearing or accept an invitation to meet my neighbours’ family? ‘Do’ participant observation in a bilingual classroom, or follow the teachers to a training course on the other side of the island? ‘Hang out’ in the village centre, surf the weblogs and forums dedicated to Corsican nationalism, or go and peruse the village or regional archives – or perhaps the national ones in Paris? Go for dinner with a neighbour, stay in the bar, or go out clubbing in a nearby town with co-workers? Fieldwork involved constant choices, and there was often no good reason to prioritize one over the other.

As a result, 13 months went by with a constant sense of incompleteness and arbitrariness, the obsessive feeling of missing out, of vagueness and unjustifiable indeterminacy, of never being in the right place at the right time. These anxieties and obsessions, which many of my colleagues shared, turned out to be productive aspects of the fieldwork experience: in time, they forced me to think critically about the kind of imaginary completeness and totality against which my own efforts seemed so
unsatisfactory. But this experience also brought home to me that what I suffered under was certainly not the tyranny of boundedness and disciplinary rigidities – on the contrary, it was very clearly the ‘tyranny of choice’ (Salecl, n.d.).

Of course, both in Corsica and later, during ‘writing-up’, I made choices, delineated topics, subjects and areas of interest, limited the extent or the depth of my research in various directions. In the second half of this paper, I will describe, through the notion of the ‘arbitrary location’, the conceptual underpinnings of the writing-up part of this process. But as for the fieldwork part, the imaginary of freedom and unboundedness made any choice, boundary or restriction feel like an illicit practice, just as the thought that ‘fieldwork’ included every possible interaction, practice or observation, left me with the uneasy sense that any moment spent alone was evidence of ‘shirking’ – nothing was out of bounds, and no time was off duty.

One might object that the feelings described above are in no way attributable solely to the multi-sited imaginary. Indeed they are not. I gather from discussions with more senior colleagues that these are fairly ubiquitous features of the fieldwork process – but they are problems which the multi-sited imaginary does not address, and in fact exacerbates. I hasten to add I am not naively suggesting that to ‘bound’ the fieldsite could provide us with a finite amount of collectable information. No geographical or theoretical bounding will eliminate the possibility of finding ever more complexity ‘within’ (Strathern 1991). But to be explicit about the necessity of leaving certain things ‘out of bounds’, would alleviate this predicament, by turning what feels like an illicit incompleteness into an actual methodological decision, one which the ethnographer reflects upon and takes responsibility for. […]

Such explicit consideration of self-restriction is at odds with the multi-sited research imaginary as I have sketched it above, and this raises a number of important issues: is it really the case that freedom is what we need to match the complexity of the world? Does not the suggestion that the traditional field-site is too limiting carry the seed of a totalizing claim to somehow represent ‘everything’? Is not there, lurking in the shadows of multi-sitedness, a strange hope that once we have burst out of our field-sites, we can conquer the seamless world?

What if, like the Dogma film makers, we decided that the best way to think about and participate in a complex world was precisely to define self-imposed limitations, to look for some methodological asceticism, to create arbitrary boundaries to what we allow ourselves to do? One aspect of this might involve framing field-sites for the delimitation of which we ourselves are responsible and accountable.

This is not a call to turn back the clock. On the contrary, the kind of bounded field-site I am proposing is premised on the realisation that any local context is always intrinsically multi-sited. Even in a small village in the North of Corsica, it is not multi-sitedness which is the problem, but sitedness. The problem, as I
hope to show in the next section, is not finding a diversity of leads to follow, but rather finding a way to contain this multiplicity.9

A Village Ethnography?

Given the high symbolic value of ‘the village’ in debates over anthropological methodology, it is with calculated irony that I present as my own case study of what I will call an ‘arbitrary location’, the village in which I did my fieldwork. The point of the exercise is to challenge two ideas: firstly, that even ‘in a village’, ethnography could ever be anything other than ‘multi-sited’, and secondly, that this dispenses us from bounding our own sites.

[Due to space constraints, and since the article is here reprinted principally for its theoretical argument, the bulk of this ethnographic section has been cut. Interested readers may find an expanded account of the village of ‘Crucetta’ as an arbitrary location in Candea (forthcoming, Chapter 1)]

How in this context, is one to conceptualize ‘the field-site’? The above account of the entity ‘Crucetta’ could already be described as ‘multi-sited’, both through the many heterogeneous spaces of ‘the village’ as a physical location, and through the many historical, institutional and conceptual spaces which an account of ‘a Corsican village’ deploys.

There is no lack of leads to follow here. Some spaces, like my own neighbourhood in the old village during high tourist season, could be thought of fondly as ‘human communities of face-to-face interaction’ (Gupta and Ferguson 1997, 15); others such as the villas taken together with the nearby town, might perhaps be imagined to form ‘socio-economic aggregates’; the networks and traces left across the island by Corsican language activists, or by the dwindling number of active shepherds, or indeed the steely web of French educational administration, could be prime candidates for a mobile analysis. In fact, of all these potential sites, ‘the village of Crucetta’ is perhaps one of the least obvious choices, since it seems to be held together by very little beyond an administrative boundary and a romanticized imaginary of an originally unitary state. But that, I will argue, is precisely its value as an arbitrary location.

By contrast, the multi-sited imaginary would prompt me to forgo Crucetta, and follow the traces which leave the village, to discover the contours of some

9 James Weiner, coming down a different theoretical path in the company of Martin Heidegger and the Foi of Papua New Guinea, has recently formulated the same concern: ‘What would a literary or anthropological “fire break” look like? It might be a mode of ethnographic inquiry, or a manner of ethnographic writing, designed to cut-off rather than extend or produce a flow of cultural and semantic associations’ (Weiner 2001, xi). Weiner’s problematic dovetails with an older Derridean concern with curbing the proliferation of interpretation (cf. Strathern 1996, 522).
wider translocal ‘cultural formation’. What this points to is the paradoxical reconfiguration of holism implicit in the multi-sited imaginary.

The New Holism

In their earliest description of ‘multi-locale ethnography’, Marcus and Fischer (1986, 91, my emphasis) already noted:

Pushed by the holism goal of ethnography beyond the conventional community setting of research, these ideal experiments would try to devise texts that combine ethnography and other analytic techniques to grasp whole systems, usually represented in impersonal terms, and the quality of lives caught up in them.

An exemplary text in this regard, is Adriana Petryna’s account of life after Chernobyl (Petryna 2002). Petryna connects sensitive ethnographic vignettes of intimate settings with informed accounts of transnational medical debates and sharply delineated life-histories, weaving together with consummate skill the multiple languages of a context ‘where individual accounts of suffering, if they are able to be heard at all, must transmogrify into numbers and codes fitting standard categories’ (ibid., 20). Petryna’s research design follows the pattern described by Marcus – from what was originally a localized study of Chernobyl ‘sufferers’, the author discovered that she had to leave the bounded site: ‘It became apparent that in order to do a fair analysis of the lived experience of Chernobyl, I had to do multisited work’ (ibid., 17). This (as Marcus’s quote makes plain) is an implicitly holistic project, in which the ‘whole’, in this case, the reality which accounts for ‘the lived experience of Chernobyl’, is taken to exist de facto beyond the contrived boundaries of any single geographic location. In itself, the desire to break out of bounded sites presupposes a totality ‘out there’ (perhaps what Marcus, above, refers to as a ‘cultural formation’) which the bounded site prevents us from investigating fully.

The new holism of multi-sitedness is also a rhetorical matter. Thus Marcus quotes Robert Thornton’s (1988, as cited in Marcus 1998, 35) claim that ‘[the] imagination of wholes is a rhetorical imperative for ethnography since it is the image of wholeness that gives ethnography a sense of fulfilling closure that other genres accomplish by different means.’ Elsewhere, the author asks (rhetorically) (Marcus 1999b, 5):

The question is whether anthropological ethnography can, or should be satisfied with “partial knowledge” thus ceding its context of holism, significance, and argument to given frameworks and narratives of theory in history and political economy that limit the scope of what ethnography can discover on its own, in terms of its own practices and the sensibilities that these encourage.
Of course Marcus’s new holism is of a very different kind, but it might seem to break with earlier calls for the fragmentary and the partial as the specific province of ‘post-modern ethnography’ (Tyler 1986; cf. Strathern 1991, 22). In fact, it brings to its logical conclusion a paradox present in ‘post-modern ethnography’ itself, one identified by Marilyn Strathern (1991, 110, my emphasis), when she notes:

The realization that wholeness is rhetoric itself is relentlessly exemplified in collage, or collections that do not collect but display the intractability of the disparate elements. Yet such techniques of showing that things do not add up paradoxically often include not less cutting but more – a kind of hypercutting of perceived events, moments, impressions. And if elements are presented as so many cut-outs, they are inevitably presented as parts coming from other whole cloths, larger pieces, somewhere.

If post-modern ethnography posited wholes by showing their fragments, multi-sited ethnography then, tries to follow and encompass these wholes themselves.

But could we not, pace Thornton, imagine an ethnography whose strength is not in fulfilling, but rather in perpetually deferring closure – whether it be the closure of holism, or that of a ‘fragmentary’ collage for a ‘fragmentary’ world?10

At any rate, the ‘multi-sited imagination’, as Marcus terms it, leaves us no position from which to imagine such an ethnography. By breaking down boundaries and removing limitations, it reconfigures partial knowledge as no more than an unsatisfactory or incomplete account, something which can be eliminated through good research design, through more unbound and fearless acts of following.

It is against this new holism, this reinstatement of completeness, that I am suggesting the value of the bounded site. To bound off ‘Crucetta’ as my field-site, to hold it together for the purpose of analysis, is precisely to highlight its fractures and incompleteness; it is to resist dissolving and resolving it into parts of wider holistic entities, be it of the old holism (‘Corsicans’, ‘Maghrebians’, ‘Corsica’, ‘France’, and so on), or the new (‘global capitalism’, ‘activist networks’, ‘transnational flows of illegal labour’, and so on). To hold on to Crucetta as an arbitrary location, one with no overarching ‘meaning’ or ‘consistency’, is to remember that all these heterogeneous people, things and processes are ‘thrown’ together, and to question, in the evidence of their uneasy overlap in one geographical space, the completeness of the ‘cultural formations’ to which one might be tempted to think they ‘belong’. Crucetta in this sense is not an object to be explained, but a contingent window into complexity.

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10 ‘A postmodern ethnography, says Tyler (1986, 131), is fragmentary because life in the field is fragmentary! Yet perhaps what is imagined as fragmentation may be no more derived from a world of fragments than what is imagined as integration comes from a world already a totality’ (Strathern 1991, 22).
Arbitrary Locations

I am saying nothing new. The kind of ‘traditional anthropological field-site’ of which proponents of multi-sitedness are suspicious, could be described as a double entity: it was on the one hand understood as ‘a found object’: a ‘really existing feature of the world out there’, a discrete spatial or human entity which was supposed to have its own consistency and meaning – the village, the neighbourhood, the tribe, the kind of entity which could become the subject of an exhaustive and comprehensive monograph. On the other hand, it was also to some extent an arbitrary location defined by the researcher as a framework for a study of something else. Thus Evans-Pritchard remade political theory in Nuerland, and Malinowski challenged Freud in the Trobriands.¹¹

Let me briefly tease out what I mean by an ‘arbitrary location’. As a heuristic device, the arbitrary location is perhaps best understood as the symmetrical inversion of the ‘ideal type’. Weber’s ideal type was an abstracted notion, nowhere existing and for that very reason easily definable, a notion which served as a ‘control’ for comparative analysis of actually existing instances (Gerth and Mills 1948, 59ff). The arbitrary location, by contrast, is the actually existing instance, whose messiness, contingency, and lack of an overarching coherence or meaning, serves as a ‘control’ for a broader abstract object of study. It is ‘arbitrary’ insofar as it bears no necessary relation to the wider object of study (‘Nuerland’ to ‘politics’, the Trobriand islands to the Oedipus complex). While the ideal type allows one to connect and compare separate instances, the arbitrary location allows one to reflect on and rethink conceptual entities, to challenge their coherence and their totalizing aspirations. If the ideal type is meaning which cuts through space, the arbitrary location is space which cuts through meaning. […]

The demise, with multi-sitedness, of the first aspect of fieldwork (the field-site as a naturally bounded entity), is to be celebrated unreservedly, and I am far from urging a return to former conceptualizations of fieldwork. On the contrary, my plea is for more consistency in their critique. For as we have seen, far from challenging the totality of the object of study, with multi-sitedness we have eschewed the contrived totality of a geographically bounded space, for the ineffable totality of a protean, multi-sited ‘cultural formation’.

This is what makes the loss of the second aspect of fieldwork (the field-site as arbitrary location) so problematic. The decision to bound off a site for the study of ‘something else’, with all the blind spots and limitations which this implies, is a productive form of methodological asceticism. To limit ourselves to arbitrary locations, geographic or otherwise (I will return to this point in the conclusion) gives us something to strive against, a locus whose incompleteness

¹¹ And neither of them, writing in a world in which ‘ethnic identity’ had not yet entered the scene, gave much thought to the definitional status of these entities (Maryon McDonald, pers. comm.).
and contingency provides a counterpoint from which to challenge the imagined totality of ‘cultural formations’.

**Conclusion: A New Experimental Moment?**

In the above, I have chosen to illustrate the use of self-imposed limitations through a spatial example, revisiting the hackneyed image of the ‘village ethnography’. But the wider point about the necessity, both epistemological and practical, of recognizing the value of limitation amidst the calls for freedom, could find many other, including non-spatial, expressions.

In 1968, the French writer Georges Perec wrote what he termed a ‘lipogrammatic novel in E’, meaning a novel written without the letter E (Perec 1968). What would a ‘lipogrammatic’ ethnography (as a written work) look like? What would ‘lipogrammatic’ ethnography (as field practice) look like? In other words, what would it mean to knowingly and arbitrarily exclude certain elements, moments, people, factors, words, concepts, from our analysis? If that seems a flippant or ‘unscientific’ suggestion, it may be worth reflecting on the extent to which we already do this, in an unacknowledged or broadly unproblematised fashion, every step of the way, from the ethnographic encounter itself to the production of the ethnographic text. It may serve to think on the number of ethnographies, or chapters, or paragraphs, or sentences, from which ideas and topics are (of necessity) excluded, while the promise or threat of some form of holism (old or new) looms in the background.

I have mentioned film and literature, but another source from which anthropologists might tentatively draw figures of productive self-limitation is archaeology. One might evoke, for instance, the figure of developer-funded archaeology, in which a physical site for archaeological research is delimited by concerns which are totally arbitrary from a research point of view (the future layout of a motorway or parking lot for instance). The site in developer-funded archaeology is perhaps the most obvious metaphor for what I have called an arbitrary location: devoid of its own intrinsic meaning from an archaeological point of view (although of course not from the developer’s) such a site can only ever be a window into complexity, and never a holistic entity to be explained.

None of the parallels or rapprochements suggested in this chapter are to be taken mechanically or literally. I am not here advocating the kind of direct borrowing of method suggested for instance by Phillip Salzman in his call for ‘team research’

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12 In fact, archaeology seems a particularly promising candidate for three reasons: the historical proximity and communication between the archaeological and anthropological projects; the complex relationship between the two disciplines’ concepts of the field-site, which has shifted continually between the poles of homology and mere homonymy; and finally, their divergent historical engagement with methodological issues of freedom and limitation.
in anthropology (Salzman 1986). It merely seems that sidelong glances at other modes of knowledge production might help us experiment with our fieldwork and writing practices, in order to recapture the value of not knowing certain things.

A Partial Postscript on Arbitrariness – Cambridge, 25 August 2008

Perhaps the best way to ‘refresh’ (as one might a webpage) the argument of the 2007 article reprinted here in an abridged form, is to refer to philosopher Alfred North Whitehead’s distinction between what he terms a nexus and a society. A nexus, writes Shaviro (n.d., 2), quoting Whitehead, is ‘a particular fact of togetherness among actual entities’ ([Whitehead 1920/2004:], 20); that is to say, it is a mathematical set of occasions, contiguous in space and time, or otherwise adhering to one another. When the elements of a nexus are united, not just by contiguity, but also by a ‘defining characteristic’ that is common to all of them […], then Whitehead calls it a society (34). A society is ‘self-sustaining; in other words… it is its own reason… the real actual things that endure,’ and that we encounter in everyday experience, ‘are all societies’ ([Whitehead] 1933/1967:203-204).

This distinction between nexus and society is a very good entry point into the question of holism (in its simplest – Aristotelian – form, the claim that an entity is more than the sum of its parts). If a nexus describes a set of contiguous parts, the society which corresponds to it is the whole. Whitehead’s distinction gives us a good way of talking about the evident fact that not every nexus doubles up as a society, that is, not every collection of parts is a whole.

So what does this have to do with multi-sited ethnography? My argument is that the multi-sited imaginary productively challenged the assumption that the local nexus mapped out by the ethnographer was also a society. It challenged in other words a simple holism (let us call it ‘holism 1’) according to which say, ‘the Nuer’ or ‘the Bhopal disaster’ could be examined ethnographically as a whole in isolation from the broader multi-local processes in which its various elements were enmeshed. What initial formulations of multi-sited ethnography did not set out to challenge however was the notion that ethnography should set itself as a task (or at least as a horizon) to map and investigate wholes. Take for instance Smith (2006, 13–5, emphasis added):

Although I was based in one town in southeastern France, Aix-en-Provence, this is not an ethnography of [Maltese] settler life in that town, but rather an ethnography of an entire diaspora. This is thus an ethnography of social practices engaged in rented halls, during day- and week-long trips, and in other temporary spaces, in the interstices of the settled villages and city neighbourhoods that have been traditional loci of study […M]ost of the settlers I spent time with [in Aix] were clear outsiders and isolated even from their closest neighbours. They were connected to other repatriates, but not
An arbitrary field-site allows one the vantage point from which to observe and engage the processes whereby wholes are made and unmade. This arbitrariness allows the site to remain a nexus, a space that cuts through meaning. In this second sense, of course, the method itself is far from arbitrary in relation to the anthropologist’s theoretical or analytical concerns. It

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13 And (pace Gatt, this volume), ‘societies’ here refers to hybrid entities or matters of concern, not to social relations as divorced from the environment (cf. Candea 2008).

14 After all, developing a holistic account may be precisely what is aimed at (for example Horst, this volume).

15 As Cook, Laidlaw and Mair once again very clearly point out. In my own case one of the driving concerns behind this entire discussion was an interest in the holistic depiction to Aixois, and I don’t think that I was ever given the telephone number or address of a nonsettler, a ‘true’ French man or woman, to contact.

Having dissociated the nexus from the society, early proponents of multi-sited ethnography prompted us to follow the society (entities which were more than the sum of their parts, such as ‘cultural formations’, ‘the world system’, ‘diasporas’ or ‘events’ – let us call this ‘holism 2’), while leaving behind the irrelevant bits of mere geographic contiguity. The focus is on the thickly interconnected entity that is Maltese settler identity, say, rather than on the minutiae of daily cohabitation in a French town where people do not know each other.

To think of one’s field-site as an arbitrary location, by contrast, is to think of it as a ‘nexus’ which bears no necessary relation to – and thus gives one a vantage point on – the ‘society’ or ‘societies’ under investigation. This is not in and of itself a better way to go about research, any more than multi-sitedness in and of itself is either sufficient or necessary to provide power and relevance to anthropological ethnography. The arbitrary location is just an alternative, which may be more or less productive than others, depending on the subject and aims of the research. It highlights, by contrast, the implicit holism of the multi-sited imaginary. However, whether one chooses to work in arbitrary locations or to map out whole entities, the broader point is that it pays to be explicit about the way one makes such choices.

Re-reading the article in the light of the other chapters in this volume, I find myself compelled to score a few argumentative own-goals. Firstly, the article failed to distinguish between methodological caution and ontological postulation. As Cook, Laidlaw and Mair (this volume) rightly point out, this is analogous to a confusion between keeping the description ‘flat’ and claiming the world actually is flat. The point is not that there are no wholes, but that arbitrary locations give one a vantage point from which to observe (and indeed engage) the processes whereby wholes are made and unmade – just like ‘flat’ description allows one to see the way actors themselves actually deploy scale.

Secondly, two different flavours of arbitrariness get entangled in the article. On the one hand there is the all-out playful, ‘what-the-heck’ arbitrariness of, say, a book written without the letter E. On the other, there is the more delimited and technical use of the term, in which the field-site is arbitrary in relation to the object of study. This arbitrariness allows the site to remain a nexus, a space that cuts through meaning. In this second sense, of course, the method itself is far from arbitrary in relation to the anthropologist’s theoretical or analytical concerns.
is particularly important to distinguish these two kinds of arbitrariness, given the concern of some readers that to describe anthropological fieldsites as arbitrary would suggest (particularly to a non-anthropological audience), that ‘anything goes’ and that there are no ‘better’ or ‘worse’ anthropological descriptions or analyses.\textsuperscript{16} On the contrary, arbitrariness in this technical sense is anything but facile whimsy: it is a purposive method which involves conscious and motivated processes of selection, an instance of what George Marcus (this volume) calls ‘a strong norm and accountability for intended, structured partiality and incompleteness in ethnographic research designs’ (p. 199).

Thirdly, this approach is also likely to be perceived as arbitrary from the purview of at least some of the people the anthropologist is working with. This raises in sharpened form the question of engagement, which the article does not address. This being said, arbitrariness in this sense does not in any way preclude collaboration, unless this is imagined restrictively as a direct and total alignment of the anthropologist’s interests upon the interests of the people they work with – it certainly does not preclude collaboration in the far more exciting senses explored by some of the contributors to this volume (Marcus; Matsutake Worlds).

Fourthly, there is a great time-shaped hole in the paper, which stops short of considering the temporal and iterative process whereby boundaries are drawn and redrawn, arbitrariness shifted, sharpened or blurred, in relation to happenings in the field, and in interaction with others (see for instance Gallo, this volume). Building this back in would form part of the answer to the previous concern, as well.

Yet the most important corrective is a matter of style. Too often, the playful/polemical tone of the piece left readers with the impression, despite explicit claims to the contrary, that this is some kind of all-out onslaught against multi-sitedness, its proponents or practitioners. It is not. In fact, this article is situated within as much as it is about the wave of methodological reflexivity generated by proponents of multi-sited ethnography. Multi-sitedness is a positive development for the discipline, although it is not a sine qua non of good ethnography. But there was certainly no intended suggestion that doing fieldwork in one place is in and of itself better than doing it in many (cf. Weißköppel, this volume): the number of sites has nothing to do with their arbitrariness (any more than it has to do, incidentally, with their ‘complexity’).

A final thought: in a penetrating and thought-provoking comment on the paper, Akhil Gupta (pers. comm.) suggested that the notion of ‘partiality’ (following Haraway) would be a preferable alternative to that of ‘arbitrariness’. This is in many ways appealing, not only because it would put my usage in line with that of categories of identity (French, Corsican, and so on). Crucetta is arbitrary in relation to Corsicanness (it does not encompass it, represent it, or bear any necessary relation to it), but not in relation to my argument about or interest in Corsicanness (see Candea forthcoming).

\textsuperscript{16} For instance Akhil Gupta (pers. comm.), and perhaps, by implication, Nadai and Maeder (this volume).
of many contributors to this volume, but also because the argument patently owes a large debt to Marilyn Strathern’s *Partial Connections* (2004). And yet, while both partial and arbitrary suggest a deferral of completeness, the relation between parts and wholes implied is not quite the same in each case. A partial account postulates and hankers after or runs from its (impossible?) whole; an arbitrary location just cuts holes through wholes: it is little more than a ‘clamp’ which keeps accounts flat, allowing ‘the actors to speak for themselves’ (see Latour 2005 and Krauss, this volume). Furthermore, the appeal of partial is at least in part due to its polysemy (as in incomplete, but also partisan, interested, not impartial), which has been very effectively used since the 1980s to wed a critique of holism to a critique of objectivity. Salutary as this demise of the ‘God’s eye view’ has been, arbitrary belongs to a different project: that of imagining new bases for a ‘post-positivist empiricism’.  

17 I borrow the term from Born (n.d.).

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A group of Brahmins is engaged in quarrelsome dispute about the nature of reality. The Buddha tells them a story – the parable of the blind men and the elephant – as follows (Udana 6.4; see Masefield 1994, 128ff). A king orders all the men in his kingdom who have been blind from birth to be brought together and led before him, each having been partially introduced to an elephant, by each being given just one part of the elephant’s body to handle. The king then asks each of these people what kind of thing is an elephant. Those who had felt its head replied that an elephant is like a pot. Those who had held its ear said it resembled a winnowing basket. Those who had held only the trunk likened it to a plough, and so on. Then, just like the Brahmins, the blind men began to quarrel. The parable is used in the Buddhist text to warn against trying to reach conclusions about the nature of reality on the basis only of the partial view of the unenlightened.

The original idea behind multi-sited research was that the partial perspective afforded by a single research site was insufficient for understanding local phenomena such as trade and ethnic identity, because these things are part of systems that operate on a much larger – specifically, on a global – scale. The contention was that a full understanding of those larger systems, and therefore of the conditions of possibility of any single local site, required one to combine the views gained from a sufficient number of different perspectives. This prescription rests on certain suppositions that are similar to those underpinning the parable of the elephant: that there is a hidden truth, available only to those who achieve a holistic, global view by transcending the particular; that this truth will explain and tie together all the partial perspectives of those who only know one point of view; and that what is seen from those different vantage points, though apparently diverse, is all really part of the same coherent, integrated phenomenon.

In his influential 1995 paper, George Marcus listed a number of appropriate topics for multi-sited field research, including the media, science, and the global political economy. Religion was absent from his list, perhaps because whereas the fields that were listed were aspects of the supposedly novel situation of...
‘late capitalism’, the globalization of religion was nothing new. In any case, its absence from the multi-sited research agenda was conspicuous in view of the attention anthropologists of so-called ‘world religions’ had already devoted to conceptualizing the relation between on the one hand the various beliefs and practices of Muslims, Buddhists, Christians, and so on, which they observed in the different local settings they had severally studied, and on the other the trans-local cultural systems (Islam, Buddhism, Christianity, and so on) of which they conceived these to be part. They had proposed various kinds of relationship in order to address this problem, from that between great and little traditions through hierarchical encompassment, syncretism, dialogue, and so on.

In other words, when Marcus’s paper was published, anthropologists of religion had long been wrestling with some of the same conceptual problems that multi-sited research was designed to address, specifically, the relationship between the global and the local and the consequences of this relationship for ethnographic research. In addressing these problems multi-sited research replicated the implicit holistic assumption that anthropologists of religion were beginning to throw off, namely, that accounting for local ethnographic phenomena must involve locating them within an encompassing trans-local ‘system’ located theoretically at a ‘higher’ level.

But what if there is no elephant? There are powerful reasons, as we shall outline below, not to take for granted the existence of any such higher level, and to reject the idea that accounting for or explaining specific ‘local’ practices should consist in locating them within a wider system. These arguments are intended as a challenge not so much to the practice of multi-sited research – which in some form or another is as old as anthropology itself – as to the way the advantages and objectives of multi-sited research have been conceived and canvassed in the last few decades. If our arguments are accepted, then the now conventional rationale for multi-sited research – that it makes possible the piecing together of the ‘wider systems’ that account for local phenomena – also falls away. If this is so then what, if anything, does multi-sited research offer? If, in particular, we abandon some of the assumptions that have underlain much avowedly multi-sited research about the relationship of ethnographic research to space, how do we establish new, less problematic relations? This chapter offers one possible answer to these questions. We propose that by conceptualizing the ethnographic field in a way that detaches it from the concepts of space and place, and thus making available the concept of an un-sited field, we can rescue the possibilities of comparison across theoretically relevant boundaries in space, the possibilities that multi-sited methods have so productively opened up.

An Obviously Multi-Sited Project?

We have developed these arguments in the course of planning an ongoing collaborative project into the new forms of Buddhism that are being adopted across
Asia. Our project is conceived as an exercise in the anthropology of ethics. We are interested in practices of self-cultivation and self-transformation – what Foucault referred to as techniques or technologies of the self (1988; 2000; 2005). These are practices through which people seek to transform themselves and which form part of historically located, more or less institutionalized ethical projects. Techniques described and analysed by Foucault include various forms of confession and self-examination, as well as dietary and other bodily regimes. Through such practices, individuals make themselves objects of their own considered reflection. This requires a developed idea of exactly what the self that must be transformed is: body, soul, mind, thoughts, conduct, character, some combination of these, or something else. It also requires a developed idea of the state of perfection towards which the self-cultivation is working (Foucault 2000, 177). These techniques or practices of self-cultivation are thus means available in particular historical circumstances and particular intellectual and cultural traditions, by which, ‘the subject constitutes itself in an active fashion’ (2000, 291).

Foucault’s writings, of course, are concerned almost exclusively with eras and locations which he explicitly regarded as part of the genealogy of the modern Western subject, even if latterly, in his studies of the Hellenistic period (1987; 2005), this included roads not taken and possibilities that have disappeared from view in the course of that history. But practices of self-cultivation are also prominent in other traditions; in none more so than in Buddhism. The ethics of self-cultivation – especially practices of study, meditation, and ascetic discipline – have been central to all main schools of Buddhism. The last century has seen the invention and development of new forms of meditation and mindfulness, their adoption by mass followings of women and lay men as well as male monastics, and their incorporation into avowedly ‘modern’ forms of life as techniques of psychological therapy and even ‘life-style’. There has also been an extensive traffic in such techniques and their adaptation across sectarian and national boundaries, and this has been accompanied by innovations in forms of religious organization and authority as well as new forms of organized Buddhist participation in public civil and social life. New Buddhist organizations have appeared, built on teaching and propagating their own distinctive practices and regimes of self-cultivation. There are some excellent studies of several of these organizations (Gombrich and Obeyesekere 1988; Queen and King 1996; Chandler 2004; Learman 2005; Levine and Gellner 2005) and the fact that they influence and interact with each other has been noted. However, neither the internationalization itself, nor the fact that this takes place crucially through traffic in sometimes-innovative techniques of self-cultivation, has been the subject of focused research.

Our project will involve fieldwork on Buddhist ethics of self-cultivation in at least three different Asian states, in each of which at least one of us has conducted fieldwork previously. Our interest is in how techniques of self-cultivation, as we find them embodied, taught, and promoted, relate to specific conceptions of the person and so to philosophical and psychological thought, and also to forms of associational and civic life. Our intention is to develop an ethnographic
description of the teaching and practice of Buddhist self-cultivation. What kinds of pedagogical relationships are involved? How are the elements of the self that compose them, and the alterations in states of mind or character at which they aim represented, both discursively and cognitively? How are success and failure identified, understood, and explained? What, if there are any, are the concepts that might roughly correspond to concepts of responsibility, agency, autonomy, duty, freedom, and enlightenment? How, that is to say, are the substance on which ethical work is done, and the telos towards which subjects direct their efforts, conceived? How do participants in self-cultivation evaluate themselves and others? How do they seek to identify, register, and perform the desired effects of self-cultivation?

A major concern of the research will be to explore the ways in which sectarian and national boundaries are established, observed, evaded, and crossed. We know already of many examples of Buddhists in one country consciously adopting and adapting, or seeking to influence and change techniques of self-cultivation practised by people in other places (see examples in Learman 2005); and in both kinds of cases pursuing these ambitions is seen as part of what it is to make oneself a good Buddhist. So, direct concern with what others do as Buddhists ‘elsewhere’ appears from the outset to be an intrinsic part of the ethnographic reality.

A topic then for which multi-sited research seems ideally suited? It is indeed for this reason that we first conceived of our project as a collaborative one, and first thought to work in several locations. But from the outset too we have doubted that it would make sense to regard our proposed fieldwork locations – or indeed any set of locations we might propose – as parts which we could aim to piece together in order to arrive at an adequate description of some greater whole, the understanding of which would in turn contextualize and explain the several instances we observe. There is no serious room to doubt that what is happening in these different sites profoundly and intricately influences and affects the others, and that some similar processes are at work in different sites. But this does not mean there is one whole of which they are all parts. While something like a multi-sited approach seems called for, we have had to rethink the rationale and the proposed objectives of multi-sited research.

World Religions, Global Systems, and Multi-Sited Research

Anthropologists and other students of religion often conceive of so-called world religions as existing trans-locally, on national, continental, or global scales, and as persisting through time. They notice that religious adherents themselves imagine spiritual or confessional kinship or community with unknown millions of others, past, present, and future. They notice that these communities are often imagined in terms of large and systematically conceived entities – a body for example – and they often more or less directly assimilate such imagery to their own conception of trans-local structure or system. Questions asked by religious authorities and reformers of whether this or that community or individual, this or that institution,
belief, or practice, is an authentic or legitimate part of the religion likewise closely resemble analysts’ questions of whether or in what sense particular beliefs of practices are ‘really’ Buddhist, Christian, Muslim, or whatever.

Such imaginings of trans-local systems – both adherents’ and analysts’ – have been enabled by the movement of ideas, practices, people, and objects: the transmission of texts along trade routes and the learning and use of them in pedagogic practice and in rites, the activities of missionaries (see Hovland this volume), the travels of pilgrims, the search for and trade in relics, and so on. Such movements have of course characterised Buddhism for a very long time. As we have already remarked, these are all exactly the kinds of movements that Marcus recommended multi-sited researchers should follow, in their studies of globalizing scientific research, labour markets, media empires, and so on, and it is therefore at first sight surprising that Marcus in his 1995 programmatic article did not consider the merits of this body of literature in proposing his method for the ethnographical investigation of globalization.

But if we now pursue the parallel between recent anthropology of globalization and more long-standing attempts to conceptualize ‘world religions’ anthropologically, some interesting points emerge, because in recent years the assumption that guided much of this latter literature – that what Buddhists, for instance, in any particular local context believe and do could be made sense of as Buddhist only insofar as it could be seen as being derived from a pre-existing and underlying system – has been subject to radical challenge, in the form of historical contextualization.2

As scholars of religion and in particular historians of religious studies have emphasized, the idea of plural religions in general, and ‘world religions’ in particular, is a relatively recent historical product, as is the consciousness among so many of the adherents of the various ‘world religions’ of their own religion as one of a range of formally equivalent, and therefore rival, universal systems. Masuzawa (2005, 200), who persuasively documents these developments, puts the matter as follows.

‘World religions’ as a category and as a conceptual framework initially developed in the European academy, which quickly became an effective means of differentiating, variegating, consolidating, and totalizing a large portion of the social, cultural, and political practices observable among the inhabitants of regions elsewhere in the world.

This is not to say that there were not ideas and images of trans-local religious community in earlier times, nor that the world-religions discourse is present even  

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2 Important contributions to this contextualization, from several disciplines, on which we draw freely here, have included Ruel (1982), Southwold (1983), Asad (1993), Harrison (1990), James (1995), Lash (1996), Saler (2000), Asad (2003), Masuzawa (2005), and important works in the recent anthropology of Christianity, notably Engelke and Tomlinson (2006), Cannell (2007), and Keane (2007).
now in all religious activity, nor that its premises are the same in all cases. But just as trans-local political communities have been imagined in modern times in distinctive ways, using distinctive media, and this has given us the distinctively modern nation-state (Anderson 1983), so the imaginary of a finite number of world religions, each a formally equivalent cultural system, is a product of a specific history. In the centuries of European and therefore Christian world hegemony, a particular model of ‘world religions’ has spread, under whose influence adherents of previously loosely affiliated local cults have sought or have been encouraged or compelled to conform to the prescriptions of some ‘great tradition’. At the same time, ‘great traditions’ themselves have been modified to fit the requirements of the template provided by the world religion model. Diverse religious traditions have seen selection and codification of canonical texts, attempts at doctrinal homogenization and reform, new forms of organization and lay participation and empowerment, and so on.3

Buddhism was the first extra-European ‘religion’ to be conceived and consolidated on this model, derived from post-Reformation Christianity. It was in the nineteenth century that Buddhism came to be recognized as a distinct religion by educated Europeans. Various strands of religious practice in diverse regions of South, Southeast, East, and Central Asia, were recognized as being part of the same tradition. In the scholastic and monastic traditions, lineage was of paramount importance, and such diverse and scattered institutions had not previously been thought of as constituting one religion in this sense, by either European observers or native practitioners. ‘In early modern times, then, there was no “Buddhism” to consolidate disparate observations gathered in and about Asia’ (Masuzawa 2005, 122). The identification of Buddhism as a world religion began with a recognition of the links and genealogical relations among assorted and seemingly separate occurrences of cultural practice in a range of countries. European scholarship embarked on a massive collective exercise which it conceived as the reconstruction of a shattered whole.

With the proper critical skills, those highly trained, monumentally devoted scholars would be in the best position, if not to say an exclusive position, to grasp Buddhism’s essential character ... One might say that Buddhism as such came to life, perhaps for the very first time, in a European philological workshop (Masuzawa 2005, 126; see also Almond 1988 and Lopez 1995).

The specifically Indian origin of Buddhism was understood as having been transcended through Buddha’s rejection of Brahmin priesthood and Vedic authority. This break with the prevailing society and order meant that Buddhism was thought to have risen above any national belonging that it might have had at its inception.

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3 A classic and exemplary study for Theravada Buddhism is Gombrich and Obeyesekere (1988).
Its doctrines, claims, and prescriptions had a potentially universal validity. Thus, Buddhism was rediscovered as a non-national world religion.

While this conception of world religions, and of Buddhism as a world religion, may have begun as an exercise in intellectual history and European scholarship, it soon became much more than this. From the early twentieth century especially it became a matter of social and institutional history (Masuzawa 2005, 308). The discourse was appropriated and reacted to by those who now identified themselves as Buddhists in a new way, now understanding this to mean that they were followers of one world religion among several. Reform and missionary movements arose to purify popular practice in the light of what was now understood to be its underlying essence, and to propagate and defend it against what were now understood as other, formally equivalent, rival systems.

For such self-images to have become influential in this way is historically consequential. People acting as if this were somehow essentially and necessarily how things are do try to impose uniformity, interpret differences as variations on a theme, prohibit divergence or innovation, or create consensus, and their attempts have variously significant effects. But this does not mean that the self-image is any more than partially self-fulfilling. People believing that behind the multifarious things Buddhists, Christians, or Muslims think and do there is in all cases and necessarily an original and authentic coherent system, does not in itself make this so. It is importantly possible that no such single system exists, and that there is nothing which in this sense makes it true that particular beliefs or practices are or are not ‘really’ Buddhist, Christian, Islamic, or whatever.4

Incontrovertibly, people who regard themselves as Buddhists in many parts of the world share the same or similar ideas, representations, and practices. The kinds of intensification and acceleration of cultural flow and exchange that played such a part in motivating the formulation of multi-sited research on ideas of globalization are also conspicuously evident here (although any attempt at quantification of that intensification would require conceptual clarification to which this chapter can only make an initial contribution). For example, distinctive Chinese organizational forms of Buddhism, originating in Taiwan, offer examples of cross-cultural movements that make use of and are shaped by globalization. The transcendence of traditional boundaries, such as those of nation states and sectarian distinctions, has been occurring within Asia and extending beyond it. Learman lists some obvious links:


4 For an important discussion of the distortions introduced into the anthropological study of Buddhism by concerns and questions of this form, see Gellner (1990).
In addition, organizations have appeared in many Asian countries promoting social welfare, egalitarianism, and/or environmentalism, all clearly influenced by each other as well as Western exemplars, and all invoking an interpretation of the true, underlying, essential teachings of Buddhism as simultaneously scientific and politically engaged (see for example Queen and King 1996), an interpretation of the distinctiveness of Buddhism among world religions that derives directly from the mission encounter and the modernist reformers who were the first self-declared Buddhists to articulate the idea of Buddhism as a world religion.

What this history means for us, as researchers of Buddhist ethics, is that we must be careful not to attribute explanatory power to an authentic, homogenous Buddhism. Insofar as Buddhism is or is becoming systematic, its always-incomplete systematicity is an effect whose production must be explained, not a cause that explains the diverse beliefs and practices in different places or such similarities as there are between them. The widespread assumption by adherents of self-consciously world religions that there must be a coherent whole of which they are part is itself a religious commitment, and one that is framed in distinctively modern terms.

A pair of assumptions strictly parallel to those in world-religions discourse – namely, that local phenomena are part of global systems, and that the explanation of local features lies in the existence of that global system – are evident in the classic formulations of multi-sited research. As Candea (2007) has argued, much multi-sited research hides an implicit holism; it suggests that by studying something in many places one can somehow encompass a totality that would escape us if the study were limited to a single place, and that it is this systematicity, located at a global scale, that explains the particularities of local phenomena. The classic holist commitments – the notion that the whole is more than the sum of its parts, and that it precedes them and explains them – are evident in much of this literature, in suggestions that the existence of a big ‘system’ out there (for example the world-system, a diaspora, or a trans-national network), beyond the site, renders the perspective afforded in a given site only partial (and therefore ‘not good enough’). As Marcus and Fischer write:

*Pushed by the holism goal of ethnography* beyond the conventional community setting of research, these ideal experiments would try to devise texts that combine ethnography and other analytic techniques to grasp whole systems, usually represented in impersonal terms, and the quality of lives caught up in them (Marcus and Fischer 1986, 91, emphasis added).

In Marcus’s 1995 paper, for example, it is taken for granted that the world’s political economy is structured as a single ‘global system’. The methodological challenge for anthropology is conceived to be that of providing a more detailed and nuanced description of ‘it’. And it is further assumed that the general outlines of that system, as a whole and as seen, so to speak, theoretically ‘from above’, are already available to us in the form of the neo-Marxist conception of ‘late
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capitalism’. The anthropologist’s task is to fill in the gaps by describing local instantiations of the various parts of the system, and in that way to describe the system ‘from the bottom up’.

It is presupposed that the research will reveal ‘the system’ and that discovering this larger, hidden, ‘social’ reality is the purpose for the research. Thus, while fieldwork sites emerge in the course of research, as they are discovered (at least to some extent) in the following by the researcher of multiple flows of persons, objects, and so on between locations, from another perspective they are conceived as being always-already determined by the nature of the larger system, even if, these being recent and ‘revolutionary’ global forces, they are also represented as always having-just-arrived. Thus particular sites, and the connections between them, are explained just to the extent that their place in the larger system is discovered. In his contribution to the present volume, Marcus remarks that multi-sited research, conceived in this way, depends upon the idea of

a map that is already understood and relied on by being expressed in some scholarly or academic literature (for example, economic or sociological models of migration, Marxist conceptions of the flow of global capital, or the proliferation of neo-liberal markets) (this volume, 187).

In 1995 Marcus cited with partial approval the Copper Belt studies carried out by anthropologists of the Manchester School in the 1960s, but criticized them for lacking a conception of an overarching and encompassing system in which their various specific empirical studies might be contextualized. Such contextualization would provide a dimension of explanation that Marcus found lacking in those studies: the particular circumstances in individual locations would be explained by identifying their place in the system.

This idea of social explanation as identifying unseen forces exerted by a distinctive kind of unseen entity – a social structure, social order, social relations, the social realm, and so on – is of course a widespread one. Bruno Latour (2005), Marilyn Strathern (1992; 2004), and others (for example Barth 1992; 1993) have pointed to the way the social sciences have fallen more or less routinely into the postulation of such special kinds of entities or processes, as if for phenomena to be ‘social’ was for them to be composed of or caused by a special kind of entity, material, or substance – as if ‘social’ were equivalent to words such as ‘wooden’, ‘mental’, or ‘linguistic’. Since there are no such special ‘social’ things or materials – because for phenomena to be ‘social’ is instead just for them to be intrinsically interactive, to result from processes of assemblage or arrangement of entities, of whatever kind – the postulation of special entities such as ‘social structures’ and

5 We note in passing the distinctly millenial quasi-religious character of this concept. For those who first coined the expression, the capitalism in question was ‘late’ because its end was seriously expected to be nigh. The continued use of the term now must either be essentially frivolous or testimony to a very interestingly persistent faith.
so on is at the very best a short cut for the more laborious process of tracing and describing those processes of assembly and disassembly, arrangement and rearrangement. At worst, it is to mistake *explanandum* for explanation. To say that a phenomenon is social is really to pose a set of questions – what kinds of processes of assemblage, interrelation, and so on, are involved? – but in these conventional formulations it appears as if to say something is social is to invoke a force or entity that can explain something else.

So we regard as persuasive Latour’s insistence that when social scientists invoke the ‘social’ in this sense, and postulate ‘social’ entities or processes, the impression that what they are doing is explanatory is essentially illusory. Latour argues that the conjuring up of such entities, alleged to exist on a higher plane or sphere and therefore on an intrinsically greater scale, appears to be explanatory because it appears to lend the picture the social scientist paints an impression of ‘depth’. He argues for the virtues, instead, of eschewing this optical illusion and instead making our observations ‘flat’.

It’s as if the maps handed down to us by the tradition had been crumpled into a useless bundle and we have to retrieve them from the waste basket. Through a series of careful restorations, we have to flatten them out on the table with the back of our hand until they become legible and usable again. Although this ironing out may seem counterintuitive, it is the only way to measure the real distance every social connection has to overcome to generate some sort of tracing (Latour 2005, 172).

Only thus, Latour maintains, can we observe the ways in which practices of switching scale to achieve depth and invoking entities located on imagined scales are among the activities of the people we study; if we are to understand, that is, how the effect of there being different dimensions is generated and maintained. Imagining ‘Buddhism’ on a trans-local and trans-historical scale is something Buddhists themselves do – in different media and with different contents and connotations – and if we are to describe and understand how they do this, we cannot take the short cut of appearing to explain their practices by ourselves employing the very same device.

So on this view, if the Manchester School’s Copper Belt studies lacked a concept of overarching structure, and therefore lacked an overarching ‘social explanation’, this makes them ‘flat’ descriptions in Latour’s terms. This we regard as a virtue, and one to which we aspire.

**Arbitrary Locations and Un-sited Fields**

So what were expected to be the most significant profits from multi-sited research – global structures that will explain local phenomena – turn out to be false coin. And there are no intrinsic reasons why a wide-ranging, multi-sited study should be richer in information than a study that is limited to a traditional, small site. We
do not change the quantity or detail of the data we encounter merely by changing scale; we simply encounter different details (Strathern 1995; 2004). If these hoped-for benefits are not, as has been thought, guaranteed by the use of multi-sited research, then the fact that pursuing research questions over great distances and across national and linguistic boundaries incurs significant extrinsic costs might prompt anthropologists to conclude that there is no good reason to do multi-sited research at all. They might retreat to the traditional bounded field site, retaining from the recent multi-sited adventure only the knowledge that the bounds of the site are, as Candea argues, arbitrary. But this was not Candea’s intention in his defence of the bounded field site (2007, 180), and nor is it our intention here. Candea convincingly shows that acknowledging and embracing an arbitrarily delimited fieldsite allows the fieldworker to preserve an important insight from the multi-sited programme – that the world is seamlessly interconnected – without falling into the trap of re-naturalizing a different set of ‘larger’ but equally bounded ‘cultural forms’, and trying to discover their ‘contours’ by following them around ‘on the ground’.

Instead, argues Candea, anthropologists need to choose the boundaries of their field sites, and, recognizing this, to take responsibility for the analytical (and political/ethical) decisions this involves (2007, 172). In 2007, Candea contrasted this recognition of the necessity of bounding as anthropological practice with the desire he finds expressed in much multi-sited research to find ready-made entities existing out there and waiting to be discovered. He has since acknowledged (forthcoming, and this volume) that this formulation fails to distinguish clearly enough between the methodological principle (‘assume no wholes’) and an a priori account of the world itself (‘there are no wholes’). We might also express this as a distinction between aiming at ‘flat description’, so that the processes through which contours are created and maintained might be captured analytically, and assuming the world actually is flat, or that all networks are equally and homogeneously dense. It is important to make these distinctions precisely because the former

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6 All else being equal, a researcher who stays put ought to have more time, money and energy to expend on face-to-face research. Weißköppel’s account of fieldwork spanning several different sites describes the consequences that spending a relatively short period of time in each site had for her ability to form trusting relationships with informants (this volume). Marcus addresses similar problems in his chapter (this volume). Bureaucratic difficulties also multiply as field sites cross national and other boundaries. Several visas may have to be sought. The space encompassed by the mobile ethnographer may not conform to the regional, national, or cultural interests of any single funding body, as Karen Leonard discovered when conducting her impressive eight-country study of Hyderabadis diaspora communities (2007, and this volume). The extra work required to attract funding from different organizations for each phase of the project will further undermine the efficiency of the anthropologist in gathering information, not to mention the problem of securing funding for what will almost certainly be a more expensive project. Where studies cross the boundaries of traditional ‘culture areas’, they are also likely to require more training, for example in order to learn additional languages.
– the methodological asceticism of abjuring assumed entities – is a precondition for bringing ethnographically into view the always-incomplete, contested, and overlapping practices by which boundaries are maintained and networks cut or blocked.

Thus we endorse Candea’s critique of what he calls the ‘multi-sited imaginary’. We also maintain that notwithstanding those arguments there can still be good reasons for undertaking the kind of spatially extended study proposed by the multi-sited research programme, even if they are not the reasons originally envisaged. Specifically, we suggest that the boundary of a field of study can be drawn to incorporate – by design – contrasts and comparisons that are germane to the theoretical questions that drive the research. So our field, like Candea’s village, remains arbitrary with respect to the untidy, multi-vocal and endless networks of people, things and ideas ‘on the ground’, but is non-arbitrary with respect to the theoretical concerns motivating the research. Actively reflecting on the boundaries of a field can enable ethnographers to get our theoretically relevant comparisons where we want them: within a single ethnography rather than only between ethnographies. Thus we can overcome the disjunction inherent in the original multi-sited programme, with theory existing at the level of the postulated system, and each ethnographic site being theoretically significant not in itself but only insofar as it is imagined to be part of that imagined whole.

However local any ethnographic study appears to be, it must in fact proceed along just these theoretically motivated lines. The ethnographer chooses a base, has certain research questions in mind, finds points in networks of people and places whose comparison promises to shed light on those questions, and then visits some of those points (people, events, and so on) in a sustained and concerted way, in the process, of course, revising the questions with which he or she began. It is only by means of a persistent (if productive) fiction that this activity could be construed as an attempt at a complete description of a bounded site. The enduring contribution of the original multi-sited research critique was to expose the conceit of holism that sustained that fiction (cf. Thornton 1988).

We should recall, however, that anthropologists never had been restricted, in practice, to small-scale, bounded field sites (from Malinowski on the kula ring onwards). It was always a rhetorical exaggeration to claim otherwise, a critique that like so many others reproduces the simplification it sets out to criticise (Strathern 2004, 20; cf. also Falzon 2005, 10).

What was so productive in the original multi-sited research programme was the opportunity to liberate ourselves not so much from a bounded site, as from the idea that the field of ethnographic research could ever be coterminous with, or the same thing as, a geographically bounded location or area. Abandoning such an idea of a necessarily sited field makes it possible to admit that it never was possible to achieve a complete description of any area or group of people; but in exchange for acknowledging that fields are always constructed out of a too-rich reality, we would gain the freedom to determine their boundaries explicitly, in relation to our research questions. This is a difficult idea and it will help at this stage to specify
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precisely what we mean by ‘field’. We propose a three-fold distinction of terms that are apt to be conflated: space, place and field.\(^7\)

**Space, Place, and Field**

The distinction between space and place has been elaborated since the 1970s in human geography, which has generally understood place as meaningful space.\(^8\) An area of 15 square miles on the surface of the earth is a space; a named city that occupies that space, with its history and political struggles, is a place. Places are experienced, subjective, and cultural, whereas space is abstract and impersonal. Places have a complicated relationship to locations in space; their location may be over- or under-determined, or they may have no fixed location at all, as in the case of a ship at sea, which, though it is always on the move, is no less a place for its crew (Cresswell 2004, 22 – as Cresswell explains, this example originates in the work of Susanne Langer).

If anthropology were to follow the lead of human geography in making this distinction, as we suggest, its topology would include villages, ‘culture areas’, ‘cultural formations’, territories, and states.\(^9\) Just as nations are one relatively distinctive set of the larger class of imagined communities, all communities being imagined, and what makes the difference between nations and other forms of imagined community is how they are imagined (Anderson 1983), so places are imagined spaces and they likewise come in myriad forms. In the nation, language, territory, ethnicity, culture, and polity are imagined to coexist within common and clear boundaries that match the boundaries of the national territory. In places in general, a variety of different phenomena that are said to characterise a place are imagined to share a boundary. Like imagined communities, imagined spaces could

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7 A related distinction between field and site has been made by Weiβköppel, who formulated it as a response to the difficulty of translating the English term ‘field site’ into German (2005).

8 This of course is a simplification of an ongoing discussion with points of controversy. Key theorists who have written about the distinction between space and place, or about related distinctions include Tuan (1974; 1977), Relph (1976), Lefebvre (1991), and Massey (2005). A useful summary of the genealogy of the distinction is given in Cresswell (2004).

9 The correspondence of these places to spaces depends on the nature of the place and how effectively and authoritatively its borders are policed. The village of Crucetta as Candea (2007) describes it as a good example of widely recognized place that does not correspond to a clearly defined space and to think of it as an arbitrary location is to endeavour to treat it not as a place (the village, a face-to-face community) but as a space (an arbitrary window onto complexity). Great effort is expended in ensuring that the borders of nation states are well known and universally observed, so that the state as a place might correspond uncontroversially to the space circumscribed by that state’s borders. Nonetheless, the fact that many borders and territories are contested reminds us that in no case is consensus automatic.
be imagined in other ways and they require work to make them consensual, work that is never conclusive.

Just as a ship can be a place while it is on the move, some anthropological entities, for example Appadurai’s ‘scapes’ (1996), do not have a simple relation to spaces. Diasporas for example are mobile but are said to have roots in the homeland; the culture of capitalism might be thought of as fundamentally universalist and international but simultaneously to retain the traces, in its nature and distribution, of its origins in northern Europe. In fact, if we take the insights of the first generation of multi-sited research theorists seriously, then we should also include under the rubric ‘place’ any community for which borders are imagined, regardless of whether those borders are spatial, including doctrinal and vocational communities. Even groups such as men, women, the young and the old, and communities of all sorts would qualify as places in our terms insofar as those groups are specifically imagined as sharing certain characteristics that they do not share with members of other groups, that is, insofar as they are found inside a boundary, whether or not that boundary is a spatial one.

We should emphasise that this is not an attempt to impose a new and counter-intuitive use of the English word ‘place’. We are simply taking the technical use of the term developed by geographers to its logical conclusion. Though communities may often be imagined through space or spatial metaphors, these may include at the logical extremes both the image of being global or universal and that of being virtual or in cyber-space, both these extremes being still certain specific relations to space.

What we wish to distinguish from both space and place, as the field, is the direct object of the ethnographer’s experience. It is the sum total of all the points in the network (in the broad sense – including people, spaces, events, ideas, and so on) examined by the anthropologist in the course of research. Though it seems initially to be a fine one, it is important to take the distinction between the ethnographer’s field on the one hand, and the spaces and places it relates to on the other, into account. Just as places are clusters of elements imagined as spaces, so the ethnographer’s field is a set of points that may be imagined as a space – as a site.

The original proposals for multi-sited ethnography were formulated in explicit contrast to a representation (simplified and rhetorical, to be sure) of ‘traditional’ anthropological fieldwork. In order to explain our own proposal, and how it differs from both that model of multi-sited research and the classical single-sited methodology those authors criticised, we shall begin by recapitulating and extending that auto-genealogy, using our own three-fold distinction between space, place, and field.

According to the traditional anthropological view, or so at least the multi-sited critique asserted, space, place and field could be studied simultaneously: the anthropologist studied a geographical area (space) which, as a territory, was coextensive with a place (the culture, or village, or tribe, or caste, or cult, or class structure, or nation). By going there to study the social structure and/or culture of a place/space, the traditional ethnographer claimed it for science and made the space into a field (see Figure 2.1).
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According to this critique, the ‘traditional’ complex saw the world as a mosaic of cultures. Cultural units have natural spatial boundaries on various scales (village, tribe, culture area and so on). The role of the researcher is to study all aspects of a culture within those boundaries; in the unitary fieldsite (represented by the shaded area) the boundaries of the field, the place and the space all coincide.

The critique pointed out that this model rested on two questionable assumptions: first, that places, cultural formations, and so on, have clear boundaries and correspond to clearly bounded tracts of space; and, second, that there is a high degree of homogeneity within places and a high degree of heterogeneity between places such that observation made inside the borders of a place can be said to stand for that place in general and can serve as terms in comparison with other places. The imperative for multi-sited research was established by claiming that while adequate in the past, these assumptions no longer held in a radically new era, as ‘forces’ of post-modernity, globalization, and transnationality, somewhat breathlessly being discovered in quick succession by anthropologists at the time, rendered the study of a village or town, a people, a culture, and such, in the supposedly Malinowskian tradition, increasingly misleading (see the lucid discussion in Falzon 2005, 10–11).

What held space, place and field together in the traditional scheme was the concept of cultures (in the plural). Consequently, the loss of confidence in the idea of cultures in the 1980s was bound to cause a re-evaluation of the relation between these three terms, and through the 1990s we saw a good deal of soul searching concerning the implications of globalization for the conditions and possibilities of anthropological knowledge (for example Abu-Lughod 1991; Gupta and Ferguson 1997). Synoptic statements that were especially influential on and among anthropologists included Harvey (1989), Jameson (1991), Appadurai (1996), Hannerz (1996), Sassen (1998), and Ong (1999). Salutary sceptical notes were sounded, among others, by Strathern (1995), Mintz (1998), Tsing (2000), and Englund and Leach (2000).
Multi-sited Ethnography

The new position proposed by the original multi-sited research theorists – and this was how they sought to differentiate their methodology from what had gone before – was that traditional village field sites do not correspond to ‘cultural formations’, and that they do this ever less as the ‘forces of globalization’ 1992; Fardon 1995; Miller 1995; Strathern 1995; Moore 1996; Gupta and Ferguson 1997a; 1997b). The new position proposed by the original multi-sited research theorists – and this was how they sought to differentiate their methodology from what had gone before – was that traditional village field sites do not correspond to ‘cultural formations’, and that they do this ever less as the ‘forces of globalization’
distribute formerly more-or-less emplaced networks further and further into ‘scapes’ and diasporas.¹¹

Let us put the arguments of the pioneers of the multi-sited methodology in the terms of our three-fold distinction (see Figure 2.2). First, they recognized two kinds of space: plural, bounded local spaces; and a unitary, ‘seamless’ global space. The former were said to be in constant, intense, and increasing interaction. In the latter, global forces (globalization, capitalism, trans-nationalism, and so on, soon to be replaced by ‘neo-liberalism’) were said to be ‘at work’.

In contradistinction to what they saw as the traditional view, multi-sited theorists argued that cultural formations, places in our broad sense, spill out of bounded local spaces. They retained, unspoken, the assumption implicit in the ‘traditional’ view, as they themselves represented it, that in order to be valid the ethnographic field must correspond to a real cultural formation, or in our terms here, a place. So, since places now spill out from spaces, the ethnographer ought to follow carriers from local space to local space and the boundaries of the ethnographer’s field ought to come through assiduous following of connections to correspond to the sum of all the connected spaces – in this sense it would be a multi-sited field. The territory of the cultural formation will be some subset of all local spaces; the field should correspond to that area as closely as possible. So in order to study a diaspora, for instance, the field should aspire to contain all the spaces to which the diaspora has spread; in order to study a market the field ought to encompass all the loci of the commodity’s production, trade, and consumption, and so on.

Since the explanatory entities – globalization, global capitalism, and so on – whose effects were credited with dissolving ‘traditional’ places and thus with the general circumstances that necessitated multi-sited research, were located in unitary global space (see Figure 2.2), there was an unintended homogenization built into the method. Although authors talked of following connections in order to establish the real shape of transnational multi-sited cultural phenomena (formations, scapes, and so on), they all tended ultimately to be seamlessly global in size and shape.¹²

One of the important consequences for the distinctions we have been proposing is that whereas spaces (or sites) and places (or cultural formations) are or may be imagined as areas in two or more dimensions, the field is not an area, but a collection of zero-dimensional points, or, perhaps better, of one-dimensional lines of comparison that connect such points (see Figure 2.2).

¹¹ Subsequently the idea of globalization itself was criticised (for example Tsing 2000) for preserving, in the notion of hybridity, the stubborn idea of bounded, whole cultures with borders; now permeated, but borders nonetheless.

¹² Important refinements to the multi-sited research methodology include the recognition, first, that the global is also local, and, second, of the importance of place-making and scale-making and the fact that the work they do is never complete, so there can be no consensus on the ‘contours’ of a ‘cultural form’ ‘on the ground’. See for example Riles (2003), Falzon (2005), and Green (2005).
Conceiving the field erroneously as an area (whether a static ‘culture area’ or a mobile ‘cultural formation’) therefore requires some kind of sleight of hand, to make those lines and points swell enough to fill the area they are supposed to represent. This can be done in at least three different but combinable ways. The most common tactic used by social scientists in order to stretch the lines and points of observation into areas of space is to explain them in terms of social forces (secularization, development, hyper-capitalism, globalization, neoliberalism, and so on), forces that are held to permeate the space. The ethnographic observations and the postulated forces are thereby established in a reciprocal (and therefore circular) relation of explanation. We have explained above why this kind of explanation is essentially empty. Another tactic that tends towards the same effect, as Marilyn Strathern has shown (1992; 1996), is based on the notion of plural cultures, according to which any part of cultural practice is interpretable as ‘the culture’ in microcosm, and so can stand for the culture as a whole. In relation to her fieldwork in Mount Hagen, Strathern observes, ‘The stereotype says that what I can learn about a cluster of clans I can (more or less) replicate for the entire population, and talk of Hagen in the singular. This demographic simplicity is an illusion’ (1996, 24). As she goes on to note, it is an illusion that can only be sustained across a distance – its problems become apparent when it is applied close to home. A third and final method of passing from points and lines to a field of spaces and places is the application of statistical reasoning. Although rarely used by social anthropologists, statistical methods of generalization depend on assumptions of normal distributions and correlations within borders that are apparently identical to the pluralism model just described.

If one recognizes that the field need correspond neither to a space in two dimensions nor to a place, a ‘whole’ cultural formation imagined in relation to space, how might and should an ethnographical field be related to space? How are anthropologists to draw the boundaries of their field? What we suggest is that rather than seeing the spatial discontinuities in the networks anthropologists study – whether they be the edges of a village or national borders or whatever – as boundaries of our field within which we are obliged to suck up as much information as possible in an attempt to describe a whole space in two dimensions, such borders may be productively encompassed by the field. By crossing borders we can use them as dividers that will make comparisons between nodes on either side possible. So whether the field is a single village or ranges over continents, there is no fundamental difference in the way ethnographers, based on their research agendas, make choices to talk to particular people, visit specific spaces, and witness specific events within the field (See Figure 2.3).

The way then to avoid the implicit holism Candea identifies in the original multi-sited research agenda, and to avoid the appeal to unseen global forces that such holism enabled, is research in which the field is importantly un-sited
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rather than being merely multi-sited: research that acknowledges that there is no necessary connection between field, place and space, that ethnographic fieldwork is something that could be done no less if one were to travel no further than half an hour’s walk from one’s study than if one were to travel all over the world.\textsuperscript{13} Even if working in a single village (more obviously so in a city) a researcher has to make decisions about what to include in the study and what to omit. One makes those decisions on the basis of the questions that are motivating one’s research. Of course, one’s research questions will be influenced by what one finds, and will generally evolve, but decisions must be made and even the most open of open-ended research must begin with theoretical assumptions that guide those decisions. It is not of course that earlier multi-sited projects have not included comparisons

\textsuperscript{13} Weißköppel acknowledges this in her chapter (this volume) where she sees multiple sites within a single city as well as on different continents.
of the kind we are proposing as the fulcrum around which an ethnographic field may be conceived, but the nature of those comparisons has often been hidden by the rhetoric of holism.

**An Un-sited Fieldwork Project**

In the academic study of world religions, it has been common to use local cases as illustrations in arguments about the purported true nature or essence of a particular religion, or, conversely, to use theories about the religion as it is imagined to be essentially, to explain data from a local site. For the study of Buddhism, this has meant a long struggle, which several generations of anthropologists have felt the need valiantly to engage in, with the problematic idea that there is a single, authentic Great Tradition by which all placed, historic variants are to be measured (see for example Tambiah 1970; Southwold 1983; Gellner 1990; Spencer 1995). This makes sense as a religious idea, because it is a legitimate religious activity to seek the correct interpretation of a transcendent teaching. It does not make sense for social scientists, who ought to be in the business of observing what people do and reporting on it. Part of the story that we should be telling is how the effects of systematicity and authenticity are achieved, to what extent and in what distribution they obtain, how they are contested, and by whom. We cannot do this if a particular position on just those questions, one of many possible positions, is built into our observations.

To avoid this, and to achieve the ‘flat’ description of an un-sited field that we have argued is required, we must avoid *a priori* decisions about which kinds of practices and beliefs, among those that are observable in any place and time, are part of a supposed great tradition that exists at a global scale. As we have seen, in the case of Buddhism, there are many things that may make world religions seem global: for example, missionaries, translations, and the characteristically modernist idea of the religion as a global community. In dealing with such phenomena, three things should be remembered: first, all of them only happen in a finite number of specific interactions; secondly, they need not occur together (so, for example, the idea of Buddhism as a world religion can spread independently of particular traditions of interpretation which are seen as the contents of the world religion by different practitioners); and, thirdly, not everyone will agree on them.

All of this has important consequences for researchers who need – knowingly or not – to define the boundaries of their field. If we accept, as most believers do, that there is an authentic tradition, then we will expect that some believers, beliefs, practices, and so on will be ‘inside’ and that others will be ‘outside’. Separating the goats from the sheep would then be a matter of studying both the universal, great tradition in order better to understand the conditions of authenticity, and studying the particular, local instantiate, in order to determine whether or not it fulfils those conditions. If, however, we recognize that authenticity and the relative scale of global/local are things that are not given by a trans-historical and universal
system existing somehow behind and beyond observable phenomena but rather are effects that need to be ‘staged’ (to borrow another term from Latour 2005), and if this staging is one of the things that we want to observe, then the boundaries of the religion are no longer given or in any simple sense there to be discovered.

No ‘local’ observations can provide the criteria of authenticity that will be used to judge that same locale, without falling into the circularity that has plagued the anthropology of Buddhism. It is all very well to say, ‘we will ask the experts’, but by what criteria are they recognized as such? The same question could be asked of any attempt to define authenticity by reference to observation.

When it comes to our research on Buddhist ethics and self-cultivation, we have begun by choosing our research questions. The next step is to construct a field that will allow us to research these questions. We are not starting from scratch. There is a degree of both serendipity and pragmatism in choosing to construct a field that includes spaces with which each of us is already familiar (the general point here is made interestingly by Coleman and Collins 2007). We start therefore not so much with a field as a research strategy: to go to certain locations, witness certain events, to seek out certain categories of person, and to do that across important borders that separate the sites in which each of us has worked previously. By combining these sites in a collaborative project we will be able to bring a single theoretical approach (that is, the focus on ethics of self-cultivation) to bear on a different range (not necessarily a greater or more intense range) of comparisons from that encompassed by our individual work. We have a particular interest in doing this because, as we explained above, we believe that the literature on Buddhism would be enriched by focused attention on the question of self-cultivation, and that detailed Buddhist ethnography would deepen and complexify our general understanding of self-cultivation projects and processes. So, because our project has theoretical or generalizing ambitions, and because existing literature, not being sensitive to questions of self-cultivation, is thin on relevant material, we need consciously to include the comparisons in our study.

By combining our resources in a way that is directed by our research interests, we aim progressively to construct for ourselves a field that will enable us to shed some light on some original questions by including comparisons across several kinds of border that are germane to the subject of our research.

One boundary that is not a parameter for our research is that between Buddhism and not Buddhism. As in other religious traditions, interpretations of what is essential and authentic to the tradition as such lump together criteria and features that do not bear a necessary relation to each other, and which therefore extend independently and non-coextensively in all directions. The expectation that there is or should be a single boundary around ‘Buddhism proper’ is, on the other hand, a very interesting aspect of contemporary Buddhist life, as of world religions in general. The Buddhism/not-Buddhism boundary is not effectively policed (less so even than in other world religions) by any power that has the capacity to make its distinctions stick.
Other boundaries, by contrast, are more effectively policed and these provide lines that cut the heterogeneous networks that constitute Buddhism, and such policing makes visible distinctions between points on either side of the boundaries it creates. We can observe this effect without having to imagine bounded homogeneity around those points.

The most important of these borders are the frontiers of nation states. States police borders that stop the flow of networks in many important ways, resulting in sharp contrasts in officially condoned ideas of personhood (citizenship); in pedagogic traditions and practices under the aegis of the institutions of state education; in the conditions under which the institutions of Buddhism relate to state authority, to other religious institutions and to the public; and in the presence and possibility of relations with foreigners. Nation states are also interested in many of the questions generated by self-cultivation – education, personhood, ethics and morality, hygiene, deference to authority, and so on – in other words in what Foucault called biopolitics. The form this interest takes differs radically across the national borders that will crosscut our constructed field.

Other borders we will be working across include those that separate lineage and sect, the divide between different ways of conceptualizing the relationship between monastic and layperson (in some cases a strong distinction from the time of ordination, in others a weak distinction because ordination is common and can be temporary, in still others weak because the laity has taken up practices once reserved for monks).

Conclusion

Although in this chapter we have expressed dissent from some of the claims that have often been made for multi-sited research, we hope it is clear that our aim has been to build upon and carry forward the project of developing a well-grounded conception of such research. We have sought, indeed, both to provide a vindication, following Candea (2007), of the bounded or ‘arbitrary’ ethnographic field (if by this we mean one that is explicitly a means of investigating, rather than pretending actually to be, a chosen object of study), and at the same time to set out a rationale for multi-sited ethnographical research (if by this we mean research in which the differences, similarities, connections, and disjunctions between several separate physical locations are analytically central). Our aim has been to specify these two aspects of anthropological research practice in such a way that there is no tension or incompatibility between them.\(^{14}\) Our distinctions between space, place, and field have been deployed in order to develop the argument that a valid ethnographic field need not correspond to a spatial entity of any kind, and need not be a holistic entity ‘out there’ to be discovered. This is true regardless of how extensively or

\(^{14}\) We do not of course deny that multi-sited research might be differently conceived and deployed for analytical purposes other than those considered here.
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not a fieldworker might travel in the course of research; the important thing is the range of theoretically relevant points of comparison that are built into the design of the field. These can be many or few, independently of how ‘big’, on the ground, the field might appear to be; independently, that is, of whether these points are within a single village (as in Candea’s Crucetta) or distributed across several large nation states in Asia (as in our own research on Buddhist practices of self-cultivation).

Unlike the blind men in the Buddhist parable, who were required by their king to try to identify something they could not perceive with the data available to them, anthropologists are free to choose for themselves analytical objectives that can actually be achieved. If there is no elephant, there is no need for us to try to imagine one.

So on this view the great advantage of multi-sitedness – the opportunity to include by design a series of points of connection and comparison – is achievable, depending on the theoretical interests motivating the research, even where the sites that are chosen are not at all far apart or are even coincident in purely spatial terms. This is why we have preferred the term un-sited to describe this kind of ethnographical research: to carry to its logical conclusion the disconnection of the ethnographic field from space which was the most signal achievement of the first-generation multi-sited research programme, and to lay to rest the holistic assumptions that have haunted and restrained that programme.

References


Think geology. Images of sediment, layer upon layer, continually pressing upon each other. Nuanced terminology for describing both slow and catastrophic change. Techniques for recognizing faultlines – the lines between tectonic plates where earthquakes are likely to happen as the plates move past one another. Historically, the earth sciences have been observational sciences; explanation was their goal. Increasingly, as the threats of natural hazards and environmental problems garner greater attention, the earth sciences are called upon to anticipate the future.\footnote{Geological terms and images have been recurrent in anthropological studies of the sciences, in part because of the influence of Gilles Deleuze, in part because geology offers a theory of change resulting from accretion, sedimentation and faultlines, which is particularly important in critical studies of the sciences given conventional narratives about big men, their genius and their revolutions. Note, for example, Mike Fortun’s critical conception of ‘fissures’ and ‘volatility’ in his book about genomics in Iceland (a geologically fissured and volatile locale) and the global economy, and Sharon Traweeks’s suggestion that thinking in terms of ‘faultlines’ in Science and Technology Studies can help us draw out ways ‘knowledge is being defined and made at the edge of times and places called modernity’ (Fortun 2008; Traweek 2000). An ‘ethical plateau’, in Fischer’s conception, is a site ‘where multiple technologies interact to create a complex terrain or topology of perception and decision making’ (Fischer 2003, 36). Ethical plateaux, like geological ones, are created through accretion and sedimentation, and they ‘work’ by providing grounds, by moulding and by pressing – sometimes cataclysmically – against surrounding formations.} Empirical observation of past and present dynamics are relied on to predict earthquakes, hurricanes, rising sea levels, climate change, and so on, and to direct public policy. As historian of science Naomi Oreskes explains, there is now a huge demand for temporal prediction in the earth sciences, and practitioners have had to develop new techniques and technologies (particularly computer models) to respond to it. They also have had to struggle with questions about how (scientific) knowledge production works, questioning different ways of using and extrapolating from observational data, figuring out what role the knowledge they generate can and should have in policy arenas (Oreskes 2000).

This predicament – regarding what can be called the promissory nature of data – is of course familiar to cultural anthropologists and is no doubt why anthropology has recurrently been linked to social reform initiatives (Fischer 2007). Like earth
science data, ethnographic data speak, so to speak, beyond what they represent. Yet representation – or at least aspiration to representation – remains key. Researchers strive to know and report on their object of concern – whether a rock or cultural formation – as faithfully to the object as possible, knowing that the meaning of what they observe inevitably exceeds what they in fact observe. And knowledge of the excessive and deferred meaning of ‘data’ in turn shapes how observation proceeds. New questions come to seem critical, and previously unrecognized causal mechanisms begin to seem significant. There is, then, a funny looping. Observation proceeds iteratively, driven by extrapolative readings of what observations thus far mean and imply. It is this looping, in my experience, that produces multi-sited ethnographic projects, and a sense of ethnography as and of open systems.

Open systems are systems that are continually being reconstituted through the interaction of many scales, variables and forces. Increasingly, such systems are the ‘objects’ of cultural analysis. Whether the system of concern is the global economy, an organization, or an individual subject, the task is mapping an array of constitutive dynamics – including but not limited to dynamics at the local level. These kinds of project differ in important ways from traditional anthropological projects while preserving in depth engagements with real world situations as a defining methodology. They are often based on complex research designs, involving ethnography at multiple sites, engagement with multiple scholarly literatures and disciplines, and fluency in many languages, technical as well as natural. At their best, these projects result in dense and complicated accounts of how the contemporary world works, which have relevance both to scholarly debates and to practical efforts to respond to social problems.

In this chapter, I discuss the scaling and visualization of open-systems ethnography, drawing out methodological rationales and challenges in interrelating multiple sites and levels of analysis. Thinking geologically, I argue, and in terms of multiple strata – from the nano level where subjects are constituted, though levels where technology, organizations, economics and other forces are in play – helps orients without determining the parameters of open-systems analysis.

Informatics also provides vital resources – literally and figuratively – for visualizing ethnography as/of open systems. Imagine digital maps that layer different kinds of data and allow readers to drill down to increasingly specific detail. Imagine being able to click through to images of ‘the same thing’, from different angles, at different scales. Imagine meta-data that describe how data

2 In the preface to the second edition of *Anthropology as Cultural Critique*, Marcus and Fischer (1999) note that many of the metaphors for the ‘functionalist’ and ‘structuralist’ vocabulary of earlier social theory derived from the mechanical and physical sciences, and how useful metaphors for understanding and describing contemporary reality can be drawn from the life and information sciences. Scientists’ way of describing bacterial and viral action, genetic transmutation and symbiosis, for example, offer ways of thinking about society and culture as emergent from mutations, viral transitivity and rhizomatic growth (ibid., xxvi).
lower down in a system are configured so that they can be found, talked about, and
more easily interpreted, shared and compared. Meta-data force things into boxes,
which are organized into sets, and linked in multiple ways. They are reductive, to
facilitate freer play.

Anthropologists, in my view, need this kind of meta-data. They need more overt
ways of talking and thinking about the data they create and use, and about the kinds
of knowledge they produce. Such articulation could help us see how different parts
of the systems we study corroborate and collide. It could facilitate comparison
across locales and anthropological projects. It could facilitate collaboration with
researchers from other disciplines. It could facilitate movements of anthropological
knowledge into the public sphere.

This is not a call for standardized methodology or a unified research programme.
It is an argument for figuring out types of knowledge that result from ethnographic
study. Results from one study can, no doubt, orient attention in other studies. But
validation is not the goal. Methodological acumen is.

Scaling

What, then, is it that anthropological study of open systems creates knowledge
about? I have found it useful to think in terms of scale, to facilitate intertwined
cultural and political economic analysis, in particular.\textsuperscript{3} It is noteworthy that one
needs to add strata to conventional scalar schema in order to articulate what
actually goes on in – and often centres – anthropological projects. Adding strata
to a conventional scalar schema also helps draw out what it will look like to
integrate Science and Technology Studies (STS) – the field I now work in – into
anthropology, methodologically rather than topically. The recursivity of it all – the
way anthropology itself is scaled as it strives to articulate scale in what it studies
– is not insignificant.

Notions of scale themselves of course require ethnographic scrutiny. In STS,
we are aware of the many different ways that ‘scale’ is understood and relied
on in different disciplines of the natural and computer sciences (Helmreich
2000, Schienke 2006). Social scientific conceptions of scales also need to be
understood as historically and culturally constructed, and as inevitably limited.
There is a tendency, for example, to think of ‘top-down’ as the way power operates
oppressively, and of ‘bottom-up’ as revolutionary. Life – whether biological or
social – is not so simple. Scale is a heuristic, which, like all heuristics, provides

\textsuperscript{3} A challenge laid out by Marcus and Fischer in the mid-1980s, which has oriented
much of my work, is ‘to find a way to embed richly described local cultural worlds in larger
impersonal systems of political-economy’ (1999, 77). A key goal is to use empirical work
at the local level to reshape ‘dominant macro frameworks for the understanding of historic
political-economy, such as capitalism, so that they can represent the actual diversity and
complexity of local situations for which they try to account in general terms’ (ibid., 88).
a way of seeing that frames and orients perspective. At its best, scale provides a way to see many types of action in motion at once, evoking a sense of the system at hand. Scale keeps track of the diversity of forces that animate a given system – whether that ‘system’ is a subject, an organization, a discourse, or a market.

I have found it useful to think in terms of seven strata, topped by what I think of as the ‘meta-level’, thinking – as I have already said – in terms of the meta-data used to make sense of complex information systems. Meta-data do not encompass what is stored lower down in the systems, but they do organize what is used, compared and considered of interest. Meta-data recognize some things and not others, at times occluding through in-action – by failing to name, categorize, or link, information, for example. What anthropologists often refer to as a dominant discourse operates on this level, and in this way. Dominant discourses shape without determining thought and behaviour. They can be more, or less, in synch with the real world.

Analysis of dominant discourse allows anthropologists to provide historical perspective on what is considered true, good and worthy of attention, and to reveal ways thought and language are organized to permit some articulations but not others. This can be done through analysis of the binaries that sustain a discourse, or through delineation of ways narrative form and ‘thought style’ delimit articulation. Understanding of ‘discursive gaps’ – holes in what it is possible to say – often emerges when the anthropologist is also working at other scales. One figures out a discursive gap by knowing what is going on at other levels. Anthropologists identify such gaps in the discourses constitutive of whatever it is that they study, and also in their own discipline.

In the aftermath of the Bhopal disaster (Fortun 2001), for example, a number of dominant discourses were in play. The ideals of modernity – science as a means to progress, for example – operated, though in somewhat unexpected ways. It was not governing elites, but activists in the People’s Science Movement who had trouble dealing with the Bhopal disaster as an example of science gone wrong. At one point during my fieldwork, these activists declined to participate in demonstrations in support of gas survivors’ demands because they could not make Bhopal square with what they felt they stood for, that is science and technology as means to progressive change. Meanwhile, governing elites were shaped by other ideals and dominant discourses. Most significant was their ‘investment’ in discourses that made progress and prosperity dependent on foreign investment and multinational corporations. This investment dramatically shaped how the Bhopal case was dealt with. When the case was settled out of court for what many considered a ridiculously low sum, for example, a government representative explained to the

4 Consider, for example, Elizabeth Povinelli’s (2002) account of ways multicultural liberal discourse recognizes, and fails to recognize, indigenous alterity.

5 The immediate cause of the 1984 Bhopal disaster was an explosion and gas leak at a Union Carbide plant that manufactured the pesticide Sevin. Less proximate causes included investments in the Green Revolution, market logics, and so on.
press that the settlement should be understood as a message to the world that India welcomed foreign investment.

Middle class activists working on behalf of gas victims were also shaped by dominant discourses, from various Left-leaning lineages in particular. Various Marxisms and Feminisms, Gandhianism, Anarchism: all had imprints. One way I tracked how these discourses operated was by talking with these activists about how they came to be activists, and by listening to their motivations and rationales in their day-to-day work. I also read what they read: anarchist tracts sent from Ireland (which, somewhat ironically, preached the value of local determination), novels and poetry. The heroic figure was often cast as one sure of his commitments and the right course of action to realize them. Such a figuration productively animated many activists but also, in my view, confounded them. Work on behalf of gas victims was far from straightforward; daily work was fraught with difficult decisions about legal strategy, about working (or not) with government hospitals, about working (or not) with different survivor organizations. Images of Che Guevara did not seem to me very helpful, indeed more often than not they contributed to burnout and in-fighting among activists.

Nor were iconic anthropological images too helpful. I certainly was not alone in the field with natives dramatically different than myself. Journalists were of course part of the scene; many of the middle-class activists with whom I worked and lived had class, educational and family backgrounds rather like my own; over time, even gas survivors – many of whom endured extraordinary economic and health insecurity – came to seem as much Same as Other, linked into a global system of industrial production and risk that I, too, lived within. Linkage between India and the USA was an obvious part of the Bhopal story from the start because of the role of Union Carbide, but this linkage took a while to register ethnographically. My research addressed the work and logic of activism in Bhopal, albeit focused on how activists’ writing practices indexed the ways they understood the worlds – local, national and transnational – that they operated within. It was only later that Bhopal as a geographical location became really de-centred for me.

Over time, I realized that there was an ethnographic response to powerful constructs of ‘Bhopal’ – by Union Carbide and other corporations, in particular – as an unpredictable, isolated and not likely to be repeated event. More fieldwork would be required, at multiple sites. I needed to look beyond the local, at the way plant design decisions were made at Union Carbide, at plant communities in the USA, at the logic and force of international trade agreements being signed as the Indian Supreme Court decided the Bhopal case.

I certainly did not anticipate at the outset what my study of the Bhopal disaster would become over time. ‘Multi-sited ethnography’ was not yet in the vernacular, nor even was ‘globalization’. My sense of how best to configure the study, and later a book, emerged iteratively, driven by ethico-political implications (the promissory nature of so much of the ‘data’) and also by awareness that much had already been written about Bhopal, and about the extraordinary, often abusive, power of multinational corporations. It was a challenge to figure out what kind of book needed
to be written in an already rich field. Thinking in terms of dominant discourses helped. With time, I realized that I wanted to talk back to dominant discourses about both activism and an emerging global order. Thus, my book’s title: *Advocacy After Bhopal: Environmentalism, Disaster, New World Orders*. One goal was to draw out how progressive advocacy really worked on the ground, at odds with many entrenched ideals. Another goal was to show how dominant (and at the time accelerating) discourses about free trade and global ‘harmonization’ were out of sync with how the world really was. Mismatch between dominant discourses and what my ethnographic material demonstrated became the book’s organizing logic.

Something else, however, also drove what my Bhopal study became. As I discussed in my book, Bhopal was a disaster in more ways than one. It confounded established ways of thinking and doing things. The disaster was complex in many ways, and corrective action never straightforward. Political ill will was part of it, but also well intended but ultimately ineffective effort. Claims about causes, effects and possible futures were usually intensely disputed, and rarely settled. Meanwhile, the overwhelming and enduring tragedy of it was omnipresent. It was extraordinarily humbling to conduct fieldwork in this context. It was this, I think, as much as scholarly theoretical commitments, that drove my sense that I needed to see ‘Bhopal’ from different angles. That it was many things. That vantage point mattered. A ‘multi-sited’ approach thus emerged.

The macro level, the level where markets, laws and other translocal institutions work, thus also became a (ethnographic) concern. Often, when anthropologists work at this level, their unique contribution is in detailing how such institutions configure in different ways in different locales. But they also work from another direction, explicating how translocal institutional forces are themselves produced, demonstrating how the macro level is itself an emergent effect.

In the Bhopal case, it was the force of law that dominated my attention. Courts in the USA declined to exercise jurisdiction, citing the doctrine of ‘forum non conveniens’ [inconvenient forum]. Invoking this principle meant that the judge concurred with the defendant (Union Carbide) that relevant witnesses and evidence could be more conveniently accessed if the case was heard elsewhere (in India). Very early on, then, crucial corporate decisions, about plant design in particular, were excluded from legal scrutiny. I also followed the case in India, where the force of law tangled quite explicitly with the force of markets and efforts to secure the Uruguay Round of trade agreements, which transformed the General Agreement on Tarriffs and Trade (in place since World War II) into the World Trade Organization (WTO). WTO’s website describes how, by the end, 123 countries took part in the Uruguay Round, and how it covered ‘almost all trade, from toothbrushes to pleasure boats, from banking to telecommunications, from genes of wild rice to AIDS treatments. It was quite simply the largest trade negotiation ever, and probably the largest negotiation of any kind in history’.  

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The Bhopal case was entangled in this. The case was first settled in 1989. The Indian Supreme Court upheld the settlement in 1991, just as trade agreements were being signed and the Indian rupee became convertible. Extra-local dynamics certainly shaped what Bhopal looked like at the level of law.

The extra-locality of ‘Bhopal’ was clear from the outset and from Bhopal itself. Making this a matter of method was, nonetheless, far from straightforward, and took time – concretely, a postdoctoral fellowship during which I had time to continue research in the United States. It was through this research that I was able to explore, from the ground up, so-called ‘harmonization’ – the effort to make product standards and regulatory policies uniform globally to facilitate free trade. Interviews with labour organizers, workers and residents of fence-line communities in the United States were useful. I also learned, from labour organizers, about Congressional hearings (held in 1991) focused on increasing reliance on contract labour and ‘near misses’ (‘almost Bhopals’) in the petrochemical industry. In these hearings, industry representatives argued that regulation requiring increased reporting of factors contributing to safety problems would not be useful because hazardous conditions were so site specific that lessons were not generalizable. Labour organizers disagreed, articulating the reality and promise of harmonization quite differently (Bedford 1992).

The meso level – that of social organization and interaction – was also critical. Here, victim and activist organizations and networks operated, alongside various government programmes, to provide compensation, health care, job training, and such. Viewed at this level, Bhopal was an extraordinary organizational challenge and opportunity. Over 600,000 people were affected by the gas. Already-taxed social and health service programmes literally went into crisis mode, and this became subject to an important critique advanced by activists. Years after the gas leak, many programmes still operated with a crisis logic that implicitly disclaimed the need for long term, sustainable programming. Activist organizing around Bhopal was also a challenge, and an opportunity. One activist explained all the conflict amongst activists working on Bhopal as indexing ‘the death throes of Marxism’ in India. Others worked to use Bhopal to galvanize an emerging grassroots environmental movement. ‘Bhopal’ acquired a momentum of its own in these circuits, tied to but not determined by what went on in Bhopal itself.

7 In the early 1990s, pressure on India’s economy came from many directions. The first Gulf War caused fuel prices to almost double, exacerbating balance of payments problems; the war also meant that many Indian workers in the Gulf came home, reducing the flow of remittances. Conflict at home – between (some) Hindus and Muslims over the Ayodha mosque, and over reservation of public sector jobs for scheduled castes – also made a difference, shaking confidence in the Indian government both at home and abroad. As it became increasingly difficult to borrow abroad, India made various agreements (with the IMF in particular) that committed the country to economic liberalization.

8 The meso level has renewed importance in many anthropological studies today given interest in the way people distributed around the world are interlinked – through migration,
The micro level is, among other things, the level of practice. Here, my focus in the Bhopal study was on writing – on what and how activists, in particular, wrote. As I described earlier, my interest was in the ways writing practices were a means by which activists made sense of the discursive terrain in which they aimed to be effective. Talking about how to cast a legal argument – for journalists or the courts, for example – was an opportunity to consider how the courts or journalists were functioning, and could be addressed. Ethnographic consideration of how activists wrote gave me insight into how advocacy really worked on the ground, while also providing me with activists’ own readings of the other scales that I came to be interested in.

Subjectivity, or ‘subject formation’ – the focus of many anthropological projects today – operates at what I think of as the nano level: below, so to speak, levels where dominant discourse, organizations and markets operate, shaped by but also constitutive of practice. Subjectivity is akin to what has been called ‘personhood’ or world view, but more overtly acknowledges the weight of history, the strange loopings of the unconscious and the space and practice come to inhabit the body. Images of sedimentation – of subjects formed through processes of accretion, sometimes harbouring faults – are particularly useful.

In Bhopal, I was particularly interested in the subject formation of middle-class activists – in the ways caste and class backgrounds, different Leftist lineages, educational and practical experience, and so on, came together to make particular things seem good or bad, possible or untenable, obligatory or optional. I was also interested, in a less concentrated way, in how and why corporate actors experienced the good, bad and ugly. Understanding the latter was clearly important in trying to understand what seemed to me to be gross corporate negligence. Trying to understand corporate actors as subjects was also called for in efforts to explicate the strategy and sincerity of corporate greening, which emerged in full force in quite direct response to the Bhopal disaster.

Operations on the technological level – which includes prosthetics of knowing (from censuses to phones to Earth Observation Systems) as well as modes of transport and production – are also critical. Technological infrastructure, like other kinds of structure, compel some things and limit others, function and dysfunction. Ethnographic examination of technological infrastructure can reveal how caste, class and other hierarchical orders are produced – ‘enumerated’, in Cohn’s (1987) classic formulation – and how some problems and people are visible and attended to, and others are not. The medical categorization scheme relied on by the Government of India in their dealings with Bhopal gas survivors, for example, capital flows, and media circulations, for example. In some ways, work to understand meso level phenomena builds on earlier anthropological studies of kinship, political and economic networks. The difference in an open-systems analysis is in the attention given to runaway reactions and blockages – to disjunction as well as function. Recall, for example, Appadurai’s emphasis on disorienting ‘disjunctures’ between economy, politics, and culture, which older approaches had discounted (Appadurai 1990).
was a prosthetic of knowing that determined who and what received care, and did not. Ethnographic examination of technological dysfunction – ‘runaway reaction’, as happened in the Union Carbide chemical plant in Bhopal, for example – is also critical. Often, an initial challenge is to map the many ways that potential for technological dysfunction is disavowed – tracking, in other words, the many ways that functionalism operates as powerfully among corporate and government planners as it does among anthropologists. Another challenge is to understand how technological infrastructure can be thought about and built with possibilities for both function and dysfunction in mind, recognizing that change will happen, and at times should be encouraged. Chemical plants, for example, can be understood as having inbuilt potential for ‘normal accidents’ (Perrow 1984) because of the vast number of linkages and pressure points that hold them together as systems. Chemical plants can also be understood, and built, as subject, or not, to change. Union Carbide USA engineers, for example, did not design for Indian conditions, exacerbating risks of disaster in Bhopal. Chemical company engineers have also denied potential to change the design of plants when confronted with local community and regulatory demands to reduce risk potential. Ethnographic analysis can thus draw out how technological infrastructure literally materializes particular theories of change.9

Finally, at the metaphoric ground, is what can be called the bio-material level. Here, anthropology accounts for the persistence of the body and the ecological – the ‘natural’, that is. Despite the breadth and depth of work in cultural anthropology and STS on the body and other constructs of ‘nature’, this level of analysis is often ignored, partly as a result of critical commitments to ‘de-naturalize’ and understand how things are ‘socially constructed’. Feminists were at the forefront of earlier efforts to de-naturalize and de-biologize, and now (in an admirable reflexive loop) are at the forefront of efforts to bring material bodies back into view. Viruses and toxins in bloodstreams, contaminated water supplies and exhausted soils need to be attended to. They weight and animate the systems they are part of. Consider, for example, the toxins incorporated into the bodies of gas survivors in Bhopal, and the toxins that continue to leach into water supplies from the factory’s sludge ponds.

All of these levels are constitutive of ‘Bhopal’. Analytically, I find it useful to differentiate them, imagining the multicoloured layers of complex geologic formations, some layers thicker than others, some with fissures, all subject to change, even if slowly.10

9 Designing for ‘vulnerability’ has become a goal of many activists and researchers focused on climate change (see Krauss, this volume). The critique of functionalism that I begin to articulate here is relevant to this challenge.

10 I also build here on critical race theorist Kimberly Crenshaw’s conception of ‘intersectional sensibility’. Crenshaw criticises identity politics for asking people to be either raced, woman or queer, and for ignoring intragroup differences – which makes it difficult to deal with domestic violence in black communities, for example, and limits the standing women, in particular, have before the law. Intersectional sensibilities involve
Ethnography of/at the Limit

A scalar schema is but one portal into what multi-sited ethnographic projects do and produce, and it is critical to recognize this. Thinking in terms of scale draws some things out, and obscures others. Recognition of this kind of limit is also critical in thinking about the practice of multi-sited ethnography. Multi-sited ethnography pieces together a picture of an ‘object’ with material from many (sometimes unexpected) places. Creativity and analysis is required to do this well; aspiration to be comprehensive is naïve and lacks critical purchase. Even so, however, multi-sited ethnography is not merely aggregative, a matter of putting the right pebbles in the bucket. Rather, it also leverages the way any take on an object is specific to the time and location of the taking. Material gathered at the varied sites of multi-sited ethnography thus provides multiple angles on an ‘object’; the ‘object’ is seen through different frames of reference. The impossibility of absolute observation, space and time is thus turned to the ethnographer’s advantage. Relativity becomes a tool; a sense of ethnography as a double game – both inductive and theoretical, observational and extrapolative, representational and constructive, of and at limits – begins to make sense.

Many imperatives drive such a conception of multi-sited ethnography. One can be considered ontological and materialist, emergent from recognition of the ways objects, context and knowledge are mutually constitutive, and from recognition of recognition of multiplicity: the simultaneous examination of race, ethnicity, sex, class, national origin, sexual orientation, and so on (Crenshaw 1989). Toxicologists would call this ‘cumulative effect’, and recognize that both scientific and legal/regulatory worlds have great difficulty dealing with it.

Multi-sited ethnography is sometimes understood as being without design, method, even theory – as somehow emergent organically without friction. Such a tendency, in my view, works against the critical potential of ethnography, and of multi-sited ethnography in particular. It is also important to recognize that multi-sited ethnography does not promise anthropological projects that do all things, comprehensively. Figuring out which scales to focus on, for how long, is a critical part of research design, for example. A scalar sensibility can help orient this figuring out.

Karl Popper sardonically described an inductive, Baconian vision of scientific knowledge production as the ‘bucket method’ – in which facts are gathered like stones and only thereafter do hypotheses emerge. Popper argued that this was absurd, insisting that science must proceed through a series of conjectures that could be refuted with facts. For Popper, hypotheses came first, then the facts were chosen accordingly (1959). Oreskes reviews this history of science in her discussion of the rise of temporal prediction (which is different than the kind of logical prediction Popper insisted upon) in her description of changes in the earth sciences, which I drew on earlier in this essay (Oreskes 2000, 33). Oreskes’s point is that what counts as good science is historically conditioned. This is worth recalling in anthropology. It is also noteworthy that critical anthropological practice today – historically attuned, striving for ‘cultural critique’ (Marcus and Fischer 1999) – does not fit the simple binary that Popper sets up.
possibilities for producing new knowledge by shifting one’s frame of reference. In other words: one pursues multi-sited ethnography because one knows, so to speak, that knowledge practices and objects are entangled, and that being differently positioned produces different perspectives.

Another imperative can be thought of as epistemological, emerging from an awareness of the way scholarly disciplines sustain critical edge through attunement to the historical context in which they operate. Cultural anthropology, in my view, has been exemplary in this regard, reliably recognizing that the ‘right’ focus, analysis or book to write at a given moment is literally subject to the discursive field in which it takes shapes and will circulate. Multi-sited ethnography allows a researcher to (often slowly) figure out what the discursive field(s) at issue are, and then to pursue an ‘object’ of concern to it. The contours of this object may take a while to become visible; figure and ground are not obvious, but something the multi-sited ethnographer must figure out. The right configuration is a matter of timing.

Finally, and significantly, there are ethico-political imperatives. Working across scale in multi-sited ethnography can produce understanding of the multiple pressure points where systems are subject to change, even if slowly or in ways hard to track. Points of entry light up, so to speak, through multi-sited, open-systems analysis. In analysing the Bhopal case, for example, I learned, through an interview in California with a former Union Carbide engineer, that the pay structure for Carbide engineers in the years leading up to the disaster rewarded the design of big projects. This, according to my interviewee, shaped the decision to design the Bhopal plant with larger rather than smaller tank storage – dramatically increasing the risk of disaster. If a small tank of toxic material had blown up, the consequences would not have been as catastrophic. Other forces also affected the magnitude of the disaster, of

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13 Consider, for example, how calls for prediction from the earth sciences were responded to, and in turn led to extraordinary developments of new techniques (particularly computational) for earth science analysis. The development of multi-sited ethnography amongst cultural anthropologists can be seen as analogous.

14 I write about oscillations between figure and ground in ethnographic work in ‘Figuring out Ethnography’, (Faubion and Marcus 2009). Here, it is important to note that allowing, even cultivating, this oscillation between figure and ground, context and scholarship, is, in some ways, at odds with a disciplined approach. But it is one way that scholarly disciplines can do something more than merely reproduce themselves, aggregating cases that confirm what they already know. The latter is important; testing knowledge across cases is critical to scholarly work, and often generates critical methodological reflexivity. Disciplinarity sets important limits. So, too, though, does the world and its temporal horizons. A temporal horizon – ‘the contemporary’, for example – can create a delimited space of work structured by something other than disciplinarity, yet leveraging what a specific discipline brings to its object of concern.

15 Not completely unlike what happens in gene expression analysis. Lights on the gene chip do not necessarily signal a problem, much less specify a corrective course of action. The lights do signal a need to pay attention to what is happening in a particular area.
course, but this information reinforced my belief in ethnography’s capacity to identify subterranean forces that can have dramatic effects.

Understanding how scalar differences can signal disjuncture and discursive gaps can also drive multi-sited ethnography. Dominant discourse about justice or about corporate power, for example, may be inadequate for actors of all sorts on the ground, signalling a need for word work – the creation of what Michael Fischer calls ‘lively languages’ (2003) – at the local level, responsive to what the world looks like from that vantage point. I took on this kind of work at the site of the Bhopal disaster, helping writing press releases and missives to the courts on behalf of gas victims. Multi-sited ethnography opens up these kinds of possibilities.

Striating such imperatives – rendering them ontological, epistemological and ethico-political – is itself a constructive move, with critical intent. It signals, once again, the need for and advantage of persistent recognition of the limits – and possibilities – of any analytical mode. Recognition of such limits, and possibilities, is true to the way knowledge really works (sic). As important is the way it compels movements to and explanation with different analytical angles and modes, leveraging rather than trying to manage away differences of perspective. Multi-sited ethnography calls for and enables this.

Again, informatics and the earth sciences are suggestive. Few practitioners in these fields would say that a given (often computational) model for understanding a system of concern is sufficient in itself. People invest heavily in particular models, striving to make them as true to what they claim to represent as possible. But there is also play and engagement with different models, and ready languages for explaining the limits of particular models and of modeling in general. This does not prevent poor or unintended use of model results by policy makers or journalists, among others. Methodological language and acumen does, however, prepare informatics and earth sciences practitioners to enter the fray, sharing the knowledge they know best how to create, aware that there will be a continual need to interpret what it means. Anthropologists should consider their example, and borrow their images.

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Chapter 4

In the Right Place at the Right Time?
Reflections on Multi-sited Ethnography in the Age of Migration

Ester Gallo

Unpredictable Movements

The main question addressed in this chapter refers to the significance of multi-sited fieldwork in my research experience among Malayali migrants in Rome and Ernakulam (Central Kerala, India). This question is particularly relevant to me as, in a way, I have retrospectively articulated fieldwork in Italy and Kerala in terms of multi-sited ethnography. The meanings of this perspective were not decided a priori when I started to work in Rome and were far from clear when I moved to Kerala.

The question emerges from my sense of unease with the often taken for granted association between ‘migration’, ‘movement’ and ‘multi-sitedness’. In his early account of the possible ways of constructing multi-sited analytical objects, Marcus (1995) identifies ‘following the people’ as one of the most obvious and conventional techniques allowing anthropology a certain degree of adaptation to contemporary migration processes. Indeed, as Hannerz (2003) notes, by the time the term ‘multi-sited’ was coined, migration studies had already recognized the importance of placing research at different ends of migrant trajectories. Nonetheless, migration has compelled anthropologists to develop new sensibilities beyond the traditional ‘research imaginary’ (premised upon the identification of people and culture with a bounded territorial dimension), as demonstrated by the well established literature on transnationalism (among others Rouse 1991; Glick Schiller 2003). The ‘research mandate’ of multi-sited ethnography ‘takes seriously the movement that constitutes the migratory processes’ (Fitzgerald 2006, 3). Nevertheless, it is my impression that the supposed obviousness of migration studies as a suitable field for multi-sited ethnography has often overshadowed the meanings of ‘movement’ involved in the processes of following people. In a thought-provoking article on Lebanese diaspora, Hage interrogates the reasons why mobility is uncritically taken for granted in many studies of transnational migration.\footnote{1 Though I agree with Hage on this point, I find his critique of transnationalism somewhat simplistic as he does not take into account the critical elaboration of this concept}
matrilocal move into his uncle’s house more significant than the move from a
Lebanese village to Boston – Hage underlines the ‘unpredictable meanings of
movement’ (Hage 2005, 470), whose articulations need to become the object
of inquiry rather than its premises. The understanding of movement is located
by the author within a constant tension of multi-sited fieldwork as pertaining to
‘either a thick ethnographer or no ethnographer at all’ (ibid., 465). What emerges
implicitly from his analysis is that this tension requires the ethnographer to make
selections and accept the ‘unknown’, a point which is not fully explored by the
author. Since he reflects on the hardship of expanding fieldwork in different sites,
one would have expected him to bring his critique of current transnational studies
to the point of interrogating the meanings of movement for the ethnographer and
how this informs the construction of a research site. Indeed, it seems to me that
one of the limits of current transnational literature on migration is the lack of
adequate attention to the implications that following people have for the way the
ethnographer makes methodological choices within an emerging range of possible
research directions that constitute a ‘site’.

Candea rightly notes that the overwhelming concern with ‘multi’ – connections,
movement, the act of ‘following’ – leads to the dissolution of the ‘site’ as an
‘anthropological heuristic device, an analytical framework’ (2007, 172). In
following people, the desire to study the multiplicity of their sites and the way they
do the bounding, obscures processes of our bounding as anthropological practice
(ibid.). Indeed, monographs on transnationalism often (unwittingly) convey the
impression that the sites of research were ‘out there to be discovered’. Comparing
these studies with my experience of fieldwork, I have often wondered if the authors’
initial methodological and research choices remained the same after engaging with
the second or third site. Could they have stayed focused despite the indeterminacy
of fieldwork? Did their relations with people change from one site to another and,
if yes, how did this shape their fieldwork and ethnographic accounts?

Certainly, my own multi-sited research on migration was the outcome of
often contradictory processes of selection in terms of what to research, if and
how to follow emerging potential paths, and, more importantly, why privilege
certain interests over others. Eventually, I had to recognize that whilst my
research developed from the intent to follow migrants ‘back’ to their native place,
this decision brought me towards unpredictable choices that made sites appear
disconnected and incoherent with each other. This is the product of a constitutive

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that followed its first launch by Glick Schiller et al. (1992). See, among others, Vertovec

2 An exception to this is Caroline Knowles’s (2000) account of her experience of
transnationalism while researching schizophrenia. While Knowles analyses personal
transnationalism through the lens of the relation between fieldwork and autobiography, I
am more concerned here with the impact of personal movement on processes of selections,
connections and dis-connections that characterise the construction of a site and the relations
between different sites of ethnographic inquiry.
paradox implicit in the different meanings given to multi-sited ethnography. As widely recognized, the importance of multi-sited ethnography is rooted in the recognition that the ‘field’ of ethnographic inquiry is not simply a geographical place waiting to be entered, but rather a ‘conceptual space’ whose meanings and confines are continuously negotiated by the ethnographer and their informants (Gupta and Ferguson 1997). It follows that the meanings of what a site will be are never predictable. Nor does multi-sited necessarily mean moving around in a literal sense (Falzon 2005). Nevertheless, the success of multi-sited research on migration has often being associated with the capacity strategically to select the ‘right’ sites, in order to limit the dangers of stretching already limited time and resources (Fitzgerald 2006).

Multi-sitedness thus implies the articulation of a priori selected geographical sites with the indeterminacy of what these sites will eventually turn out to be. This, it may be argued, is far from being new in anthropology, as one of the maxim students receive from their elders when leaving for fieldwork is to be ‘open’ to its unpredictability. However, multi-sited ethnography implies not only the capacity of being ready to put into question theoretical assumptions and research expectations that precede fieldwork; it also requires the researcher to put into question previous sites of ethnographic inquiry in light of new ones. This paradox is probably best understood if we consider the shift from the more formal understanding of multi-sited ethnography as articulated by Marcus (1995), to a more radical one. In the first stance, this strategy emerges from the objective of following a ‘known conventional process’ – this being a person, object or idea. But, in my experience, through the same process, what was (partially) known became almost ‘unknown’, as I had to locate my initial interests within a new set of emerging, unexpected relations and research possibilities. This brings us to a more recent elaboration of the concept, in the sense that the multi-sited field emerges from collaborations which fieldwork begins. However, following Candea, this understanding requires making explicit the ‘processes of bounding … which any ethnographer has to undergo to reduce the initial indeterminacy of field experience into a meaningful account’ (2007, 169). Making explicit processes of selections implied by following people does not lead us away from multi-sitedness towards a more traditional idea of fieldwork, as Marcus seems to imply in his earlier elaboration of the concept (see Candea 2007); rather, I believe, it is exactly what multi-sited is about. It should indeed be noted that partiality is implicitly recognized as a constitutive part of multi-sited ethnography, the latter being the product of knowledge of ‘varying intensity and quality’ (Marcus 1995, 100).

While I agree with Candea about the mandate to make explicit processes of selection that constitute bounded field sites, my analysis takes a somewhat different direction. Placing ‘bounding’ and the partiality of knowledge at the heart of multi-sited ethnography steers me clear of putting the ‘bounded field site’ in a dichotomous relation with a ‘multi-sited field’. Once we move away from the holistic aspirations of multi-sitedness, we can look at how its application results in the cross-fertilization and reciprocal limitations between different levels of
ethnographic perspective. Indeed, I am concerned here with an idea of multisitedness that implies both expansion and limitation of the ‘site’, as analytical framework and relational practice. An idea that goes beyond the Weberian ideal type towards its ‘corrupted’ and consistent forms.

The Sites … after Fieldwork

Before turning to discuss the choices that defined the sites of my research, let me just give a brief account of the way I look at them now. They reflect two moments in my formation as an anthropologist: as an undergraduate in Rome working on gender and family among lower-class Malayali Syrian Christian migrant women (1996 to 1998); and as a doctoral student, working in Kerala (2000 to 2002) on kinship, memory and the inter-generational politics of the family among Malayali urban elites.

My first project focused on migrant domestic labour and its impact on women’s family lives. It took the shape of a ‘community’ study, which initially traced the role of Italian political and religious institutions in shaping migrant experiences of journeys and labour. At a later stage I analysed the formation of women-centred kinship networks and their role in sustaining – financially, socially and emotionally – the arrival of younger relatives from India. A preliminary inquiry showed that women were mostly arriving in Italy alone and unmarried, and that marriage was a significant event in connecting their lives in Rome with their family and social background in Kerala. Arranged marriages with men in Kerala marked an important turn in women’s role as household providers. Their achieved maturity implied that they had to sponsor younger family members from their natal and conjugal families. Family connections and the enhancement of enduring kinship ties across Italy and Kerala were made more significant by a progressively restrictive Italian migration legislation. At the time of my research the safest ways to reach Italy were through family reunions for immediate consanguines and affines, or via ‘sponsorship’ with domestic work permits. The latter implied the presence of a relative already working for an Italian family. As I elaborated later, influenced by recent work on transnational households (Gardner and Grillo 2002; Salih 2003), being a legal migrant in Italy was intimately dependent on the possibility of belonging to growing kinship networks spanning different countries (Gallo 2006). My undergraduate thesis eventually explored the meanings of kinship in contemporary global migration and in sustaining people’s connections with different places. I limited my research to migrants’ narratives and personal genealogies as a framework of analysis, and used migrant marriages as a prism to explore wider processes of forging kinship across different countries.

My doctoral research addressed migration from a ‘marginal’ perspective. It aimed at understanding local ideologies of migration as a transforming process

3 Syrian Christians are a large and high status community of Malayali Christians.
in personal and collective biographies, but also to place mobility within the wider framework of lived experiences of political activism, social reform and intergenerational family conflict. Migration thus became one of many idioms expressing the formation of youth and elder culture in a society marked by a past of supposedly ‘revolutionary’ social reform movements and by a particular social development in terms of literacy, health provisions and birth control. An intergenerational approach allowed me to explore history and memory at the heart of family change and to explore conflict as an important renovating feature in kinship ideologies and practices. As will become clear below, I took an historical look at global mobility (from 1920 to 2000), and at the articulation across generations of different meanings of migration. My research reiterated previous experience in being urban focused; yet, compared to Rome, it required higher mobility on my part between different towns and neighbouring villages to follow family histories and relations. My Kerala ‘site’ departed from the previous one in that it nuanced the communit- based approach towards a more dialectical and comparative one across religious and class diversity. Finally, archival research and indigenous sources brought the issue of family and intergenerational politics into past and present public view.

Looking at these experiences through the ‘filter of biographical time’, they can be interpreted as two important moments in a ‘personal research trajectory’, where different interests developed thanks to the processual feedback between related ‘sites’ (Coleman 2006, 39–41). Processes of social mobility – upward and downward – within a framework of increasing competition (Italian vs Malayali, Malayali Hindus vs Malayali Christians) are an underlying feature that structures continuity between ‘my’ sites. The relation between religion, migration and labour ideologies is certainly another one. Yet what is also relevant in this multi-sited trajectory is that these two sites of research, while framed by reciprocal connections, also maintain their own individuality and ‘boundedness’. They are both sites where knowledge about Malayali social and geographical mobility was produced, but this knowledge does not necessarily speak about connections and openness. This resulted from different choices I made during the last months of fieldwork in Rome and, later, in Kerala. My second fieldwork period made Italy and Malayali life there more intelligible, in a way that would not have been possible otherwise. At the same time, it raised many questions and doubts about my previous experience and its naivities and partialities, and inevitably left ongoing processes of change among Malayalis in Rome significantly not known to me. This is not only understandable in terms of the ‘depth problem’ raised by the common sensation of chasing people around the globe – after all, traditional anthropological research has never been based on some timeless continuity in the ethnographer presence in the field. After my graduation I would have probably had to be away from my field in Rome in any case, even if I had not moved to Kerala to start new fieldwork.

I do not think here that ‘depth’ is the diagnostic distinction between traditional and multi-sited fieldwork, not least because what ‘depth’ is is itself
subject to contestation. The ‘myth of complete ethnography’ remains so both in ‘traditionally’ single-site studies and multi-sited ones (Falzon 2005). Equally important is the recognition that the cross-fertilisation of sites inevitably opens interstices of unanswered questions. For instance, the decision to focus on intergenerational discontinuities and conflicts in Kerala both resulted from and enhanced a preliminary interest of family change among Italian families (which made the presence of migrants so crucial). Looking at this phenomenon in Kerala highlighted the meanings of frequent tensions between migrants, Italian families and Indian families, but left many interrogatives on a changing ‘Italian’ family culture unexplained. Each site made migration as historical and social process more understandable in relation to the other. But the shift in focus, which seemed fundamental to me to understand the ‘Italian site’ from a broader perspective, also disconnected it in many ways from my subsequent research. Rather than seeing this partiality as a ‘negative’ departure from an idealized fieldwork, I look at it as a constitutive process of research sites as material and intellectual spaces.

Building a Multi-sited Research: Problems and Possibilities

Why go to Kerala? My Malayali friends in Rome were often talking places in their previous experience of migration, in their future life projects, or as destinations they knew about through relatives and friends. Their relation with Italy can be exemplified through the words of Salih when she notes (Salih 2003, 22), with reference to Moroccan women in Bologna, how:

The local space constituted by the actual town, city or rural area where women lived was but one and often not the most important, among the sites where Moroccan women developed their social and cultural relations. The women’s network stretched to multiple location that included in a continuum other European countries … Morocco and other locations that go beyond the local place which women physically inhabit.

Likewise, investing – financially and emotionally – back home or in other future projects of migration was for Malayalis a reaction to constrained life and working possibilities in a discriminating society. Beyond that, migrant narratives were indicating that what constitutes ‘Malayaliness’ – being educated, adjustable, movement oriented, and committed to one’s family – was intimately linked to being part of a network of cosmopolitan relations spanning across India, the Middle East and Western countries. Without being diasporic – as in the case of Sindhis (Falzon 2005) – Malayali geographical dispersion and sense of connections represented a constitutive feature of identity across different generations, where migration stood as an expression of personal and community life-cycle stage (Osella and Osella, 2000), often aimed at a future return to Kerala. The feeling that while researching in Rome I was encountering a wider reality of people’s lives, that their experience of migration in Italy was deeply informed by a map
of places and of possibilities of alternative lives, was a key factor in deciding to extend the research to a related context. In Rome I was often told that if I wanted to understand Malayalis I should not go to their native place but to the Gulf, where social life was conceived as retaining many traits of Kerala but expressed in a more modern and cosmopolitan way (see also Osella and Osella 2008). I excluded Gulf countries for funding reasons and because, despite the advice of my informants, what interested me most at that time was to understand how women’s pioneering role in the creation of transnational families was entwined with a long standing Malayali history of male dominated geographical mobility (Zachariah et al. 2000). Conflicts with relatives in Kerala over questions of family loyalty, dowry, honour, and consumption were addressing the persistent importance of this place in shaping and contesting meanings of what a ‘family’ and a ‘good migrant’ should be. Forced domesticity with Italian families and family expectations in Kerala seemed to be equally determining in shaping women’s labour experiences, household relations and future life projects. Moving to Kerala was conceived as a first step to map the ‘constitutive connections’ of emergent global phenomena umbrella termed as ‘feminization of migration’ (Anthias 2000), an expression that indicates the growing presence of unskilled and flexible women in the global labour force.

Subsequent fieldwork (2000–2004) was the product of at least two persistent methodological doubts. The first addressed the question of whether or not to concentrate on a select number of migrant families and ‘follow’ them genealogically. I had initially planned to limit my stay in Kerala to a few months and to focus on Syrian Christian families who had relatives in Rome. In my plan, fieldwork in Italy would have remained the temporal and spatial ‘centre’ of my research. In fact, for reasons that will become apparent below, Kerala progressively became the key place in my multi-sited trajectory. This decision came, partly, after I started to interrogate the reasons why it was so difficult for me to find continuity between my research experiences in Italy and Kerala. For my informants, a sense of connection between places was recognized as constitutive of their biographies and even of their relations with me. I knew many Malayalis in Rome and had visited some families during my holidays in Britain and Switzerland. This made me already expected and known in Ernakulam by many families. They knew many things about me and my family, my life in Italy and career plans. My initial ascription in Kerala to a certain network of relations connecting Ernakulam and Rome weakened the idea itself of physical and cultural separation entailed by the conventional idea of fieldwork. Indeed, despite spatial discontinuity between research sites, my membership in a translocal web of relations made my presence in different sites socially continuous. In Kerala I was already ascribed a position as a member of the Malayali Syrian-Christian community, not primarily because of my presence there or a common religious faith, but because of my association with Italy. Yet, while for the people I was meeting in Kerala a sense of connection between places seemed to inform the meanings of their daily relations with me, I
sometimes found myself imposing a separation between my research in Rome and the one in Ernakulam, and between my different positions among Malayalis.

Different representations of/by women were crucial in this. In Rome women were keen to present themselves as self-made individuals, and their visible role in communal public life resulted from their ability to mediate between Italian institutions, employers, migrant organizations and other Malayalis. I was conceived by most of them as a sort of younger niece who needed support for her work. In this vein, I was often asked to baby sit their children, which I happily accepted both to finance my fieldwork and to spend more time with them. Women took an active part in my research – as assistants, in finding materials, suggesting families to visit, taking me around, or tracing the genealogies of their families.

In Ernakulam women were hardly present in public life. I found myself hemmed in the domestic space and restrictions on travel and meeting people were applied to me as to the women I lived with. Still, this did not make me ‘one of them’, as additional restrictions came with my being a ‘western woman’, thus potentially raising issues of decency. Beyond the fact that this constituted participant observation and allowed me to gain certain insights into women’s lives, it also raised many questions about changes and continuities in my relations with them between Italy and Kerala. Despite solid friendships, my relations with men and women were tainted with tension, competition and suspicion that raised many questions on the role of conflicts between the ethnographer and informants in shaping fieldwork relations and outcomes (Gallo 2009). I found myself thinking about Malayali kinship and family life in Italy as radically different, if not antagonistic, to the more ‘traditional’ Kerala. Importantly, my different relations with migrant women in Kerala – if compared to the ones that characterized my fieldwork in Rome – made me come to understand their lives in Italy as a ‘liberation’ from oppressive patriarchal hierarchies. Whilst some Malayali friends agreed with this interpretation, I became progressively aware of the naiveties of this way of looking at women’s translocality. For instance, how to explain women’s active engagement in migration in the face of what seemed to me oppressive gender relations? Keralite conservatism was frequently addressed in Rome, but I struggled to explain the support women received from their families to migrate abroad, without falling in the stereotypical opposition between a ‘modern and liberating west’ and ‘conservative traditional orient’ (see Gardner and Osella 2003; Salih 2002).

It obviously took me many months to articulate the problem in this way. I spent a long time wondering if it was worth limiting the research to the movement of Syrian Christians between Italy and Kerala or if my interrogatives would be better addressed through a broader perspective on kinship, politics and family change among a not yet well circumscribed group of Malayalis. Should it be an ethnography of connections, or one of wider social mobility and historical transformations? I eventually took my research away from Italy and planned longer term fieldwork in Kerala. I progressively decided to locate migration to Italy within a deeper historical perspective on gender and mobility, linking this to the colonial and postcolonial debate on the ‘Malayali modern family’ and household reform. Also,
it encouraged me to interrogate the meanings and confines of the nuclear family model and the role played by migration in it. This meant, among other things, a more focused use of biographies across generations, a comparative interest in class diversity, and documentary research. My new orientation was not the outcome of a one-off hunch but resulted from my being multi-sited also in terms of being located in different institutional settings (De Neve and Unnithan 2006; Gallo, forthcoming). The possibility to share insights and doubts with Malayali academic peers and seniors helped me considerably to nuance my initial assumption and to take different fieldwork directions.

One important issue that emerged from these interactions was the need to understand the ambiguities of women’s active involvement in migration in light of public discourses on modernity between the 1920s and 1940s, when women’s role within the household as active domestic subjects was an integral part of a public project aiming at the consolidation of the conjugal/nuclear family (Devika 2007). During this period, paid employment for the women belonging to the emerging Hindu and Christian high status/middle class was projected as an extension – and thus a continuation – of their domestic agency within an ideal model of modernized patriarchal order (ibid.). In the same vein, one could interpret the success of family planning and sterilization policies in postcolonial Kerala (Devika 2002), as a device to ‘liberate’ women from domestic affairs and to free them to work outside the house. Placing the history of Syrian Christian migration to Italy within a deeper historical perspective of family reforms and gendered mobility, helped me nuance my initial way of looking at contemporary women’s migration as radically antagonistic to a traditional patriarchal order.

Women who migrated to Italy since the late 1960s come from the lowest subsets of the Syrian Christian community and, relative to their better placed counterparts, have not benefited from previous forms of social mobility. Indeed, influenced by the work of Malayali historians, it seemed possible to me to interpret this specific pattern of migration as a strategy aimed at developing active domestic subjectivity and creating modern nuclear families in Kerala within a dominant frame of modern patriarchal relations (Gallo 2006). In relation to my everyday field relations, this perspective helped me be more conscious of the family dynamics involved in women’s migration and of the ambiguities surrounding their role in paid employment. Yet, the affirmation of a conjugal family in postcolonial Kerala seemed to a certain extent problematic in light of the changes that migration brought about. Field data in Rome and Kerala indicated that the meaning of ‘household’ and the gender hierarchies underlying it were more complex and nuanced than the ideal type envisaged by the colonial reformers in the decades before Independence and, later, by the Keralite (family) legislation and reforms. Transnational households were shaped by the imperative of the

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4 Mainly the higher subsets of Hindu Nayars and Syrian Christians. These communities benefited greatly from migration, educational and occupational possibilities promoted by the colonial presence since the early decades of the twentieth century.
nuclear family as a modern achievement but also by the necessity to make kinship an important resource and, no less importantly, by the willingness of migrants to renew a common sense of family belonging across places. Following Waldinger and Fitzgerald (2003, 21), the genesis of the transnational household could be interpreted as the ‘collision of the social organization of migration, and its state spanning results, with those processes undertaken by states and civil society actors to produce state-society alignment’. Indeed, if my research on kinship may be defined as ‘transnational’ in the literal sense of the term, it is in that it attempts to bring Malayali national family projects and reforms, the role of Italian laws and domestic service regulations, and processes of household formation, into a single analytical framework.

In the process of expanding and bounding the sites of my research, many important connections became relevant. It was the result of cumulative prolonged fieldwork, where related sites were both constituted through reciprocal interrogations and questioning but, no less importantly, by the acceptance of incompleteness. The strategy I chose helped me understand how women’s presence in the global labour force, and the concomitant formation of transnational households, intertwined with ongoing historical processes of family change in Kerala. But, at present, and despite subsequent fieldwork in Rome, my knowledge of the impact of ongoing Italian legislation on migrant family and labour strategies is very fragmented. I know nothing, for instance, about marriage practices among migrants who arrived after 2000. However, it is through the raising of questions, and the recognition that these may not be answered by our personal multi-sited research trajectory, that sites feed back into another and open future research possibilities. I shall now turn to my second methodological doubt.

**Competing Histories and the Politics of Places**

In 1995 Marcus (1995, 112) underlined how

> In contemporary multi-sited research … the ethnographer is bound to encounter discourses that overlap with his or her own. In any contemporary field of work, there are always others within who know (or want to know) what the ethnographer knows, albeit from a different subject position, or want to know what the ethnographer wants to know.

My second doubt emerged from the realization that in Kerala migration is popularly a hotly debated and sensitive subject. Deciding ‘what sort of migration’ or ‘on which caste, class or religious community’ one might concentrate, entails political implications for our field relations (Gallo, forthcoming). This is because the networks produced by people’s movements across countries generate ‘not one but a multiplicity of imagined communities, organized along different, and often conflicting principles, whether related to the scale of aggregation (local
In the Right Place at the Right Time?

In Ernakulam different historical and geographical experiences of migration informed the construction of community identity but also the establishment of a constitutive difference within and across communities. In this highly differentiated context, some expressions of geographical and social mobility are conceived as more prestigious and representative of ‘Malayaliness’ than others. Histories of migration are inscribed in collective biographies in a way that differentiate and privilege certain practices to the exception of others. It is against this reconfiguration of social spaces that new migrants from the socially and economically marginalized sections of Malayali society have to find recognition. Thus, I became aware that female migration to Italy is considered demeaning compared to current skilled migration of high class Hindu Nayars, Brahmins, or Syrian Christians. This perspective, commonly shared by many middle and upper class Hindus and Christians, rules out that contemporary skilled migration is partly the outcome of a status achieved in previous decades through women’s migration within the British colonial space (Kurien 2002). My field interest in lower class women working in the domestic sector was not welcomed. Lower class female migration reiterated a degrading image of servitude but also the contested identification of Christians as money oriented people who do not mind jeopardizing their family honour by hiving off their daughters. Interestingly, my being Italian was often associated with less prestigious forms of migration, if not criminal ones. Quips along the lines of ‘Italy is not well known for exporting doctors, is it?’, ‘We have not exported the Mafia but engineers’, or ‘You are just like those little girls travelling alone abroad without their family’, represented frequent thought provoking statements aimed at building hierarchies and distance between differently mobile subjects.

Fieldwork in Italy was assuming new meanings in light of the encounter with divergent and competing histories of translocal lives. It raised the point that in order to understand the meanings of Malayali migration to Italy, I should have located it within a more complex network of power relations reflecting different migration strategies and their public representations. During the first months of research I began to realize that people’s everyday lives in Kerala were deeply informed by proximity and competition between different sections of Christians, Hindus or Muslim society, who may have made migration a successful source of recognition back home. My impression was that the significance of transnational networks and cosmopolitan orientations coexisted with the crucial role of Kerala as a place where processes of social mobility achieved through migration had to be legitimated or sanctioned, and that this exercised a persistent importance in migrants’ personal and collective biographies. When travelling between places, people are also moving within ‘localized’ and everyday ‘experienced hierarchies’ – with respect to the cross-cutting dimensions of class, caste, gender and religion. Gains achieved through migration and transnational lives were often legitimated with reference to these hierarchical social spaces. This does not mean that these hierarchies were predetermined or predictable. Rather, they were continually
transformed, reinforced or put into question by people’s experiences of different places. Places are located by people’s discourses and practices of migration within a hierarchy of possible destinations, and this has significant implications on the way migrants build and negotiate their identity in contemporary Kerala. As noted by the Osellas, ‘the particularities of one’s position in country of origin and destination are more salient in shaping experience and evaluation than any commonly shared experience of migration’ (Osella and Osella 2008, 149). These complex particularities are often organized in Malayali discourses in a triangular framework in which Kerala is pitted against the ‘West’ and ‘Gulf Countries’, each corner reflecting different experiences and imaginaries of mobility (ibid.). What was emerging from my field was that this tension between migration as a common feature of Malayaliness and its hierarchical representations was expressed through ‘essentialist’ associations between places and naturalized attitudes and qualities. Brahmins, for instance, asserted their recent presence within the skilled migration flow to the USA, making natural an ideological association between the intellectual capacities of a superior caste with the elitist possibility of migration to prestigious places. Similarly, migration to the Gulf was often associated with the lowest class or the nouveau riches. The configuration of places such as Italy as a degrading destination where unskilled jobs are available for the lower section of Malayali society, was more often than not a discursive strategy of the established upper middle class to de-legitimate competing processes of social mobility. This hierarchical geography of places deeply informs present day processes of social mobility and identity renovation in Kerala. The relation between identity and places also illuminates processes of subject construction (Gupta and Ferguson 1999). Within this process a tendency towards what the authors describe as ‘an erosion of the cultural distinctiveness of places’ (ibid., 37) – entailed, for instance, in Malayali discursive assertions such as ‘Kerala is becoming like the Gulf’ – seemed to be ambiguously accompanied by the formation of an essentialized geography of places, where people are often conceived as more ‘suitable’ or ‘predestined’ than others for peculiar destinations.

During my second fieldwork sojourn in Kerala I decided to broaden the spectrum of my initial interest to place the histories of migration to Italy within longer and divergent histories of mobility. This should have allowed me to gain better insights into the Italian case but also to look at migration across competing histories. This would have meant not only a comparative perspective between different communities, but something closer to the approach adopted by Caroline and Filippo Osella in their study of Izhava (Osella and Osella 2000b). It presupposes a dialectical openness that takes into account how each competing social stratum defines itself through contrapuntal relations with others, whilst maintaining a focus on a single community. I decided to concentrate on the urban elites – namely Hindu Brahmins, Nayars and Syrian Christians – for two main reasons. First, because their common belonging to the newly established or

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5 See Osella and Osella (2000b).
aspirational ‘modern’ urban middle class was specifically based on the ideological refusal or celebration of certain forms of migration. I thought that an ethnography of how elites respond to spreading forms of competition from lower class migration would have, in Bourdieu’s sense (2004), highlighted from a different perspective the use of ideologies and practices of migration as a device of social distinction in past and everyday social competition. Second, this elite embodied different and often conflicting histories of migration: the fact that present forms of professional mobility and transnationalism were based on generational engagement or refusal of migration since the early twentieth century, made them particularly interesting as exemplars of global forms of mobility within an historical perspective.

Conclusions

I have reflected here upon the possibility of looking at multi-sited ethnography as a research path that entails the coexistence of different levels of ethnographic perspective and that goes beyond the opposition between a ‘traditional’ single site/in-depth fieldwork and ‘modernist’ field-openness and translocation. In migration studies, multi-sitedness has all too often been associated with the movement of people and less so with the implications that ‘following people’ has on our methodological orientations and field relations. The apparent opposition between different modes of ethnographic inquiry becomes less sustainable once we explicate and analyse processes of expansions, selection and bounding that equally pertain to a single site as well as its relations with possible others. Making this process the object of our analysis highlights the dialectical process of feedback and reciprocal limitations between sites; indeed, as the product of our changing intellectual, relational, methodological and geographical positioning. In sum, what is so particular about multi-sitedness is the possibility it offers to interrogate the ‘site’ of research, not as a preconstituted dimension of social inquiry, but as relational process and methodological device.

The choice of conjugating different modes of doing fieldwork between Italy and Kerala has certainly created many questions on how to construct a coherent object of analysis. Engaging critically in this process has left me with many doubts on the research path I have forged during these years – on its ramifications and partialities – that are recurrently present in the products of my research. What I have tried to highlight through the analysis of the doubts that oriented my research are the different meanings of multi-sitedness and their role in shaping my research. A multi-sited approach meant for me the attempt to locate the ‘Italian site’ within a lived geography of destinations and power relations and to move with people in their different histories of mobility. Multi-sitedness had a lot to do with following people between different sites but, more importantly, it entailed the readiness to question these sites in light of others.

In my experience, multi-sitedness represented a methodological device to understand continuities and disruptions between competing histories of
translocal mobility and to gain a certain insight on the lived historical depth of this phenomenon. It certainly offered insight into the ambiguous relation between ‘Malayaliness’ as an idiom of cosmopolitanism, openness, and connections, and its closures and boundedness in hierarchical differentiations. It made me more aware of the tensions between the attractive possibilities of self-identity mongering that translocal lives promise, and the reality of locality as a place of struggle where to find legitimate recognition as emerging subjects. In this respect, placing histories among other histories of migration and following ideologies of translocality across generations and class diversity may contribute to re-historicize present forms of global mobility. Indeed, while multi-sited ethnography may constitute an important step towards researching global phenomena, it could also be interpreted as a strategy that absolves us of the dichotomy between a ‘closed past’ and an ‘open present’ (Waldinger and Fitzgerald 2003, 24) or, better, a sort of hyperglobalism, which in this sense means an over-emphasis on the novelty and the importance of global connections and displacement in people’s everyday lives.

References


6 I take this expression from Assayag and Fuller (2005).


Glick Schiller, N. (2003), ‘The Centrality of Ethnography in the Study of Transnational Migration’, in Foner (ed.).


During the course of my fieldwork with Friends of the Earth International (FoEI)\(^1\) it became apparent that the spatial relations FoEI activists experience in their interactions with each other around the world were not simply different spatial ‘configurations’. The sites where FoEI activists engage with each other, and produce what it means to be a FoEI activist, are not different to ‘single-sited’ ones in the sole sense that they are geographically ‘placed’ differently to other social groups. Rather, FoEI activists experience and engage with different types of emplacements, or topologies, simultaneously through the different environmental relations they engage in as part of their daily practices. As a result my fieldwork could be described as neither multi-sited nor single-sited. What was required of me was to pay attention to the different environmental relations that co-produced different experiences of emplacement.

The practice/theory of multi-sited methodology stimulated, at least in part, the rich discussion about ‘the field’ in anthropology (Marcus 1995; Marcus 1999; Hannerz 1998; Marcus 2007; Gupta and Ferguson 1997; Amit 2000; Candea 2007; Coleman and Collins 2007). The sensitivity towards ‘sites’ implicit in this body of work provided the analytical and practical tools that enabled me to explore FoEI activists’ experience of emplacement. In this chapter I first outline how multi-sited practice/theory enabled my research project. In the subsequent section I sketch the discussions about ‘sites’ and ‘the field’ which point to an issue that deserves further analysis. Namely, that the notion of immobile, bounded cultures has led to

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\(^1\) The fieldwork for my doctoral research project consisted in six months’ participant observation with FoE Brazil, five months with the FoE International Secretariat in Amsterdam, six months with FoE Malta as well as the three years between 2003 and 2006 during which I engaged with FoE Malta as an activist; I also attended nine international meetings between 2003 and 2007, and did continuous email participant observation throughout the period from February 2003 to December 2007. I am grateful to the University of Aberdeen’s Sixth Century Studentship award for funding my doctoral research project. I would also like to thank Tim Ingold, David Anderson, Mark-Anthony Falzon, Arnar Arnasson, Jo Vergunst, the FoEI activists in Malta, Brazil and Amsterdam, and the participants in the University of Aberdeen’s writing-up seminar, in particular Sophie Elixhauser, Katy Fox, Amber Lincoln and Richard Muscat for their comments on earlier versions of this chapter.
a strengthening of the approach that posits people’s relations to the environment as separate to their social relations. I then trace the source of this separation, discuss its flaws and present the alternative as already developed in anthropology. This discussion shows how I arrived at the particular conclusions about the co-production of emplacement by FoEI activists and their environmental relations. First, however, it will be useful to provide an outline of FoEI so as to elucidate the instances in which these issues arose.

**Friends of the Earth International**

On its website FoEI is described as the ‘world’s largest grassroots environmental network, uniting 69 national member groups and some 5000 local activist groups [on every inhabited continent]’ (<www.foei.org>). It goes on to say that the network unites around two million activists.

FoEI differs from many other environmentalist federations because, rather than being solely concerned with nature conservation, its approach is also inclusive of social issues. A number of scholars (Doherty 2006; Timmer n.d.) as well as many FoEI activists contrast FoEI with Greenpeace. The two organizations are thought to differ primarily due to FoEI’s emphasis on a decentralized organizational structure. Indeed, FoEI expresses this focus on decentralization by referring to itself as a ‘grassroots’ federation. Although the actual discourse and practice of decentralization in FoEI deserves further analysis, the ideological aim of decentralization in FoEI has resulted in a significant diversity of member groups and types of environmentalism.

FoEI membership is restricted to one FoE group per country. Common membership requirements include independence from religious or ethnic movements, political parties and economic interest groups, and a democratic, non-sexist structure. However, specific group agendas are significantly diverse, ranging from nature conservation – such as Global 2000 (FoE Austria) – to environmental and human rights advocacy – such as Legal Rights and Natural Resources Center, Kasama sa Kalikasan (FoE Philippines) – and each group has its own mission statement. The value given to being a ‘grassroots’ federation is partially expressed in the importance assigned to this internal diversity. Also, a central interest of many activists is maintaining and developing the discourse and practice of decentralized decision making as an important component of their environmentalism.

FoEI was formed in 1971 as a loose network of environmentalist organizations from France, the UK, Sweden and the USA for the purpose of coordinating their international collaboration. It was initially modelled upon the representative structure of the United Nations (FoEI Archives, Amsterdam). The aim was to maximize the representative value of their claims: the claims made by FoEI activists to

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2 Except in the UK, where there is a FoE Scotland and a FoE EWNI (England Wales Northern Ireland), and in Belgium, where there is a FoE Flanders and a FoE Wallonie.
representatives of national governments at international meetings were meant to echo and supplement campaigns being run in national contexts. Such international contexts included United Nations and other intergovernmental meetings, such as those of the International Whaling Commission. Decentralized decision making was one of the central ideological/political choices that informed the foundation of the network and its subsequent growth (FoEI Archives, Amsterdam). At this time the activities of the network were primarily annual meetings bringing together activists from the national FoE groups, where they exchanged ideas and strategies for their plans for the upcoming year. Beyond the meetings, little else was formally required of the groups.

However, in the 1980s, the activities of and interactions between the national FoE groups became more structured and in 1981 an international secretariat was established. The secretariat rotated, operating from a different member group’s office each year (FoEI Archives, Amsterdam). A number of current activists interpret this rotation as a result of two factors. First, it was a necessity imposed by the lack of resources of any one group to maintain their national work as well as the secretariat’s for a lengthy period. Second, it was a deliberate means of keeping in check the power of the secretariat and preventing the control of the secretariat by any one group. In 1983, when the network had 25 members, the secretariat stopped rotating and settled in Amsterdam.

In the first 15 years FoEI activists organized annual meetings, met at international intergovernmental meetings where they were campaigning, visited if they happened to be on holiday in the vicinity of a FoE office, and corresponded (substantially less than nowadays) by means of a mailed newsletter, mail and, later, fax. Today, FoEI activists interact considerably more. Biennial General Meetings (BGMs) are statutorily required. During these meetings an Executive Committee is elected to run the day-to-day decisions of the Federation for the following two years. This Committee is composed of activists from different FoEI regions in order to obtain ‘regional balance’, that is, approximately an equal number of representatives from each region. The Executive Committee meets at least quarterly. A large international meeting is also organized in the intervening year between BGMs.

The FoEI regional groupings in turn hold their own annual meetings. Currently, the regions are Africa, Asia Pacific, Latin America and the Caribbean (ATLAC), Europe (FoEE) and North America. Although FoEE was set up in 1985, and ATLAC in 2001, the FoEI website and other FoEI-wide activities became sedimented along ‘regional’ lines during the early stages of a ten year Strategic Visioning and Planning Process (SVPP) that the network embarked upon in 2004.

FoEI campaigns are organized into programmes. Each programme has a coordination group, also composed according to ‘regional balance’. Depending on the programme, the coordination teams meet three to four times a year. FoEI member groups often participate in joint projects – sometimes restricted to FoEI members, at other times in collaboration with other organizations – which may entail further meetings.

Apart from face-to-face meetings, activists interact by telephone, telephone conferences (Internet telephony services such as Skype have made telephoning even
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more widespread), email and email groups. ‘FoEI All’ is an emailing group open
to any FoEI activist who asks to be added to the list of addressees. Also, individual
programmes, campaigns and projects each have their own emailing group. Over and
above, the groupings with which I did fieldwork, FoE Malta, FoE Brazil, and the
International Secretariat, each have their own internal emailing groups.

In most of these contexts only FoEI activists participate; however, as part of
their work activists also engage in numerous other forums with activists from other
networks and also with non-activists. This intense exchange, no doubt catalysed by
the new communication technologies is, amongst other things, an expression of the
drive to produce communal decisions, campaigns and outcomes. In FoEI communal
decision making is an ideological objective possibly on a par with that of environmental
protection. It is recognizable in the activists’ concern to ‘have mandate’ from their
group to take particular positions and decisions. This objective is expressed in FoEI’s
organizational structure and requires considerable effort to enact.

Multi-sited Practice/Theory as Enabling

At all stages of my research, discussion about multi-sited\(^3\) methodology provided a
thought provoking interlocutor with which to identify resonances and differences
within the milieu of FoEI. It was also pivotal in stimulating my sensitivity towards
‘sites’ and emplacement. The work on multi-sited methodology was enabling in
three particular ways.

First, it suggested particular methods with which to get started. In the light of
FoEI’s organizational structure it was necessary to explore the relations between
FoEI member groups in order to understand the constitution of the group. Marcus’s
(1995, 106) programme offered the practical suggestion to follow people, their
lives and biographies (ibid., 109). The ethnographies of Garsten (1994) and Falzon
(2005) suggested that for various reasons, three was a practical number of locales
to work in. Likewise, I chose to carry out fieldwork with three FoE groupings: the
FoE International Secretariat, FoE Brazil, and FoE Malta. These methods were a
guideline by which to go about trying to understand how FoEI ‘hangs together’
(Hannerz 1996, 4).

Second, the discussions about the binaries global/local and system/lifeworld
and nation-state (stimulated by multi-sited practice/theory) were particularly useful
in formulating questions of relevance to FoEI due to its particular ideology and
structure. FoEI activists seem to live a contradiction. On the one hand the prevailing
environmentalist ideology in FoEI recognizes Earth as a continuous, interconnected
and interdependent world. On the other, its membership structure, divided along
country borders, implicitly endorses the appropriative logic of the nation-state, which

\(^3\) The term ‘multi-sited’ includes research that is defined as transnational. For this
reason I use the term multi-sited practice/theory to include the literature cited about
transnational research.
does not incorporate the continuity and interdependence of ‘the environment’ as understood in classical environmentalist terms. As such the points Marcus (1998) raises about the dichotomies lifeworld/system and global/local resonate with the experiences of FoEI activists, who also deal with these dichotomies.

Marcus’s (ibid., 40) critique of the dichotomy lifeworld/system is that it homogenizes the notion of the ‘system’, while assuming that ‘local’ (subaltern) lifeworlds are diverse. Multi-sited research offers a possible means to collapse the dichotomy by providing ‘ethnography of complex connections’ (Marcus 1999, 50) between places that are produced in/of (as well as produce) a world system. Haraway (1991, 190) proposes that since all knowledge is situated, in order to ‘see well’ it is necessary to explore different situated knowledges. My choice to carry out fieldwork with the particular three groupings in FoEI aimed to explore different types of situated knowledges within the FoEI federation.

Third, the discussion around the constraints of multi-sited methodology offered a rich body of thought from which to ask further empirical and theoretical questions in my research context. The most common concern raised about multi-sited methodology is the loss of depth. The compelling response to the problem of depth is that a multi-sited project is not by definition comparative (Hannerz 1998, 237). Besides, Falzon agrees with Hannerz that ‘the notion of a “complete ethnography” was always something of a myth’ (Falzon 2005, 21). As such Falzon shows how his ethnography of the Sindhi diaspora focuses on one important aspect of the lives of the Sindhis he researched: the translocal aspect. The practice/theory of multi-sited methodology emphasises this point. It is the connections and the relations across what are considered at times to be different sites that are investigated (Marcus 1995, 1998) – creating in effect a single research project.

Appadurai (1991) proposes extending Anderson’s imagined communities to ‘imagined worlds’ in order to understand such relations. In a similar vein Rouse (2002) describes the ‘lifeways’ of Mexican migrants to the USA as a ‘community dispersed in space’. He elucidates this spread-out community through their travels back and forth between Mexico and the USA, the fact that they equip their children with familiarity of both contexts, but also through their continued participation in social obligations and relations at a distance primarily by means of the telephone. The emphasis of multi-sited practice/theory on people’s relations that do not necessarily depend on continual face-to-face interaction achieved two things: it destabilized the model of participant observation and its inherent bias towards bounded sites (Gupta and Ferguson 1997; Amit 2000; Coleman and Collins 2006); and it drew ethnographic attention to how such relations are lived (Garsten 1994; Rouse 2002; Falzon 2005). Finally the practice/theory of multi-sited methodology equipped me with an indication of what to pay attention to during fieldwork in order to explore, among other things, the non-face-to-face relations (if there were any) that kept FoEI ‘hanging together’ (if it did). This education of attention⁴ is

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⁴ For an exposition of how learning is an education of attention in a person’s total field of relations, see Ingold (1993) and Ingold (2001).
what allowed me to perceive some discrepancies between the lived experience of such relations and current anthropological discussion about them that this chapter addresses.

Multi-sited Practice/Theory and Analyses of the ‘Field’, ‘Location’ and ‘Site’

The context of recent discussions of the ‘field’ and ‘location’ in anthropology is closely related to the practice of ethnography that is in some way multi-sited (Gupta and Ferguson 1997; Amit 2000; Coleman and Collins 2006). The burgeoning studies addressing migration, transnational corporations and occupations, media, tourism and cyberspace (Hannerz 1998) implicitly challenge the anthropological fieldwork method of long term engagement in a single place. The anthropologists engaged in these areas encounter people whose lifeways are constituted to a significant measure by movement and flow – either their own or that of images, ideologies, commodities or capital (Inda and Rosaldo 2002). The life experiences of such people challenge the immobility and boundedness demanded by the view of the world as a mosaic of geographically delineated cultural wholes. Anthropology remains committed to developing theory drawn from empirical findings, or that at least does not contradict the experience of informants (Coleman and Collins 2006; Tsing 2002; Wikan 2002). As a result multi-sited ethnography has reinvigorated a rejection of a bounded understanding of cultural difference. In practice this has generated an attention towards ‘sites’ (Marcus 1995, 112).

The interrogation of anthropological ‘field sites’ has brought into question the limits of ethnography. The Malinowskian archetype fieldwork – fieldwork based on prolonged participant observation in a relatively small, geographically delineated place – has become the hallmark of anthropology, distinguishing it from other disciplines, such as history and cultural studies (Gupta and Ferguson 1997, 3). Crucially, Gupta and Ferguson (ibid., 12) show how the mosaic model of cultures, though rejected as a theory, is nevertheless perpetuated in the notion and adherence to the Malinowskian archetype of fieldwork. They examine a number of aspects of how fieldwork is carried out and talked about that in particular maintain the notion of boundedness. These include tropes of entering and exiting the field, the hierarchy of field sites and the emphasis on face-to-face participant observation.

Gupta and Ferguson (ibid.) point out how discussion of the fieldwork method inevitably calls up discussion about the field of anthropology as a discipline. But they, as well as Coleman and Collins (2006), denaturalize the imperative of Malinowskian fieldwork by tracing the history of the methodology, and highlight other methodologies, orthodox and heterodox, in anthropology’s past. They demonstrate how Malinowskian fieldwork has not always defined what anthropology is and does, thereby enabling a wider discussion of methodology in anthropology.

Almost ten years later, Coleman and Collins’s (2006) introductory chapter, addressing very similar issues as Gupta and Ferguson’s (1997), is still relevant and topical. This possibly suggests that the Malinowskian fieldwork archetype
is still hegemonic. In addition to deconstructing the centrality of Malinowskian fieldwork Coleman and Collins (ibid.) trace how boundaries, embodied in the distinction of a single geographically delimited field site, have been useful for cultural comparison in anthropology. Synecdoche and bounded sites coexisted in creating the anthropological subject, providing the cultural difference anthropology is supposedly the specialist of. However, with ‘globalization’ the synecdoche local/global does not work, since the global has no lines of division along which comparisons can be made. Thus, while for Gupta and Ferguson (1997) multi-sited practice/theory challenges the limits of Malinowskian fieldwork method by destabilizing a dependence on the archetypal ‘natural laboratory’ of the bounded field site, Coleman and Collins (2006) draw attention to the perceived threat to the anthropological context (created by regional comparison) that ‘globalization’ studies usher in.

Neither authors accept that the challenges to Malinowskian fieldwork signal the demise of ethnography; like Amit (2000), they offer proposals for a reinterpretation of fieldwork. Gupta and Ferguson (1997) argue that the anthropological ‘site’ is a social construction. Thus in order for contradictions between theory and practice to be avoided, what needs to be explored is the ways people construct locations (ibid., 35–9). While they do not propose any single alternative methodology to single-sited fieldwork, they do propose ‘shifting locations’, as opposed to ‘bounded field’ as a way of defining where and about what anthropological fieldwork is done.

Amit (2000), criticising the persistent image of the solitary fieldworker, argues that in the current atmosphere of reflexivity it does not follow that ethnographic texts omit fieldworkers’ personal relations. She argues that the ‘field’ is constructed by disentangling and describing certain relationships and not others, in order to provide one contextualisation and not another out of multiple possibilities.

What Coleman and Collins (ibid.) add to the notion that the ‘field’ is a social construction is that anthropology remains committed to investigating the lifeways of its informants. As such fieldwork is emergent from both the agency of the researcher and the serendipity of the unexpected. They suggest ‘performing the field’ as a way to liberate fieldwork from ‘the purely spatial metaphor of the field’ (ibid., 12) while maintaining the mutual creation of ethnographic knowledge by researcher and informants. In resonance with ethnographic experience of mobility and flow, these writers are reacting against the immobility and boundedness of single-sited fieldwork. The solutions they suggest – ‘shifting locations’, ‘the constructed field site’, and ‘performing the field’ emphasize the social construction of place, location and field site.

In sum, multi-sited practice/theory has stimulated a productive discussion about ‘place’ and ‘location’. This discussion, whilst describing current trends, also brings them and the issues they raise into mainstream attention, allowing them to be more widely discussed. These analyses pave the way for methodological change in orthodox understandings of fieldwork as well as addressing the feared demise of ethnography or anthropology as we know it that such a change would purportedly bring. The proposals that liberate fieldwork from geographically
bounded metaphors enable anthropologists to do research into different or new areas of human experience, as it enabled my research with FoEI. Additionally, reflexive analyses about fieldwork also encourage further investigation and debate. In fact there is one point in particular in the solutions against the bounded site that calls out to be debated: the proposal that regarding location and situation (including the location of the field) as being socially constructed has the effect of detaching sociality from emplaced lived experience.

The detachment of the ‘social person’ from the human organism and the non-human environment, in other words from their lived emplacement, is explicit in most of the literature discussed above. Gupta and Ferguson call for location to be understood as social construction, yet they do not mention the environments in conjunction with which such constructions are made. Coleman and Collins explicitly state their position that anthropology is about social relations, agreeing with Hannerz that it is only derivatively about place (2006, 12). Marcus (1998, 45) suggests that multi-sited fieldwork transcends the local/global dichotomy by ‘experimenting beyond place-focused … ethnography’. In his defence of the bounded field site Candea (2007) makes the same shift as those promoting multi-sited fieldwork. He proposes that field sites offer windows onto social complexity and that the location of the site is arbitrary – which implies that any particular environment bears no relationship with the social relations that occur there.

In order to understand the relationship of how social groups cohere, anthropologists have gone from one extreme to the other; from immobility and bounded cultures to dis-emplaced mobility and flow. On one hand the shift has enabled the extension of anthropological research into areas of human experience that the bounded site methodology did not allow. Yet, inadvertently, the recourse to detaching people’s social relations from specific environmental, emplaced relations in order to explain mobility and flow, perpetuates the same assumptions about human make-up that established single-sited fieldwork in the first place.

The Detachment of the Social from Emplacement

Gupta and Ferguson (1997, 6–8) highlight that Malinowskian fieldwork was part of the same movement as fieldwork in natural history. The movement considered that empirical information collected in the ‘natural environment’ was the most reliable. Another assumption inherent in this movement is that so-called ‘primitive’ societies were closer to nature. Therefore ‘complex societies’ could not be studied in their ‘natural environment’ because civilisation had the effect of detaching people from ‘nature’.

Durkheim expounds on the different proximity of societies to ‘nature’ in The Division of Labour (Biernacki and Jordan 2002). He argues that if the practices of a group of people – provided that group was not too extensive – are attached to a concrete landscape, say to a river or a forest, then individuals would automatically follow shared norms and beliefs (ibid., 136). When, however, people interact with
others from different places (with different rivers and forests), their relationship to the environment becomes abstract. Collectively they do not relate to a forest, but to the abstract notion of forest (ibid.). Through this abstraction, social beliefs, norms and relations form society that is sui generis and follows its own internal mechanical laws not influenced in any way by the environment. The social connections between people therefore become social space, and rather than relating to the environment, social collective beliefs transfigure the non-human world (Hatch 1973).

As Biernacki and Jordan (ibid.) show, social space abstracted from ecological relations is understood in the sociology of Durkheim to be an inevitable outcome of history. The more complex the division of labour in a society, the more social relations become detached from the environment. Biernacki and Jordan go on to show how, similarly to Durkheim, Henri Lefebvre argues that with the onset of modernity people’s relationship with lived places became increasingly dominated by abstract space. In fact, Lefebvre (1993, 123) admonishes anthropologists from investigating ‘social space’ where no modernity is to be found, since they will not find it there. This ontology, shared by Durkheim and Lefebvre, separates those people who on the one hand have ‘social relations’ and who inhabit ‘social positions’ or ‘social space’, but therefore are not influenced by the ‘environment’ and on the other hand those who have ‘culture’ and who need to be studied in their ‘natural environments’.

The practice/theory of multi-sited methodology follows the same line of thought as the Durkheimian/Lefebvrian association of modernity with a detachment from ‘place’, that is relations with a non-human environment. The progression that is envisaged goes something like this: from pre-modern/close relations with the environment to modern/social relations abstracted from the natural environment. This is a shift from understanding people as inhabiting places to inhabiting abstract space. Places are ‘loci defined by the layout of a setting for practice’ (Biernacki and Jordan 2002, 134, my emphasis); in other words the multiple fields of relations people work through and with in their daily lives. On the other hand space is abstract and conceived as

> a naturally given, grid-like platform for human conduct … reducible (or already reduced) to a formal (that is, empty) schema or grid. It establishes a featureless, purely extensional arena for action apart from its occupation by an embodied agent (ibid., 133).

When for instance Marcus suggests that multi-sited fieldwork can trace relations that are on a ‘differently configured spatial canvas’, the implicit notion is that of isotropic space, a background canvas upon which social relations can have different arrangements. In this view, ‘the landscape itself is rendered uniform; it is reduced to “space”, a vacuum to the plenum of culture’ (Ingold 1993, 226).

The majority, if not all, multi-sited, transnational and globalization studies reviewed by Marcus (1995), Hannerz (1998), and Inda and Rosaldo (2002) explore in some way aspects related to capitalism or colonialism. Further, although it
is always qualified, the recent interest in mobility and flow is explained by the development of communication and transport technology (Inda and Rosaldo 2002; Eriksen 2003). Transport physically moves people and commodities from place to place; communication technology moves capital, ideologies, images, emails and voice. As such, due to the perceived ubiquity of the abstracting and detaching tools of modernity, people’s relations with the environment are also understood to be everywhere abstracted, mediated by social positions. Thus the understanding that social relations are separate from people’s relations to places – in other words environments implicitly endorse the ontology of natural history in which Malinowksian fieldwork was established. Only that now ‘natural environments’ no longer exist and everything is about ‘social’ location.

Haraway argues that infinitely mobile vision is a disembodiment from situated vision and as such is an extension of the ‘god trick’ (1991, 188–9). Visualizing technologies have turned the illusional ‘god trick’ of seeing everything from nowhere, into seeing everything from everywhere and over and above insert this into everyday practice (ibid.). Therefore, the proposal to focus entirely on social relations as a solution to the immobility and boundedness of single-sited fieldwork perpetuates the ‘god trick’ by not exploring the particular environmental relations in conjunction with which mobility and flow are made. Of course I am in no way trying to revive the cultural ecology of the 1960s. Rather, the proposed answers to this conundrum are found in the relational approach in anthropology as well as in science and technology studies.

In a parallel discussion in anthropology, Ingold (2000, 215) has argued that ‘the ancient division of the human being into two parts or aspects, social subject (person) and biological object (organism)’ has also been maintained in the scholarship on landscape and that on embodiment. The Cartesian mind/body separation, which in anthropology has been discarded as an overarching theory, is maintained by the continued lack of interest in ecological relations of the organism-person as a whole. Therefore he suggests that to avoid this untenable separation, researchers should direct their attention to the organism-person who participates in a field of ever-unfolding relationships with the manifold constituents of their environment (ibid.). Latour (1987) coins the word ‘actant’ to denote the mutual constitution of a milieu by human and non-human aspects.5 Haraway (1991, 198, my emphasis), similarly insists that

Situated knowledges require that the object of knowledge be pictured as an actor and agent, not a screen or a ground or a resource … In some critical sense that is crudely hinted at by the clumsy category of the social or of agency, the world encountered in knowledges projects is an active entity. 

5 Actants are not ready-made entities. Latour (2003) shows how they emerge in the ongoing process from being matters of concern to being temporarily instituted as ‘essences’, that includes humans.
Ingold and Lee (2006) provide a practical example of what this sort of research would look like. In a project about walking in Aberdeen they show how whole persons, as organism-persons, and the environment they walk through co-produce routes of sociality (ibid., 72). Sociality here does not denote a division of human social persona from the physical world and non-human aspects of the environment, but indicates the mutual constitution of that ‘route’, that ‘way of life’ by humans, the road, the weather, other people.

Finally, apart from perpetuating the Cartesian mind/body split, the assumption that environmental relations are detached from social relations has a specific implication for research. In imagining places as isotropic space, this notion is projected onto the lived experience of the subjects of research, when in fact their experience of emplacement is far from homogenous. In the following section I attempt to convey some of this diversity through a few aspects of FoE activists’ different experiences of emplacement. These do not ‘transcend’ place, but are made possible and co-produced by what the manifold non-human as well as the human constituents of their environments afford.

**Neither Multi-sited nor Single-sited; rather, Topological Multiplicity**

My fieldwork with FoE Malta continued while I was in Porto Alegre and in Amsterdam. I kept in touch with FoE Malta activists on an almost daily basis first via email, then Skype. I was active in FoE Malta email discussions, taking decisions, proposing ideas, editing documents and press releases. Though FoE Malta and FoE Brazil have regular contact with the International Secretariat, they did not at the time directly cooperate on any projects together. On the one hand FoE Malta and FoE Brazil activists consider themselves constituent actors of FoEI, and they feel that any work done by FoEI in any part of the world is part of their FoEI too. Thus, my fieldwork was single-sited in the sense that I was working with a circumscribed group tied by thick social relations, albeit dispersed in a much larger geographical area than traditional single sites. On the other hand, certain distinct historical, geographical, and ecological aspects of the places FoE activists inhabit – including the nation-state – are the basis upon which the FoEI structure is modelled. In this sense the fieldwork was multi-sited since the geographical distinction between sites is a constituent part of their very relations. The spatial assumptions implicit in the terms used in anthropological methods did not match the FoE activists’ experiences of space. I came to realise that neither the term ‘multi-sited’ nor ‘single-sited’, adequately described the places I was doing fieldwork in.

FoE activists experience, inhabit and co-produce at least three types of places simultaneously. They consider the whole world a single site – indeed the whole universe, according to those FoE activists who are also budding astronomers (a belief common to the environmentalist ideology of the Gaia hypothesis) (Milton 2002, 31). FoE activists will recognize and act upon a mutual recognition of belonging to FoEI regardless of the explicit diversity of the individual FoE
activists and their divergent ways of being environmentalists. At the same time, the activists engage in certain practices and hold certain beliefs that configure their lived emplacement into bounded sites – divided along national lines, divided into different ecosystems with fuzzy borders, and often along political positions or group affiliation. Each of these very different ways of experiencing places and the social relations they afford, are emphasised depending on the activity at hand, but they are always at play even if not being focused on at all times.

The different experiences of emplacement are contemporaneous aspects of how activists constitute and experience being part of FoEI. At first sight these different types of emplacement appear to be what Tsing (2000) refers to as ‘ideologies of scale’. However, as Strathern (1991) has argued, ’scale’ belongs to a particular intellectual tradition and as such the particular ways FoEI activists construct ‘scale’ itself need further examination. In fact, the experiences of emplacement that I encountered with FoEI activists resonate closely with Mol and Law’s (1994) work on different topologies.

The notion of topology is useful in discussing FoEI activists’ emplacement for various reasons. Firstly, topology being the study of how place is experienced, it incorporates the imperative of questioning environmental relations, rather than assuming what places/sites are and how they are constituted/experienced. Secondly, as proposed by Biernacki and Jordan (2002), ‘abstract space’ may still be a part of people’s experiences and as such should be used both as an analytical tool as well as recognised in empirical investigation. Similarly, the topologies Mol and Law describe include attention to how ‘abstract space’ is enacted in what they call ‘regional topology’. Thirdly, the notion of topology also avoids the further binarism of place/space, which as a dichotomy – since binary opposites imply mutual exclusivity – makes it difficult to understand how both can be simultaneously part of experience. Although Mol and Law (1994) also separate the social from its lived environment, stating that their topologies are only metaphorical for social situation, their development of the notion of topology, as well as the actual ethnography they write, affords attention to environmental relations.

Through their research on anaemia, Mol and Law (ibid.) identify three, non-exclusive, non-exhaustive types of emplacement. In other words three types of topologies: regional, network and fluid. Crucially, the different topologies are not mutually exclusive. They coexist and their particular constitutions are emergent from the relations between the different types of topologies (ibid., 663).

Regional topologies are created when the differences within a geographical region are subsumed to create a homogeneity that matches with geographical, metric borders (ibid., 646). Therefore the organizational structure of FoEI based on national member groups, as well as their endorsement of geographically distinct ecosystems, resonates closely with ‘regional topology’. In the offices of FoE Brazil (Nucleo Amigos da Terra, NAT) in Porto Alegre, Brazil, Mariangela had just downloaded Google™ Earth on to the computer where she worked. Amy, Mariangela and myself

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6 All personal names are pseudonyms.
sat around the computer as they tried it out for the first time. They used it to identify the locations of their activities in Porto Alegre, in other parts of Brazil and to spot my house in Malta. We spent the afternoon zooming in to different regions of Brazil and the world. Mariangela particularly enjoyed the way the names of cities, towns and localities appeared as the satellite image on the screen zoomed closer to the ground, the way that when the image zoomed out to view the world from ‘Space’ you could click on any visible land mass and the three dimensional globe image would rotate smoothly, simultaneously beginning to zoom in, the land taking up more and more space on the computer screen, till the names of countries, then cities, then towns began to appear. Google™ Earth is one of the visualizing technologies that according to Haraway (1991, 189) disseminate the illusion of disemodied (from situated positions) vision. As such, it is through Google™ Earth, the world maps on the walls of their office, aerial photographs, air travel and other such experiences that by their daily usage incorporate FoEI activists’ experience of regional topology.

By contrast, in a network topology, proximity is not metric. Rather it depends on access to parts of a system (or an infrastructure) (ibid., 649). Therefore the FoEI activists’ close ties that are developed over the Internet, telephone and such, exist in and create a network topology. During a FoEI General Meeting in Nigeria in 2006, Internet was only available on two computers. Most of the activists were not used to this. Whereas normally they would be following international news websites, the activists now came to know of the news that North Korea had officially launched a nuclear programme through a text message (which service was in any case only sporadically available) to the representative of FoE Japan. Someone jokingly commented that not having their usual access to communication media felt like they were holding the meeting in a bubble. Calhoun (cited in Hannerz 1998, 248) argues that the Internet and telephone are an indirect means of interaction. But many FoEI activists, indeed many people who regularly use the Internet and telephony (Rouse 2002) do not feel it is indirect. While they do not confuse it with face-to-face presence, the various communication media are experienced as a distinct type of presence. This is a different experience of place; a network topology. However it can only successfully occur in the way it is currently experienced by FoEI activists if, among other things, the landscape (for mobile phone reception), the tools (for Internet access to computers, plus climate control since computers tend to be allergic to hot and humid conditions) and the infrastructure (access to a relatively stable electrical supply), join forces. The meeting in Nigeria felt like an isolated ‘bubble’ because certain constituents of the environment did not afford the kind of presence co-created in a network topology.

Finally, whereas in network topology what creates ‘places’ are sets of similar elements and similar relations between them, fluid topology denotes experiences where both the elements and the relations between them can vary, while remaining recognizably distinct. Fluid topology is defined by this ‘invariant transformation’ (Mol and Law, 658). When in 2003 I met Anastasia, the coordinator of FoE Cyprus in a project staff meeting in Malta, we were the only two FoE activists among a group of environmentalists from other groups and contexts. After I gave my presentation about
FoE Malta and our particular work on the project, Anastasia approached me and set about asking after FoE Malta members whom she had got to know over the years. We had never met before, yet she spoke to me with familiarity and pointed out who other people were, and told me stories about FoEI meetings and about environmentalist politics. Although the proximity that I experienced could be taken as ‘social’, in the sense that what counted to Anastasia was the FoEI diacritic of identity, by attending subsequent meetings and meeting more FoEI activists there I realised that Anastasia was teaching me how to behave as a member of FoEI. However, I was not only learning with my social person, my whole self was learning, learning how to sit slightly inclined towards the other FoEI activist, learning how to look and notice if others had a FoEI sticker on their laptop or diaries, and learning how to welcome fellow FoEI activists through an enthusiasm not communicated in words, but in orientation expressing interest with the whole body.

The different types of emplacement that I encountered with FoEI activists elucidate that any sociality needs to be understood as being mutually constituted by a field of relations within which the FoE activists, as whole organism-persons, are engaged, rather than established between disembodied social personae. Moreover, due to the different environments activists constantly encounter, their experience of place, location or sites is far from isotropic. Different types of presence or emplacement can be felt simultaneously, and these places change depending on the environment the activists are relating to, including, but not exclusively, other people.

**Conclusion**

In the process of theorizing about mobility and flow – as opposed to the implicit orthodox immobility and boundedness of the ‘field’ – multi-sited practice/theory has so far proposed a further detachment of sociality from emplaced lived experience. This solution perpetuates the assumption of division of mind and matter without exploring neither non-dualistic experiences nor the different ways such a duality could be lived (see for instance Porath 2007). I have shown how the notion that people’s social life is detached from their environmental relations, now more than ever because of increased mobility, forwards the same social theory embedded in Malinowskian fieldwork that couples modernity with such a detachment. However, the debates multi-sited practice/theory have stimulated have drawn attention to the issue of ‘sitedness’, which for my research project provided a backdrop of thought against which to explore how FoEI activists relate to their environments. Through my analysis of some aspects of FoEI activists’ experiences I have sought to show that it is necessary to further develop our sensitivity to how people relate to their experience of emplacement and the environmental relations together with which such places are co-produced. This shows that there is more to say about people’s experience of ‘place’, at least that of FoEI activists, than a detachment of the social from the environment allows.
I have conveyed the emplaced relations of FoEI activists with their environment in my ethnographic descriptions in order to try and demonstrate the mutual constitution of the different types of places. However, an open question remains as to which are the different, and which the most useful, ways to understand and describe those fields of relations organism-persons are part of. What I hope to have done in this chapter is to have brought the discussions about environmental relations (Ingold 2000; Ingold and Lee 2006; Latour 1987; Latour 2003; Haraway 1991; Biernacki and Jordan 2002) to bear on issues of multi-sited practice/theory in a way that stimulates reflexive analysis and exploration of method that is perceptive to organism-persons in their emplaced relations.

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Chapter 6
Expanding Sites: The Question of ‘Depth’ Explored
Cindy Horst

Introduction

A widespread observation in the literature on multi-sited ethnography is that multi-sitedness is inevitable in dealing with the realities of many people’s lives today but at the same time compromises the anthropologist’s ability to conduct in-depth, holistic fieldwork. However, in a multi-sited approach the ‘fieldsite’ is perceived in a transnational way which questions common assumptions about what is in-depth and what is not. Further, most of the more interesting multi-sited research that has been conducted, has been based on in-depth fieldwork among one ethnic group, with sites added on over a period of time. In this chapter I shall argue that, when doing fieldwork on transnational networks and flows amongst members of the same community in different contexts, depth and multi-sitedness can well be combined.

I will illustrate this through my own experiences during a decade-long involvement with Somalis, which took me from the Netherlands to Kenya, the USA, Norway and Somaliland. In these different sites, I worked on various types of transnational engagements between Somalis. Originally, one of the issues I studied was how, due to transnational flows of information, refugees in remote camps in Kenya compare their lives in the camps to those of others elsewhere and cultivate dreams of resettlement to a third, typically western, country (Horst 2006a; Horst 2007b). Besides these trajectories of desire, made possible through the expansion of the global media, I studied financial flows between different sites. Focusing on remittances, I was mainly interested in the implications of transnational financial ties between Somalis for their local livelihoods (Horst 2006b; Horst 2007a) and social interactions (Horst 2008a). Another aspect of transnational networks and flows that I am increasingly interested in is the political engagement of Somalis with their country of origin (Horst 2008b).

The experiences of many of the Somalis I spoke to were multi-sited, and could not have been understood to the same extent without my first-hand knowledge of some of these different contexts. Knowledge about both the contexts informants themselves had lived in previously, as well as those they were connected with transnationally or lived in currently, was crucial in my ability to understand and analyse my data. Further, contacts in one site allowed me to gain access to another, and over the years my network expanded to include Somalis in a good
number of sites. Whereas I believe that the expansion of research sites over time has great potential for research on transnational practices, I also see a number of challenges. Engaging in simultaneous research through teams of qualified researchers based in the different sites of study, and developing electronic tools for collecting data that are not ‘located’, may address some of these.

In this chapter, I first discuss changing perceptions of the fieldsite. I then analyse the linkages between multi-sited ethnography and theorizing on transnationalism, and their methodological implications. In particular, when studying transnational networks, the question is whether matched samples are needed; when studying transnational flows, decisions need to be made on whether to do this simultaneously or not. I also discuss various analytical and practical challenges in doing multi-sited fieldwork, and possible ways of dealing with them. Finally, I argue that, since present day ethnographers commonly work in multiple fieldsites, we need to start addressing different questions than the ones that stem from a concern with a ‘holism’ that is still based on assumptions about the ‘rooting’ of people (Malkki 1992).

Changing Perceptions of the Fieldsite

Multi-sited ethnographies have over the last two decades become an increasingly common product of social scientific research, in particular that which studies the movement of people, objects and/or ideas (Appadurai 1986; Levitt 2007; MacGaffey and Bazenguissa-Ganga 2000; Stoller 2002). It is firstly argued that this type of approach is best suited to observing different types of ‘motion’ (Fitzgerald 2004, 2). In one of the classical articles on multi-sited ethnography, Marcus (1995, 106–10) distinguishes six examples of this approach, namely following the people, the thing, the metaphor, the plot, story or allegory, the life or biography, and the conflict. Secondly, and relatedly, various authors hold that multi-sited fieldwork allows us to study networks particularly well (see for example Hannerz 2003b, 21). In sum, applying multi-sited methods enables us to study the field as a network of localities which are linked to each other through various types of flows.

Thus, a multi-sited approach does not just mean a multiplication of the number of fieldsites. Rather, the turn from single-sited to multi-sited methodological approaches may be seen as a critique of traditional fieldwork in light of what these types of studies do and do not allow us to study. It has been argued that single-sited fieldwork is insufficient for capturing the complexities and multiple causalities of contemporary social systems and structures. The move towards multi-sited methods is commonly discussed with reference to models of globalization and ‘modernity’, in the framework of which individuals lead increasingly segmented lives and the world becomes increasingly connected (Hannerz 1996; Eriksen 2003). These observations have led to intense debate on the meaning of place and space, and on the meaning of the fieldsite in anthropology and other social sciences (Fog Olwig and Hastrup 1997; Gupta and Ferguson 1997; Hannerz 2003a). Through
these debates, the model of constructing the field as a closed local society has been discredited in both anthropology and sociology.

The general debate that attempted to break with the idea of ‘community’ and ‘culture’ as a bounded entity, was stimulated in the 1980s and carried on throughout the 1990s (see for example Kearney 1986; Marcus and Fisher 1986; Kearney 1995; Marcus 1995). Marcus, for instance, argues that activities and identities are ‘constructed by multiple agents in varying contexts, or places, and ethnography must be strategically conceived to represent this sort of multiplicity’ (1989, 25). Not only has the assumption that a geographical boundedness justifies studying a location in isolation been countered with the idea that sites are not separate but continuously connected, but various authors have stressed the fact that ‘the local’ is shaped by ‘the global’ and vice versa. Marcus and Fisher (1986) point to the fact that research on local and regional worlds tends to underestimate the transnational political, economic and cultural forces that shape the local context. At the same time, as others have indicated, the implications and transformations of global processes are ‘grounded in cultural constructions associated with particular localities’ (Fog Olwig and Hastrup 1997, 12). It is crucial to understand the relevance of local contexts in mediating the scope and depth of migrants’ transnational practices.

Whereas, according to some, multi-sited methods have evolved because of the changing realities of the people studied, others have argued that they have become more popular also because of the changing realities of the scholarly life, with fieldwork nowadays being carried out ‘now and then’, ‘in between’ our lives (Hannerz 2003a, 212–3). These more mundane issues may be far less visible in the debate but are no less important; although it is necessary to have an understanding of the methodological reasons for the development of multi-sited approaches, these practical explanations and implications need to be explored as well. In my view, sites are usually selected on the basis of a combination of factors related to their relevance and convenience. For example, the fact that Norway became a site for my research on Somali transnational activities and networks from the perspective of the ‘wider diaspora’ (Van Hear 2002), can mainly be explained by two very practical facts: first, I have a young family and am neither able nor willing to spend months doing fieldwork in refugee camps; second, I was offered a researcher position in Oslo. At the same time, there were also some very solid reasons for studying remittance sending practices amongst Somalis ‘at home’ in Norway: Norway hosts 20,000 Somalis, which makes them one of the largest refugee groups and the largest African population in the country. Further, the only available way to transfer money to Somalia, the hawala, is operating illegally in Norway. This has weighty implications for how transnational remittance responsibilities and local ‘integration’ processes are negotiated by remittance senders and receivers, which is a key question in our current research.
Simultaneous and Matched?

As mentioned, the use of a multi-sited approach has been identified as especially suitable for studying movement and networks. As such, there is a clear link between the burgeoning literature on transnational flows and networks, and the increase in multi-sited studies. The concept of ‘transnationalism’ indicates social relations and exchanges over and beyond nation-states, without however disregarding the importance of borders. Basch et al. (1994, 7) define transnationalism as ‘the processes by which immigrants forge and sustain multi-stranded social relations that link together their societies of origin and settlement’. The authors term these processes transnationalism ‘to emphasise that many immigrants today build fields that cross geographic, cultural and political borders’ (Basch et al. 1994, 7). Similarly, multi-sited fieldwork is linked to the epistemological shift described earlier, in which the field is not a geographical place waiting to be entered but a conceptual space whose boundaries are constantly negotiated and constructed (Gupta and Ferguson 1997). In this understanding, the focus is on transnational interconnectedness and the links between ‘the local’ and ‘the global’. As such, I hold that the term ‘multi-sited fieldwork’ should be limited to describe fieldwork that has a specifically transnational focus – as opposed to scholars who use the term to discuss comparative case studies (see for example Bloch 2007).

Mazzucato (2008) suggests that the greatest methodological challenges in multi-sited transnational research are, first, how to capture the linkages between the different locations, and, second, how to understand the simultaneity of the flows with which these linkages are created and maintained. In Table 6.1, I identify a number of different options for dealing with these challenges in different ways, reflecting the most common approaches taken. Multi-sited research may be carried out by a team of researchers, but it is often also carried out by a single individual. If it does involve a team, research can be carried out simultaneously or step-wise. Simultaneous research would be research in which the flows studied are approached from both (or more) sites at the same time. If the research is carried out by an individual, it is inevitably step-wise. Many anthropologists use this approach (Al-Sharmani 2006; Horst 2007a; Stoller 2002), which Riccio (2009) terms ‘multi-sited ethnography as a virtual spiral’.

To my knowledge, a research project by Mazzucato et al. (2006) is the only study so far that has produced data on transnational flows simultaneously. True simultaneity implies studying the flows of information, money and goods from the sending and receiving sides while they occur. Such an approach improves the quality of data due to triangulation opportunities and the fact that it is possible to study people’s actual actions rather than their discourse on those actions (Mazzucato 2008). Whereas these are very convincing arguments for simultaneous research, conducting multi-sited fieldwork in a step-wise manner has the advantage that each stage benefits from the previous one, both practically and intellectually. An individual researcher can benefit from a step-wise approach by building on existing networks in one site to develop connections in another site (Fitzgerald
Expanding Sites: The Question of ‘Depth’ Explored

In fact, as Hannerz (2003a, 207) argues, even the selection of sites is the product of a gradual and cumulative process as new insights develop, new acquaintances are made and new opportunities come into sight.

My research experiences over the last decade are a clear example of a ‘spiralling ethnography’ of the transnational practices of a specific group of people. A one year fieldwork period in the Dadaab refugee camps in northeastern Kenya, and shorter, intermittent fieldwork periods in Dadaab and in Nairobi over the years, formed the starting point of my academic journey. The camps are located in a semi-desert area and currently host around 224,000 refugees at the time of writing. The large majority of refugees originate from Somalia, with smaller numbers coming mainly from Ethiopia, Eritrea, and the Sudan. My original research interest concerned the effects of refugee life in the camps on existing social security arrangements of Somalis, which I defined as consisting of a high degree of mobility, strong assistance networks, and dispersed investments in a variety of people, places, and activities. I found that, for a majority of the population in the camps, these strategies, which had developed from the local circumstances of life in Somalia, were still relevant in Kenyan camps but had acquired a far more transnational character (Horst 2006b, 3). In fact, the camps could be seen as one of the many points of complex and constantly shifting networks among Somali families which included refugees, internally displaced persons and citizens of a variety of other states.

Whereas refugee camps are commonly imagined as isolated places in borderlands, my research shows how inaccurate this image is. When camps are studied as bounded units, researchers remain blind to the fact that these camps are connected to a wide variety of places through flows of remittances, images, goods, and people. Refugees in Dadaab move between the camps and Nairobi or Kismayo; they receive remittances from Johannesburg and Minneapolis; and they communicate with relatives in Cairo and London. Further, visions of these different places reach Dadaab through for example Bollywood movies, Somali theatre plays and pop songs dubbed in Columbus or Toronto, brochures from distant universities, and pictures of relatives and friends in faraway places. Thus, images of the lives of others and a rich, ever-changing store of possibilities to be lived are presented (Appadurai 1996, 53). In this process, the lines between the

<table>
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<tr>
<th>Sample</th>
<th>Researcher</th>
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<th>Team</th>
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<td>Simultaneous</td>
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Note: \(^a\) Partly matched.

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2004; Monsutti 2004).
real and the fictional are blurred, especially for those far removed from the reality described (Horst 2006a).

A transnational focus allowed me to explore some of these issues while in Dadaab, but mainly from the perspective of those who lived in the camps (although I had worked for the Dutch Refugee Council for many years and thus had some understanding of the position of refugees in a European setting as well). This limitation was pointed out to me while I engaged in discussions on my research on Somali websites, receiving excellent feedback from readers worldwide (Horst 2006c). A number of Somali students in Minneapolis told me that I would not be able to understand the full depth of the transnational connections that enabled refugees to survive in Dadaab by infusing money and dreams into the camps, without having studied the position of the providers of those remittances and images. So I accepted their invitation and conducted a preliminary study on transnational connections in Minneapolis, later followed up in Norway with a more in-depth study on the interrelations between local lives and transnational remittance practices, and the implications of this interrelation for the social dynamics between senders and receivers.

Throughout this cumulative process, not only were sites added with increased insights and to increase insight, but, more importantly, my understanding of the transnational field gradually became more in-depth. My research focus in Norway was shaped by the work I had done earlier in Dadaab and Minneapolis, and subsequent studies in Dadaab benefited from insights gained in Minneapolis and Oslo. For example, in Dadaab I learned how some of the refugees put pressure on their relatives to send money, so in Minneapolis and Oslo I tried to understand what this pressure meant for those who were expected to send. At the same time, having stayed in Dadaab for extensive periods, I could understand the real moral challenges Somalis faced while in Oslo or Minneapolis, themselves struggling with their daily lives but at the same time being constantly reminded of transnational inequalities. Arguably, a similar process of understanding would develop for work done simultaneously, though it would be far more compressed. These are important considerations to take into account when deciding on a step-wise or simultaneous approach.

Another essential choice to make in multi-sited fieldwork is whether to have a matched sample. A matched sample is where the unit of analysis is constituted of networks of people who are connected across national boundaries (Mazzucato 2008). In my case, this would have meant that, during my research in Minneapolis, I should have interviewed the relatives of those refugees I interviewed in Dadaab, in order to understand from both angles the transnational field that connects them. As Mazzucato (2008) rightly points out, a considerable number of projects study transnational flows generically, without actually matching the sample of senders and receivers involved. Matched samples have advantages in providing more precise information about the inner workings of transnational flows and linking migrants’ actions directly with those of people back home, rather than assuming
such linkages on the basis of discussions with comparable ‘categories of people’, such as remittance receivers.

At the same time, matched samples are extremely time consuming to establish and maintain, and the results are not necessarily that dissimilar from those produced by unmatched samples. More importantly, there are pressing ethical dilemmas involved in matched samples where the researcher has more detailed private information than the informant themselves about the situation of close relatives or friends. The question on how to deal with this information, for example whether or not to share it and what implications this might have for the research findings and the writing process, is particularly challenging. Relatedly, I experienced difficulties when using matched samples between Kenya and Norway, in two ways. On the one hand, contacting the Norway-based relatives of people interviewed in Dadaab increased the expectations of those located in Oslo as to my ability to assist their relatives through family reunification programmes. On the other, asking for the contact details of relatives in Norway led to suspicion amongst those interviewed in Kenya. In general, relatives do have knowledge about the difficult legal position their relatives in Europe are in, and they know that some of the information that their relatives provided to the authorities may not be accurate. They obviously do not want to jeopardize their relatives’ legal status. Further, some individuals have quite detailed knowledge about the position of Somalis in Norway, and are concerned that the information they provide may be used adversely because of the negative discourse on Somalis in the media (Fangen 2006) and the fact that remittances cannot be sent through legal means from Norway.

Challenges in Expanding Sites

Whereas the value of a multi-sited research approach in studying transnational practices is clear, I wish to explore a number of the reservations that have been expressed about the method, in particular on a practical level. One of the most commonly cited scepticisms about conducting fieldwork in several sites, is that it compromises one’s ability to understand a single context in sufficient depth; it is argued that a ‘thick’ understanding, commonly associated with anthropological fieldwork, is rather difficult to obtain. This clearly leads to questions about the value of the research, and questions on whether it deserves to be called ‘ethnography’ at all (Fitzgerald 2004; Hannerz 2003a). As Burawoy (2003) bitingly puts it, ‘bouncing from site to site, anthropologists easily substitute anecdotes and vignettes for serious fieldwork’.

This criticism is countered by several authors who argue that it is based on an all too conventional understanding of the field and the site (Hannerz 1998; Hannerz 2003a; Hendry 2003; Wittel 2000). As Wittel (2000) convincingly illustrates, the debate on whether multi-sited fieldwork can have sufficient depth is really one about different modes of ethnographic complexity. In a way, one may argue that fieldwork in one location is as partial as multi-sited fieldwork is, since it fails
to analyse connectedness (Hannerz 1998, 248; Hendry 2003, 500) and focuses instead on boundaries. Long term participant observation in a locally limited area may well over-privilege face-to-face relationships and overlook more mediated forms of interactions. It also privileges permanent residence at the expense of movement (Wittel 2000). This when, as Appadurai (1996, 54) argued over a decade ago, ‘ethnographers can no longer be content with the thickness they bring to the local and particular’.

Serious ethnography is commonly thought of in terms of ‘thick description’ (Geertz 1973), which is based on the premise of full immersion in a certain culture as the only way of understanding its complex ‘web of meaning’. This seems to imply that multi-sited research can only produce ‘thin’ descriptions of each specific site and culture. Yet the purpose of multi-sited fieldwork is not a ‘full’, located, understanding of ‘culture’, but rather one which targets transnational networks and flows. This kind of study ‘neither searches for deep dimensions within a culture, nor for hidden layers of meaning. Instead, culture is created in the area of the ‘in between’, it is a dynamic process about becoming and fading away’ (Wittel 2000, 5). Thus, multi-sited research might not be able to provide ‘thick’ description of the individual nodes, but it does guarantee ‘thick’ description of the network, its dynamics and the interplay of relations between people, things, activities and meanings. In my view the ‘depth’ with which transnational practices are understood increases with the expansion of sites. At the same time, however, we should realize that there is an inbuilt incompleteness in multi-sited transnational work as well, in the fact that the sites selected only provide a partial (and often small) overview of the total network and flows within it (Hannerz 2003a, 207).

Related to this first, most commonly encountered, criticism and its counter, is the fact that many practical difficulties have been identified in relation to guaranteeing quality multi-sited research. The bottom line is that it is often considered to be too expensive and/or too time consuming. There are two aspects to the issue of time management. First, the researcher has to apportion their available time between different sites. Obviously, the greater the number of sites, the less time available for each individual node or connection between nodes. Second, a number of authors argue that within a given multi-sited project, it will take longer to build up and gain access to a network of informants. Whereas in single-sited fieldwork, it is common to make use of only a small number of gatekeepers, who often double as key informants and take formal or informal leadership positions in the community, in multi-sited fieldwork the same procedure has to be rehearsed for each site, allowing less time for the actual research (Wittel 2000, 5). In practice, however, the researcher may be able to utilize the very transnational networks studied to find gatekeepers even before physically arriving at a site. In truth, my own experiences in Norway were not very encouraging in this respect since, because of their marginalization, Somalis in Norway turned out to be wary of being researched. Consequently, trust building swallowed up a considerable portion of my research time in Oslo.
Another commonly expressed practical concern is the fact that multi-sited methods may be difficult to implement by an individual researcher as they require multiple linguistic and cultural competences. Multi-sited work tests the limits of a method usually thought to rely on deep, local knowledge of everyday interactions as a means to understand social relations. The requisite intensity of fieldwork and linguistic competence may be difficult to achieve in multiple sites, with consequences to the quality and consistency of the work (Fitzgerald 2004; Hannerz 2003a; Marcus 1995). Ideally the researcher should be able equally to understand all local contexts included in the research, but this raises questions as to whether it is possible to have the sufficient cultural competence to deal with multiple sites (Hannerz 1998, 249). In my case, I was clearly disadvantaged in not knowing the Norwegian context well enough, or being fluent enough in Norwegian to have a deep contextual understanding of Somalis in Norway. One obvious solution to this problem is collaborative research and/or teamwork; this is discussed in the next section.

Possible Strategies towards an In-depth Multi-sitedness

Various suggestions have been made as to how to deal with these various challenges, including conducting focused research that is theory driven, working as part of a team, and conducting multi-disciplinary and multi-methods research. One proposed solution is to be very focused in the kinds of issues that are studied transnationally. Rather than cultivate holistic ambitions, multi-sited studies ought to stick to ‘built-in assumptions about segmented lives, where some aspect (work, ethnicity or something else) is most central to the line of inquiry, and other aspects are less so’ (Hannerz 2003a, 209). The embeddedness of this specific aspect in the local contexts is of interest, but this does not require a holistic ‘emersion’.

Whereas my original approach in Dadaab was fairly open-ended and inclusive (with a broad interest in social security mechanisms), successive field trips to other sites were far more focused, following up ideas that developed from my original work in Dadaab. I became, for instance, fascinated by what the Somalis termed *buufis* [an obsession with resettlement], and sought to understand its underlying causes. *Buufis* in my view exemplifies the ways in which flows of information and remittances can impact people’s ambitions to move. Thus, in Minneapolis I studied what sort of information was passed on to relatives in Somalia and in refugee camps, and for what reasons. In various sites, I combined this interest with research on remittance sending practices as forms of socio-economic exchange. Tellingly, my interest in the topic matured and become increasingly focused throughout the years, due to insights gained in Dadaab, Minneapolis and various other places.

In this sense it is fruitful to concentrate on a limited number of well-defined questions and a select number of in-depth case studies. Precisely because of the risks of spreading resources too thinly, successful multi-sited fieldwork is highly dependent on a clear theoretical orientation (Fitzgerald 2004). This does not
necessarily presuppose a deductive approach as the only suitable option. Rather, the dialectical engagements of a priori theory with ethnographic data should guide the ongoing construction of the field and decisions on refining the focus of the research (Snow et al. 2003). This approach then also adds to the value of case studies as a contribution to further middle range theoretical developments. Whereas it is logical to assume that multi-sited fieldwork will contribute to the refinement of theories on transnational networks and flows, it is also crucial to allow this work to move beyond the framework of thinking these theories offer.

Another commonly advocated strategy is to work as part of a team of researchers. According to Stoller (1997, 91), ‘the hybridity of transnational spaces demands … multidisciplinary teams of researchers’ to conduct longitudinal studies. While this is now common practice in the social sciences, and has been for decades, the practical difficulties it involves are rarely described explicitly (the present volume is refreshingly untypical in this sense – see for instance Matsutake Worlds Research Group). For starters, when multi-sited collaboration involves researchers with different disciplinary backgrounds, and especially if interaction between them is limited or even absent, the question arises as to the extent to which the data produced are commensurable. Which is why such an approach requires sustained coordination in the research design, fieldwork, data analysis, and writing up phases. It also stands to benefit from a certain level of flexibility to incorporate local differences, and extensive piloting and evaluation of each phase of the project (Bloch 2007). Some of these demands, it must be said, do not match established anthropological practice very well.

I have found it useful to train local assistants to collect information and assist in the analysis for specific research projects. Abdullahi Mohamed Qambi, for example, is an IT student based in Dadaab and Nairobi who has been working with and for me for a decade now, doing fieldwork in Nairobi and Dadaab independently. During my one year stay in Dadaab I conducted a number of methodological training sessions, which he attended; currently, his input is transmitted through regular electronic communication and occasional meetings, in which we discuss research questions and objectives, the relevant literature, and interview guides. During my last stay in Dadaab, I trained a team of 12 Somali refugees from the camps and three local inhabitants from Dadaab town to do fieldwork, and I am currently working with a smaller team of three to five Somali students on a similar project in Norway. When I think of the transnational and multidisciplinary teams that are seen as one of the ways forward in multi-sited research, I have in mind working with these competent individuals in local contexts in ways that fit my original preoccupation with engaging in ‘shared knowledge creation’ through participatory approaches.

Another strategy that is advocated to deal with some of the challenges to conducting high quality multi-sited research, is to work multidisciplinarily and through multiple methods. One of the disciplines that has not been incorporated sufficiently in transnational studies is history. Fitzgerald (2004) advocates historically sensitive research as one of the main strategies of dealing with the
incorrect assumptions of an overly ‘postmodern’ approach that over-emphasises the particularity of the present. As he argues, ‘the major flaw in the notion of a “deterritorialized” world is the assumption that social life was “territorialized” at an earlier point’ (Fitzgerald 2004, 14). Multi-disciplinary teams by definition correct for these and other disciplinary blinders. Further, each individual within a team will likely consider a particular type of data especially relevant.

As Hannerz (2003b, 33–5) argues, new types of material have become interesting in contemporary field studies. Rather than relying solely on interviews and/or participant observation, for instance, these studies lend great weight to texts and electronic media. This opens up a whole new area of study, with new methodological, practical and ethical challenges. When engaging in research in complex transnational spaces, we may find that the key to success ‘devolves less from methods, multidisciplinary teams, or theoretical frameworks – although these are, of course, important – than from the suppleness of imagination’ (Stoller 1997, 91). The incorporation of the Internet both as a field and method of data collection, for example, opens up a whole new range of imaginative opportunities while creating a host of new challenges (Horst 2006c; Miller and Slater 2000; Wittel 2000).

The Internet is highly suitable to study flows, as a methodological tool of data collection and analysis, and to maintain a transnational network of researchers. Yet, especially when using the Internet as a field of study, it is important to address the validity of the data obtained, the relation between virtual and real spaces, and the implications of understanding the Internet as a network. Some of these issues are addressed in the relatively new field of virtual ethnography, which grew out of a cross-breeding of anthropology, Internet research in general, and science and technology studies. This field applies participant observation and the traditional notion of the field beyond face-to-face interactions to social relations that are mediated electronically (Dicks et al. 2005; Hine 2000; Hine 2005). In doing so, it tries to create virtual counterparts to ethnographic concepts and aims to change notions of fieldsites as localized space into an understanding of the fieldsite as a network of interlinked encounters.

Although I am a relative outsider to this new field, I have some experience in using the Internet as a tool for engaging in dialogue on my data and analysis, which has proven extremely valuable. Understandings of ‘shared knowledge creation’ led me to use various participatory research tools during fieldwork and to engage in multiple dialogues relating to my data and analysis after fieldwork, largely by electronic means. This has allowed me to refine my analysis, as illustrated on various occasions in my monograph (Horst 2006b). Of course, great benefits can be obtained from using the Internet as a communicative tool during transnational simultaneous research with a team of researchers. I remain somewhat more sceptical about the Internet as a field of study when it is not matched with ‘located’ research that proceeds from one of the nodes of the network one is interested in studying. In my view, such research can only make claims about the nature of the virtual exchanges it studies, without being able to locate these exchanges within the real lives that people live in different places around the world.
Conclusion: Anthropology Today

It may be a truism to state that the contemporary anthropologist is different from anthropologists in the ‘pre-global age’. Yet, whereas this has been a topic of discussion for well over a decade, leading to explorations on concepts like ‘field’, ‘site’, ‘culture’ and such, I do not feel that anthropology or other social sciences using ethnographic methods, has truly let go of the image of the lone, isolated anthropologist spending years in one locality. This image is so ingrained in our vision of what ‘true’ anthropology entails, that it still colours many of our assumptions, including the assumptions that are made about the main weakness of multi-sited methods, namely its lack of depth.

As perceptions of the fieldsite change, we urgently need to take on board the important implications of this shift. Sites are continuously connected, both in the sense of being part of transnational networks and practices, as well as that of being shaped by and shaping the ‘global’. Multi-sited work enables anthropologists today to capture these facts with greater depth than they ever could if they focused their energies on one site over an extensive period of time. This awareness allows us to move beyond concerns with ‘depth’ and ask new questions on the challenges of doing multi-sited research, and on the implications for ethnography of applying new strategies to overcome some of these challenges. I feel that, as researchers carrying out multi-sited ethnographies, we need to move beyond the question of depth to increase the debate on some of the practical, methodological and ethical issues we face.

In my case, studying Somali transnational networks, flows and activities in different locations has helped me develop a holistic understanding of specific transnational fields. At the same time, expanding sites has posed a number of challenges that needed to be addressed in the process. My work in Minneapolis, Oslo and other places was complicated by not knowing the subtleties that determine Somali life in these specific localities well enough. Further, trust building came to resemble unfinished business, different for every context. In Norway, negative stereotyping in the media and the illegal status of the hawala tremendously complicated access to the Somali community there. It also became increasingly difficult to keep track of the changing contexts in all the different sites I conducted research in. Not only was I not capable of keeping in touch with all the informants I met in different places, many of whom moved location during my fieldwork, I also found it hard to keep fully abreast of the local, regional and national changes that affected their daily lives. Dadaab, the site where my journey started, in the end may remain the only place I keep going back to.

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Follow the Missionary: Connected and Disconnected Flows of Meaning in the Norwegian Mission Society

Ingie Hovland

George Marcus (this volume) reflects on how and why multi-sited ethnography can claim to be ethnographic. Its particular claim does not lie in the sustained examination of a single (geographically bounded) site, as in traditional Malinowskian fieldwork, but rather in its return to—and reconfiguration of—some of the key tenets that have become associated with fieldwork, such as a commitment to ‘the native’s point of view’, that is, engaging with and working through subjects’ points of view, an interest in how these subjects imagine and act within their (multi-sited, distributed) worlds, and a concern with one’s own situatedness, partial insights, and accountability in the midst of research. Marcus is especially interested in how this modality of multi-sited ethnography can be used to understand contemporary social and cultural formations, such as various effects of globalization.

These formations might be akin to what Appadurai (1990) terms ‘scapes’—‘ethnoscapes’, ‘technoscapes’, ‘finanscapes’, ‘mediascapes’, and ‘ideoscapes’. Two examples of multi-sited ethnographic research that examine such contemporary scapes are Bestor’s (2001), which follows the transnational commodity chains of the trade in Atlantic bluefin tuna, and Rotenberg’s (2005) work on the transnational connections forged through podcasting. In this chapter I am concerned with some of the same underlying topics—including connections that cross national boundaries, and how these connections work through flows of communication and meanings between sites. Bestor (2001) examines relations between fishers, traders, entrepreneurs and sushi consumers; Rotenberg (2005) examines connections between podcasters, producers, advertisers and listeners. Importantly, however, the connections that they examine also function through—and set up—non-communication and new disjunctures. In Bestor’s case, the disconnections emerge for example in the ways in which Japanese traders remain incomprehensible to many North American fishers; in Rotenberg’s, disjunctures are revealed in the inequalities of access and power in the podcasting world, and in how podcasters set up alternative bases of power within neoliberal regimes. My work similarly focuses on transnational linkages that work through both connections and disconnections. It differs, however, in that I have not examined these connections in relation to previously ‘unconnected’ people.
(such as tuna fishers and traders; podcasters and listeners); rather, I have examined connections and flows between people who are already connected within one organization – within one ‘ideoscape’, if you like – though operating in dispersed geographical sites.

There are two strands of thought that weave through this chapter. First, that multi-sited ethnography should not claim to be a sort of über-triangulation that gives the researcher a ‘complete’ ethnographic understanding. Rather, it can in certain cases bring out a still partial but better understanding. I would not have been able to pay such attention to connections and disconnections within the organization I studied – and the significance of these connections to the people concerned – had I focused on a single site only, and in this sense multi-sitedness gave me a deeper understanding. But it did not provide a ‘complete’ picture.

Second, there is a certain analogy to be drawn between the organizational staff described in this chapter, and ethnographers. Both groups grapple with questions around localization, and how best to establish one’s local commitment. While the staff described here may not have found any more durable and satisfying answers than the ones currently tried out by multi-sited ethnographers, their questions alert us to the fact that grappling with multi-sitedness is a facet of research that can be responded to in a variety of ways, and in a variety of discourses. Let me turn now to the organization.

**NMS: A ‘Spread Out’ Organization**

In 2003–04, I spent a year doing fieldwork with the Norwegian Mission Society (NMS). NMS is an international Christian mission and development non-governmental organization (NGO) based in Norway, with field operations in 12 countries in South America, Africa, Asia, the Middle East, and Europe. The organization has been active since 1842, and its work entails supporting local churches and funding development projects related mainly to education, health and agriculture. At the time of my fieldwork it had about a hundred field staff stationed in these countries. The field staff are called ‘missionaries’ – and, as will become evident through the rest of this chapter, the missionary is both an important and a contested figure in NMS. In addition to being ‘spread out’ across the world map, NMS is also ‘spread out’ across Norway. The head office, with around 70 employees, is located in Stavanger on the west coast, but the organization relies heavily on funds raised by a network of thousands of members and supporters, who are located throughout the country in nine local offices and around 2,500 local ‘mission groups’.

In addition to this plethora of geographical locations that goes into making up the organization, there are also historical ‘locations’ that matter. In NMS, the struggles of previous generations of missionaries are still remembered – or rather, they have been transformed into particular organizational memories – and past missionaries are usually presented as heroes and adventurers, people of strong
faith and generous of heart. In this way the cherished memories of the past serve as one of the ‘glues’ that bind the current organization together. As my fieldwork got under way, then, I began to feel that my ‘field’ was not a normal one at all, but rather was ‘here’ and ‘there’, ‘now’ and ‘then’.

I have elsewhere written about one aspect of this multi-sited exploration, namely how linguistic questions in NMS reveal underlying organizational contests over how to conceptualize the world, or the ‘ideoscape’ that missionaries operate within (Hovland 2009). I have explored how mission metaphors, as well as the term ‘heathen’, are used with different meanings in different ‘sites’ within NMS. I have also argued that multi-sited ethnography, when used to understand a dispersed organization, should not be viewed as a method that simply adds perspectives together (as in one site plus one site plus one site equals multi-sited ethnography); rather, it should pay particular attention to the site and system awareness of the research subjects themselves, and to the kind of associations and connections that they make. In Marcus’s terms (this volume) one might say that I tried to map my (multi-sited) field as it was found in the field itself, among the people I studied. In this way multi-sited ethnography does not just add together perspectives that the researcher encounters, but instead prompts the researcher to change perspective. Multi-sited ethnography then is not a question of comparing like categories across different locations, but rather a matter of questioning the way that these categories are constructed.

This is the (related) theme I now explore. I shall examine the various meanings that are attached to the idea of ‘the missionary’ in different parts of NMS – in other words, how people who are differently ‘sited’ within the organization relate to the organization’s field staff, namely the missionaries. ‘The missionary’ is a complex figure in NMS, and, at the time of writing, at the heart of much tension and uncertainty within the organization. I argue that multi-sited ethnography brings this out in ways that single-sited ethnography cannot, within this ‘spread out’ organization. My point of departure will be the NMS head office, from where I shall proceed to NMS meetings held at other NMS ‘sites’ in Norway, before turning to some illustrative episodes from the missionaries themselves in Madagascar. Finally, I shall discuss the implications this multi-sited approach has for ethnography in general.

The Missionary Hero

In November 2003, the NMS head office organized a staff retreat and hired in a management consultant to talk to them about organizations. The consultant first made a point about an organization’s consumers, and asked the assembled staff what NMS’s consumers were called. This led to a brief, awkward silence, since NMS’s primary consumers are that large and amorphous group that used to be called ‘the heathen’, but that now, for various reasons, is no longer supposed to be called so. (I discuss this incident in Hovland, 2009). The consultant then
asked a second question: ‘And what do you call the organization’s heroes, the people who deal directly with the customers?’ This time there was no pause – ‘The missionaries’, somebody immediately replied.

This image of the missionary as the organizational hero has deep roots in NMS. When NMS was founded in 1842, it was with the explicit aim of educating and sending Norwegian missionaries to ‘far off’ places such as Africa. And although the organization’s strategy has shifted considerably since, and now involves far more partnership with established African churches, the basic idea of a Norwegian missionary travelling to the other side of the globe still evokes a rich emotional resonance in the organizational imagination. It is therefore not surprising that the head office staff at the retreat did not have the slightest difficulty identifying the organization’s heroes.

About two weeks after the retreat, I was having lunch in the NMS canteen. The staff at the head office usually eat lunch together and then listen to a brief devotion, which is delivered by one of them. This time the person holding the devotion planned to do something unusual: he was going to call one of NMS’s missionaries in Thailand. He had arranged to put him on speaker telephone so that the whole canteen, in Stavanger, could hear him. He asked him to talk briefly about what his family and himself were doing. The missionary talked about what he and his family were doing for a Norwegian Christmas in Thailand, and also about the preparations that were taking place in the local Thai church that he was a part of, where they were busy organizing Christmas visits to each of the congregation’s members. He then referred to a verse from one of the Gospels. The conversation did not last long and concerned mostly mundane matters, yet as the staff left the canteen, I noticed a ferment in the air. Two of the women whom I had shared a table with were smiling and commenting on this special kind of devotion, and had clearly been touched by the experience of hearing the missionary’s voice ‘live’, as it were. This experience was confirmed by many other encounters that I had with the head office staff. Many of them sought to learn the names of all of NMS’s current missionaries; they had pictures of the missionaries hanging in one of the head office corridors; news from the missionaries was distributed around the head office; and any especially urgent or important news from any of the missionaries would be announced in the head office canteen during lunch.

Now, if the nature of my research project had only entailed participant observation in one site, among head office staff in Stavanger, the matter of the missionaries might have stalled at the point at which, among the majority of regular staff at the NMS head office, the image of the ‘missionary hero’, although not completely unproblematic, is still highly meaningful and a source of inspiration. The thought of the missionaries provides important motivation for most head office staff. However, because my research project involved a multi-sited approach, I soon became aware that this was only a small part of a much bigger story concerning the figure of the missionary within NMS.
The Recalcitrant Missionary

Let us first follow the idea of the ‘missionary hero’ up two flights of stairs, from the staff canteen to the NMS leadership offices. In some ways I came to view the top leadership of NMS as a separate ‘site’ within the organization, even though their offices are located in the same office building. This was partly because those in higher positions sometimes referred to the missionaries in a somewhat particular way. They were always careful to emphasise that the missionaries were ‘human beings too’ – with various issues, emotions, and uncertainties, just like any other. But from the leadership I also heard frequent references to the organizational problems that surrounded this particular group. First, the leadership were more or less agreed that over time they wished to reduce the number of Norwegian missionaries that were sent out by NMS – partly for financial, partly for strategic reasons. They wished increasingly to channel NMS’s mission work through partnership with local churches and through local capacity development projects, rather than pay for Norwegian personnel to travel across the world. While they still thought it necessary to hive off missionaries to a number of strategic positions, they did not particularly wish to foster the image of the missionary hero. Nor did they wish to equate ‘mission work’ with ‘sending Norwegian missionaries’, as they thought that mission work could just as well – and perhaps better – be carried out by local personnel.

The missionaries posed organizational challenges in other ways. From time to time I heard the leadership express their frustration when missionaries ‘in the field’ did not understand, or flatly refused to follow, head office policy, or to join in new initiatives. I also heard leadership staff try to figure out how to get missionaries to take up offers of supplementary courses, which the leadership thought the missionaries needed, but which missionaries themselves apparently did not see the need for. The leadership also tried to organize curriculum vitae (CV) training sessions for missionaries who had come to the end of their contracts, the concern being that after having worked for NMS in Africa for long periods, the missionaries might not know how to put together a CV for the job market in Norway. All in all, this image of the recalcitrant missionary who needs further training but does not acknowledge this, or who does not know how to put together a CV, is a far cry from the image of the ‘missionary hero’. From the point of view of the leadership ‘site’ in NMS, the missionaries were still respected and appreciated, but the challenges that they posed (including how to reduce the number of Norwegian missionaries) and the problems that they caused for the organization (including partially implemented policies and additional costs), were also strongly felt.

Missionaries as Adverts

In keeping with the project of following the missionary, let me now add another layer of complexity by taking my inquiry to a third site, namely that of NMS’s local
members and benefactors in Norway – the people, that is, who donate funds to the organization. The regional meeting that was held in June 2004 about an hour’s drive outside Stavanger, was fairly typical. Around five hundred people had come together for a day for this annual meeting, held specifically for NMS members and supporters in the local district. The vast majority of those attending were over 60 years old, which is typical of NMS supporters in Norway. The programme for the day included biblical study, the election of the new chair of the regional committee, an accounting exercise of how much money had been collected and spent, an overview of the previous year’s activities in the region, mission news, and presentation of missionaries from the region who had recently returned to Norway or who were about to leave for the ‘mission field’. The General Secretary of NMS had been invited from Stavanger to address the meeting. One of his duties included chairing the session that was devoted to thanking the missionaries who had recently returned to Norway. He introduced the session by saying:

The mission is God’s mission. None of us own it. God sent his Son, and then in the next round he sent us. It is God’s mission. But he needs people to carry it out. Therefore we have missionaries … and therefore we have staff [in Norway].

This was a careful blend of strategy and public relations. On one hand, the General Secretary subtly emphasised that Norwegian missionaries were not the only tool that God could use for his mission. On the other, he wished to inspire members and encourage them to continue to donate funds to the organization, and one of the most effective ways of doing so within NMS being to make the work tangible and personified in the figure of the missionary. Which is why he then proceeded to invite the ten or so young and middle aged missionary couples who had completed their sojourns to come on stage, couple by couple, as he read out their names. Once they were standing in front of the audience, he repeated that ‘The mission has not been yours, it has been God’s. But in order for Him to carry out His mission, He has called some, and you have been a part of that’. He then gave a gift to each of the couples, and was about to end the session by calling a round of applause. One of the local organizers of the meeting, however, clearly felt that the crowd needed a higher pitch of emotional intensity, and he gestured for everyone to give a standing ovation. He then declared that the anthem of NMS should be sung standing up and looking towards the missionaries on stage.

Following this performance, one of the missionaries, a young woman whom I had previously met, walked past me and pulled a face, indicating that she was glad to exit the limelight. I later asked her and her husband what they had been given. He pulled out a cake knife and added, in jestful but ironic nature, ‘Maybe we should have knelt down to be knighted’. He clearly felt that the applause and the singing of the anthem had taken it all a step too far; he wished to be thanked for his work, but he most certainly did not relish this ‘hero’ treatment. Later his wife told me, also smiling and shaking her head at the whole ritual, that she had found it a bit over the top. I observed that it had been an acclamation for the heroes.
She agreed and said she thought it was the outcome of a ‘cultural clash’: what the General Secretary had said was fine, but the local organizer’s antics showed quite a different frame of mind. The latter was used to the ‘old way of thinking’, she said; he had attended gatherings in the olden days when missionaries were presented while the NMS anthem was sung. ‘Still today for many of the regionals’, she added, ‘the missionary is the very personification of the mission, of what they give money to’. She expressed her dissatisfaction with this idea, explaining that in her view the missionaries in NMS should no longer embody this all-important role. Today they ought to work as part of a team, together with local churches and local staff in whatever country they are sent to – which is what she felt that she had done as a missionary. The very thought that she should be made to play the exalted role of the ‘missionary hero’ made her laugh.

I had similar experiences at other local gatherings, where one or more missionaries were held up as examples – as personified ‘adverts’, if you will – for the mission. It is usually clear that this is exactly what the assembled NMS supporters want. They want to know that they are giving money to a tangible cause, something that can be personified, something familiar – and the figure of the heroic Norwegian missionary, who travels overseas to help people, delivers the goods. If the meeting facilitator wishes to play to the crowd, the missionary figure is always exalted. From time to time, however, I also encountered the kind of sceptical, bemused reaction shown by the younger missionary couple described above, from other individuals at these meetings.

At another meeting, a former missionary to Madagascar was presented in typical heroic fashion, the audience being told that he had spent ‘years at the forefront of the spiritual battle in Madagascar’. In this case, the missionary himself responded favourably to this presentation, and also recounted to the audience a number of fine deeds by missionaries whom he named individually, in order to give a glimpse into the mission work in Madagascar. His drift was the bravado of living in remote rural areas, surviving exotic illnesses, establishing new schools against the odds, and devoting one’s entire life to being a missionary. His narratives resonated well with the audience and he was rewarded with a hearty round of applause.

Afterwards, however, one of the very few audience members who was under 60 years old, indicated to me that he was not sure that this was the way the mission ought to be presented to supporters. He had thought a great deal about NMS’s shift in strategy away from the strong reliance on Norwegian missionaries, and felt uncomfortable with the strong personification of mission at this meeting, and the way that it was centred on the Norwegian missionaries. Another former missionary, also relatively young, expressed her dissatisfaction that people in these kinds of groups always seemed to think that missionaries were doing the work on their own. She said that people had asked her and her husband, when they were missionaries, ‘Are you really all alone at the [mission] station?’ She shook her head in disapproval at this kind of question; she and her husband had always worked in tandem with the people around them. ‘But that’s how they phrased the question’, she complained.
These episodes render the figure of the missionary within NMS complicated. On one level, missionaries are used as personified adverts of the mission work, in order to keep funds flowing from the local supporters in Norway. At the same time, this image is far more than just that – it also represents a worldview. The image of the exalted missionary carries with it the implication that the missionary travels to exotic locations in order to work, single-handedly, with people who badly need help because they are sick, uneducated, unenlightened, irreligious, or in some other dire predicament. There is little space in this worldview for equality and interrelations between Norwegian missionaries and local people. Some of the missionaries I spoke to identify with this worldview, and look upon the idea of the heroic missionary as an image that truly conveys some of the underlying reality of the world as they see it. Others, however, disagree, and treat the exalted image of the missionary with irony and amusement. It is, however, difficult for them to challenge outright the ingrained image of the missionary hero, and to project their own version of the modern, team-working missionary instead. The regional members and supporters of NMS across Norway frequently prefer and cultivate the first type of image based partly on historical precedent and also perhaps in order to lend legitimacy to the work they are donating funds to.

In fact, the will to maintain this particular image is so strong among the grassroots members and supporters of NMS, that they will sometimes explicitly defy the NMS leadership over the matter. This was most clearly expressed at the General Assembly in 1999, where the leadership of NMS proposed a reduction in the relative number of Norwegian missionaries. The delegates to the General Assembly, most of whom were representatives of local mission groups, held a long debate over the matter, and in the end voted against, noting that NMS should work instead to increase the Norwegian component. The leadership of NMS subsequently had to overrule the vote in order to be able to continue with their strategic shift towards partnership modes of working, with fewer Norwegian personnel (for example, NMS 2000).

The contested image of the missionary has far reaching (in more than one sense) implications in NMS. In order to seek to understand these implications, we now travel to a fourth site, namely the ‘mission field’ in Madagascar.

A New Kind of Missionary

In 2004 I visited Madagascar, with the aim of talking to as many as possible of the 35 Norwegian NMS missionaries and volunteers stationed there. On one of my first days in the field I went to a Sunday service held by the mission. I sat down next to a friendly-looking missionary and asked her about her work. She told me about it, and then, perhaps because she had heard that one of my research questions was about how NMS had changed over the past century, she proceeded to tell me all about how, in her view, missionaries had changed. Previously, she said, missionaries felt they were called by God to travel to Madagascar and to
stay there for a substantial part of their life – in other words, for several years. Now, however, missionaries stayed for shorter periods, typically two to four years. Besides, she added, they seldom learned to speak Malagasy fluently, and would even go back to Norway on holiday maybe once a year – something that was previously unheard of. The new missionaries, she mused, felt that these new and shorter periods of time more accurately reflected their missionary calling. My informant was not quite sure how to explain this significant shift in what it meant to be a missionary, but hazarded that it seemed very strange that God’s calling should so move with the times, so to speak.

Later that day I sat outside with some of the younger Norwegian missionaries. We joked about which came first to Madagascar: refrigerators or Norwegian brown cheese. (It is not uncommon today for Norwegian missionaries to take a supply of brown cheese with them to the field.) They smiled at the humorous suggestion that perhaps missionaries in the past had refrained from lugging cheese around, because this would have been a ‘luxury’ that would have ill-fitted their ideas of the hardship and sacrifice of a missionary calling. One of the young missionaries intoned, ‘Yes, because they had a different understanding of the calling’, and everybody laughed. The parody conveyed a sense of how these younger missionaries perceived their situation: they did in fact feel that their understanding of God’s calling was and should be different today, and they did not wish for a return to the (sometimes self-imposed) hardship and sacrifice that they thought had characterized previous generations of missionaries. Perhaps too the parcels of brown cheese also signified their shorter sojourns in, and therefore commitment to, the field in Madagascar. At the same time the situation threatened towards the accusatory: they were wary that they might be accused of not being as tough, as committed, and as admirable, as the Norwegian ‘missionary heroes’ of old. Their laughter suggested both recognition, uncertainty, and the relief of being able to poke fun at an uneasy issue.

During my interaction and interviews with the missionaries in Madagascar, I repeatedly encountered this streak of uncertainty concerning the missionary role. For some, it had to do with the changes they perceived among the ‘newer’ type of missionaries, who went back to Norway once a year, who signed a contract for perhaps two or four years in Madagascar, and who transported brown cheese in their luggage. For others like our young jokers, the uncertainty manifested itself as a source of some discomfort, as they felt they could potentially be measured against an older yardstick and be found wanting.

Finally, a common concern for all of them was the fact that the NMS leadership was reducing the number of Norwegian missionaries in Madagascar. They were already fewer than they had been a few years earlier. Many of them wondered if the leadership at the head office in Norway was intentionally slowing down recruitment to Madagascar, and they felt somewhat in the dark as to what exactly the purpose behind the reduction in missionaries was, and what the leadership wanted from them. There were feelings of vulnerability around whether or not the leadership in Norway really appreciated their efforts. Many of the missionaries felt that since they were the field staff, they should be regarded as the ones who
were ‘doing’ the mission for NMS, and that NMS’s leadership in Norway ought to function simply to service them; this, however, was not always consistent with what they saw happening at NMS. Most of the mission work carried out directly by staff from the NMS head office, such as partnership agreements, did not really figure in the missionaries’ representation of what mission work was about. For most of them, their own role, namely the role of the missionary, was central. This left them in a tricky position, and slightly at odds with NMS’s official strategy, which was to reduce the number of Norwegian missionaries, and no longer to regard a high number of Norwegian missionaries as a sign of success.

**Conclusion: The Missionary as Equivocal Input**

How do we make sense of the different and complex images encountered as we followed the missionary through the spread out organizational structure of NMS? How do we interpret the connections between all these images within NMS? The list of suitors is long. There is the image of the missionary hero, that of the recalcitrant missionary and the one who needs to be taught how to write a CV, the exalted missionaries on stage at a regional meeting, the young missionary who pulls a face at me as she walks past, the irony and bemusement that is evoked in some when they encounter the exalted missionary role, the seriousness that is evoked in others, the importance of the Norwegian missionary as a personification of mission in relation to NMS’s supporters, the uncertainty of missionaries in Madagascar who see ‘newer’ missionaries do things differently, the uncertainty of these newer missionaries who are not sure if they are measuring up, the uncertainty among missionaries regarding what the purpose of a reduction in missionary numbers is, and their feelings of vulnerability in relation to the NMS leadership in Norway.

As touched upon in the introduction, the flow of meanings surrounding the idea of ‘the missionary’ within NMS works through both connections and disconnections. To my mind, my multi-sited approach brought this out more clearly than a single-sited one ever could. Across the multiple sites, the complex combination becomes apparent – the missionary images that are used in different sites carry over similarities and take shape in relation to one another, while also expressing quite different perceptions of mission strategy and even differing worldviews. The missionary has always been an overdetermined phenomenon in NMS – that is, there are more causes that act to produce this behaviour than are necessary for it to occur. Multiple layers of causes have been in operation in relation to the behaviour of sending missionaries. Some of the past and present causes include the wish to spread the gospel, a feeling of being called by God, a desire to help people in need, a sense of adventure and a wish to explore uncharted territory, a wish to spread Norwegian Christian values, a wish to share the experience of one’s own Christian conversion with others, requests for Norwegian missionaries from local churches, a theological understanding of the responsibility of the church to come to the aid
of other churches, a need to legitimate NMS’s existence in relation to its donors, and a need to spend the organizational budget. It follows that the significance of this practice – sending missionaries – is also plural and equivocal within NMS.

Seeing missionaries as an equivocal input in NMS implies that there are different strands of meaning attributed to the image of ‘the missionary’ (see Weick 1979). These should not be conflated, even though they all refer to the same term – ‘the missionary’. Precisely because it is an equivocal input, the image of the missionary may be given more attention within various sites in NMS than if it had been perceived as an unequivocal phenomenon. Especially among the leadership, among the missionaries themselves, and among former missionaries, this seems to be the case. The fact that the image of the missionary is an equivocal input also means that it is potentially more adaptable. Again, the leadership and some of the missionaries, especially the newer arrivals, seem to be able to experiment with alternative interpretations of the missionary role in NMS today, and no doubt over time some of the images will change further. They will probably not, however, converge on a single, unified image of ‘the missionary’; if they did, it would be a sign that the organization had become much smaller, that the multiple sites of the organization had lost their flexible connection to each other, and the organizational creativity had been stifled. In the same way as Bestor (2001) and Rotenberg (2005) found that the transnational connections they examined were constituted through flows of both communication and non-communication, connections within NMS retain the same double-sidedness.

Let me return now to the two underlying lines of thought that I briefly touched upon in the introduction. The first concerns the method of multi-sited ethnography. In a case such as the one described here, where multi-sited ethnography is meant to facilitate an ethnographic understanding of an idea across several sites, it seems to me that it is important to keep the open-endedness of the method in mind. There is a temptation, when dealing with connected sites, to treat them as pieces of a jigsaw puzzle – to try to ‘fit’ the ideoscape together. Then the aim of the method becomes to force the pieces into perfect connections to each other, and to form a ‘complete’ and understandable whole. I would argue, however, that richer ethnographic material can be generated when the sites are not treated as jigsaw pieces, but rather as aspects of an incomplete whole (see Puget 2002) – a more shifting, distributed ideoscape.

Laurel Richardson (1998) has called this attitude ‘crystallization’. Crystallization recognizes that the research topic – like a crystal – has many sides, a complex web of reflections from any light that hits it, and is difficult, if not impossible, to pin down to one accurate description. ‘Crystallization provides us with a deepened, complex, thoroughly partial, understanding of the topic. Paradoxically, we know more and doubt what we know’ (ibid., 358). In some ways this is a reaction to the research method of ‘triangulation’, which can at times (though not necessarily) come to be used within a positivist frame of reference – for example looking at different research sites in relation to one research topic in order to validate the consistent ‘facts’ across the sites – and hints at the possibility of drawing up a
‘complete’ ethnographic view. As Des Chene (1997) has observed, this builds on a laboratory understanding of the field, and is an extension of the idea of the single bounded field site, rather than a step toward multi-sited understandings.

In sum, it seems to me that when trying to follow the idea (or ideas) of ‘the missionary’ within NMS, the outcome is neither jigsaw nor triangulation, but rather a deeper understanding of aspects of an incomplete, shifting, transnational organizational ideoscape. This partial and multi-sited understanding can in certain cases enable us to return to some of the core tenets of fieldwork: to better engage with and work through subjects’ points of view, and to address the question of how these subjects imagine and act within their multi-sited, distributed worlds, while keeping in mind the partiality of our research (Marcus, this volume).

The second underlying line of thought that has woven through this chapter concerns the analogy between missionaries and fieldworkers. Ethnographers who attempt multi-sited research may find themselves wondering about the exact nature of flows and connections between sites, and how best to localize their research within these flows, while knowing that to some extent their siting will be both arbitrary and contestable. The mission organization that I studied grapples with some of the same type of questions. My informants, like us, pay careful attention to different sites, and argue over the best localizing strategy for their staff and work – even as localization remains contested and somewhat arbitrary. Their wish to retain deep local commitment in their work, while gradually shifting away from the idea that such commitment has to be personified in a ‘hero’ who travels to a locally bounded site and remains there for a lengthy period of time, is reminiscent of anthropological debates around how to retain local commitment if this is not tied to the fieldworker’s presence in a bounded site for a certain period. Historical images (the missionary hero, the intrepid fieldworker) may be difficult to dispense with; new images (the team-working missionary, the multi-sited ethnographer) may not carry the same symbolic weight – and may solve some problems only to cause new ones. In the midst of this multi-sited awareness and questioning, siting operations stand to remain equivocal for some while yet, whether in missionary or ethnographic circles.

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Climate change is real. This lowest common denominator of climate science and politics has increasingly caught on. But how to keep it real? What kind of phenomenon is climate change, and how to localize it and study it ethnographically? Wherever the anthropologist goes, climate change is already there. So too in my case when I set out seven years ago to study conflicts concerning national parks on the German North Sea coast and ended up studying climate change. It has been my subject since, and I have pursued it in various projects and cooperations in diverse academic settings. As such, my research entered the realm of science studies and, more specifically, of actor-network theory. In this chapter I will present some instances from my fieldwork in order to demonstrate the deployment of my multi-sited approach and its relevance to current discussions on climate change.

Both nature and climate are ‘universals’, and ‘global connections are everywhere’ (Tsing 2005, 1). But universals are particulars, too, and one of climate’s particularities is that ‘global climate is a model’, as Tsing (ibid., 101) notes further. Whether we talk about the possible effects of climate change on low-lying coasts, the fuel-based economy or the future of the planet, we always rely implicitly on scientific models and symbols such as the famous ‘hockey stick curve’, which shows so impressively the parallel rise of emissions and temperature since the onset of industrialization. But what does this mean for a specific region such as the North Sea coast? How does one localize climate change? Localizing is an arbitrary process, as Candea (2007) convincingly demonstrates, with multi-sited ethnography torn between the ‘big picture’ and the boundaries of a restricted field site. From the perspective of climate science, localizing has a specific meaning. It is the procedure of downscaling from global climate models, with the help of proxy data and diachronic measurements of temperature, storm frequencies and so on, in specific places. Thus, localizing is a calculation, the resultant of another model. But how ‘real’ is this kind of localizing, and how does it relate to other factors that make up the coast?

In this chapter I argue that identifying climate change and localizing it through scientific expertise is an activity that is much more complex than ‘simply’ calculating. A close examination of scientific practice makes clear that localizing is as much a problem for climate researchers as it is for ethnographers. This holds not only for the interconnectedness of the global and the local climate, but also for
the separation of climate change as a ‘scientific fact’ on the one hand, and a ‘matter of concern’ on the other. Climate research offers an insight into a messy world of ramifications, surprising activities and unexpected ‘social’ content.

Actor-network theory as defined by Latour can be read as a radical form of the multi-sited approach. Not only does it open up science as a field for ethnographic research, it also empowers the ‘informant’ as an actor in an encompassing sense. According to Latour, actors have their own theories about what they do; they are not mere informants who have to be taught about ‘the context “in which” they are situated and “of which” they only see a tiny part, while the social scientist, floating above, sees the “whole thing”’ (Latour 2005, 32); actors also have ‘their own theories of actions’, they are ‘full-blown reflexive and skilful metaphysicians’, and they have to be taken seriously as intermediaries (ibid., 57).

Working with climate researchers opens a Pandora’s box and offers an insight into the social construction of climate change. Far from producing ‘only’ models or calculations, climate science is already loaded with social content. Thus, the task is not to add something to the climate models in order to make them more real, but to follow the scientific construction of climate change. In doing so, it becomes obvious that climate science does not produce authoritative ‘facts’ which society is obliged to follow; instead, science itself is a social activity and articulates propositions. According to Latour (2004, 247), climate as a proposition is ‘an association of humans and nonhumans before it becomes a full-fledged member of the collective, an instituted essence’. As a well crafted and articulate argument, climate change turns into a ‘thing’ that assembles, connects, and finally finds its way into democracy. Multi-sited ethnography of climate change means for the purpose of this chapter to follow climate scientists and to enter the network where climate change is simultaneously constructed as a universal and localized as a particular. It is not there to add complexity or social context to the activities and findings of climate researchers; my multi-sited ethnography, for example, is intended to show the complex web of ‘humans and non-humans’ that connects the North Sea coast to the alarming climate curve. Actor-network theory thus offers a methodology that is itself ‘a narrative or a description or a proposition where all the actors do something and don’t just sit there’ (Latour 2005, 128).

A multi-sited approach brings to light that the world of climate science is not separated from the social world. In this chapter I develop, using examples from my fieldwork, a storyline that overcomes the artificial opposition between social and scientific. Climate researchers become testimonies in global climate discourses such as Al Gore’s recent film, and they also actively produce these discourses in connecting for example singular extreme weather events with climate change. But the story does not end there: even in the innermost circles of climate science, the social and scientific merge in peculiar and multiple ways. Take the scientific debate about the famous ‘hockey stick curve’ (see below). While it seems commonsensical that climate researchers are responsible for the ‘natural’ and social scientists for the ‘social’, ethnographic evidence shows that this is not necessarily the case. Climate researchers can be well aware of the social relevance of their activities
and, in a kind of reflexive turn, some of them actually see this ‘social noise’ as an integral part of their scientific work. Tracing these multiple connections in a single narrative means smoothly integrating a multi-sited approach into the realm of an encompassing actor-network theory.

**Research Design**

My research had its beginning in an interdisciplinary project on the conflict over a national park on the German North Sea coast that focused on the concept of nature.¹ The transition from nature to climate arose from the collaboration with a participating Institute for Coastal Research. The particular interest of the Institute was the multi-dimensionality of coast and nature as addressed by our project. This is unfamiliar terrain for hard-boiled scientists. As it turns out, however, even the positivists cannot all be tarred with the same brush. Positivists are capable of reflection, just as reflective cultural anthropologists can turn into part-time coastal researchers. Out of a sporadic cooperation, the director of the Institute and I developed what Marcus (1998, 116f) calls ‘complicity’. The director opened up his Institute to ethnographic research, and I reciprocated with ethnographic expertise. The Institute became my part-time employer and commissioned me to conduct a media analysis of the Elbe River flooding in August 2002 (Krauss and Rulfs 2003); later I conducted a project entitled ‘Anthropology of the coast as a knowledge landscape’ (Krauss 2007), reflecting the social construction of the coast and the role that coastal research plays in this process.

For the duration of these projects, I pitched my tent on the island of rigorous science. This gave me ample space to participate and observe. The semi-official terminology was that I would be studying the ‘tribe of scientists’ and their ‘cultural backpack’. Both sides got along well with this formulation, which Latour and Woolgar also use in their introduction to *Laboratory Life* (1979). These metaphors from the foundation mythology of cultural anthropology formed the discursive basis of our collaboration and easily opened doors for me in this otherwise inaccessible world.

During the time of my stay in the Institute, a group of climate scientists were working on the reconstruction of past climates. The director of the Institute, Hans von Storch, is a renowned climate researcher, who has travelled around the world, representing the Institute and participating (and initiating) debates on the global level. The activities ranged from high-water management to the hockey stick debate, from policy advising and public information to the management of the Institute. Climate and coastal research is a nervous system full of interdisciplinary and transnational projects and a wide range of research topics; I tried to make sense of it and to follow some of these activities. Practically speaking, following

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the actors in a literal sense was possible only to a very limited degree, because
the mobility of scientists easily exceeded the bounds of my research budget.
But climate change is a hot topic which raises all sorts of questions in the public
sphere. Regularly, the research results of the Institute were published in the media,
the director gave interviews and public speeches, and there was a steady output of
scientific articles and papers. Scientists are also avid bloggers, and publications in
_Nature_ or _Science_ usually spill over into the blogosphere. Following these virtual
communications helped me enormously to understand the relevance of some of
the findings within and beyond the scientific community. Scientific publications,
media presence, in-depth interviews and ongoing informal contacts allowed
me to follow more or less continuously the process of research that takes place
on the coast, at the Institute, at transitory sites and in virtual space. I learned to
understand the scientists of this Institute as actors in a network that encompassed
far more than ‘only’ numbers, statistics, models and calculations. My multi-sited
approach, covering a series of seemingly unrelated projects, enabled me to follow
these actors in their effort to define global climate change and to localize it in
the ‘real’ world. In what follows, I present some of these efforts and try to bring
climate change back to the coast, where everything started. I start with global
climate discourse and identify the complex role that climate researchers play in it.
I then browse through a scientific debate on one of the central symbols of climate
change, the hockey stick, which was initiated by the Research Institute. In the final
part I argue that reflexivity is not the prerogative of social science, and that climate
research brings climate change back to the coast not as an apocalyptic sign, but as
a proposition.

**Climate Change and Global Discourse**

Climate change is everywhere, and it enters public discourse, everyday talk and
changes our perception of the world. It is a media phenomenon, and it borrows
its authority from science. It is useful to start off with an example on how climate
change is presented as an indisputable ‘scientific fact’, and critically to discuss
its ideological consequences. Climate change as a media phenomenon follows in
the footsteps of environmental discourse, as Nordhaus and Schellenberger show
convincingly in their pamphlet ‘The Death of Environmentalism’ (2004) and their
book _Breakthrough_ (2007). The climate-related discourse of former USA Vice-
President Al Gore will serve as a perfect example to show this continuity and to
illustrate its implications.

Al Gore’s efforts to bring climate change to people’s attention was recently
honoured with both an Oscar and the Nobel Peace Prize. This double bonus has
undoubtedly made him the most prominent admonisher of climate change in the
world. As a spin-off, and also due to the fact that the Nobel was actually equally
shared with the members of the Intergovernmental Panel on Climate Change
(IPCC), climate science gained even more public attention and credibility. There
is no escaping this global discourse, not even for the more hard-core and reclusive climate researchers.

In the film *An Inconvenient Truth*, Gore illustrates in a series of animated slides the consequences of climate change. Along with many other localizing examples, he shows a scenario for the North Sea coast from Holland to Germany and Denmark. A blue line symbolizes a wave that advances inland and washes away the entire coast (including, alas, my field site). Obviously, this is a didactical scenario intended to raise awareness and influence public thought. While such a scenario of localizing climate change is based on scientific findings, it surely deserves a closer look.

The North Sea coast has always been a somewhat vulnerable location, prone to storm tides that have on more than one occasion wrought devastating damage. Dyke-building techniques have, however, improved, and in recent years the coast has survived peak tide levels largely unscathed. In a recent keynote speech in Holland, Latour pointed rightly to the fact that that nation owes its existence to a politics which has taken dykes seriously and which has therefore practised a good ‘politics of things’ (Latour and Weibel 2005). On his part, however, Al Gore does not talk about how to deal with the consequences of climate change; differences disappear behind the big picture of apocalypse.

In doing so, his presentation rests on the idea that society is both static and passive. In the end, he is not interested in the individual case, which is only another example in a series of anticipated catastrophes. It is catastrophe itself that takes centre stage, as a warning and a summons to action. Yet this summons is one-sided; it calls only for the reduction of greenhouse gas emissions. Mitigation is doubtless a major concern, but it is striking how little time Gore has for adaptation to climate change. Nordhaus and Shellenberger rightly suggest that Gore sees adaptation rather as sinful: ‘The truth about the climate crisis is an inconvenient one that means we are going to have to change the way we live our lives’ (Al Gore in Nordhaus and Shellenberger 2007, 106). His summons to action culminates in an exhortation (during the end credits to his film) to replace one set of light bulbs with another – an infinitesimal gesture in view of the dimensions of the problems that he demonstrates. Nowhere do we find even a ripple of hope for the North Sea coast, and everywhere else too rescue attempts seem futile. ‘That’s because, for Gore’, Nordhaus and Shellenberger observe, ‘it can’t be. As surely as the Bible begins with a fall and ends with apocalypse, humankind’s sins against nature will be punished. “It’s human nature to take time”, Gore says, “But there will also be a day of reckoning”’ (ibid., 107).

Yet it is not only the (imputed) religious intention that surrenders the North Sea coast to wrack and ruin. A further obstacle to a realistic presentation of climate change and its consequences for the coast is Gore’s one-sided use of scientific data. When Gore comes down so heavily on the side of mitigation in the debate between mitigation and adaptation, he is using science to score points against climate change sceptics, who are up to their mischief particularly in the USA. Science thereby becomes for him a producer of irrefutable truth, and climate
change turns into an indisputable ‘scientific fact’. As if this lacked sufficient persuasive power, Gore stresses repeatedly that he counts on many scientists as his friends, and uses a soft focus lens to show the stairway leading to the sacred halls of science. In continuation of environmental discourse where nature was writ with a capital ‘N’, Al Gore writes climate with a capital ‘C’ and science with a capital ‘S’. Nordhaus and Shellenberger attack this attitude at length in order to open up climate discourse for new strategies of action – no longer a single issue politics like environmentalism, which ends in the creation of a national park or a nature reserve, but rather a climate politics that is worthy of the name and implements far reaching political strategies. Paradoxically their critique of Al Gore’s vision is part of their attempt to take climate change seriously and to ‘keep it real’ as a political challenge.

Al Gore’s merit must surely be that he has anchored climate change in public consciousness. But this is localized only in a vague Somewhere or Disneyland, as the collage on a title page of an issue of 
\textit{Vanity Fair} illustrates: Leonardo di Caprio poses before an iceberg with Knut, the German polar bear. The North Sea coast, however, was and is \textit{really} threatened by floods, as are many other low-lying coastal regions. Al Gore bluntly uses the authority of science with a capital ‘S’ to nail the sceptics and advocate mitigation over adaptation. But even though Al Gore may appear to take science hostage, my next example shows that many scientists themselves eagerly jump onto this bandwagon.

\textbf{Climate and Catastrophe}

When in August 2002 the river Elbe in eastern Germany burst its banks as a result of heavy rains, the Institute for Coastal Research commissioned me to analyse media coverage of the disaster, particularly with a view to the role of science.\footnote{On research under the conditions of a radically changing German knowledge politics see Krauss (2007).} The Institute is home to specialists on Elbe hydrology and had sent a research ship up the river during the floods to take on-the-spot measurements. One of the objects of the commissioned analysis was to measure the success of such actions in the media – an important factor in times of the reorganization of the research landscape. Above all, however, the work afforded me surprising new insights into the nature of climate change.

What was striking was that, from day one, the flood disaster was placed in relation to climate change. Climate experts appeared on talk shows, and journalists penned admonishing commentaries in which they interpreted the event as punishment for man’s heedless treatment of nature. Commentators declared the Elbe flood to be the first real climate catastrophe in Germany. Although the climate researchers on the talk shows tended to be cautious, most confirmed that such a connection was more than possible. Only few voices were raised that pointed out
the lack of scientific evidence. And, in spite of these objections, the media now repeatedly and automatically produce a direct connection between climate change and specific events, be they Hurricane Katrina or the recent cyclone in Myanmar.

But there were other and more solid lessons to be learned from the Elbe flood. The media followed the actions of the emergency services, disaster control and politicians in minute detail; they called for donations and shows of solidarity and waxed euphoric about all of these as manifestations of an active German reunification following the fall of the Berlin Wall. In comparison with Hurricane Katrina, the Elbe flood was managed fairly well (von Storch and Krauss 2006) and, thanks to government compensations, many of the victims were better off after the flood than before.

In a cause analysis at a workshop of the Institute for Coastal Research on the subject of flood management, it became clear that the bulk of the damage resulted from settlement and the subsequent loss of flood plains along the Elbe. Yet such insights hardly penetrate to the public sphere. Time and again in the public sphere the debate has shifted from the necessity of disaster control and foresightful politics to mitigation as the only answer to climate change, and this continues to the present day with each fresh catastrophe.

Reference to climate change adaptation is also a flash point within climate research. When, after the Elbe flooding, an interview with von Storch was carried in a German weekly under the optimistic heading ‘We’ll whip that’ (Stampf and Traufetter 2003), his colleagues in climate science wondered loudly if this had undermined the effort to rouse public awareness. In his interview, however, von Storch pointed to social dynamics and to the uncertainties attached to the scenarios, without disputing the existence of a man-made climate change. He emphasised that adaptation is a preventive measure, since natural disasters will happen in any case. Climate politics becomes cynical when it does not comprise disaster control, even if many a climate researcher has surfed into the limelight on the wave of a catastrophe. Climate research polarizes; it is a fight with no holds barred. But climate is also a thing that assembles and shapes new coalitions or enforces old ones. Time, then, to turn to the inner circles and discussions of climate research itself.

Constructing Climate Change: The Hockey Stick as a Symbol

‘Global climate is a model’, Tsing (2005, 101) rightly observes. It is a scientific construction by means of global climate models, which rest in turn on weather statistics. As we all know, it does not look altogether good: all values – emissions, temperatures, and such – are heading upward. Following the 2002 IPCC report, the curve describing this tendency, called the ‘hockey stick’, became an icon of man-made climate change far beyond the boundaries of the scientific community.

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3 Intergovernmental Panel on Climate Change; see Houghton et al. (2001).
Whereas over 900 years of climatic variation yields a fairly level plot, the curve rises steeply with the advent of industrialization – this is the blade of the hockey stick. The curve is also called the Mann Curve after its creator. My first intense encounter with this diagram was at the IGBP4 Global Change Conference in Amsterdam in 2002. The conference was a run up to the negotiations on the Kyoto treaty in the Hague and its goal was to use scientific evidence to direct public attention to dramatic climatic developments, to provide the press with material and, possibly, to influence the negotiations. The array of keynote speakers from climate research was correspondingly impressive. Four years before Gore’s film, world class scientists delivered one PowerPoint™ presentation after another. The pictures showed the beauty of planet Earth with all its diversity of species – but also droughts, floods, starving children, wars, malaria, and more. All this was repeatedly interwoven with the hockey stick graph for scientific support. The message was ‘climate change is real’, backed up scientifically and visually. Since then, the hockey stick has become a world wide symbol for man-made climate change. The accompanying apocalyptic rhetoric is not only the work of the media, but is also often enough, as was the case in Amsterdam, preset by climate researchers. Yet delicate connections via the hockey stick arise not only between science and the public, but also within science itself (from which the media are never entirely excluded).

**Breaking the Hockey Stick: The Politics of Climate Research**

Back at the Institute, the contrast between this media-oriented conference and the everyday routine of climate researchers seemed to me immense. One work group at the Institute was studying the paleo-climate, reconstructing climate phases that lay in the Earth’s distant past. I learned something about the use of proxy data and various climate models, and become somewhat acquainted with concepts such as downscaling, nesting and the like. Still, I often had the feeling that I did not really understand the language of this tribe or the excitement they felt about their research. This ‘lag period’ at the Institute was brought to an end by two consecutive events that created a great stir in the global research community and in the public sphere, and again afforded an insight into the networks that produce climate change, with the Institute as an important actor. In both cases, the director of the Institute, Hans von Storch, took centre stage – first as short term editor of a scientific journal and ostensible champion against the climate sceptics, and then as a climate researcher who was himself suspected of being a sceptic.

Von Storch was one of ten editors of the peer reviewed journal *Climate Research*, all of whom were on a equal footing. In 2003, the journal published a controversial article by Soon and Baliunas in which apparently doubtful data were used to question the theory of man-made climate change. Climate sceptics

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4 International Geosphere-Biosphere Program.
in the American Senate invited Soon to a hearing with the intention of using the
(allegedly) scientific evidence to undermine the plans of their colleagues McCain
and Lieberman for the reduction of greenhouse gas emissions. There was protest in
the scientific community that the article rested on false data. As a consequence, the
publisher of Climate Research appointed von Storch as the sole editor so as better
to supervise the peer review process which the disputed article had passed without
objection. The first thing von Storch did was to write an editorial for the next issue
in which he criticised Soon’s article and the evidently miscarried peer review.
The publisher, however, blocked its publication because all the editors had first to
agree to it, something von Storch argued would be impossible since at least one
among them had approved Soon’s paper. Von Storch resigned as editor in protest,
followed by half the editorial board (Goodess 2003). These events caused a flurry
not only in the climate blogosphere; the New York Times, the Wall Street Journal
and the Journal for Higher Education also reported them. Soon was suspected of
being funded by NASA, the NOOA and the oil industry, and having deliberately
played into the hands of the sceptics.

Von Storch and his Institute soon found themselves in the thick of further
debates, which gave me as an observer a better insight into climate research in
times of climate change as a ‘universal’. In a publication in Science, von Storch
and others demonstrated that the hockey stick curve rested on false methodological
assumptions (von Storch et al. 2004). The criticism pertained not to the blade
(that is, the rapidly rising curve), but rather to what Mann and his colleagues
had interpreted as the relative constancy of climate in the last thousand years.
The modelling calculations of von Storch et al. refuted the data computed by
Mann et al., detecting, for example, an earlier warm phase that approximately
resembled the present one, and a greater variation during the ‘Little Ice Age’ than
had been assumed. This was obviously a purely methodological criticism of the
Mann curve, but it caused a great stir. The hockey stick had disappeared or at least
became unrecognizable. The article triggered a vehement debate in the climate
research community; everyone’s nerves were frayed. Was von Storch playing into
the hands of the sceptics? Was it ethically defensible to publish such an article in
view of the imperative to do something against climate change? Was the criticism
by von Storch of the climate community’s treatment of proxy data, which formed
the basis of their objections, justified? Was Mann’s work slipshod? Why did von
Storch not quote two papers that had already come to the same conclusions a year
before? Had not von Storch himself helped compile the very IPCC report in which
the hockey stick theory played so prominent a role? The debate conducted in the
blogosphere reads like a scientific detective story. All participants spoke out in a
blog of the journal Nature, where these points were sometimes vehemently and
very inconsistently discussed.

Such an event is of course highly political, and representatives of American
politics again played a role. One politician sent questionnaires to three outstanding
figures in the debate; von Storch spoke about his findings before a committee of
the House of Representatives (von Storch 2007b; von Storch and Zorita 2006). He
insisted that politics should not determine research, and that the peer review process and methodological debate must remain at the centre of a serious discussion about climate science.

In an interview to the German news magazine Spiegel, which again had broad repercussions in the scientific blogosphere, von Storch called the hockey stick theory ‘nonsense’ (von Storch 2004); in an article in the same magazine bearing the eloquent title ‘A Climate of Staged Angst’, he said that the atmosphere in climate research reminded him of the McCarthy era (von Storch and Stehr 2005). There was reason to suspect, he continued, that the gatekeepers of publications sometimes acted like censors to exclude dissenting opinions. Journals like Nature or Science, on the other hand, tended to publish only research with high visibility and public effect, neglecting work from the scientific community that was unintelligible to the outsider but methodologically important. Climate research, he concluded, had been trapped between mitigation and adaptation, between sceptics and admonishers, between liberals and Republicans (in the USA).

Von Storch illustrates this situation using the example of two bestsellers that deeply influenced public perception of climate change at this time: Roland Emmerich’s film The Day After Tomorrow and, as its opposite number, Michael Crichton’s State of Fear. Both, according to von Storch, use partially false elements in order to render certain realities vivid. In Emmerich’s film it is the admonishers whom no one heeds; in Crichton’s book it is the agents of the environmental movement who unleash an intellectual (and then later a real) terrorism in order to persuade the world of climate change.

In a blog von Storch commented in retrospect that he was satisfied with the debate over the hockey stick (in Pielke 2005b). It triggered, he said, a moment of reflection about the peer review process and methodology, and its repercussions reached to the IPCC and its new reports, which reflected positively the results of the debate and obviously changed the climate curve, which now looks less like a hockey stick but leaves the message that climate change is real and that it is urgent to act, intact.

It seems a long way from the everyday routine at the Institute for Coastal Research, where the ‘nerds’ sit before their computers and model climate, talk in whispers, drink coffee and eat in the canteen, to the IPCC report that pitches the world into a state of alarm. In between lies a world full of excitement, dispute, and openly or tacitly fought out conflicts, with renowned climate researchers like von Storch serving as influential actors and intermediaries. Climate research does not take place outside the world but in it, and von Storch harbours no doubts about the social construction of scientific facts, though he insists on the accuracy of methodological approaches in climate science. Astonishingly for a cultural anthropologist heavily laden with prejudices, von Storch not only sees the clamour that accompanies climate research in the world as a component of his own scientific curiosity and work, but makes it part of his climate research activities.
The Reflexive Positivist

Climate change cares little about disciplinary boundaries or the borders between science and the public sphere. For years now von Storch has been working together with the sociologist Nico Stehr. Together they have published a series of articles and a book (von Storch and Stehr 1999) that put climate change and the fear of climate change in historical perspective. They have shown that human actions have always been ascribed as having positive or (especially recently, mainly) negative effects on climate. Both the improvement of climate by human progress and its endangering by natural catastrophes are precursors of contemporary climate angst. Deforestation, the invention of the lightning rod, atom bomb tests and the burning oil fields of the Iraqi war have all been interpreted as catalysts of a ‘nuclear winter’ or the like. These fears have proved to be largely mistaken, which distinguishes them from the man-made climate change that has been detected today. Von Storch and Stehr have shown that both climate angst and climate research are socially conditioned (von Storch and Stehr 2000) and have emphasised that uncertainty is a constant of science. This uncertainty consists above all in the vast, dynamic unknown called ‘society’. Earlier climate researchers erred in their predictions because they were unable to foresee social developments such as the steam engine. Today we are again confronted by developments that would have been inconceivable only a few years ago. All scenarios rest on the assumption, and herein lies the greatest error, that the present is an immutable condition.

Yet false assumptions often trigger the right actions, as Storch and Stehr (2006) have shown by reference to a case described by the Swiss environmental historian Christian Pfister. Scientists traced flooding in a Swiss valley to the deforestation of surrounding mountains, but they forgot that the river had always overflowed its banks. Nevertheless, the consequent political decision bore fruit: reforestation and its positive consequences, even if on the basis of a false scientific analysis. Such examples show that climate research is a science located ‘between academic curiosity and cultural conditioning’ (von Storch 2007a). Does it follow from this that there is no truth about climate change but only more or less good climate research? At any rate, the climate scientist as a reflexive positivist is extending the networks and adding new actors such as ‘society’ that in turn influence results and the direction of research.

In order to strengthen climate research, Hans von Storch and other climate scientists in Hamburg have since 2001 annually awarded the Brückner Prize (named after the founder of modern climate research). Since then, its recipients have included Christian Pfister and the American science studies researcher Pielke Jr., both of whom have been honoured for their interdisciplinary and border crossing work.

The politics of climate research is the speciality of the sociologist Roger Pielke Jr., who is the director of the Science Studies Centers in Boulder, Colorado. In his publications and blogs, he has analysed and commented upon the previously

5 <http://sciencepolicy.colorado.edu/prometheus/>.
discussed debates and the policies and politics of climate science. In publications like *Nature*, he is an important exponent of adaptation, and so of the necessity of being prepared for disasters whether or not they are caused by climate change (Pielke 2007a). His criticism of the interpretation of individual disasters as symptoms of climate change rests on both an intimate knowledge of the debates within climate research and the debates in the public sphere. To bring back society into the climate debate is one of his main arguments against those who exclusively focus on mitigation efforts; climate change is already happening, and adaptation saves lives (Pielke 2005a). In his new book, Pielke introduces the idea of the ‘honest broker’ (Pielke 2007b). The honest broker is a scientist who enters into social questions and offers society scenarios or propositions that both include uncertainties and provide various opportunities for discussion appropriate to the questions. Pielke distinguishes the honest broker from three other types of scientists: those who completely shut themselves off from the outside world, those who openly advocate and work for a specific goal, and finally those who serve as stealth advocates – who, for example, draw a direct connection between climate change and an individual event like the Elbe flood for didactical reasons. In doing so, the last take an illegitimate shortcut on the way to localize climate change.

**Localizing Climate Change: Bringing it all Back Home?**

The North German coastal landscape is the result of a centuries long interaction between human and non-human actors or, more simply put, of land reclamation by means of dyke building in a region constantly threatened by storm tides. The predicted rise in sea level is, of course, bad news for this coastline. Climate is here a post-environmental problem, too, in a very literal sense: in the mid-eighties, the coastal shelf was declared a national park, a move which was fiercely contested by local inhabitants. It took almost two decades to resolve this conflict, in which locals had suspected science as being in cahoots with environmentalism. While the ‘nature people’ propagated ‘Nature’ as a moral authority based on irrefutable ‘Science’, their actual practice differed significantly. It was only after negotiations about each and everything that their national park was accepted. Today ‘nature’ is an attraction for eco-tourists, and the coastal shelf one of the best researched coastal ecosystems worldwide. Most of all, the dykes have been left untouched by environmentalism.

It was one of the central arguments of the local protest that coastal protection always has to be privileged over nature conservation.

Localizing climate change comes after localizing nature; in order to be successful, there are many lessons to be learned from these previous conflicts. The North Sea coast is one of the Institute’s main areas of study, and here as on all low-lying coasts predictions about climate development are of particular interest. By means of improved models and thanks to the relatively good data

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6 For a detailed discussion of these conflicts see Krauss (2006).
records (long and reliable series of measurements), it has become easier to make realistic forecasts of regional climate developments (Woth and von Storch, 2008). In the nineties there was an increase in the number of storms and storm tides that made people fear the worst. But newer research at the Institute shows that this increase was restricted to a certain phase and that storm activity has recently abated. On the other hand, the scenarios based on the new calculations show that the predicted rise in sea levels and longer duration of storm tides will exert higher pressure on the dykes in the long term, so that preventive steps must be taken. The predictions also forecast, however, that the present dykes currently should be sufficient with good maintenance. Interestingly enough, the media reported after an interview with the climate researchers, that there would be no problems for the next thirty years; this is a time span that reportedly was not mentioned in the interview, but invented by the journalists.

Parallel to these new studies, the Institute presents its findings to the public for discussion. The Institute has set up a permanent ‘Climate Office’ that provides information to coastal politicians and inhabitants, and the director of the Institute gives repeatedly talks to regional institutions and organizations. He never fails to mention that climate change will happen and that it is man-made. But he invariably adds that he can only present scenarios on the basis of actual knowledge, with all its uncertainties. On a higher level, there are already institutionalized cooperations.

During the debate on the national park, the subject of coastal protection and the dykes was a flash point. The dykes were non-negotiable for the coastal population; without a fixed agreement on the principle of ‘coastal protection before nature protection’ no conversation was possible. Proposals to try out new forms of coastal protection over the longer term fell immediately under the general suspicion that conservationists had placed the protection of nature before the safety of the people. Once this debate came to an end and the national park was more or less accepted, a new discussion slowly began to unfold. There have been hints of this in recent debates on the effects of climate change in which local representatives of coastal protection and coastal research have discussed various scenarios with respect to rising sea levels.

Climate change is sure to produce a new collectivity, a new coast, a different coastal population, just as it is bound to change climate research. The discussions about the hockey stick and the future of the coast are linked in manifold ways. Localizing the global involves necessarily redistributing the local (Latour 2005, 173); this is true for nature as well as for climate change.

**Conclusion**

It is a long way from the Research Institute to the North Sea coast. This is not only true for the ethnographer, but also for the climate scientist. Global climate is a model, and downscaling is on first sight nothing but a complicated calculation. But a closer look offers an insight into the many ramifications, processes and values
involved. A network of actors and assemblies is attached to climate, seamlessly transforming scientific facts into matters of concern. Anyway, climate science is not a closed system from which political action can be deduced, but it can offer well crafted arguments and well made propositions. The debates about mitigation versus adaptation and sceptics versus admonishers have not only shaped public discourse, but become an integral part of climate science. Following climate scientists and their debates seems to be a good way to find out what it means ‘to keep climate change more real’; such a multi-sited approach is not about debunking climate science or adding social dimensions, but about showing its inherent richness and helping to bring it into democracy.

Multi-sited ethnography, consisting in research sojourns of variable duration and occasionally in unusual methods, can afford an insight into how climate change is a construction that needs to be handled carefully. Localizing has proven to be an activity that points in all directions, crossing the boundaries of disciplines, but also of science and society. Climate change is real, and in order to keep it real science and democratic decisions, functioning administrations and networks, rectifying conflicts and a certain pragmatism are needed. Only that is doable which can be done locally. Climate research cannot shorten the democratic detour, but only offer ‘if/then’ scenarios on the basis of the best possible data. Climate is part of the spheres of human existence which we must keep stable and livable, whether we like it or not. And localizing climate change is an activity that has only just begun.

References


Chapter 9

Changing Places: The Advantages of Multi-sited Ethnography

Karen Isaksen Leonard

Hyderabad city and the former Hyderabad State in southern India were my starting points for an exploration of migration and social memory, an exploration that would have led to very different results were it not multi-sited. By looking across sites and over time, the interactive nature of identity reconfiguration in the diaspora became clear. Investigation of any single site of migration abroad would have failed to reveal the multi-sited lives of the emigrants, the ways in which they thought of moving, decided where to move, and decided which social and cultural networks to maintain and which to initiate, including marriage networks for their children.

The ‘native state’ of Hyderabad experienced dramatic ruptures in the mid-twentieth century that pushed some of its inhabitants abroad, and to not just one but many sites. Hyderabad was India’s largest and most important princely state and it tried to stay independent after the partition of British India into India and Pakistan in 1947, but the Indian Army secured its surrender in 1948. Then, with India’s 1956 Linguistic States Reorganization, Hyderabad State was dismantled and its three linguistic regions of Telugu-, Marathi-, and Kannada-speakers joined the new states of Andhra Pradesh, Maharashtra, and Karnataka respectively. Hyderabad city became the capital of Andhra Pradesh, and Telugu-speakers from formerly British ruled coastal Andhra flooded into the city and controlled state politics. In the 1960s, the UK, Canada, the USA, Australia, and the Gulf states opened up opportunities to study, work, or immigrate, and many Hyderabadis and other South Asians became migrants.

By the end of the twentieth century, Hyderabadi migrants were in seven major sites around the world: Pakistan, the UK, Australia, Canada, the USA, Kuwait, and the UAE.¹ Mine was a study carried out in those seven sites, really eight, as I included the homeland, over a period of more than ten years, from 1990 into the early twenty-first century. As an historian and anthropologist, I believe in fully contextualizing the voices and conversations which guide my writing. Yet there are space limitations, and I regret that here, rather than giving detailed accounts of the seven sites, the streams of migrants to them, and the ways in which the

¹ Many go to Saudi Arabia to work as well, but I could not secure a visa to do research there. I am a widow and needed a male blood relative to accompany me; I hesitated to ask my brother or my son to spend a precious vacation accompanying me to Saudi Arabia.
migrants and those left at home presented themselves, I can only include material that illustrates the methodological and ethnographic observations I make below—despite Fine’s (2003) insistence that we include evidence to show our claims are justified.²

The first methodological issue is that simultaneous research in my eight sites, as argued for by Mazzucato in this volume, was not possible, partly because I worked alone and also because no single source of funding enabled me to move quickly from one site to another. Research funds, I found, generally related to one nation-state or another, not to several; also, ‘Hyderabadi’ was not a population defined by emigration, immigration, citizenship, or census record keepers, so a cross-national statistical study, such as might have been funded by the National Science Foundation (NSF), could not be designed and proposed. But there were many advantages to doing a longitudinal study. As an historian of Hyderabad State before I became an anthropologist, I knew the homeland history and culture well, but I needed time to become familiar with the new contexts for those settling abroad. Because my own travels continued for more than a decade, my contacts with Hyderabadi migrant networks kept expanding as people in each place gave me new names of migrants in other places. Sometimes I met migrants whom I had already met elsewhere, people with whom I could compare observations about the sites and Hyderabadi experiences in them. I was able to see changing educational, occupational, and marital patterns over time, especially as members of the second generation abroad matured, and I could compare them from one site to another.

The second methodological issue concerned the relative importance of the starting point to identity reconfigurations abroad. How much did one need to know about the homeland of the Hyderabidis who were leaving India’s Deccan plateau?; How unique was Hyderabadi history and culture, and how lasting was its imprint? Of course, people have moved about for centuries to South Asia and within it, leaving old homelands and finding new ones. A major historical theme in Hyderabadi and Indian history has been the relationship between indigenous people and immigrants, natives and newcomers. The definitions and occupants of these categories have changed over time. In the Deccan, the terms in medieval times were Dakhni [of the Deccan, countryman] and afaqi [foreigner, outsider], and, more recently, mulki [countryman] and ghair-mulki or non-mulki [foreigner, outsider]. The native/newcomer theme has not only shaped all of India’s history, it has obvious relevance to contemporary world patterns of migration, to modern nations and concepts of citizenship. I will return to this point at the end, but here we need to know more about the emigrants I studied.

These emigrants were rooted most firmly in the former Hyderabad State, the native state ruled by the Nizam (hereditary governor) of Hyderabad until 1948. Because my own initial work in Hyderabad city was in 1966, relatively soon after the state’s 1948 incorporation into independent India, and because I worked on longtime Hindu nobles and servants of the state, the diaspora I researched from

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² My book (Leonard 2007) does provide detailed evidence.
1990 to just after 2000 was that of the ‘old Hyderabadi’; those who termed themselves *mulkis* or countrymen, citizens of a state that had Persian and then, from 1883, Urdu as its official language. Drawing on Raymond Williams (1961, 41–71 et passim; 1973) and his phrase ‘structures of feeling’, it is important to note that Hyderabad persisted as a stronghold of Indo-Muslim culture in an India increasingly influenced by British imperial culture and the English language. Ramanujan (1970, 229) postulated three successive hegemonic cultures in South Asia, cultures based, crucially, not in linguistic regions but in cities. He wrote, ‘Sanskrit is an urban-centered but non-regional language, as Urdu later was and English is now’. Some (see van Buitenen 1966, Basham 1961) lamented the passing of the vital early Sanskritic Indian civilization, while others welcomed the next phase of generative civilization, that begun by earlier Muslim rulers and solidly established by the Mughals. In the Indian context, this change was not loss but difference, with new migrations bringing Persian and eventually Urdu urban culture: this was the basis of the old Hyderabadi identity that is passing from the contemporary scene. British imperialism, new trade and political connections, brought a gradual shift to the English language for administrative and educational purposes. The impact of English proved decisive in most of India, although it was resisted in Hyderabad, which remained an outpost of Indo-Muslim civilization into the mid-twentieth century. This resistance made the eventual establishment of English- and Urdu-medium western education schools in Hyderabad in the early twentieth century a significant reorientation for Hyderabad’s middle and upper classes. By the end of the twentieth century, English, as an urban centred international language and ‘structure of feeling’, engaged Hyderabadi and other South Asians in an increasingly global economy and society.

The third methodological issue concerns the limits of one of the founding assumptions of the research, namely that I was tracing a cohesive cultural formation, and multi-sited research helped highlight these limits. In this case, it was the so-called ‘Deccani synthesis’, a cultural synthesis drawing on the regional Telugu, Marathi, and Kannada languages as well as the dominant Persian and Urdu based Indo-Muslim urban culture. While recognizing that the designation ‘Hyderabadi’ inaccurately projected ‘a group of people as a unified actor’, as Handler (1985, 178–79) puts it, I nonetheless found it useful to follow self-identified ‘Hyderabadis’ of diverse backgrounds for the project. In tracing their movements, I not only agreed with Marcus (1995, 106) that ‘an initial, baseline conceptual identity … turns out to be contingent and malleable as one traces it’, I also confirmed that no single sense of Hyderabadi citizenship and culture had existed initially.³ Thus while it was no surprise that ‘Hyderabadiness’ was being reproduced or produced very differently in the new locales, only through multi-sited research could these

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³ Jim Ferguson’s oft-quoted observation (1999, 208) is apt: ‘Here there is much to be understood, but none of the participants in the scene can claim to understand it all or even take it all in … Anthropological understanding must take on a different character when to understand things like the natives is to miss most of what is going on’.
differences be discovered and documented. The re-visionings of old Hyderabad from abroad did shed much light on the migrants’ varied and fluid identities, highlighting the inadequacy of notions of fixed, inherited identities in both past and present and establishing the importance of interactions in new contexts.

The fourth methodological issue concerns these very interactions between contexts or places and people. What was my research field, and how was it delineated? The new sites themselves were nation-states, bounded sites that exercised considerable influence over immigrants in the constraints and opportunities they offered for settlement, citizenship, and work. However, the Hyderabadi migrants remained unbounded, maintaining multiple connections both emotionally and physically as they led multi-sited lives. Thus, in this volume Maeder and Nadai talk about ‘fuzzy fields’ and Candea defends bounded field sites but stresses their incompleteness, their partial views of more complex phenomena. Certainly the Hyderabads were cosmopolitan, moving and putting down new roots, and relatives and friends of theirs also moved and put down new roots, in not just one but several places, and people kept in touch. My research involved, then, not a field of bounded sites but (following Hine 2000) fields of relations, relations across sites and across time too, because the project took many years to complete. Hyderabads in the diaspora saw their worlds very much in terms of relationships among people, as Sanjay Srivastava, in Australia, writes about South Asian sensibilities. Hyderabads stayed in close touch with their own relatives and friends (particularly their schoolmates and classmates), and these relationships spanned nations. However, the fields of relations have shifted for the young, the members of the second generation abroad. I argue in the conclusion that there is really no diasporic second generation, an insight possible only through multi-sited observations of the second generations abroad.

A final methodological issue concerns just who or what one is tracking across sites. I started by following the subjects, but I was also following ideas, ideas about old Hyderabad and the identities held by individuals, families, and communities there. And this brings us to the power of ideas about Hyderabadi history and culture: could those ideas continue to lend meaning to the lives of Hyderabads abroad?

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4 Hine (2000, 60) suggested moving from a notion of field as bounded site to field of relations.

5 Srivastava (2005) wrote that South Asians attach as much, if not more, importance to the relationship between humans as an aspect of belonging and attachment than to fixed places (ancestral villages or ‘native places’). Asserting that journeying, moving, going and coming, have been a recurring leitmotif in South Asian life, and that notions of home, ancestral place, and attachment have always been complex, Srivastava moved toward theorizing a South Asian cultural sensibility that contrasts with western notions of motility.

6 Hine writes similarly (2000, 60): ‘Ethnographers might still start from a particular place, but would be encouraged to follow connections which were made meaningful from that setting. The ethnographic sensitivity would focus on the ways in which particular places were made meaningful and visible. Ethnography in this strategy becomes as much a process of following connections as it is a period of inhabitance’.
Although the emigrants were translocal or transnational, imaginatively participating in social worlds grounded elsewhere and able to mobilize resources in states other than those in which they resided, their Hyderabadi identities essentially fell apart in the diaspora. This happened despite important institutional and personal networks that linked many first generation Hyderabadi emigrants. What I found in the new sites was that the meaningfulness, the utility of ideas about the Hyderabadi homeland and its cultural synthesis, varied greatly. Immigrants thought about themselves and placed themselves abroad in very different ways, working not only with the interface of old and new national ideologies and policies but with new configurations of fellow citizens and immigrants as they empowered themselves by retaining, reconstituting, or erasing components of their homeland identities.

Now I turn to the ethnographic findings, the answers to questions about the partially shared culture of Hyderabad. What held it together in the homeland and what pulled it apart abroad? Could the migrants make Hyderabad, and themselves as Hyderabads, meaningful in new places? How did they present themselves to others and to their own children in each place? Multi-sited research showed that the new contexts played powerful roles in reconfiguring immigrant identities, for it is not enough simply to follow a designated group from the homeland and focus on it. One needs to see what opportunities and constraints characterize each site, not just the laws and policies governing immigration, citizenship, and work conditions but who else is settled there, what the interactions have been and are among all those living and working there, what aspects of one’s identity are most salient to others there.

There were nodes of communication and institutions linking first generation Hyderabadi emigrants together outside India, but over time some linkages weakened. These nodes included the Hyderabadi Urdu newspapers to which many emigrants subscribed and several Urdu literary associations that crossed national borders, as well as the Hyderabadi associations founded in some but not all of the overseas sites. More personally, families and communities tried to build and retain marriage networks across national boundaries and families also tried to reunite over time in an overseas site, strategizing about where, among the new sites, immigration and citizenship opportunities and facilities for care of the elderly were best.

Nodes of communication in Urdu, Hyderabad State’s official language from 1883 to 1948 and the language of Osmania University (inaugurated in 1918), India’s first vernacular medium university, were quite important to first generation emigrants. The Urdu newspaper *Siyasat* and especially its Sunday overseas edition, along with *Rehnuma-i-Deccan* and *Munsif*, provided important links to the

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7 In his important study of Sindhi businessmen, Falzon (2005) discusses transnational linkages.

8 Here is another advantage of non-simultaneous and therefore longitudinal multi-sited research: for instance, Weißköppel (this volume) traced linkages over two years, but I was able to do so for more than ten years.
homeland for many Hyderabadi emigrants. Urdu literary societies sprang up in various sites and spanned them, competing with each other. Other speakers of Urdu from South Asia, depending on the site, were also members of these societies, and Hyderabalis participated enthusiastically in them. In Australia and Canada, some of these societies were clearly local, while in other sites the societies functioned transnationally, awarding prizes to writers in North America and Europe.

Some sites, however, were off the transnational radar. Urdu was proclaimed Pakistan’s official language, but Hyderabadi Urdu was a negative identity marker in Pakistan. Furthermore, Punjabi literary activities, intended to promote a ‘minority’ language (although Punjabis constitute about 70 per cent of Pakistanis), were more vigorous than Urdu ones. In Kuwait and the UAE, where Hyderabalis expatriate workers at all socioeconomic levels stayed for years at a time, Urdu literary societies (the Bazm-i-Urdu and Bazm-i-Deccan) helped keep expatriates in touch with the homeland but did not seem to be linked to other overseas sites.

Hyderabali associations were founded in some sites but not others, depending primarily on the ‘fit’ of the Deccani cultural synthesis notion with the new national projects. Pakistan was the least hospitable to a Hyderabadi identity, despite substantial contributions of talent and money to the new Islamic state by Hyderabalis moving to it from 1947 into the 1960s. Muslims from India were termed *muhajirs* in Pakistan, meaning refugees or exiles, and the once proud *mulki* Hyderabalis hated to be termed that. One man asked me, ‘How could we be refugees, coming to our homeland?’ (Leonard 2007, 57). Pakistani Hyderabalis strove to put their Islamic and Pakistani identities forward and erase or suppress their Hyderabadi identities. They re-visioned old Hyderabad: some saw it as a failed Islamic state and blamed the cultural synthesis for displacing Islam, while others denied that there had been a cultural synthesis and claimed Hyderabad had been a successful Islamic state until its conquest by Hindu India (Leonard 2007, 65–71, 73–4). They also devalued Hyderabadi Urdu, a distinctive dialect ranked below North Indian standard Urdu by the more numerous other *muhajirs* in the new state, and they formed a Bahadur Yar Jung Academy, not a Hyderabadi association, in Karachi. In Pakistan, there was no celebration of Hyderabad city’s 400th anniversary in 1990, and it was only in the 1990s that ‘old boy’ and ‘old girl’ associations emerged as ‘fronts’ for Hyderabadi identity.

The first Hyderabadi associations abroad had been formed in Britain by students from Hyderabad long before 1948. Hyderabad State and the Nizam were fairly well known in Britain, and the Nizam’s government had regularly sent students

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9 *Siyasat*’s editor Abid Ali Khan gave special attention to stories from and about the diaspora, and he travelled abroad himself to be chief guest at Hyderabadi events, particularly when Hyderabad city’s 400th anniversary was celebrated in Kuwait, Riyadh, and London in 1990 (Leonard 2007, 224–5).

10 In Dubai, two English language newspapers, the *Khaleej Times* and *Gulf News*, employed Hyderabadi journalists from both Pakistan and India and sometimes highlighted homeland issues.
there from the beginning of the twentieth century. Many Hyderabadi students in
Britain in 1948 stayed on, thinking of it as a third space that was neither India nor
Pakistan; they also invoked Hyderabad State’s status as Britain’s ‘faithful ally’
rather than its former colony, wanting to be received as equals. Other Hyderabads
from both India and Pakistan joined them, and in the 1970s an English name, 
Hyderabad Deccan Association, was chosen when the Hyderabadi organization
was officially registered. In the 1980s, the association began inviting the last
Nizam’s younger grandson, Prince Muffakham Jah, who resides in London half
of the year, to attend functions, and attendance increased, culminating in a grand
celebration of Hyderabad city’s 400th anniversary in 1990.

The USA and Canada opened up for Hyderabadi students and economic
migrants from both India and Pakistan by the late 1960s, and the Deccani synthesis
idea seemed to fit with national goals of pluralism or multiculturalism. Led by
those close to Prince Muffakham Jah, the Hyderabad Foundation of Chicago was
founded in 1985, followed by associations in Los Angeles in 1986, San Jose in 1986,
was formed in Toronto, Canada, in 1986, again with princely encouragement. In
Hyderabad itself, a Hyderabad Deccan Society was started in 1983 and registered in
1989. The association in the homeland had connections through Prince Muffakham
Jah and others that were especially strong to associations in the United States.
Conscious of these other efforts, the London association proclaimed itself ‘the
premier Association of Hyderabads in the world’ in 2000 (Leonard 2007, 102).

Australia too loosened restrictions on immigrants in the 1960s but the older
‘white Australia’ policy really lapsed only in 1973. Anglo-Indians with British
passports could immigrate before that, and many Hyderabadi Anglo-Indians
did go there immediately after 1948. Despite the presence of the last Nizam’s
older grandson and heir, Prince Mukarram Jah, in Perth for many years, no
Hyderabad association was formed in Australia.12 This was probably because
multiculturalism was so late to be officially embraced, and although the post-1948
Hyderabad Anglo-Indian immigrants did speak Urdu, cook Hyderabad food,
and feel nostalgic for their homeland, they had become Australian in significant
ways by the time Hyderabad native speakers of Urdu came in numbers in the
1970s and 1980s. By then, too, the old society in Hyderabad had fallen apart with
the decisive political empowerment of Telugu speakers from coastal Andhra. As
discussed below, Hyderabadi Muslim immigrants in Australia have emphasised
Urdu literary and Islamic activities.

11 The Hyderabad Deccan Association of [southern] California’s 1990 celebration of
the city’s 400th anniversary was the event that started me on this multi-sited ethnographic
project.

12 In both Melbourne, Australia, and Dubai in the UAE, I was invited to dinner parties
ostensibly to discuss founding a Hyderabad association, in my role as a ‘circumstantial
activist’ (see Marcus 1995, 112–14). However, I thought the hosts were not serious about
this, although they wanted to know about associations elsewhere.
Hyderabads from both India and Pakistan and of all classes, linguistic, and religious backgrounds have been going to the Gulf states of the Middle East as expatriate workers since the 1970s. Restrictions on entry, property ownership, and citizenship combine with heavy regulations governing employment to make these sites transitory ones for Hyderabads. In the UAE, a Bazm-i-Deccan society functioned as a Hyderabadi association until class divisions brought it down in 1991. In Kuwait, a Hyderabad Association formed in 1990 by the leading Hyderabadi Indian businessman there, Hoshdar Khan, celebrated the 400th anniversary of Hyderabad city but then lapsed; a Hyderabad Muslim Welfare Organization formed there in the 1990s was small, only for Muslims, and worked on social and economic problems back in Hyderabad as much as in Kuwait. When Prince Muffakham Jah was trying to develop an international Hyderabad association in the 1990s, branches in the Gulf states were not envisioned because Hyderabads there could not be citizens and it was difficult for them to maintain even longterm residency in most cases.

All of these Hyderabadi associations initially stressed old Hyderabad’s Deccani synthesis and its ‘fit’ with the plural or multicultural societies of Britain and North America. However, they have changed over time, depending on the site. In some cases associations have become distinctly Muslim rather than multicultural in tone. Across the research sites, most of the Hyderabadi associations have failed to engage the second generation, chiefly because events tend to be formal dinners featuring speeches and poetry in Urdu.

By the late 1990s, the major organizations celebrating Hyderabad culture and maintaining cross-national networks among Hyderabad emigrants were newly-emerging ‘old boy’ and ‘old girl’ associations of the schools dominating the educational scene for Hyderabad’s middle and upper classes by the mid-twentieth century. These were both English and Urdu medium schools imparting western education.

Analysing the Urdu literary associations, Hyderabadi associations, and ‘old boy’ and ‘old girl’ networks described, it is clear that they vary in their transnational coverage and effectiveness and also vary in those over time. I draw attention to the inability of the second generation to appreciate and participate fully in these associational activities, based as the activities are in a sure command of literary Urdu and in school attendance in Hyderabad rather than in the new sites. Now I turn to networks that are based in family and community, ones that could possibly offer continuing translocal or transnational linkages to members of the second and subsequent generations of emigrants from Hyderabad.

Foremost among these must be marriage networks, as most first generation Hyderabadis of Hindu, Muslim, Parsi, and Sikh backgrounds are trying to arrange or semi-arrange the marriages of their children within customary caste and community boundaries. This has meant, often, not only consulting relatives and friends in several sites (including Hyderabad) about potential brides or grooms for young Hyderabadis abroad, but actually making marriages across national boundaries. Here Pakistan, with its many Hyderabad Muslim settlers of the 1950s,
emerges just behind Hyderabad, India, as a source of potential Muslim marriage partners and a site for marriages in South Asia. Marriage making, as a process, involved considerations beyond the matching of two individuals, such as where relatives and friends were settling abroad and where one’s aging parents might best be resettled in the future.

‘What’s happened to the Mughal princess?’, one immigrant put it, trying to frame for me the subject of the marriages of young Hyderabadis abroad. A Hyderabad Hindu who went to California in 1962 told me that, brought up in old Hyderabad in the culture of the Nizam’s days, his boyhood dreams were always of a Mughal princess, an image very familiar from Persian and Urdu poetry and Indo-Muslim miniature paintings. Possibly many young men from Hyderabad who went abroad before the late 1960s dreamed of a Mughal princess, but who stands in the place of the Mughal princess or prince for younger members of the Hyderabad diaspora communities, for members of the second generation? Some answers are emerging, ones not quite conforming to the boyhood fantasy above – for the record, my informant married a Greek American and his children have married Arab and Filipino Americans.

Certainly there was and still is something of a transnational marriage market for the young people of Hyderabadi heritage. One could discern patterns of spousal preference, by countries and by gender, as people discussed marriage making considerations across the various diasporic sites. The directions of bridegiving indicate a new North American centre of the Hyderabad global networks. Few brides were sent to India or Pakistan from abroad, although some families continued marriages that drew on close relatives regardless of location. A few brides went to Australia but it was clearly not a magnet for brides from Hyderabad and Karachi. Young Hyderabadis went to Australia as couples, or Hyderabadis men tended to marry local Australians or New Zealanders, or grooms were brought from Hyderabad, pointing to the good prospects for employment there in much of the late twentieth century. Britain drew upon Hyderabad and Pakistan for brides and also sent brides to the USA, although the dominant pattern was to arrange marriages between young people of Hyderabad background in Britain. The USA and Canada attracted both brides and grooms from all other sites, although young men brought up in India or Pakistan were thought far more difficult to ‘socialize’ than young women from anywhere. Young women of Hyderabadi or Karachi background were, however, apprehensive about marrying men brought up in the West. Young girls in Hyderabad, particularly, talked warily about wanting to avoid such marriages.

Settlement patterns within and across nations significantly influenced marriage arrangements, which fostered multi-sited research on my part even within the sites. In Pakistan, young men on their own in places other than Karachi tended to marry non-Hyderabadi Pakistanis, while members of the large community of Hyderabadis in Karachi preferred to arrange marriages within that community. The earliest students in Britain, the USA, and Canada, sometimes married outside their communities, as did many Anglo-Indian immigrants to Australia. The numerous
later-arriving professional immigrants in the western countries tried to maintain the practice of arranged marriages. Matchmaking proceeded through relatives, advertisements, and, sometimes, functions held to bring youth together. Families and communities preferred marriages of various sorts (cousins, regional or national origins), with Muslims either deliberately crossing or maintaining the Pakistan/India boundary depending on family preferences. Marriages also reflected parents’ political prejudices and orientations to the future, and in this way, marriages continue to reflect transnational considerations and dis-/connections.

However, the notion that extended family membership or previous relationships among members of the first generation abroad could overcome differences of national socialization of the members of the second generation has now been tested often and is increasingly found wanting. Unsuccessful cross-national marriages have inclined people toward matches made within the countries. Marriages arranged with or between expatriates, even between relatives, sometimes ‘went bad’, and one heard of such troubles in family after family. Usually these stories concerned daughters given in marriage to a man working abroad, especially in the western countries. Divorces are not uncommon among second-generation Hyderabadis abroad in all sites now. Some occur so soon after an arranged or semi-arranged marriage has taken place that one wedding photographer waits to edit his wedding videos two or three weeks because some couples call off the marriage that quickly(!) Marriages across caste and community boundaries have increased, with ‘old Hyderabadis’ marrying other ‘old Hyderabadis’ both in the city and in the diaspora. Such instances included not only love marriages but arranged or semi-arranged marriages. The preferences, then, are for broader cultural and national categories such as ‘old Hyderabadis’ or mulki Hyderabadi or American, British, Australian, and so on rather than for narrower traditional categories.

Wedding festivities themselves are major events connecting Hyderabadis in the homeland and abroad, even as fewer matches are being made across national boundaries. One partner in a marriage might be from Hyderabad or Karachi, or the weddings of young second generation Hyderabadis abroad are held wholly or partially back in Hyderabad or Karachi, or relatives and friends fly here and there around the world to attend weddings. Relatives in Hyderabad or Karachi, with more and more weddings being held abroad, sometimes participate by sending costumes or other items.13 But weddings are still magnets pulling emigrants back to the homeland even if only temporarily, and weddings where both bride and groom live abroad are held there for several reasons. Most relatives might still live in the homeland, or the family’s audience, those it wanted to impress with its success abroad, might still live chiefly there. Then, the older generation has more success in maintaining the tradition of five to seven days of ceremonies in Hyderabad or Karachi, and stories abound of

13 In a Los Angeles wedding, jasmine was flown in from the Philippines and special needles for threading flowers were sent from Hyderabad so that family members in LA could produce the garlands. The groom was from Pakistan and the bride, born in London, lived in LA.
young independent women, living in their own apartments in the USA or Canada, made to sit meekly for hours in heavy wedding finery in an Indian or Pakistani function hall. Another reason is the availability and cheaper cost of experienced caterers and suppliers of everything needed for a Hyderabadi wedding. Although it is more costly and difficult to hold Hyderabadi weddings outside Hyderabad or Karachi, the balance is tipping toward holding weddings in the countries where the young couples or their families are residing.

In conclusion, let me consider what held Hyderabadi culture together in the pre-1948 state and city and what pulls its bearers apart in the diaspora, analysing the findings and relating them to the methodological issues initially discussed. The selective emphasis or erasure of various aspects of Hyderabadi identity could only be seen through multi-sited research. These translocal or transnational linkages – based on Urdu newspapers and societies, Hyderabad associations, ‘old boy’ and ‘old girl’ status, and second generation marriages – were shown to be of varying reach and longevity. The measurement of longevity was possible because simultaneous research in the eight research sites could not be achieved (the first methodological issue), but the longitudinal research, the tracing of transnational linkages from multiple sites over time, proved an advantage. In addition to tracing the transnational linkages, one needed to consider the associations anchored in the new sites that could link Hyderabidis with non-Hyderabadi fellow citizens, and these relationships also changed over time. In Hyderabad State, for example, adherents of the Indo-Muslim court and administrative culture included Hindus, Sikhs, Parsis, and Anglo-Indian Christians as well as Muslims, and religious identities were important in the private, not public, realm. In some of the overseas sites, however, religion and co-religionists have exercised strong influences on immigrant Hyderabadis, and religion has become a far more significant part of public identity than it had been in Hyderabad.

Again, comparisons across sites and over time highlighted the differing emphases placed upon religion in identity reconfigurations. The following discussion of Hyderabadi Muslims in particular illustrates that methodologically, simultaneous research in the sites is not necessary, but that multi-sited research at its best is interactive research and that it is an advantage to carry it out over time. Those Hyderabadi Muslims who moved to Pakistan did so for primarily religious reasons, seeing their needs best served in a state by and for Muslims. Yet once they were there it was the Pakistani nation and the construction of a strong Pakistani political identity that became most salient. Rather than talk about Pakistan’s contemporary efforts to build an Islamic state or about their own beliefs and actions as Muslims, they talked to me about the past, about the Nizam’s failures either to create or defend an Islamic state. Their Islamic identity was taken for granted and it was their national identity that needed defending, it seemed, for both first and second generation Hyderabadis in Pakistan. The first generation Hyderabadis in Pakistan suppressed their Hyderabadi identities but the second generation Hyderabadis there replaced them with local and regional identities such as Lahori, Karachiite, or Pathan.
When I did the research in Britain, ‘black British’ coalition politics rather than religious identities dominated identity politics in the public arena. This coalition was, however, in the process of splitting, as the champions of West Indian/South Asian unity efforts heard South Asian Muslims protest that their interests were different and separate (and young Hindus had not yet mobilized). Hyderabadi Muslims in Britain did not see themselves as part of the black British coalition, nor did the Hyderabadi Muslims (the largest group of Hyderabidis in Britain) see themselves as allied with the many other South Asian Muslims in Britain, most of whom were working class Pakistani Punjabis and Kashmiris. Hyderabidis in Britain, at least in their public organizational expressions, remained loyal to the Deccani synthesis ideal; they did not build or attend particular mosques and their religion was a private matter.

In Australia, in contrast to Britain, Hyderabadi Muslims (coming in substantial numbers from the 1970s) joined the mosques and Islamic organizations founded by the Lebanese and other Arab Muslims who had preceded them; these Muslims worked together to develop an Australian Muslim identity. Similarly, the first Hyderabadi immigrants in Australia, the Catholic and Protestant Anglo-Indians, joined existing churches rather than organize separately from other Christians. There was even some shifting of national labels: some first generation Hyderabadi Indian Muslims in Australia joined a Pakistani association (my research there took place shortly after the destruction by Hindu fanatics of the Babri masjid [mosque] in 1992 in northern India), and some second generation Anglo-Indians felt more comfortable with the national label ‘Indian’ or ‘Australian’ than with the ‘Anglo-Indian’ identity their parents had fought to establish in India. Interestingly, unlike in Britain, where most Muslims were South Asian but working class, in Canada, most Muslims were South Asian but professionals, and in Canada Hyderabadi Muslims from both India and Pakistan took up public roles as Muslims. When the first Hyderabad association in Toronto folded, its funds were donated to a local mosque.

In the USA the religious landscape was more complex than in the other sites abroad, particularly with respect to Islam and Muslims. In the USA, most Muslims were working class African Americans (estimated at 30 to 42 per cent), while multi-class Arabs (some had migrated in the late nineteenth century) and post-1965 middle and upper class South Asians vied with each other for second place (estimates for each ranged between 20 to 30 per cent). These and other very diverse Muslims were, from the 1980s, trying to build national political coalitions, and Hyderabidis were among the leaders. Other Hyderabidis, Muslims included, engaged in building inter-faith coalitions or South Asian political coalitions.

Finally, religious identities for Hyderabidis working in Kuwait and the UAE continued to be expressed privately rather than publicly. Not all Hyderabidis working in the Gulf sites were Muslims. Many Hindus and Christians and a few Parsis and Sikhs from Hyderabad also worked in the Gulf, and in the UAE Hindu temples as well as Christian churches were permitted. Despite the dominance of Islam in the public arenas of the Gulf states, Hyderabadi Muslims often felt that
the practice of Islam in the Arab heartland differed greatly from their own. On the whole, religious life as well as social life was quite separate for citizens and expatriates in the Gulf.

Turning to the second and third methodological issues, the importance of the starting point and whether the emigrants being traced constituted a cohesive cultural formation, multi-sited longitudinal research provided insights into the past as well as the present. These are issues where the researcher’s initial knowledge and stance are important but where further discoveries are invariably made in the course of the research. In the material presented above, one can see the continuing influence of the history and culture of the homeland, even as the destination sites, the new nation-states, assume prominence in migrant narratives. The stories told by the emigrants linked them in one way or another to the former court culture and administrative structures centred in Hyderabad city. But Hyderabad State was vanquished in 1948 and vanished in 1956, and the emigrants have by and large taken up citizenship in their new nations. Yet the continuing importance of concepts of belonging and non-belonging, of words like mulki and muhajir, was clear. These terms, denoting access or lack of it to full citizenship in Hyderabad and in Pakistan, showed the importance of movement in the past, the importance of locating oneself and one’s family and community in a place and a polity. Mulki had had a legal meaning in old Hyderabad: mulki status defined access to jobs and privileges, and there were rules about attaining that status after moving to Hyderabad. The term was still meaningful abroad, as people identified others as mulki or non-mulki; some questioned why I was speaking to those they considered non-mulkis. The continuing meaningfulness of mulki or countryman with its layers of contested belonging made the continuing designation of the Hyderabadis in Pakistan as muhajirs even more painful. This relates to the fifth methodological point, concerning the tracking not just of people but of ideas and their continuing relevance to the emigrants’ lives.

The fourth methodological issue concerned the interactions between contexts or places and people. Along with an awareness of the constraints and opportunities offered by each destination state, people showed a strong awareness of the multi-sited human landscape, the relatives and friends located in several places and the ways those relationships figured in migration decisions. The power of the new states in attracting migrants and shaping identities was evident from the interactions of state policies and diverse national populations with respect to class and religion, in particular, as we have seen. The role of transnational ‘structures of feeling’ also proved crucial here, and this too came from the past and was reinforced in the present. The strongest networks among members of the first generation abroad, the networks that continued to cross religious and caste lines, were those of classmates and schoolmates, and these were rooted in the institutions of western education developed in Hyderabad city by the mid-twentieth century. The middle and upper class migrants who had been schooled together in the developing English-medium schools in the 1930s and 1940s were already moving from one ‘structure of feeling’ to another,
moving from old Hyderabad even before it ended into what Thrift (1996) has described so well as an ‘emergent structure of feeling’. They had become part of a transnational and cosmopolitan world, a globalizing economy and society dominated by the English language, and many of them, including some from Pakistan, ended up migrating to the western nations that opened up after the 1960s. These cosmopolitan migrants illustrate, again, the diverse layers of culture in the homeland and the trajectories shaped by it.

These fields of relations, of human connections across nations and of family strategies developed with several sites in mind, characterized members of the first generation, as we social scientists term those who move. All of these reconfigurations of Hyderabadi identities emerged through ethnographic work across sites and over time. Multi-sited research proved rewardingly interactive, providing rich comparisons and contrasts among the multiply-connected emigrants from Hyderabad and their unbounded, transnational lives.

References


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Attempts to do multi-sited ethnography push ethnography (and the professional craft of fieldwork) to the limits of its classic aesthetic or ‘feel’ (expressed in all the quite informal, but crucially regulative, shop talk about what is and is not good ethnography), for which I will let the Malinowskian paradigm or complex stand as a short hand in this essay. Much social and cultural anthropology still operates within the limits governed by the Malinowskian complex in the operation of professional culture. The ethnography of peoples, places, and cultures in situ, and their immense contemporary transformations, is, piecemeal, alive and vigorous in all sorts of interdisciplinary venues which define anthropology’s participations and research agendas. However, particularly over the last decade anthropologists have also been trying to do something quite different with ethnography, and not with just ethnography plus other methods (such as archival research, literary analysis, theoretical exegesis, and so on), which is a common solution to the limits challenge of making ethnography multi-sited. This entails the valorization of methodological bricolage and spectacular performance which are cultural studies styles that have had a profound influence on the anthropological culture of method (see Marcus 2007).

An unreconstructed Malinowskian practice does indeed make the idea of multi-sited ethnography as the major modality of basic research difficult to practice. Yet, there is also a considerable desire for and attraction to the idea of multi-sited ethnography within this very same tradition. There is something about the way traditional units or objects of study – for example culture, cultures, community, subjects – present themselves nowadays, combined with the near revolution in theory, that has immensely complicated the way these classic terms are understood operationally, and that makes one want to conceive of Malinowskian ethnography within time-space frames that instil pragmatic doubt about its very feasibility under the current regime of research norms. Ethnography, deeply, is a certain sort of mise en scène, and when its conditions cannot be produced, its virtues are counterfeit. In adhering to this position, multi-sited ethnography is very difficult to do indeed within the current way research is taught in anthropology (see Faubion and Marcus forthcoming).

1 A different version of this chapter appears in Multi-sited Ethnography: Problems and Possibilities in the Translocation of Research Methods, edited by Simon Coleman and Pauline von Hellermann (2009).
My particular vantage point on the challenges that multi-sited ethnography pose to anthropological research has been as a supervisor of doctoral dissertations over the past 20 years—a period of tectonic change in the way that classic training is instilled by negotiation with student talent of shifting demographic character and intellectual motivation. The dissertation is a strategic site in several respects. The creation and implementation of an alternative practice of fieldwork out of the Malinowskian tradition are possible where disciplinary metamethod has most effect, where ethnographers are made at this critical point in the mode of professional reproduction.

There was a rupture through the 1980s, and a shift in the centre of gravity in the research orientation of anthropology from topics like religion, kinship, myth, and ritual, to the kinds of issues that mostly development anthropologists had studied before. Still, the culture of method or metamethod has continued authoritatively. Students are now muddling through—often very interestingly—in multi-sited space, with skeins of theory, traditional practices that bound projects of research, and familiar, comfortable topoi like identity and exchange sustaining the anthropological framing in research ventures that could very well (and I think should) be about something else, in terms of idioms found inside the realm of fieldwork itself. Reading new work—how projects of changing fieldwork challenges still manage to fit into the genre of ethnography such as it is—and also studying the muddling through at the very beginning of careers, at the core of the training model, have been for me the most interesting materials to think with in contemplating multi-sited ethnography as a distinctive paradigm of alternative research practices, while still remaining true in specific ways to the Malinowskian complex.

I understand making ethnography multi-sited to challenge four pillars of the still regnant Malinowskian complex, expressed as worries or anxieties. Foremost, there is the worry about the further dilution of an already diluted practice since the ruptures of the 1980s with past disciplinary agendas, and especially in the doing of apprentice research projects which launch careers. The Malinowskian ethos of ethnographic research as focused, sustained, intensive life in communities of distinctive difference, and its North American emphasis on interpretation—working through the logics of subjects’ perspectives as the mode of developing ethnographic analyses from fieldwork—are endangered. Lurking here is the idea that anthropology will become even more like cultural studies and its interdisciplinary inspirations, which in a sense captured its imagination and research agendas after the 1980s.

Second, and relatedly, there is the fear that ethnography will become more about systems, institutions, formal organizations, the structures of Western rationality, progress, modernity, and the thought of experts, than about the conditions of common experience, observed as everyday life in its own idiom. Indeed, multi-sited ethnography has been most creative, critical, and interesting where it has been involved with the study of formal systems and the effects of expert knowledge in their interfaces with ordinary life (mostly within the growing field of science and technology studies, but not only there; see the 2004 volume by
Ong and Collier, for a sense of the diverse structural, systemic interests – political, economic, scientific, and so on – brought together in multi-sited imaginaries).

But there is something uncomfortable for the anthropological ethos in this delving into the plans and practices of bureaucracies and their protocols of substantive rationality. Take the recent work of Marilyn Strathern (Strathern 2004). After work on new reproductive technologies, keeping it symbolically anthropological by skilfully connecting this research comparatively to her established Melanesian work, she has moved boldly into the study of audit and policy cultures, homo academicus today, the ethnography of her own frames of work in knowledge production, without such ‘markings’ that guarantee its anthropological identity. Following her own student Annelise Riles into the ethnographic study of the ‘already known’ (Riles 2002) – the predicaments of bureaucrats and designers of interdisciplinary programmes, and how the knowledge they produce is circulated and dispersed – Strathern evidences a sensitivity that what she is doing may no longer seem ethnographic in the anthropological tradition. She is in a terrain where there is nothing ‘nail biting’ – meaning the life or death situations of the everyday which create interest in ethnography in anthropology. She refuses either the identity or exchange markers of ‘otherness’ that make ethnography anthropological these days in unsettling, or defamiliarizing, ‘natural’ understandings of familiar institutional terrains. Strathern delves into the known, or simply more technical (read boring?) worlds of bureaucracies for a different kind of purer ethnographic result, true to what is found in those sites.

My point here is that she is ironically uncomfortable in her ethnography and its possible incapacity to attract a readership expecting ethnography in either its more familiar, frankly exoticizing, idiom, or its ‘ordinary life’ idiom. I would argue instead that it is precisely in the yet to be articulated metamethod of multi-sited design as a context for such pioneering work that Strathern and others are doing that a vital, innovative continuity with the Malinowskian tradition depends. Strathern’s gesture of demur thus conveys something of the stakes of making ethnography multi-sited – to create a broader disciplinary constituency for its most innovative contemporary work, by not using the well established technique of the tying of such projects to the traditional ethnographic archive, in the way that Strathern did in her earlier work, for the sake of a different kind of result that is not always or only about the usual – identity, symbolic expression, or exchange. In sum, multi-sited work does not guarantee that ethnography will be about its expected tropes. This threatens the identity of ethnography itself but also produces a sense of excitement in finding new terms for ethnography within the doing of fieldwork itself.

Third, there is the worry that the demonstration of significant difference (for example, through the technique of defamiliarization) as a signature result or product of ethnographic research will vanish in multi-sited research, that ethnography will lose its distinctive rhetoric with which its functions are deeply bound up. Again, there is the worry here of ethnography as entry into the already known (the dynamics of policy, media, academic problematics which attract
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anthropological research projects). What is distinctive about the anthropological project – that it works through perspectives, categories, logics of subjects who are presumed to be other – leads to a liking aesthetically for the argument or result that produces defamiliarization or unsettling displacement. Multi-sited ethnography, oriented to process and connections, seems to threaten this, when the subject’s perspective is no longer so clearly other, but in the realm of the already known. The past habit of Malinowskian ethnography has been to take subjects as you find them in natural units of difference – cultures, communities; the habit or impulse of multi-sited research is to see subjects as differently constituted, as not products of essential units of difference only, but to see them in development – displaced, recombined, hybrid in the once popular idiom, alternatively imagined. Such research pushes beyond the situated subject of ethnography toward the system of relations which define them. Such subjects are potentially paraethnographers of their own conditions, and the intellectual partners of ethnographers when found – counterparts rather than others. Such subjects are key to the distinctive nature of multi-sited research. In contemporary settings, what is shared is the perception that local realities are produced elsewhere, through dispersed relations and agencies, generating a multi-sited imaginary, one that is practical for the subject, and that is a found design of a mobile ethnography for the anthropologist.

Fourth, culture area expertise remains basic to the formation of the anthropological ethnographer, and, to a degree, it calls the tune in how multi-sited ethnography might in a preferred way develop in this discipline. Multi-sited research incorporates factors of systematic cultural distinction but does not give them priority. So there is the worry that the emergence of this form of ethnography might undermine a component so basic to the Malinowskian complex. There have been many developments in culture analysis in recent years that have made more complex any area frame of expertise (area specialists are certainly not what they were in the 1950s through the 1970s), but the proposal of multi-sited ethnography troubles the heart of this construction of professional identity. Multi-sited ethnography indeed tends to cut across the geography of area studies, but without denying the factorial importance of particular cultural histories. It flexibly has it both ways, or all ways, but this hardly promotes the capital of area expertise

2 Paraethnography (see Holmes and Marcus 2005) is not merely a matter of identifying a new ethnographic subject – an accomplished autodidact. Rather, it opens far deeper questions about how culture operates within a continuously unfolding contemporary and where everyone, directly or indirectly, is implicated in and constituted by complex technical systems of knowledge, power, health, politics, media, economy, and so on. What is at stake in our conceptualization of the paraethnographic are formations of culture that are not fully contingent on convention, tradition, and ‘the past’, but rather constitute future-oriented cognitive practices that can generate novel configurations of meaning and action. Indeed, this gives rise to our most radical assertion – that spontaneously generated para-ethnographies are built into the structure of the contemporary and give form and content to a continuously unfolding skein of experience.
itself which is another of the pillars of being a traditional ethnographer of peoples and places.

So — primarily, dilution, and less articulated worries about the observable everyday, about the demonstration of difference as a result, and about the diminishing of the core importance of ‘peoples and places’ cultural expertise — this is the anxiety reaction formation to the idea of multi-sited ethnography, in sum.

Now as against all of these concerns about the effects of making ethnography multi-sited in any radical or unconventional way, there is also perceptible a real hope for the multi-sited idea as overcoming the felt limitation of ethnography — could it be more than just the production of case studies in the service of the agendas of whomever or whatever project that find them interesting or useful? If its own professional community is not a reliable judge of what it produces, can ethnography generate its own self-sustaining constituencies and contexts of reception within its own research design? This possibility of making reception itself a dimension of ethnographic method recreates the questions about the reflexive, dialogic nature of ethnographic research raised in the 1980s but limited then to the classic Malinowskian mise en scène. The questions just posed are formulated for a different terrain and design for ethnography suggested by the idea of multi-sitedness.

If ethnography no longer serves the ethnographic archive or knowledge bank, then it either serves other broader agendas or can create its own through the very practices of ethnography, derived from the major points of the 1980s critiques. So there is a welcome ambition in the idea of multi-sited ethnography, a speculation, that would expand the intellectual functions of post-1980s ethnographic research, despite the doubts about its practicality and its fealty to the powerful aesthetics of professional culture. What intellectual weight and ambition can the ethnographic project bear, beyond the case study? Crossing between zones of expert and common knowledge as most multi-sited projects entail, generates functions for ethnography beyond the analytic and descriptive characteristics of the case study. This is the warrant for programmes of trying to experiment in imaginative ways with the basic premises of the Malinowskian complex such that multi-sited ethnography does not mean mere extensions of them into added-on sites but a more theoretical rethinking of fieldwork itself.

My initial response to the worried reactions to multi-sited ethnography was to pose pragmatically a doctrine of ‘ethnography through thick and thin’ (Marcus 1998), and there are still norms to be advocated in relation to this, such as a strong norm and accountability for intended, structured partiality and incompleteness in ethnographic research designs. Where the strength of ethnography in multi-sited projects is variable, it should not be merely excused (for example as problems with differential access to certain sites or subjects), but should be justified by ethnographic design and argument itself (for example, in certain projects, certain sites are more strategic for intensive investigation than others). It is interesting and important to argue why some sites should be treated ‘thickly’ and others ‘thinly’ in terms of the loci and design of particular projects. So within the ‘dilution’
worry is actually an entire unexplored level of thinking ethnographically about a research problem, where the traditional subject or conditions of ethnography is not stereotypic. So thick and thin is as much a theoretical question as a matter of fieldwork pragmatics.

Once the idea that different related sites can be designed differently for ethnographic treatment, I then moved into questions about the alternative ways that multi-sitedness can emerge as a research space, not given by existing representations or understandings of processes, but rather as the mapping of a space or field of social action that is found in the field itself through closer work and collaboration with certain subjects. And this finding of multi-sited ethnography through the orienting work of ethnography itself suggests a modality of research that leads to a restatement of many aspects of the Malinowskian complex.

I want now to present in schematic and fragmentary form a certain line or progression of thinking about a modality for multi-sited ethnography that is a reform or reimagining of the Malinowskian complex in which I was brought up as a student, but for which to pass on to present students requires precisely forms and norms of ethnography that are stimulated by the emergence of multi-sited conditions of research.

1. What a project of multi-sited ethnography conceived and pursued by an individual is capable of

Some have suggested that the potential problems of multi-sited ethnography might be resolved with the return (or increase from the few well known traditional examples) of collective, coordinated research projects like the Rhodes Livingstone Institute, the Chiapas project, and such. Maybe. But the formal structure and forms of ethnographic research projects in anthropology remain resolutely individual, and I see this continuing into the foreseeable future since it is so programmed into the making of anthropologists professionally. However, individually conceived research becomes de facto collective in at least two ways – by the derivation of the space of fieldwork from orienting, strategic collaborations at the outset of fieldwork, and by the incorporation of forms of reception within the frame of research itself into the reporting and results of ethnography to academic and other constituencies.

The need to develop forms and norms in research design to anticipate and manage this collective nature of ethnography, despite its individualistic form in professional culture, is at the core of reforming Malinowskian method in multi-sited projects. These same issues were strongly raised in the critiques of the 1980s, but those critiques stayed within the classic Malinowskian complex. Multi-sitedness displaces the anthropologist-other binary, and creates collective aspects of research that might become a standard part of authoritative standards for ethnography. At the moment the revised tropes of ethnographic authority after the 1980s critiques offer some capacity, yet still very impaired, to express the
collective relations of research on which the design and conduct of multi-sited research vitally depends.

2. The alternative ways in which the multi-sited field might materialize in research

The multi-sited field is either conventionally a map of a process in various senses, but a map that is already understood and relied on by being expressed in some scholarly or academic literature (for example, economic or sociological models of migration, Marxist conceptions of the flow of global capital, or the proliferation of neo-liberal markets), or this field is found in the field itself, even in full knowledge of the academic literatures, through an orienting ethnographic process conceived as collaboration. In the formal mode, multi-sited ethnography emerges from the objective following of a known conventional process, or an unconventional process – following a commodity chain/productive process, migration networks, or following a plot/narrative, a metaphor, or circulation of an idea. This is the kind of multi-sited research that I discussed in my 1995 article (Marcus 1998). It challenges the Malinowskian complex but does not radically deconstruct and reconstruct it – Bourdieu’s scholastic point of view is sustained (Bourdieu 1990), rather than seeking epistemological mutually interested alliances with partners or counterparts as subjects, or with research ‘in the wild’, as Michel Callon (1998) has termed it.

During a recent (2004) year on a resident research fellowship and since, I have been interested in developing a modality for multi-sited ethnography that embraces this more radical rethinking of Malinowskian premises. This involves understanding the multi-sited field emerging from strategic collaborations with which fieldwork begins. The thorough discussion of the conditions and evolution of such a collaboration is at the core of working out the particular modality of multi-sited research in which I am interested.\(^3\) The conceptual apparatus and design of a research project is derived not from academic literatures or theories, but from ethnography itself by working through a selected subject’s or group’s paraethnographic take on a problem cognitively shared with the ethnographer. There is much to be filled in here about issues of research practice: how collaborative

3 On moving from Rice University to the University of California, Irvine, in 2005, I founded a Center for Ethnography, organized around a number of topical projects that define challenges to the classic ethnographic method. One of these is ‘Ethnography as/of Collaboration’. This project includes small conferences, conversations, research, and pedagogical experiments (such as staged ‘para-site’ interventions in the process of dissertation research) that explore past and emergent forms and norms of collaboration in the range of ethnographic practices (see <http://www.socsci.uci.edu/~ethnog/>). Also, collaborative models are at the heart of conversations between Paul Rabinow and myself about the nature of anthropology in contemporary times (see Rabinow et al. 2008).
alliance emerges, the various ways the connection to paraethnography can be established, what paraethnography, in practice, is, and so on. Most ethnography today passes through zones of specialized, technical knowledge before it defines the traditional fieldsite; it can no longer afford to ignore these existing representations in deference to the authority of the academic. What is distinctive about anthropology and precious to preserve in the Malinowskian ethos of ethnography is the pretense and claim to be able to work through subject positions, perspectives, and meanings in order to establish one’s own knowledge. What produces this commitment at the core of ethnography in multi-sited research is the strategic engagement with paraethnographic perspectives in research, epistemologically equivalent to one’s own, and working through them literally into other sites of fieldwork. Independent ethnographic knowledge is a derivation of this process.

There is a literal and figurative odyssey here that defines this way of thinking about multi-sited research. The design of such research is reflexive in a sense that goes beyond the way this operation in traditional research was raised in the 1980s critiques and which has now become thoroughly clichéd as a norm of conventional practice.

In the Malinowskian complex, reflexivity becomes the norms and standards for the designing of ethnography through collaborations and eventual departures from them. It is the necessary account of how the multi-sited field emerges in any project. In this view, multi-sitedness arises from how one sort of subject (often experts but not necessarily) sees the world versus how another, the anthropologist, sees the putatively same world. Out of this relation comes the literal movement of the anthropologist beyond it, but within the ken, so to speak, of this strategic relation.

Now there are many ways to express this modality. The one I have been cultivating is the idea that the field exists in a world of distributed knowledge systems and this is often the frame and subject of finding paraethnography. In the anthropologist’s striving for a labile multi-sited ethnography that works through processes and in locales, distributed knowledge systems encompass, but replace the dominating conceptual role of culture.

To give a sense of this, I quote a recent email exchange between myself and a former student, Kim Fortun, known especially for her 2001 book *Advocacy After Bhopal*. It represents a brilliantly evolved rendition of improvisations through the stimulus of the necessity to write a book for tenure about what a multi-sited project of the sort in which I am interested here might be within the standard professional work process in how careers begin. She went to Bhopal three years after the accident in 1984 and lived for two years with activists working on social justice and environmental issues. She wrote her dissertation on the work of activists at Bhopal but even then the multi-sited dimensions for the ethnography were apparent (during that period when rhetoric was a powerful analytic influence in generating ethnography, her work was on the forms of everyday and specialized writing that these activists produced).

She actually wrote a dissertation on advocacy as a mode of thought and practice, but advocacy itself encompasses a kind of paraethnography, as she
demonstrates. From Bhopal over the succeeding years she followed the reach of that paraethnography elsewhere in diverse settings but always in conversation with and amendment of the Bhopal material. The published ethnography, a ‘messy’ text, an experiment pushing the limits of the ethnographic form while keeping it under control, is in conversation, in a double voiced way, with both the ‘from the field’ discourses of advocacy and the relevant academic literatures. It is clearly Malinowskian in its anthropology, but within a practice of ethnographic research still lacking articulation, standards, and expectations.

**An exchange with Kim Fortun, 2 May 2005**

George Marcus (Center for Advanced Study in the Behavioral Sciences): This has been a pretty good year – me alone with my thoughts – I find that in order to get into the reformist project on ethnographic metamethod that I have in mind, I have to give a reader a fairly clear idea up front of the sort of world that fieldworkers (especially apprentices/neophytes) encounter now. I don’t want to use assemblages, regimes of living, emergent forms of life – these are ok but they reflect the STS (social studies of science) project. I want something more generic – I like the notion of encountering and finding oneself amidst ‘distributed knowledge systems’ rather than ‘other cultures’.

The template for teaching students ethnography is still that they go out into the world and find other cultures – some do – the Malinowskian scene – but many more (those who I tend to supervise) tend to find themselves in the middle of distributed knowledge systems, which is the way that other cultures manifestly present themselves these days anyhow. These are not the contexts of culture in villages and communities etc. – but the form that culture takes so that even if you are not studying experts first, or as such, and are working in villages, you are also operating in distributed knowledge systems which are the challenge of fieldwork to figure out and operate within. Ethnography needs a new set of understandings of itself as metamethod still in the anthropological tradition to come to terms with this, etc.

Anyhow, what I need here is some stimulation about how to develop the idea of distributed knowledge systems – something you don’t map completely before fieldwork but something you map as a function of fieldwork itself.

Kim Fortun: On distributed knowledge systems ... a few things come to mind, some of which may already be obvious to you ... but to start ...

Thinking about culture as manifested through distributed knowledge systems seems to me related to, or a partial effect of, thinking about the (ethnographic) subject as manifest at the nexus of cross-cutting discursive, political-economic, cultural currents ... So ethnographic subjects need to be accounted for as nodes in distributed knowledge systems. Each has her own specificity; each subject is a tangle of a particular set of forces. So there is ‘culture’ in the trans-individual sense, but it settles into different subjects in different ways. And these are particular kinds of subjects – very subject to change because they operate in always moving currents of information, political economy, etc. The need for active sense making, often without known to be reliable
criteria, is incessant. There is a lot of figuring it out as these subjects go. So it is about knowledge making, rather than knowledge holding. So what these subjects DON’T know, and often know they don’t know, is critical – and different than the simply conceived ‘enlightenment subject’. So to understand ‘the subject’ in ethnographic projects, one must map the distributed knowledge systems that constitute and continue to iterate them. The object of ethnographic inquiry is thus a moving object.

(I once wrote about these kinds of subjects as subject to the ‘aleatory’ in the sense that John Cage uses the term – i.e. subjects confronted with much that conventionally would be considered noise, but set up to listen anyway – in my case, to try to understand toxics, which conventional scientific idioms have a hard time hearing.)

But understanding subjects in ethnographic projects is often not the ends, but the means – i.e. the means to understand distributed knowledge systems themselves, using engagement with subjects (conceived as above) as the way into these systems. This is what I imagine we were up to in the Late Editions project. And what I think you and Mike (Fischer) told us to do in the World Historical Political Economy chapter of Anthro as Cultural Critique ...

In keeping ethnography accountable to subject perspectives, a distributed knowledge system is not mappable outside the derivation of it from subject points of view. Keeping ethnography ethnographic in the Malinowskian sense means not falling for the temptation to allow given networks or technical systems to be the objective space of ethnography. For example, connected/virtual IT relations and networks suggest a natural context for multi-sited ethnography, but often, the tracks of ongoing processes in relation to such systems are not isometric with the course that multi-sited research takes in its development. The map of such research is to be found in the collaboration, ‘native points of view’ that are found in fieldwork as orienting ethnography.

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4 Late Editions 8 vols (University of Chicago Press, 1992–2000) was a collective project under my editorship of producing annuals during the last decade of the twentieth century, reflecting an ethnographic (documentary) approach to the heightened sense of endings and beginnings, characteristic of the fin-de-siècle (in this case, the fin-de-millenium, as well), but that was sensitive to the trenchant critiques of realist representation that were so salient at the time. Most of the contributions in these volumes were experiments with the dialogic/interview/entretien form. As such, they reflected the taste for reflexive strategies of writing of that time (and since), but were also anticipatory as well of the refunctioning of the mise-en-scène of ethnographic inquiry sensitive to the multi-sited terrains in which such inquiry has increasingly been conceived.
3. The dissertation as strategic site of innovation and reform

Again, the dissertation and the process that produces it is the most strategic site not only for seeing new norms and forms of multi-sited research in the making, but also for bringing about reforms of metamethod in anthropology.

Dissertation fieldwork and ethnography are where the shape of anthropological research gets collectively and normatively defined in the shadow of its tradition. How this is so is not a straightforward story of indoctrination, but rather a more complex one of the ways in which anthropology has accepted and negotiated influential interdisciplinary models over the past two decades. If basic change is to come it would be in this context.

Systematic discussions are needed of the institution of explicit norms of collaboration (see Lassiter 2005, Westbrook 2008): the nature of such relations, how to extend, abandon, or move beyond them in a project of research, and what they are expected to produce as data. The use of theory in setting the analytic frames and writing of much ethnography today is a place holder, I would argue, for better practices in the pursuit of inquiry that has a multi-sited character. Theory substitutes for modalities of inhabiting ethnographically relevant, vital sites for certain projects. This can most clearly be seen in how contemporary complex subjects are rhetorically produced in the dissertation process within the authority of the Malinowskian training complex.

Thus, given the significance of graduate pedagogy in understanding the desire for and resistance to multi-sited ethnography as well as it being an ideal laboratory to work out its dynamics within the Malinowskian complex, I am using dissertations that I have supervised with others as a data set for thinking through the modality I have in mind.

4. A sketch, in question-response form, of key metamethodological issues that address the worries and hopes about the practice of multi-sited ethnography

At stake in these questions is preserving the ‘feel’, aesthetic, and distinction of ethnography despite the considerable changes that multi-sited projects engender in the Malinowskian scene of ethnography.

Questions: What prevents the fieldwork from becoming overwhelmed by the multiplication of sites, and what gives multi-sited fieldwork a boundedness and an intensity?; What preserves the sense of working through subjects’ points of view rather than mainly being in conversation with social theory or other studies of social science with subjects’ points of views configured as ‘data’?; What replaces the trope of ‘being there’ so central to conventional ethnographic authority, of inhabiting place?; What preserves the sense of difference, of the favoured trope of ‘defamiliarization’ as a mode of argument in multi-sited projects?
The responses reflect one possible modality of multi-sited ethnography that I have been thinking through with the special vulnerability of the ethnography as dissertation in mind:

a. Multi-sited projects potentially overwhelm the norms of intensive, patient work in ethnography. The response is an accountable norm of incompleteness whereby a bounded relation or juxtaposition is exhaustively explored by the traditional norms and ethos, while the larger map is ethnographically inferred, and imagined on the same plane, so to speak, as the lived-in space of a set of relations, which is the intensive object of ethnography (an example of ethnography where a ‘relation’ is the object of study, yet a solely observed ‘place’ is the scene of fieldwork is Paul Willis’s *Learning to Labour*, 1981).

b. Multi-sited ethnographies begin with orienting collaborations within certain sites, the interest of which is an appropriation of paraethnographic perspective. Fieldwork is actually designed in this relation with a counterpart (as in a recent work I have produced with a Portuguese nobleman, see Marcus and Mascarenhas 2005). This is where ethnography is thickest perhaps, not so that an account of this site can be written, as, for example, an ethnography of expertise or elites would entail, but so that the space-time of ethnography can be created. In this modality of ethnography, a complete account of the collaboration is necessary, not in the mode of 1980s reflexivity, but as a means of ethnographically justifying the point of view/situated knowledge to which the anthropologist commits. The object of collaboration is to move the study to other places, imagined, but not literally visited by collaborators, and eventually to bring ethnography back as inputs to those collaborations. These movements conceptually establish the relations that are the object of study of a multi-sited ethnography – not the relations, or literal path, of the research, but the independently existing relations – imagined and real – that these designed movements of ethnography explore by fieldwork.

In my own recent work the laboratory or workshop for exploring this modality has been collaborative research on central bankers (with Douglas Holmes) and a project on Portuguese aristocrats. In these inquiries, fieldwork is not simply a schedule of interviews but is very often stage managing in collaboration connected events of dialogue and independent inquiries around them. This produces a rich set of materials equivalent to the corpus expected of classic Malinowskian fieldwork.

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5 This is where such ethnography is most Malinowskian; it is working through a ‘native point of view’. Indeed, it is as if I am taking the felicitous improvisations of Kim Fortun’s study (the ethnography of advocacy, intensively studied, leading to a mobile study of global environmentalism over a marked period of its recent history) and making the norms/forms of metamethod out of it.
c. One moves beyond the relation of collaboration, or with it, to other sites by exploring a juxtaposition, an assemblage, or network as object of study. This is ethnography, variantly both thick and thin, the specific densities of which depend on being in constant conversation with the orienting collaboration as a map or design, so to speak, of the project. Other sites might be literal or orchestrated – events, observations, convened seminars, attendances – but they are anchored in the orienting paraethnographic engagement.

d. Temporal concerns and anxieties displace the classic trope of ‘being there’. In multi-sited projects, location in space is not the salient factor in defining its context of significance as much as location in time – its detailed situatedness in ‘the contemporary’. Such ethnography primarily addresses tempos of change, moments in the flow of events, and is trying to produce something relevant – the kind of knowledge that is as much modulated in temporal terms as placed in spatial terms (see Rabinow 2008).

e. Accountabilities are built into the study, into the very relations that generate the data, so to speak. The data are accountable primarily to the orienting collaboration, but also to other combined constituencies for/subjects of the research.

f. Multi-sitedness represents three things: the objective relations of a system which can be studied independently of ethnography (for example a network); the relations set into play as an artefact of a research design (this is important to account for as the reflexive dimension of the fieldwork); and the paraethnographic perspective, the clockwork or ‘native point of view’, which is always situated and specific in spatio-temporal terms, that the ethnography works within for its own purposes and produces results in conversation with. In this modality, ethnography produces its most distinctive and traditional result in line with the Malinowskian complex. It apprehends a system or systemic relations from within subjects’ expressions. The key act is the commitment to develop ethnography from embedded perspective which often entails fieldwork that begins at home. The field is no longer objectively ‘out there’. Rather, one networks oneself into a concept of the field through relations of ethnographic research all the way along. Connections are of equal importance to the fact that the fieldworker may find themself in Poland, in Nigeria, in Indonesia, or in India, for example, at the beginning, middle, or end of a course of research.

5. The redesign of fieldwork for multi-sited ethnography as a challenge to the strong influence that the genres and conventions of ethnographic writing have had on the norms of fieldwork

The ‘writing culture’ critique (Clifford and Marcus 1986) was widely appreciated as being about texts and only implicitly about fieldwork. What was perhaps
missed is the powerful regulative influence that the textual forms of ethnography have had on what is expected of fieldwork in professional culture. Indeed, the methodological significance of ethnography has traditionally been as a frame to discuss the materials and design of fieldwork, and to create expectations for it.

Since the 1980s, the ethnography has not sustained this relation to fieldwork and has in fact become a genre that bears a much heavier theoretical weight for which it was never designed. In the case of multi-sited projects, the limits of ethnographic writing conventions further constrain their possibilities.

Perhaps multi-sited fieldwork and research design anticipate a certain writing problem of a complexity that exceeds the conventions that still hold the ethnographic genre’s identity in place – such as the trope of ‘being there’. As noted, it is the dimension of temporality rather than place that primarily situates and frames multi-sited ethnography. This requires a different sense of the appropriate textual forms coming out of multi-sited projects, of which the classic ethnographic genre, or what is left of it, may or may not be one.

In the meantime, just as the worry that multi-sited projects might dilute the intensity of classic ethnographic fieldwork, so the surviving genre tropes of ethnography provide a difficult fit for the scope of multi-sited projects. The fact that the textual needs for writing multi-sited ethnography might exceed the capacities of the ethnographic genre means not that multi-sited research designs should change, but perhaps the sense of what the written ethnography might be, should. While the problems of multi-sited ethnography are largely about the shape and design of fieldwork, it ends by being again about writing culture, or rather, ethnography, in a different era.

At the level of graduate pedagogy, the dissertation should not be a rough draft of an eventual book, but some sort of middle range production of texts that engage intensively with the kinds of materials that it produces. As a colleague of mine has said, what is needed are practices of composition somewhere between fieldnotes and finished texts. In other words, far from diluting ethnography, multi-sited projects show potential of returning the focus of ethnography to the materials that projects produce – they put the ethnography back into ethnography, so to speak.

6. Finally, multi-sited ethnography in the modality worked out here suggests the reffunctioning of ethnography itself

Yes, this modality is still partly about description, modelling, and analysis of processes in the world – producing a result for a scholarly community that is going to do something with it, for example, in comparative analysis. But multi-sited ethnography is also about mediations and interventions. Michael Fischer (2007)

6 Indeed, sociological ethnography has remained focused on such results for the purposes of the professional community, for example actor-network theory moving toward Michel Callon’s interest in markets (1998). Such ethnography is a related, mainly objectified
thinks of this as the forging of third spaces – reflexive domains within scenes of social action – regimes of living, global assemblages – in which questions of ethics are considered; the anthropological ethnographic intervention is distinctive here. What seems basic is that once ethnography becomes multi-sited and engaged intellectually with its subjects, its arguments and articulations have constituencies within the field and unpredictably beyond it. These are constituencies that exist in relation to and alongside the professional constituency.

However these relations are worked out or ordered, which is a task also for the rethinking of method and standards in anthropology, the mediational character or form of knowledge produced from ethnography cannot be suppressed or shifted to other pursuits such as activism. The ethnography, as report to the discipline, then can be no more than a version of knowledge or results of research extracted from its circuits of mediation, so to speak, for the specific purposes of the discipline. What these purposes might be in light of the refunctioning I suggest is perhaps the most pressing task for rethinking the anthropological tradition of ethnography as a study of contemporary change. Ethnographies of globalization, I would argue, do not add up to an anthropology of globalization, the emergence of a coherent subfield. The purposes and reception of such ethnographic projects – multi-sited by reception alone – are already within the confines of the field, and what the stakes of anthropology as a discipline are in such an attractive arena that pulls many of its best young researchers remains to be articulated. Such an articulation does not depend on new reference theories, but on a project of reform of the classic culture or aesthetic of method, what I term metamethod, and the complex issues of practice and theory involved in that. The contemplation and attempt to do multi-sited ethnography in one or more of its alternative modalities opens onto this seminal project.

References


version of the modality which I am discussing that stays clear of the problem of how results are derived from collaborations.
Chapter 11
Strong Collaboration as a Method for Multi-sited Ethnography: On Mycorrhizal Relations

Matsutake Worlds Research Group (Timothy Choy, Lieba Faier, Michael Hathaway, Miyako Inoue, Shiho Satsuka, and Anna Tsing)

Introduction: In which a Mushroom Leads the Way

Once there was a mushroom that so enticed everyone with its spicy, sweet aroma that people would pay a fortune for a sliver in their soup. The mushroom hid and prospered in cool mountain forests, where deer and squirrels followed its delicious scent. And the people, too, came to gather it, plucking it from the roots of pines. From its revenues, mansions were built in Tibet and village cooperatives prospered in Oaxaca. War refugees from the hills of Laos gathered it in Oregon. The North Korean army claimed its harvest as a security objective. Nature lovers reshaped the landscape to revive it in Kyoto, and ecotourists sought it out in Finland. In time, a group of anthropologists became curious about these goings on. How could one mushroom create so many different social-natural worlds? What might we learn from studying its diversity within the interconnections of its commodity chain? This chapter is our first report on the conditions of collaboration that can make such a multi-sited study possible.

Once there was a discipline that had become known for its ability to glean the texture of everyday life and the force of communal sociality. But it longed to speak to the wider scope of modernity and perhaps address the majesty of the whole blue-green planet Earth in its bed of clouds. When the term ‘multi-sited ethnography’

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1 The Matsutake Worlds Research Group is an anthropological collaboration to study global commerce and science involving matsutake mushrooms. Our research has benefited from grants from the University of California Pacific Rim Research Program and the Toyota Foundation. Our project includes ongoing fieldwork in the Pacific Northwest (USA), British Columbia, Japan, and China. Tsing’s fieldwork with Southeast Asian American mushroom pickers in the Pacific Northwest takes place in collaboration with Hjörleifur Jonsson as well as undergraduate students Lue Vang and David Pheng. Satsuka’s fieldwork was funded by Albion College Faculty Development Grants and the University of Toronto Connaught New Staff Matching Grant.
was coined (Marcus 1995), it caught like wildfire. Perhaps this method could sustain the legacy of ethnographic intimacy while extending it across the globe. Suddenly, everyone claimed to do multi-sited ethnography, whether it was a study of two organizations in the same city or a comparison of two countries. Random anecdotes, loose journalism and statistical surveys were signed up. The term leapt past its capacities and threatened to lose all meaning. This is a good time to take another look. What kinds of projects might bring pride to the concept of multi-sited ethnography? In this collectively authored chapter, we argue for a model with ‘strong collaboration’ at the centre.

Our concept of collaboration requires a reflexive methodology for working across and with difference. Our collaboration is not data gathering under a common theoretical umbrella. Instead, our collaboration requires negotiation across epistemologically diverse terrains and partially ‘articulated knowledges’ (Choy 2005); this is collaboration with friction at its heart (Tsing 2005). Following Sandra Harding’s (1993) term ‘strong objectivity’, we call this strong collaboration. The methodological work of collaboration should not be hidden; the knowledge we gain depends on it. We argue that strong collaboration can lead to ethnographically substantive multi-sited research.

At first glance, the use of collaboration in multi-sited research seems straightforward. To the extent that the sites are different from each other, expertise and commitment are necessary to study each site, and this is most easily accomplished with more than one researcher on board. Multiple researchers can carry the burden of multiple languages, area studies, and histories in the study.

Yet advocating a practice of deploying multiple expertises is dangerous business; it might be mistaken for a division of labour, where all the parts studied – and analyses produced – would add up to a coherent whole. To steer around such spurious holisms, our claim for strong collaboration goes further. First, we acknowledge that ethnographic fieldwork requires a disciplined full-body immersion. Having multiple bodies working across sites of immersion does not merely add data; it requires explicit and active work in translation and interpretation. Such work changes the research project: the basic questions, the framework of analysis, and the very object of study do not remain stable. Strong collaboration is the work of continually remaking the project.

Second, the forms of sociality we study are themselves formed in encounter and translation (Faier, forthcoming). Multi-sited research almost always looks for global interconnection, and our project is no exception. The social networks we study are already strongly collaborative in just the senses we give to our own research; they are formed through awkward negotiations across difference. Strong collaborations become the object of our study, as well as our own practice. Our self-consciousness about the mechanisms of collaboration illuminates our social object.

Third, such collaborations are not limited to humans. Strong collaborations between humans and non-humans make up the partial systematicities of ecosystems. Multi-sited research opens the possibility of comparison across
varied eco-collaborations, in which non-humans are enlisted into contrasting roles (Hathaway 2006). Strong collaboration among researchers facilitates appreciation of non-human agency in all its site specificity.

Indeed, the entanglements of matsutake (see below) and host trees – technically known as mycorrhizal relations – offer an intriguing model for our own research objects and practices. Mycorrhiza are joint structures made from the interaction between tree roots and fungi. Mycorrhizal relations transfer sugars from plants to fungi while making soil nutrients available to plants. The encounter itself is productive, but whether for better or worse for each partner is hardly settled. Mycorrhiza call to mind all the pleasures and dangers of intimate encounter. Without simple hierarchies or predator-prey dichotomies, mycorrhizal relations suggest theories of entanglement and assemblage to understand both social and natural relations of collaboration, love, and value.

In another moment of agency and authorship of our research collaboration, Mogu Mogu (that is, Satsuka and Choy in entangled collaboration, 2007) develops a Deleuzian optic to draw parallels between the intimate relations of matsutake and pine, on the one hand, and the dreams and plans of foresters, mycologists, merchants, and connoisseurs, on the other. Just as the relations between mushroom and tree are co-nourishing, so too are these economies of esteem, prestige, knowledge, and resource management.

In our project, then, we follow two objects: collaboration itself and a mushroom called matsutake. Matsutake is in origin a Japanese word, and, indeed, the mushroom is most valued in Japan. Some of our Japanese informants see matsutake as an icon of Japanese culture: ‘You can’t understand Japan without knowing matsutake’, explained one connoisseur. But this is a ‘Japan’ made in global encounter. Since the 1980s, most matsutake is gathered outside of Japan: in Korea, North and South; in China; in the USA and Canada; and in Mexico, Morocco, Siberia, and northern Europe. Our project has begun intensive fieldwork in just a few of these sites: central Japan; North America; southwest China. In these sites, we learn from all kinds of foragers, from Southeast Asian refugees and Japanese American heritage pickers in the USA to Tibetan villagers in Yunnan. We speak with scientists, foresters, and nature lovers, as well as mushroom buyers, wholesalers, grocers, and gourmets. In this, we make use of the expertise and experience of team members. However, none of us has become an expert on just one area; each of us savours the transnationalism of the project. We have experimented with overlapping and joint fieldwork. We work on interpretation together. Our goal is continually to reframe the project through the collaboration. Thus, unlike more conventional projects of scientific collaboration, we bring humanistic processes of evaluation into the heart of project formation – but with more than one researcher. It is this feature that offers the most interest for multi-sited ethnography.

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2 For related ethnographic research on global commodity chains, see Bestor (2001), Friedberg (2004), Collins (2003), and Fisher and Benson (2006).
Consider the design of the project. We did not gather mushroom researchers and ask them what they knew about each site. Instead, we gathered anthropological theorists of nature and transnationalism and asked them to think about mushrooms. In this way, we have built strong collaboration as the distinctive feature of the research, the feature that makes it ‘multi-sited ethnography’ rather than just research reports from here and there.

In this chapter, then, we refrain from telling you much about matsutake; instead, we grapple with the question of how strong collaboration can contribute to multi-sited ethnography. As might be expected, the format of this chapter itself reflects a particular moment of strong collaboration in our project. Collaborations are never easy. Our group includes scholars at different stages in their careers, and with different priorities. Everyone in the group has solved the problem of finding an academic appointment, but this introduces further anxieties about advancement to tenure. The younger scholars do not want to be overwhelmed by the older ones. Collectively written prose – generally the most readable and useful product of collaborative work – is still awkward for us because we have not agreed upon a ‘voice’ that can reflect everyone’s position. So we begin by including something more basic: short, signed contributions each by a single author. This need not be our ending point, but it represents a beginning: and one that highlights the process itself.

Our first two contributions address strong collaboration in field research. In ethnographic fieldwork, the researcher’s body – with all its senses – is a tool for learning, and the primacy of sensual immersion urges fieldworkers to pursue their learning alone. Here we consider how one might use collaboration to aid the senses. Lieba Faier discusses the possibilities of ‘echolocation’ as fieldworkers share their ‘sensitive skin’. Shiho Satsuka explores the question of translation, as collaboration may stimulate attention to the crucial ‘gap’ across forms of understanding in which ethnographic knowledge begins to appear. Strong collaboration has the potential, both argue, to sharpen the senses, facilitating that acute, shape-shifting discomfort upon which anthropology depends.

Satsuka offers examples of how translation between Japanese, North American, and Chinese projects for managing nature challenges the terms for studying each. She thus leads us to our second section, which asks how strong collaboration might open up new conceptual spaces. Miyako Inoue explores the difference strong collaboration makes within the unifying practices of modern knowledge. How can dialectical and dialogic research practices offer an altered perspective on the analytic process? Anna Tsing asks what kinds of anthropology might be open to strong collaboration. She introduces some features of a collaboration-friendly scholarship, in which we might slow down the academic process to allow the continual reformulation of our work.

Our final section places our project in the context of other experiments in collaboration. Michael Hathaway explores the negative possibilities of collaboration; collaboration can be smothering, traitorous even. Timothy Choy asks why collaboration has become so attractive among cultural anthropologists today. How does our collaboration sit among others? He then takes us back to the
‘mycorrhizal relations’ to which the mushrooms lead us: the living entanglements of roots and fungal cells that advise us of the strong collaborations with which both humans and non-humans sustain themselves. In connection, and for both intellectual and material sustenance, we are in their debt.

Fieldwork and the Senses

Snakes and Skin – Lieba Faier

‘What kind of people pick matsutake? Only men?’, I tried to engage Morikawasan as he drove me up the narrow mountain road that led to one of his matsutake-gathering spots. In Central Kiso, a cluster of mountain towns and villages in southwestern Nagano, Morikawa-san was known as a matsutake no meijin, a matsutake expert. He responded to my question matter-of-factly, his eyes fastened on the winding road ahead: ‘No. Both men and women pick matsutake’. He explained that he was sometimes joined by his wife and granddaughter. ‘But’, he chuckled, ‘you can’t be afraid of spiders or snakes, or have sensitive skin’.

In a collaborative research project, what happens to the spiders, snakes, and sensitive skin of data collecting? How can we incorporate the affective, embodied, experiential dimensions of fieldwork when working with so many different researcher-bodies?

We brought together so many researchers with various area studies expertise to address what George Marcus has referred to as the ‘dilution’ of Malinowskian ethnographic practice that can occur in multi-sited research (Marcus 2005). We are committed to basing our analyses, at least in part, on intimate, focused, and extended immersions in given communities. When it comes to thinking about how to analyse our data, we cannot ignore the phenomenologies – the experiential materialities – of bodies and places: the fear I felt driving up the winding thread of the mountain road in Morikawa-san’s tiny Mini-car; the way the gentle compression of the ground reassured me as we slowly made our way down the slippery mountain slope; the sense of surrender I felt as a lissom breeze made its way through the trees; my self-conscious embarrassment with the way that Morikawa-san’s daughter-in-law and granddaughter greeted me – introducing themselves very politely and pointedly in English and extending their hands to me for a shake – when he invited me into his home after our short excursion; the spongy feel in my mouth of the wild mushrooms we ate together that afternoon.

Taste, sight, sound, touch, smell, heat, body awareness, pain, anger, frustration, balance, weight, scope, acceleration, logic, instinct, hunger, belief. The senses we engage when we conduct fieldwork are nodal points between our ethnographic environments and us. Through them, we become ethnographers. Through them, our bodies become our research instruments.

What, then, does it mean to do research with multiple sensing bodies? What does it mean to produce ethnographic knowledge with so many differently
embodied research instruments? How can we work across the different subjective forms of knowledge they produce? To some extent, this is a problem of translation. How could it ever be possible to translate diversely embodied experiences into a single analysis?

Rather than thinking of the diversity of our embodied, sensory experiences as an insurmountable obstacle, I suggest that we approach it as a challenge that will enable us to understand both our bodies and what it means to be ethnographers in new ways. This is a challenge of ethnographic writing as much as it is one of diverse perspectives. A strong collaboration research animal can develop new collaborative-fieldwork senses; the sensory whole of our fieldwork experiences can offer something different than the sum of our research parts. For example, lately, I have been thinking about echolocation, the ability to navigate through spaces by interpreting reflected sounds. Although humans do not have this sense, bats, toothed whales, and some kinds of birds do. Echolocation is an interactive sense that enables a creature to find its way by reaching out to other bodies with sounds that return to it transformed. Consider, then, the diverse ways that Anna, Miyako, Shiho, and I described our interactions with a matsutake wholesaler we all at some point met in Tokyo’s Tsukiji market. As we bounced ideas off each other based on our meetings with him, we found that our impressions of this man ranged from ‘generous and charming’ to ‘obnoxious and pretentious’. A more multidimensional picture of him emerged than any single ethnographic perspective could have provided. Perhaps ethnographic echolocation is one of many new kinds of senses that can be cultivated through multi-sited, strong collaborations.

Translation in Collaborative Fieldwork – Shiho Satsuka

While conducting joint fieldwork in Kyoto, Anna, Michael and I heard several scientists mention that the best matsutake forest is one where a woman can walk in high heels with an open parasol. Because matsutake is a weak competitor among fungi and microbes, the forest floor should be clear, relatively dry, and solid; the soil should not have enough nutrients for other species to thrive.

This gendered image of matsutake forest caught Anna’s attention because she, if I may borrow an expression from Lieba, ‘brought her eyes’ from Oregon, where matsutake forests are often associated with wildness and masculinity. The tame, cultivated vision of matsutake forests contrasts with environmental vision in Oregon, where even managed forests are considered ‘wild’. Michael brought his eyes from Yunnan, and pointed out the irony of cultivated landscape in Japan. On the one hand, he was surprised by the extended web of paved roads in Kyoto forests, which narrowed to winding paths in mountainous areas. Compared to the landscape in Yunnan, where the erosion of unpaved roads causes serious problems for farmers, the land seemed well maintained. On the other hand, he was struck by the widespread phenomenon of matsutake forests being ‘abandoned’, not taken care of properly for matsutake, because of rural depopulation and the fossil fuel revolution that has replaced firewood collection since the 1960s.
Matsutake yama, or matsutake-growing landscape in Japan, can be translated as ‘matsutake forest’, thus lending commensurability with forests in Oregon and Yunnan. Although the dictionary translation of ‘yama’ is ‘mountain’, the cultivated aspect of ‘matsutake yama’ may be sometimes captured better by ‘forest’ instead of ‘mountain’, because of the latter’s connotation of solitude and wilderness. For example, Saito and Mitsumata (n.d.) use ‘forest’ in their analysis of the communal land usage of ‘matsutake yama’. Translation forge the equivalence and the ground for knowledge exchange and articulation. Translation makes linkages with landscape in different locales, and this is integral for our study of transnational matsutake networks. Yet, as Lydia Liu (1995) points out, the making of linguistic equivalence is not innocent from power connotations.

The politics of translation is striking in the translation of iriai, the practice of communal land use in Japan, as ‘the commons’. In a workshop Anna and I attended in Tokyo in January 2005, the meta-theme seemed to be how to situate iriai in relation to the commons. According to Tomoya Akimichi, the workshop convener, the English commons and iriai differ for several reasons. For example, while ‘the commons’ assumes universal rights of human resource use, ‘iriai’ is based on the idea that a certain group of people temporarily borrows the resource from kami [deities]. While the commons organizes the ownership of the land, iriai land ownership and use rights do not necessarily coincide. While the subject who uses the commons is an individual natural resource user, the subject of iriai is the community itself, including both human and non-human actors (Akimichi 1999).

While the workshop participants were all aware of the difference, they discussed iriai as a Japanese version of the commons. This translation creates the visibility of iriai beyond the particular context in Japan; it allows them to compare iriai with similar natural resource histories and to understand its significance in a global context. However, by making iriai equivalent to the commons, the translation mobilizes the commons as a universal category. Translation offers the commons a distinct status that transcends local difference.

Similarly, equating yama and forests enables us to compare forest landscapes in Kyoto, Oregon, and Yunnan, and to analyse the connectivity of these landscapes. At the same time, the gaps among yama, forest, and shan [mountain/forest] or any other Chinese folk terms for forest, should not be forgotten if we want to trace how the linkages came into being and to analyse the politics of transnational networks.

Spivak reminds us of the responsibility of the translator to recognize the gap while being an active agent of bridging it. She says, ‘in translation, where meaning hops into the spacy emptiness between two named historical languages, we get perilously close to it ... The task of the translator is to facilitate this love between the original and its shadow, a love that permits fraying, holds agency of

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3 This process is similar to how money became distinct from other commodities, and obtained a privileged position as a currency with claim to universality and exchangeability.
the translator and the demands of her imagined or actual audience at bay’ (1993, 180–1). She cautions, ‘unfortunately it is too easy to produce translations if this task is completely ignored’ (ibid., 181). If we ignore the ‘spacy emptiness’ between ‘yama’ and ‘mountain’ or ‘iriai’ and ‘the commons’, translation is only too easy to produce; a lot is lost in translation. Translation requires an uneasy commitment of the translator: to be an agent of bridging the gap through simultaneous transgression and submission to what we seek to translate.4

In our collaborative fieldwork, when the trajectory of each researcher intersects with clusters of strings coming from their previous interactions in other contexts and other places, an interesting chemistry happens in the effort to make sense of this difficult, irritating and uncomfortable gap. This is a gap that a researcher who has already immersed themself in a field would easily ignore. This is nothing new to ethnography. Every ethnography involves cultural translation, and an ethnographer constantly juggles this gap, struggling to find a position and point of view. But fieldwork collaboration highlights the process even more, making it inevitable and hard to ignore. The collaborators’ historical trajectories, and the social strings attached to them coming from different places, make the constitution of the gap evident and traceable, and highlight tensions and the generative potential of translation.

Remaking the Academy

Collaboration and Modern Knowledge – Miyako Inoue

We grew up professionally and live in a sub-discipline that envisions collaboration as a marked category. We were all trained and disciplined to think of scholarship as a normally individual, even isolated, enterprise. We know this is an abstraction, indeed, a distortion. We know that intellectual labour is social labour. Scientists, engineers, biological anthropologists, archaeologists: they all regularly work in teams of specialists with dovetailing knowledge. Corporate research and development is, of course, never based on a model of individual pursuit of knowledge, but is always organized in teams of complementary specialists. So what is up with collaboration as far as sociocultural anthropologists are concerned? Are we just trying to catch up with everybody else, from corporations to archaeologists, or is collaboration for us something different?

I think that what we are trying to do here is different in three specific ways. First, strong collaboration is dialectical not synthetic. There is more to strong collaboration than what is highlighted in the hard sciences and corporate research and development. The benefit traditionally imputed to research teams is the integration of complementary specializations, the pay-off that comes from dividing

4 Derrida (1991) likewise discusses the difficulty of translation as simultaneously necessity and impossibility.
up the work into functional categories and assigning it to different team members. What we are trying to do here is different. True, we have all assumed ‘specialized’ roles in this project. But the model is not based on an intention to synthesize our separate analyses into an organic, complementary whole. On the contrary, our works may show that there are views of our object that are not assimilable to others; we might have to be content with productive tensions among our conclusions. The analogy in politics is the recognition that a will to consensus is often a will to power in that somebody has to yield to others in order to arrive at ‘consensus’. We are often better off agreeing to disagree. While all of us are familiar with this model of working together, we should mark its distinction from the traditional understanding of collaboration. Clifford (1997) has written wonderfully about the multivocality of collaboration between anthropologists and indigenous peoples; they do not arrive at synthesis and perhaps not even agreement, other than to work together by representing their distinct standpoints. But, generally speaking, scholars have been less open to this kind of multivocality as a ‘positive’ outcome of collaborations with each other. Our project gives this model the attention it deserves.

Second, we depart from the traditional model of collaboration in terms of the partitioning of knowledge. While scientists on teams recognize that value comes from interdisciplinary or inter-speciality dialogue, there is generally little sense that disciplines or specialities might change their basic relations with each other or undergo internal reorganization on the basis of dialogue. Our project opens up that possibility. While it is not comfortable to consider rethinking one’s research in the midst of doing the work, we need continually to ask how our frameworks for the project are hierarchically related. Consider a case in point that I am always struggling with as a linguistic anthropologist: How is the act of speaking by individual actors and its study by linguists related to political economy, to capitalism? Can I take that question seriously without a theory of social reproduction that either privileges language or mode of production? I have not answered that question, but I continue to wrestle with it because it is good to think with. My point is that a collaboration among critical sociocultural anthropologists can be an opportunity to think deeply about core ideas in our discipline and our specialties within the discipline.

Third, we depart from the traditional model of collaboration to the extent that we focus on connections across different geographical sites rather than complementarity across different specialities. Bringing supposedly different ‘cases’ to the table is not new in anthropology, but we are not interested in conventional comparison. Rather, we see these sites as socially and materially connected by a commodity. Marx’s concepts of the commodity and of commodity fetishism are useful guides. Marx was not interested in ‘comparing’ the site of production of a commodity with the site of its consumption, as if they were two different ‘cases’ in the Human Relations Area Files. His work guides us to trace concrete connections between production and consumption, what Lila Abu-Lughod (1991), after Eric Wolf, has called ‘phenomena of connection’. Besides, such connections are not best understood in terms of the functional requirements of capitalism or
an integrated world system. That is precisely how we should not be trying to understand phenomena of connection. Localities – places, as geographers call them – are sui generis; some of them are more powerful than others and have the ability to tie the others to them – and sometimes remake the others – in ways the less powerful have little choice over. What are those ways that powerful places have at their disposal, and how do less powerful places respond? How do less powerful places get caught up with the more powerful places to begin with? While it is possible for a single scholar to address these kinds of questions through multi-sited ethnography, it is collaborative work that is most promising.

*Four Propositions and Three Stories in Six Hundred Words – Anna Tsing*

Why do ethnography? One reason is to spurn spectacular capitalism, which fills our screens with glamorous happy thin elites playing with their globally-standard expensive toys. The world – in its materiality and its diversity – is worth more than that, as ethnography can remind us. But anthropology too is full of glamour stars, all in a rush to ‘brand’ their ideas and market their way to the top. What might it take to build a slower, richer scholarship, in which we might connect with the living sensual textures of our still diverse world? Might strong collaboration help?

One: The joy is in the detail  Miyako joined our group when I brought her a dozen bags of matsutake. It was 2004, the best season for matsutake in the Pacific Northwest for many years. Miyako had never seen so many; one slice was a treat. She gasped, she sighed – and she cooked for us. ‘Don’t cut the mushrooms with a knife’, her mother had said, ‘it ruins the flavour and texture’. We ate the mushrooms grilled with no oil but with a bit of lemon and soysauce. Ah! It was the first time I appreciated the flavour, easily ruined by Chinese-style stir-fries.

When you are studying a mushroom, everything is a ‘detail’ that most people – scholars and otherwise – do not care about. But the pleasures of ethnography have always been in the detail. It is what makes ethnographic research long – and worthwhile.

Two: Collaborations are defined by their difficulties  H, a Japanese matsutake dealer, flirted with Lieba and refused to address me, despite my age and status. Both Lieba and I felt intensely uncomfortable. Only when I went back some months later and spoke to H on my own did I realize the value of that complicated initial scene. H was formal, and he was shocked when I quoted for him the most interesting thing he had said: ‘Matsutake traders are like the Mafia who controlled international ports before the second world war.’ He had been showing off for Lieba, going out on a limb beyond his reserve; he had allowed himself some useful indiscretions.

This is, of course, an absolutely ordinary fieldwork story, and a self-consciously benign one at that. But contrast it with a conventional understanding
of ‘collaboration’, in which a predetermined research paradigm is brought to fruition by a team. That kind of collaboration is never ethnographic, although it might be multi-sited.

Three: Writing should be slow Everyone says the opposite, of course, but wouldn’t our field be better if there were fewer books and articles and each more beautifully crafted?\(^5\)

Four: All knowledge is experimental knowledge Shiho, Michael, and I spent several weeks in Kyoto in joint fieldwork. I was terrified. I thought we would step on each other’s feet. In fact, it was a brilliant moment. Shiho understood the scene; we would have been hopeless without her. Michael has a documentary soul, able to record everything around him. I was the business card, able to open doors. Best of all, our memories are different, and we can play on each others’ strongest suits. Such experiments do not always work. But an anthropology without them has lost its edge.

Alan Christy (forthcoming) has described the innovative, if sometimes contradictory, experiments of the Japanese Native Ethnology movement in the early twentieth century. He reminds us of the spirit of mix and play that pervaded the early history of ethnography, and not just in Japan. Multi-sited research will benefit from that kind of spirit, and strong collaboration can be an inspiration for lively new forms.

Comparative Collaborations

Traitorous Collaborations – Michael Hathaway

One day, over an improvised meal of tea, watermelon, marinated tofu, and Japanese mushroom-shaped chocolates, we, a group of anthropologists, gathered to talk about matsutake and ways we might undertake a collaborative project. We were encouraged that, within our discipline, interest in collaboration was on the rise, challenging well-entrenched traditions of carrying out independent fieldwork.

The idea of collaboration has much appeal, but some of us wondered if collaboration is always desirable. Definitions of the term reminded us of its divergent meanings. Collaboration is not always about equal contributions: in the projects of ‘big science’, for example, some participants only gather data, and do not play a role in theoretical considerations. Collaboration can also mean abandoning one’s key insights for the good of the group. Or it can mean capitulation, or, worse still, betrayal, as we see in the American Heritage Dictionary (2006) definition:

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\(^5\) In agitating for a ‘slow books’ movement, I have at the back of my mind the model of the ‘slow food’ movement. Mark-Anthony Falzon calls my attention to the related ‘slow city’ movement Cittaslow.
1. To work together, especially in a joint intellectual effort

2. To cooperate treasonably, as with an enemy occupation force in one’s country

The second definition might help to explain the hesitations and difficulties we encounter in our own desires to collaborate, as well as the difficulties in ongoing forms of collaboration. Collaboration is not necessarily positive, even where it informs desires for social justice or environmental sustainability.

Collaboration in the academy has its own rituals, set by institutional demands. To consider the possibilities of collaboration, it is useful to look further. Outside of the academy, we can often see many forms of collaboration in networks, partnerships and other forms of joint work. In some sectors, collaboration is in itself a goal. To look at the multivalent quality of collaboration, I will briefly look at non-governmental organizations (NGOs) and their international nature conservation efforts, where collaboration has become de rigueur. NGOs foster linkages between themselves, major funders, individual members, academics, corporations and many other agents, trying to effect change across the globe.

Nature conservation has adopted a self-consciously global perspective, which requires, in turn, a global staff. To carry out their plans hatched in Gland, Tokyo, or Washington D.C., nature conservation organizations require alliances with national resource managers. Until the 1980s, national representatives were almost always state officials, natural resource bureaucrats, and park guards.

Anthropologists and others critiqued such top-down plans as examples of the second, ‘treasonable’, form of collaboration, whereby national agents colluded with foreign forces, evicting rural citizens in order to create and enforce newly made nature reserves. Many critics contended that such conservation projects were often crafted through forms of traitorous cooperation. Worse still, this was not only true among representatives of the nation-state, but also through community members themselves, as I explain below.

Over the last decade, other forms of collaboration, such as ‘community based’ programmes have vastly increased, gaining momentum and funding support from major organizations. Community based programmes can, however, hide vast power differentials, and the now obligatory requirement for ‘local collaboration’ has stimulated some rather unusual plans. Anthropologists who look past the rhetoric have revealed structural injustice still in operation. Examples are easy to find. In my own research in southern China, a conservation group provided loans to village families, but on two conditions. First, each family had to sign a pledge promising not to harm any of the elephants that occasionally plodded through the village. Second, the family was required to invest the loan in money making ventures. Later, the conservation staff asked neighbours how the families really spent the money. If they figured that a family had violated any of the conditions, the conservation group took away the family’s valuable possessions. These details were not publicized, and the project was heralded by outside analysts as a
successful example of community collaboration. Yet community members were expected to ‘cooperate treasonably’ against their neighbours.

Of course, there are also many ambiguous stories and spaces created by conservationists who are trying to avoid some of the common problems in collaboration; many examples of such conservationists are described by Lowe (2006) and by Brosius et al. (2005). In my own fieldwork in China, I saw a number of creative alliances, which spanned national boundaries and attempted new forms of partnerships and cooperation, traitorous and otherwise (Hathaway 2006). Still, stories about nature conservation offer a warning to those of us who attempt new forms of collaboration: beware the divergent meanings and possibilities inherent in both the term and the practice.

Other Collaborations: Human and Non-human – Tim Choy

Collaboration in anthropology is not new. Not only do we collaborate regularly with our ‘informants’; a cursory look at the history of the discipline shows many projects in which several researchers based themselves in the same area – perhaps each of them studying a different aspect (for example, ritual, spatial relations, kinship, economy, and so on) of a ‘culture’ understood to be geographically delimited. Today, we see fewer of these regional basecamp models, at least in American anthropology, but it is important to note that even our individual efforts are always in relation with others – drawn into relation with other efforts through our analytics and topics of choice.

Still, there are moves afoot in anthropology to make collaborations more explicit and intentional and to stimulate different ways of working together. The Savage Minds blog (<http://savageminds.org>) strives both to develop a community of discussion among anthropologists and to share anthropological ways of posing questions more widely. The Anthropology of the Contemporary Research Collaboratory (ARC) brings together anthropologists concerned with biopolitics and governmentality with the aim of developing a common language, common standards, and common questions for illuminating contemporary configurations of knowledge and power. There are more occasional and more loosely knit collaborations as well. The Center for Ethnography at University of California-Irvine (see Marcus, this volume) stages conversations among ethnographers, as well as between anthropologists and their interlocutors on topics of shared expertise. Several California-based researchers in science and technology studies, meanwhile, are cultivating a multi-disciplinary network through summer retreats and campus exchanges. The Asthma Files Project, spearheaded by Kim and Mike Fortun, is equally broad in its disciplinary scope, while focusing particularly on collecting multiple ways of knowing about and representing asthma and air pollution.

We did not have these other collaborations in mind when we began, just as they most likely did not have us in mind when they began to work together. But it is striking to see emergent at roughly the same time so many efforts to make working
together a methodological priority. A conversation between different collectives about the means and ends of collaboration might be due. Let me offer some thoughts on what our matsutake collaboration has to offer such a conversation.

There is something decidedly interactive in the fieldwork collaborations we have described. More crucially, perhaps, there is something sensory to them – they highlight moments of perception and apprehension. In the story we have told, we are a many-sensing animal, and the triangulation of signals from our various sensory apparatuses – none of them prior to our training, our competencies, our always-informed affordances – allows us a heightened, almost super-saturated, empiricism.

This differs somewhat from the characterization of collaborative work offered by the Anthropology of the Contemporary Research Collaboratory. In an email exchange with George Marcus, Stephen Collier and Andrew Lakoff explain their practice of taking empirical ‘soundings’ in order to fine tune ARC’s concept work. This concept work, they explain, follows a process of informed observation, the formulation of tentative concepts and distinctions, sounding them against empirical tests, discussions among collaborators, and reformulation.

ARC’s description of this process could apply just as well to our own matsutake research and the discussions we have had while writing this chapter, just as I am sure that ARC researchers have stories to tell about learning how to apprehend as much as comprehend. It is striking to note though, the extent to which, when talking about the benefits of working together, the discussions we bring to view are ones where we calibrate our senses – the means through which empirical details come to presence in our research imagination. Comparatively, ARC focuses on conversations that hone their analytics. This opposition between perception and concept work dissolves upon closer view, of course. Empirical details are noticed by conceptually informed senses, as much as concepts come from striking perceptions. So, then, what is the point of these different emphases? What is the effect?

For us, the answer might lie in the senses we are inspired by matsutake to hone. One major family of senses we are trying to develop is that for sensing qualities of agency and relation that are not necessarily ‘human’. Much has been made recently in the anthropology of science of the role of non-human agencies in shaping the worlds we live in, but too frequently we locate such non-human agencies by imputing in them qualities that we tacitly assume are necessary to human agency – such as subjective intentionality, autonomy, and so on. We should know better: feminist anthropology, for instance, has problematized such assumptions in feminist politics looking for a particular kind of feminine or political subjectivity in other places (for example, Mahmood 2005). Nonetheless, theories of non-human agency – and subsequently, of agency in general – rely upon tropes of speech, recalcitrance, and so on (for example, Latour 2004). We count among our collaborators matsutake itself with a hunch that doing so will teach us to think more creatively and subtly about the forces that shape the worlds we live in, our
own human capacities and agencies, and the intersections and collaborations that generate such forces.

Concluding Thoughts: The Agency of Smell

Our reflexive consideration of the phenomenology and concept work of collaboration works together with our commitment to take the mushroom seriously as a collaborator in our research. This is the heart of our use of the metaphor of ‘mycorrhizal relations’. We want to learn from the mushroom as well as each other. However, we are aware that this forces us to consider forms of agency and relation rarely noticed in anthropology. What kinds of sensual and conceptual responsiveness on our parts include matsutake?

Consider smell. People and animals appreciate matsutake for its smell. Matsutake, in turn, attracts useful predators through smell. Smell is a form of chemical agency – a relation established through chemical bonds. We smell as our neurosensors react to volatile chemicals; matsutake emits volatile chemicals to communicate with potential predators, who willy-nilly spread their spores. Chemical bonds are agential in other ways as well. Matsutake and their host trees respond to each other – and to soil bacteria, nematodes, and slugs – through an exchange of chemicals. The question of non-human responsiveness has enlivened the study of human-animal relations (‘And say the animal responded?’, asks Derrida 2003); perhaps this question is equally important in appreciating wider ecologies. Chemical interactions, including smell, offer one register of relationality in which humans and non-humans, alike, can participate.

Yet cultural differences are particularly marked in considering smell. Thus, for example, white North Americans distance themselves from smells; white pickers in Oregon generally describe the smell of matsutake as distasteful. White pickers who eat the mushrooms disguise the smell by smoking or pickling them. To immerse oneself in questions of smell requires a multi-cultural palette whether one is considering humans or non-humans. Multi-sited ethnography, with its ability to follow ‘smells’ across diverse natural-cultural landscapes, is particularly useful to develop such a palette. Attention to multiple forms of human-non-human relationships in different geocultural sites is essential, we argue, to this intriguing task. In our project, smell opens up multiple linked sites of exploration, from Japanese non-verbal semiotics to multi-cultural and multi-natural ecologies.

Mushrooms remind us: We are all collaborators. Just because matsutake is not cultivated does not mean it does not collaborate with humans and other beings. Rather, matsutake urges us: Strain to find lines of connection. Just as matsutake forms relations with host trees in its essential becoming, strong collaboration makes us remember that all becoming is relational. Taking non-humans – not just

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6 Wood and Lefevre (2007) have shown that one of the aromatic components of the North American matsutake smell repels slugs.
fungi but also trees, animals, and climate – as collaborators stimulates surprise and wonder. Non-human forms of recognition are not our forms. Thus they open up the frameworks through which we appreciate relationality.

The challenge of taking mushrooms as collaborators brings us back, then, to multi-sited ethnography. All ethnographers fight against the social scientific tendency to know what we are looking for in advance – and thus to blind ourselves to what turns out to be interesting. ‘Immersion’ has been the ethnographer’s weapon against the problem of social scientific blindness, but multi-sited work makes full immersion less practical and more conceptually challenging. Multi-sited ethnographers must struggle to hold on to a studied openness about what matters and how, despite superficially similar phenomena across varied sites. Strong collaboration is a useful tool for this task. Strong collaboration, through its very difficulty, keeps us alert to the lessons of what Marilyn Strathern (2004, 38) calls ‘compatibility without comparability’, that is, the constant work of making connections within the recognition of difference. The multi-sited ethnography we advocate never solidifies into a well-defined comparative grid. It must instead grow interactively. The challenge is to keep shifting our knowledge of the research object as we learn from the collaborative process.

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Chapter 12
Bridging Boundaries with a Transnational Research Approach: A Simultaneous Matched Sample Methodology

Valentina Mazzucato

Introduction

Conducting research on a transnational topic poses the challenge to researchers of finding a good balance between depth and breadth. Transnational phenomena, by their very nature, cross nation-state borders – be they related to people, ideas, goods, or institutions. Various scholars have suggested that multi-sited research lends itself well to understanding these cross-border flows. In his seminal article, Marcus (1995) proposed six possibilities: follow the people, things, metaphors, stories, lives, and/or conflicts. Yet for researchers who have since employed this or similar research approaches, the challenge has been to combine multiple locations with an in-depth understanding of the different localities, as well as to be able to contextualize the often fragmented information that one gets from multiple sites (Rutten 2007). So while Appadurai (2000) argues that local area studies are a thing of the past (given that most social phenomena involve or are affected by cross-boundary flows), others, like Mintz (1998), Burawoy (2000) and Rutten (2007), argue that the local anchoring of research is necessary to gain in-depth knowledge of globalizing processes.

In a review of multi-sited, empirical transnational studies that I conducted in 2005 (Mazzucato 2008), two characteristics stand out. First, all researched two or more sites in a step-wise fashion. That is, a researcher first worked in one location, and then moved on to a second and possibly a third. Second, the majority of the reviewed studies obtained their primary information from interviews involving

1 This chapter reports on the results of a collaborative research programme between the University of Amsterdam (AMIDSt), Vrije Universiteit Amsterdam (AOE), the Amsterdam Institute for International Development (AIID), and the African Studies Centre (Leiden) in the Netherlands, and the University of Ghana (ISSER) in Ghana, entitled ‘Transnational networks and the creation of local economies: Economic principles and institutions of Ghanaian migrants at home and abroad’ (Nederlandse Organisatie voor Wetenschappelijk Onderzoek (NWO) grant number 410-13-010P). I wish to acknowledge the useful comments of Mark-Anthony Falzon on a previous version of this chapter.
short contact with respondents. The latter finding supports Rutten’s (2007) argument that social scientists often claim to use anthropological or ethnographic methods when in reality they rely on qualitative, in-depth interviews.

Simultaneity and networks are two important features of transnational phenomena that emerge from the theoretical literature. Transnational flows do not occur in a vacuum but require trans-border networks along which to travel. Second, thanks to modern information and communication technologies, people can be simultaneously engaged in two or more countries. Simultaneous engagement enables linkages between dispersed people to tighten, new livelihood opportunities to emerge, social institutions to change, and hybrid identities to develop. These changes have led to qualitative differences in how migrants, the cities in which they live, and their home communities, are impacted by migration (Foner 1997).

This chapter reports on a simultaneous matched sample methodology developed for the Ghana TransNet study in which we attempted to bridge the boundary between breadth and depth and to incorporate simultaneity and networks directly into the methodology. Simultaneous matched sample (SMS) methodology means using a sample of informants who are linked to each other by being part of the same social network and studying these informants in a simultaneous fashion so that information obtained from one informant in one locality can be immediately linked up with that obtained from another elsewhere.

A Simultaneous Matched Sample Methodology

The Ghana TransNet research programme examined how migrants’ transnational networks affect the principles and institutions on which local economies are based. Through flows of goods, money, services, and ideas between migrants and people they know in their home country, values, knowledge, economic opportunities, and means of social assistance are changed, adapted and transformed, ultimately impacting the institutions that shape local economies both at home and abroad.

The research programme takes migrants’ simultaneous engagement in two or more countries directly into account methodologically (Mazzucato 2000). As argued by Levitt and Glick Schiller (2004), simultaneity is one of the distinguishing features of transnational phenomena. That is, contemporary technologies make communication across large distances easier, faster, and cheaper, and facilitate and make widespread the ability of migrants to be simultaneously engaged in two or more countries at the same time. SMS methodology takes this burgeoning quality directly into account, in two ways. First, the unit of analysis is a network of people who are not necessarily based in the same nation-state. Rather than an

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2 Of the 23 studies reviewed, all were conducted in a step-wise fashion and 15 used methods requiring a one-off or short contact with respondents.

3 Readers wanting more information about the project can consult <www2.fmg.uva.nl/ghanatransnet>. 
individual migrant or their household back home, as was typical of migration studies in the past, the unit of analysis includes the migrant but also their friends, family, colleagues and others with whom they engage in trans-border exchanges. Second, simultaneity is taken into account by conducting the study through a team of researchers based in the main locations of migrants’ networks.

The programme combined three projects each based in an important node of Ghanaian transnational networks: Amsterdam, where most Ghanaians in The Netherlands reside, Accra, the capital city of Ghana where many migrants have lived or passed through, and rural to semi-urban villages in the Ashanti Region of Ghana to which many migrants trace their roots.\footnote{Later, a smaller study was added in Kumasi, the regional capital of the Ashanti Region, where migrant network members were also located in large numbers.}

The five year research programme was conducted in two phases. In the first, lasting one and a half years, contact was made with Ghanaian migrants in Amsterdam, a network survey carried out, a research team established, Amsterdam-based respondents selected, and preliminary fieldwork conducted in Ghana. In a second phase, lasting two years, Ghana-based respondents were contacted, similar research tools developed for each research location, and fieldwork carried out in each of the three research locations. The last one and a half years of the programme were spent analysing and disseminating results in academic and policy circles. These phases are described in detail below.

First, contact was made with Amsterdam-based Ghanaian migrants. Of the official figure of 18,000 Ghanaians in The Netherlands in 2006,\footnote{Unofficial estimates pointed towards 40,000 in 2000 (Mazzucato 2004).} approximately 60 per cent lived in the wider Amsterdam region and, of these, almost 80 per cent resided in one neighbourhood, Amsterdam South East (Dienst Onderzoek en Statistiek 2006). Initial contact was made by frequenting the neighbourhood, attending church ceremonies and social events, and working together with Ghanaians on a cultural project.

As there exists no general survey of Ghanaian migrants in The Netherlands, we conducted an initial network survey by selecting migrants encountered in as many different social settings as possible (two churches, one cultural project, two community leaders, three hometown associations, one workplace, chance encounters in markets, and initial contact with migrants’ families in Ghana).\footnote{Snowball sampling, often used in migration studies when there is no baseline survey, was not used so as to avoid the risk of obtaining access to only certain types of migrants.} The diversity of gateways helped ensure that we came into contact with a wide variety of migrants with different individual and network characteristics. Based on this survey we then selected case study individuals and their network members to follow in depth.

A network survey based on 17 name-generator questions was conducted among 106 Ghanaians. The name-generator questionnaire is a tool used in quantitative social network analysis (Burt 1984; Campbell and Lee 1991) in which questions
are asked with respect to the exchange of emotional and material supportive content between ego and alters (McAllister and Fischer 1978). In our study, we asked questions pertaining to both positive relations (such as friendships) as well as negative relations (such as people one argues with) and strong and weak ties (Granovetter 1973). Respondents were asked to mention as many names as they could think of for each question and along with the names, also the location of the person and the relationship with the respondent (kin/non-kin and, for each, specifying the kind of relationship, such as ‘business partner’ or ‘mother’s sister’). The tool was tested for cultural relevance of questions and saturation.\(^7\)

A selection was made of respondents with whom to conduct the second phase of research based on individual characteristics of the migrant (sex, age, income, education, and length of migration period) and the network (size and density), trying to get as much diversity as possible. This step required asking those selected if they would take part in our research and if they consented to having us interview their network members in Ghana. Whenever possible, two to three meetings were held with each respondent before asking them to take part, so as to create a feeling of trust. This was important, given that many respondents were in vulnerable positions (either because they themselves were undocumented and/or because they were related in some way to others in a similar predicament).

Meanwhile, two additional researchers had been recruited and based in Accra, the capital of Ghana, and in a rural location in the Ashanti region (to which many migrants trace their roots).\(^8\) A preliminary fieldwork visit to Ghana followed. As migrant respondents were being found in Amsterdam, their network member names and addresses were communicated to the researchers in Ghana. In the selection of Amsterdam-based respondents, care was taken to select migrants with network members located in a cluster of rural locations. Spatial clustering was necessary in order to keep distances manageable (the Ashanti region is approximately 24,400 km\(^2\)), given that researchers would visit each respondent on a weekly basis. Four clusters were chosen, three larger towns and one cluster of three, more difficult to access, smaller villages. We were aiming for 30 to 40 networks, and, after getting the consent – and some rejections – of various migrants, we ended up with a sample of 33 networks,\(^9\) or 115 respondents between the three research projects.\(^10\)

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7 Saturation refers to eliciting as complete a network with as few questions as possible. The complete questionnaire can be found on <www2.fmg.uva.nl/ghanatransnet>.

8 Including the Amsterdam-based researcher, the team was composed of a development studies scholar, a rural sociologist, and a development economist. All had several years of field experience in developing countries.

9 In four cases the migrants did not want to take part in the research, thus only their network members were interviewed.

10 Including the smaller Kumasi study, we had a total of 131 respondents. However, here I report only on the respondents in the three main locations as these were the ones with whom all methods were employed.
It was often difficult to trace network members in Ghana and to overcome their initial mistrust. During that period Dutch immigration policy was extremely restrictive, going as far as sending detectives to the towns and villages of origin of visa applicants to check if the information on their application forms was correct. If any member of the extended family gave discrepant information, this was considered sufficient grounds for a visa refusal. This made migration a highly sensitive topic in Ghana and there was a general mistrust of foreigners asking questions about migrants. Working with local research assistants helped assuage the suspicion of local residents. However, the most helpful, indeed crucial, tactic in getting respondents in Ghana to collaborate in our study, was asking the migrants to telephone their network members in Ghana and explain that the research programme was a bona fide academic exercise (that is, that we were not working for the Dutch immigration police). Some migrants sent gifts or letters to Ghana through researchers and this helped generate trust. In the urban areas (where

11 In our specific case, when we tried to get a Ghanaian PhD candidate to work with us in The Netherlands on this project, his grandmother, who had more than 20 grandchildren, had forgotten that the candidate was born in Accra, and gave a different answer than was on the visa form. The candidate was rejected a visa based on these grounds and thus could not work with us on the project. He ended up getting a PhD degree from the London School of Hygiene and Tropical Medicine.
addresses are by no means easy to locate) we soon learned that first meetings with respondents should be arranged at well known locations, from which we would proceed to the respondents’ homes. Because of these and other intricacies, this phase lasted ten months, from September 2002 to June 2003. Figure 12.1 shows schematically what the networks that we studied looked like. Respondents in the three research locations are demarcated by a circle. It can be noted that in some instances we had information about flows from both sides because both sender and receiver were respondents. In other cases, we had information about flows only from one side, because the network member resided outside our three research locations.

Once the respondent matrix was in place, the second phase of the programme could begin. The research team jointly developed questionnaires and question lists so that the same questions would be asked simultaneously in the three research locations to the members of the same networks. First, a transaction study was developed to record all transactions\(^\text{12}\) on a monthly basis conducted in eight domains of daily life, identified from the literature and preliminary fieldwork as being important in the economic lives of migrants and people back home. These were housing, business (including farming), funerals, church, health care, education, remittances for general sustenance, and community development projects. For each transaction the name, location, and relationship of the transaction partner were recorded. The transaction questionnaire was administered on a monthly basis during the period July 2003 to June 2004. Thus, rather than a one-off visit, this questionnaire required 12 visits to each respondent. Second, in-depth interviews were carried out on each of the eight domains, paying specific attention to the role of network members therein. Third, life histories were compiled. Fourth, participant observation in social events were employed in Amsterdam from June 2002 to August 2005 and in locations in Ghana from May 2003 to August 2004.

The various research techniques required visiting respondents at least once monthly and sometimes more frequently. This allowed us to build relationships with respondents, which fostered feelings of trust and improved the quality of response. Besides, the ‘excuse’ of a questionnaire or interview created opportunities to observe respondents in their different contexts: at work, at home, at the market, or in a neighbourhood chop bar. The relationships we established with respondents also meant that we were often invited to ceremonies such as funerals, marriages and baptisms, giving us further opportunities for participant observation.

\(^{12}\) Transactions were defined as an exchange of information (through telephone, post, audio cassettes, physical travel, or computer), goods, money or services.
Characteristics of SMS methodology

Working in a Team

Working in a team of researchers presents researchers with methodological advantages as well as organizational and attitudinal challenges. As mentioned earlier, multiple sites pose serious challenges to the established idea of the ‘lone ethnographer’, who alone is in a position to interpret their data. One of the greatest challenges is gaining depth of information and being able locally to contextualize information. If a researcher spends time in multiple sites, this limits the duration of work in any one site. Further, information obtained from multiple sites can appear ‘splintered’, making it difficult to combine the various pieces into a whole picture (Rutten 2007).

One way to combine in-depth analysis with multiple sites is by working in a team in which each researcher covers a different site. This allows them to spend a longer amount of time at a site than if they were to cover different sites individually, thus giving them the time to embed themselves in the everyday life and activities of respondents. The side benefit is adequate time in which to build trust – especially important as migration can be a sensitive topic that requires a trusting relationship between researcher and informant/s. In our case we needed to know about the income and asset wealth of informants to see to what degree people in Ghana were reliant on migrant remittances; for migrants, we needed to be able to estimate how much of their income went into remittances. In the urban contexts of Accra and Amsterdam, these topics could only be discussed in a reliable fashion once trust was established. The same is true for the legal status of a migrant. We found that legal status was very important for understanding migrants’ vulnerability, their access to services, housing and jobs and their reliance on social network members (Mazzucato 2007). Yet it was only possible to discuss issues of legal status once a relation of reciprocity was established with respondents.

Having researchers based in multiple sites allowed us to piece together the different chunks of information. Each researcher was kept updated with information from other parts of the network. This had two advantages: first, being able to contextualize knowledge about one’s research site within a broader picture of what was happening elsewhere, and, second, gaining more in-depth information from respondents by adapting questions in light of this translocal knowledge.

An example of the first advantage is our study of the economics of funerals in Ghana. Each researcher followed events related to a specific funeral that spanned three locations: the village where the three day ceremony was celebrated; the regional capital where many of the provisions for and guests at the ceremony came from; and the national capital where the body was preserved for months in order to allow migrants to prepare for the funeral and travel to Ghana. By sharing information between researchers we were able to document the different transnational activities that took place to make the funeral possible, to place these within the culturally relevant context of funerals in Ghana, and to see how the
outcomes of the funeral – such as who benefited financially from it, who gained in social prestige and how relationships between migrants and their home town were negotiated – were influenced by events and activities that took place in the three locations (Mazzucato et al. 2006).

An example of the second advantage is the increased ability to obtain information at our own sites that this gave us. The following was a typical scenario.

While travelling to Spain, Nana Asasa was detained at Schiphol airport due to lack of proper documentation. She was taken to a prison approximately 30 km from Amsterdam where she was given a phone card and called her sister in Amsterdam to ask for help. Her sister went to visit her and brought her money to purchase more cards. Nana Asasa then called to tell me what had happened and asked for help. I emailed a summary of my field notes to the researcher who was studying Nana Asasa’s parents in Kumasi. Although Nana Asasa had already been in prison for a few weeks and her sister had contacted her parents to let them know the situation, the researcher in Kumasi had not been told about the situation by the parents. Following my email the researcher told the parents that she knew about Nana Asasa’s situation. The parents thus saw that Nana Asasa trusted us with the information and subsequently let the researcher in on the details of what was happening. We found out how they were experiencing the event, and the activities that they engaged in as a consequence of it. These involved making phone calls to the Netherlands, staying at home to wait for calls from the Netherlands, abandoning agricultural activities on the family farm located a day’s travel from Kumasi so as to be present for any decisions that needed to be made concerning Nana Asasa, and attending a ‘prayer camp’ for two weeks with fasting and prayers and in which donations were made. These were all activities that, had we not known about Nana Asasa’s situation in the Netherlands, we would only with difficulty have detected, as they blended in with daily activities of people in Ghana.

This contextualization of information, however, can only be achieved through close collaboration and data sharing within the research team. This requires a particular mind frame of researchers, as well as infrastructure for data sharing. Researchers need to be ready to share data, which can be quite a shift from the model of the lone social scientist who individually produces and owns data. Clear agreements on a set of co-authored publications can facilitate this sharing process. Researchers also need to accept that they are less free to make independent choices. For example, researchers based in Ghana were not free to select their own field sites, as this depended on the respondents who were selected in Amsterdam and the locations of their network members. Finally, an event in one location may require a researcher in another to readjust their schedule in order to be able to follow those activities associated with that event.

A second period of intensive sharing among researchers emerges during the development of research tools. In order to achieve many of the advantages of SMS methodology, it is necessary that the same questions be asked in the different research locations, simultaneously. This allows comparison across sites, provides
additional information with which to improve questioning in each site, and allows researchers to note discrepancies in response patterns. Methodological tools are thus required that can be applied to each research site simultaneously. This necessitates that researchers draw on their own previous knowledge of their site to develop tools such as the questionnaires for the network and transaction studies and interview keys for the in-depth interviews and life histories, together, ensuring that they are relevant for each context. For the Ghana TransNet project, this exercise was conducted between the first and second fieldwork phases, when all researchers were in The Netherlands and could together develop the different tools to be used. Information on later and necessary changes, discovered during the testing phase in each of the research sites, was communicated through intensive email exchange.

Working in a team cannot substitute for the lived experience of a researcher. However, we built in various ways of helping researchers contextualize information. The principal investigator who was responsible for integrating the three projects, and who was herself conducting the Amsterdam-based fieldwork, made various visits to the urban and rural field sites in Ghana and was present at at least one interview with each informant. A second means that we used to help researchers contextualize information was by sharing information on a regular basis through bi-weekly reports. During our research, we circulated bi-weekly field reports in which we summarized progress at each of the sites and proposed a list of possible topics/themes of interest to be asked to respondents at the other sites. Each researcher tried, as far as possible, to include these themes in their regular interviews with respondents at their site. Such close and frequent communication within the team was only possible because all researchers had access to the Internet, making it possible to share field notes via email.

Simultaneity and Multiple Sites

Having a team of researchers located at the most important nodes of Ghanaian migrants’ transnational networks enabled us partially to overcome one of the challenges of researching a mobile population. It allowed us to trace the immediate consequences in one site of actions taking place in another, and to trace the link between the two. It also made it possible to verify discourses about migration by verifying people’s actions in both sending and receiving countries. It is worth expanding on these three points.

One of the difficulties in conducting research with methods that require more than a one-off visit to a mobile population is that one can easily lose track of respondents – people change addresses, mobile phone numbers, and migrate to other countries. Indeed all of these situations presented themselves during our research. Marcus’s suggestion to ‘follow the people’ is appropriate but how does one achieve this in practice when mobility is such a variable and fickle process? Locating researchers in the most important nodes of Ghanaian migrants’ transnational networks, using the same tools at the same time, and sharing information, enabled us to continue
working with our informants who moved between one location and another. So, for example, when one young student, the daughter of a migrant, graduated from Kumasi National University for Science and Technology and moved to Accra for work, we continued to interview her through our researcher based in Accra.

SMS methodology allowed us to trace the direct consequences of Dutch immigration policy on people living in Ghana. Stringent requirements for verification and validation of professional diplomas in the Netherlands had led some migrants to seek better employment opportunities elsewhere in Europe, as was the case for one of our respondents, Joy. Because of our following of Joy’s network members in Kumasi, we were able to document the effects of Joy’s move on her school-age nephew.

Joy is a qualified Ghanaian nurse who, in the nine years that she resided in The Netherlands – seven of which with the appropriate documentation in hand – was never able to have her nursing diploma validated, and worked in the lowest ranks of elderly care. During our fieldwork, Joy was at the end of her tether and increasingly impatient with the consequences to her self-esteem. She ultimately decided to move to the UK where she had better chances of getting her diploma recognized. This would have its financial demands, since she would need to pay for her trip and housing, and it would take some time for her to obtain a nursing job. Her husband, who was working two jobs in The Netherlands, used his income to support her during this transition phase, which lasted almost a year. The costs of this move were documented in the transaction study and at the same time we were able to follow the consequences this had in Ghana. Joy and her husband were financing a nephew’s schooling in Ghana; as a consequence of Joy’s move they were unable to pay the fees for half of the academic year. By the end of our fieldwork no one in Ghana had been able to make good and the child had been taken out of the school.

These insights were only possible through the simultaneous, multi-sited, production of transactional data. Joy and her husband had not mentioned their inability to pay school fees for their nephew, either because they had not thought of it, or because they were embarrassed. While some aspects can be obtained through recall during interviews or life histories, simultaneity allowed us to deal with the dynamics and order of time of memory, to capture smaller transactions or events which may otherwise have been forgotten, and to establish linkages which respondents themselves may not have been aware of.

There are many discourses on migration that exist in Ghana and among migrants overseas, and one often finds them reproduced in migration studies that rely solely on what people say (interview data, that is), without recognizing that this does not necessarily correspond with what people actually do. We found that migrants often have more room for manoeuvre than they might portray. Matching data on requests made from network members in Ghana with actual migrant behavior in The Netherlands and knowledge of migrants’ financial situations revealed the ways in which some migrants skirt requests or are able to avoid requests from some people
in their network. These are aspects of behaviour that migrants will not easily reveal in an interview. These strategies only became clear to us because we could observe migrant behavior at the time of requests. Finally, we found that migrants are also dependent on their relations in the home country, particularly during certain phases of their migratory trajectories, making them net receivers rather than givers of help. This partly explains why they continue to send remittances despite the fact that they say they feel oppressed by requests (Mazzucato 2006). This reliance on home did not emerge from the interviews with migrants themselves, because they often associated it with a ‘failed’ migration project. Rather, it came out of observing how migrants solved difficult situations (such as losing a job, being detained, or getting cheated in a marriage-for-residency-permit), the requests they made to people in Ghana, and the ensuing actions that network members in Ghana undertook on their behalf. As the example of Nana Asasa’s detainment shows, we were only able to arrive at these various conclusions by observing and collecting quantitative data from the different stages of the migration process at the same time.

Another dominant discourse is that migrants show off their hard-earned income in their country of origin, leading to the misconception that ‘abroad, money grows on trees’. The discourse has it that this conspicuous behaviour of migrants results in youths wanting to migrate and in extended family members making constant requests for money and goods from migrants. Having researchers in different locations meant that we could observe migrants’ behaviour on their home visits. It emerged that people in Ghana, especially in the cities, had a very realistic picture of life in developed countries and were well aware that their compatriots were often working and living in difficult conditions. We found that migrants were usually not explicit to their network members about their own personal circumstances, but that they explained how living conditions were difficult in general and sometimes gave details about people they knew. In fact, some of our young respondents who were able to secure a decent job in urban Ghana did not express any desire to migrate. This showed that while the ‘money growing on trees’ discourse may have reflected reality in the beginning of Ghanaian emigration overseas in the 1980s, it is now outdated. It may, however, still be relevant in rural areas (Kabki et al. 2004) or areas of Ghana from where not many people emigrate (De Lange 2003).

Finally, a third emerging discourse amongst aid donors, both governmental and non-governmental, is that ‘doing development together’ with migrants – what has been termed ‘co-development’ – leads to more sustainable results. This is because migrants have direct links to their home communities and are thus better aware of the needs and equipped to reach communities without having to navigate bureaucratic channels. Reports and academic publications reporting these advantages, however, are based on interviews with migrants and migrant organizations. Very little work exists in which what migrants say is actually traced back to the home country. Our SMS methodology allowed verification of migrant discourses with what they actually do in their home areas. Comparing the five towns and village clusters where we worked in the rural areas of Ghana, we found that in certain instances, particularly with small-scale projects such as the building
of classrooms, the electrification of small villages or the provision of equipment to local clinics, the common wisdom was indeed the case (Kabki et al. 2008). In the more extensive projects in larger villages, however, migrant projects were not always viewed positively by the local population. Different factions existed that expressed different needs, and local leaders at times felt that their authority was threatened by the often-prestigious projects initiated by migrants.

*Working with Networks*

Working with networks rather than households or kinship groups as our unit of analysis taught us that kin relationships are not necessarily the most important. We included non-kin relations among our respondents – preachers, business partners, secondary school friends, girlfriends, and such. This enabled us to notice that certain domains of migrants’ activities in their home countries were in the hands of non-kin relations. For example, many migrants strove to build a house in their home country, in the town or village to which they traced their roots; in these cases, it was often a member of the extended family who helped (Mazzucato 2008). However, in a significant number of cases migrants preferred to build their house in the regional capital or Accra, where they may have had more social ties, or where they could avoid the slew of requests which comes with being seen as wealthy (Smith 2007). A survey we conducted among 106 Ghanaian migrants in Amsterdam showed that 10 per cent would entrust the construction of their house to non-kin – a significant percentage given the oft-noted Asante tendency to organize economic activities along kin lines.

Second, it is important to note that the networks are never complete in that it is too costly to have researchers at all the locations they span (in our case some networks extended to seven different countries), and that even in areas where researchers are located, some network members may not want to or be unable to participate. Indeed the number of informants we followed during our research represents 80 per cent of all network members that were mentioned by Amsterdam-based migrants. This means that for some ties in a network we could collect information from both ends, while for others only data from one end were forthcoming.

Third, networks were defined by migrants in Amsterdam. We thus ran the risk of missing out on network members that were more isolated or marginal, for instance those to whom the migrant did not remit; clearly, these types are necessary for a fuller understanding of transnational networks. (A well-designed name-generator questionnaire, however, minimizes this problem.) In fact, the Ghana TransNet programme included various respondents who never received a remittance from migrants during the entire stay abroad of a migrant. This allowed us to investigate questions as to why these people were so marginalized, and how they survived without remittances (Kabki 2008).
Considerations for the Use of SMS Methodology

There are some practical considerations when deciding whether or not to use SMS methodology. A first consideration is the time investment it requires. In all research dealing with vulnerable populations, time is needed to gain people’s trust. With SMS methodology this aspect is accentuated because researchers typically ask informants for detailed information about their network members who are located far away, effectively allowing them less control and oversight over the outcome of the research exchange. Indeed, in our case we visited each respondent several times before contact details of network members were asked for. To gain trust and avoid misunderstandings, the entire first year of the Ghana TransNet study was used to make diverse types of contacts and to gain people’s trust. Not a single interview question was asked in that year. Rather, time was spent socializing, going to important events, ceremonies and outdoor markets, and conducting a cultural project together with migrants.

The mechanics of drawing up a matched sample of respondents in a transnational setting is also very time consuming. Time was needed for Amsterdam-based informants to identify network members, explain the intention of the research, and seek consent to pass on contact details to researchers. Once we received the contact information, the next (equally time consuming) task was to trace the contacts in Ghana. One difficulty in the urban context, given the large scale combined with the absence of street addresses typical of cities or areas of cities in developing countries, was locating residences.

This phase of making contact with the matched side of the sample took approximately three months with each researcher in their location intensively reporting back to Amsterdam so that migrants could be questioned again on the contact information or asked to contact their network members to enlist their participation in our project. Finally, time was needed to gain the trust of the matched side of the sample so that they would agree to participate as engagedly as SMS methodology necessitates.

Another consideration to make before using SMS methodology is related to cost – teams of researchers do not come cheap. The Ghana TransNet programme employed three full time researchers for four years and a total of 16 research assistants hired during the two years of fieldwork. (And this was a pilot study with a total of 115 respondents – if one were to apply SMS methodology to greater numbers, even more researchers would be required.) The total project cost almost 500,000 EUR (750,000 USD). SMS methodology is a major undertaking and is not really applicable to small projects.

An interesting sideline concerns researcher participation. Being part of a team located at the nodes of transnational networks, it is inevitable that researchers get involved in the flows that knit locations together. When we travelled to and from Ghana and The Netherlands we were invariably asked by respondents to transport money, gifts, photos, audiocassettes, mobile phones and such, for their friends, family and business partners. Thus the researcher becomes a courier, which is also
a form of partial reciprocity for respondents’ cooperation. More importantly, these situations provide a wealth of hands-on knowledge about what is sent and how these flows are organized.

**Conclusions**

This chapter has reviewed some of the main characteristics of SMS methodology: working in teams, in multiple sites simultaneously, and with networks. It has highlighted some of the advantages such as the triangulation of results, the possibility to collect supplementary information with which to improve the quality of data obtained, and getting beyond dominant migration discourses. It also discussed some of the difficulties, such as the different research mind frames needed to be able to work in a team, the fact that it is virtually impossible to work with complete networks, and the investment of resources needed to work with members of networks scattered in different countries.

However, the main advantages of SMS methodology are its ability to address the most important challenge of researching transnational topics: that of bridging the boundaries between depth of information and breadth of research sites. Theoretical and methodological literature on multi-sited fieldwork (Hannerz 1998; Marcus 1995; Stoller 1997) indicates its importance in understanding contemporary social phenomena. A number of studies of transnationalism have taken on this challenge. However, most works study multiple sites sequentially, thus omitting a simultaneous component. Empirical studies thus leave open the question whether simultaneity, which has been identified in the theoretical literature as being a characteristic feature of transnational flows (Levitt and Glick Schiller 2004; Vertovec 1999), results in a qualitative difference in transnational social phenomena.

**References**


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13 There are too many to cite fully, but some examples are Basch et al. 1994; Gardner 1999; Goldring 1998; Guarnizo et al. 1998; Levitt 2001; Osili 2004; Schmaltzbauer 2004.


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Chapter 13

Contours of the Field(s): Multi-sited Ethnography as a Theory-driven Research Strategy for Sociology

Eva Nadai and Christoph Maeder

Introduction

Ethnography in the classical anthropological tradition has long aimed at producing relatively holistic representations of more or less clearly bounded, fairly small groups. This programme has been criticised and redirected from different angles, culminating in the so-called ‘crisis of representation’ (see for instance Clifford and Marcus 1986; Atkinson 1992; Van Maanen 1995). These discussions raised a number of problems ranging from questions of writing, power, the embedding of small scale ‘traditional’ communities in larger systems, ethical issues, and others. Twenty years ago Marcus (1986, 172) concluded that most anthropological ethnography failed to tackle the question of ‘why this group rather than another, why this locale rather than another’, because such issues were simply not considered an important problem. While this is still true for textbooks, where one learns about field access, field roles, handling relationships in the field, writing and organizing field notes, and the like, but hardly about what constitutes an adequate field for a given research question, the problem of the field is a major issue of current theoretical debates. Mainly due to the changes associated with the process of globalization, anthropology had to find ways to investigate research objects disembedded from the local and inextricably connected to global forces and systems of symbols and knowledge. These developments threaten to shatter the very foundations of ethnography as a research practice. As Gille and Ó Riain (2002, 271) note, ‘The potential and uneven de-linking of the spatial and the social under conditions of globalization upsets ethnography’s claim to understand social relations by being there and thus demands that we rethink the character of global ethnography’.

The forces of globalization and the ensuing problems for adequate research have engaged sociology just as much as anthropology. However, the respective literature and calls for a ‘mobile sociology’ (Urry 2000) are rather theoretical than

1 This chapter is an extended and revised version of a previous paper on multi-sited ethnography in sociological research (Nadai and Maeder 2005).
empirical, and if empirical mostly not concerned with ethnographic methods. But from the outset sociological ethnography has been confronted much more immediately with the problem of selecting a field. Even though the early studies of the Chicago School on subcultures, ‘deviants’, urban slums, and so on, still focused on some sort of bounded small communities, sociology could never claim to deal with an ‘integral spatio-temporal isolate’ (Marcus 1986, 178). Whyte’s (1943) corner boys, Anderson’s (1923) Hobo, Gans’s urban villagers (1965) and Levittowners (1967), and other famous ‘natives’ of sociological ethnography could never be depicted as cultural islands isolated from the surrounding world. Nor could they be mistaken as simply a fragment of the larger society mirroring all its culture. Therefore sociological ethnography has to be more self-conscious regarding its concepts of the field. Unfortunately this has not led to more theoretical and methodological debates. Interestingly, although there is also a trend to multi-sited designs in recent sociological ethnography (see Kusenbach 2005), it is not accompanied by intense methodological reflections (for notable exceptions see Burawoy et al. 1991; Burawoy et al. 2002; Duneier 1999). Even highly sophisticated sociological ethnographies using multi-sited designs as a systematic analytic strategy do not even frame their work in these terms (for example: Klinenberg 2002; Newman 1999; Knorr Cetina and Bruegger 2002).

In this chapter we would like to contribute to an overdue discussion of the construction of a ‘field’ in theory-driven sociological ethnography. We argue that sociology inevitably has to deal with ‘fuzzy fields’ (Nadai and Maeder 2005), that is fields without clear boundaries with respect to many dimensions. Using the example of a research project on the practical relevance of the discourse of the entrepreneurial self for processes of social integration and exclusion, we shall demonstrate how theoretical considerations allow us to link such seemingly incongruous spheres of Swiss society as the Human Resources Management of big companies and programmes for the unemployed operating within the social security system. This will lead us to the question of the fuzziness of such fields in relation to the traditional concept of the field in ethnography. To what extent does ethnographical sociology need a ‘field’ and what is the function of the field in theory-driven qualitative research?

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2 The project ‘The enforcement of the entrepreneurial self’ was funded by the Swiss National Science Foundation as part of the National Research Program 51 ‘Social integration and social exclusion’ (Grant No. 405140–69081). In addition to the authors Matthias Hofer participated in the project as research assistant. His work was funded by the research commission of the University of St. Gallen. For an overview of the research see Nadai and Maeder 2006.
Theoretical Framework: The Entrepreneurial Self as a Mode of Governance

35 years ago the liberal American sociologist Daniel Bell heralded in his bestseller *The Coming of Post-industrial Society* (1973) the arrival of a new society. He announced the governance of an expert elite in state, economy, and science, which, based on the superiority of their professional knowledge, would shape and guide the fate of Western societies. What was not foreseeable at that time, however, was who exactly would conduct this symphony of knowledge and expertise. Three decades on, we can solve this puzzle: it is obvious that the regime of management (not of managers) has become the leading semantics of and for the social. Managerial rationality invades virtually all the spheres of life from professional careers to private relationships to social problems of all kinds. Managerialism can be seen as the dominant form of governmentality that is as a conglomerate of techniques and rationalities for ruling others and the self (Foucault 2000).

Managerial thinking culminates in the character of the entrepreneurial self who rationalizes their whole life according to market imperatives and knows how to seize opportunities. A number of discourse analytic studies have argued that the ideal of the subject as an entrepreneur managing their life has become the predominant mode of (self) regulation in every social sphere (Burchell et al. 1991; Bröckling et al. 2000; Boltanski and Chiapello 2001; Rose 1996). Individuals are called upon to fashion themselves according to the ever-changing demands of the market in order to survive economically and socially. Those who cannot live up to this norm risk social as well as labour market exclusion. Having become dependent on some form of welfare, they are still not relieved of the pressures of autonomy and self-responsibility but are subject to the activation and workfare programmes of the enabling or social investment state (Gilbert 2002; Lødemel and Trickey 2001).

The ideal of the entrepreneurial self simultaneously constitutes an appeal, a threat, and an apology. Shrewd self-management leads to success, but these days success is only temporary. On the one hand, in times of rapid social and economic change neither individual performance nor entitlements on the basis of status secure social positions in a stable way over an extended period of time. On the other hand, as Bröckling (2002) has shown in his analysis of management textbooks, the model is itself characterised by inherent paradoxes leading to excessive demands on the individual. One can never really live up to the standards of the ideal and therefore one is structurally forced to try even harder to survive under the ‘unconditional dictate of the comparative’ (ibid., 186). Just as success is attributed to the individual, they have to take the blame for failure. Those who for whatever reason do not or cannot conform to the model of the active and entrepreneurial self, are at risk of being excluded as ‘redundant’ (Bude 1998; Castel 1995). In sociology this reappearance of the category of the ‘expendable’ is discussed within the theoretical framework of inequality: as a cleavage between ‘inside’ and ‘outside’, as instances of ‘exclusion’ or ‘underclass’. We cannot go into this debate here but want to stress the dynamic aspect of exclusion which,
following Castel (1995), we conceptualize as a product of mechanisms starting ‘in the centre’ of social structures like for instance in the labour market.

The sweeping diagnosis of the omnipresence of the entrepreneurial self certainly captures broad societal trends. However, stemming mainly from discourse analysis of texts, it raises the question of the extent to which the normative model is actually operative in different social contexts. To whom is it applied and in what ways is it transformed in the process of adaptation? Our study addressed the missing link between discourse and practice by analysing how the model of the entrepreneurial self informs practical action and daily routines in observable ways. In other words, we were interested in tracking discourse in action. We examined organizational technologies and processes of integration and exclusion in the labour market and adjacent institutions of the welfare state, namely work programmes for the unemployed. Since we were interested in cultural knowledge that is actually in use, not just in expert knowledge laid out in written form, or in mere accounts, we opted for an ethnographic research design. As we all know there is no ethnography without a ‘field’ inhabited by some sort of ‘natives’. But where is our field when we track a highly theoretical concept with supposedly near-unlimited applicability?

Constructing a Multi-sited Field for a Theoretical Question

Sociological ethnography in and of complex societies cannot deal exclusively with clearly bounded groups in single places. Therefore the field of sociological ethnography cannot just be found ‘somewhere out there’, but needs to be constructed by the researcher. Although ‘street-corner studies that rely heavily on “hanging out” as the primary research strategy are not threatened with distinction’ (Kusenbach 2005, 70), more complex strategies, that try to map the inherently fragmented, yet connected, spatial and social spheres of modern societies, such as Duneier’s (1999) extended place method or Burawoy’s (2000) global ethnography, are on the rise. Once we transcend the ‘single tribe’ approach and aim at social structures that are constituted across multiple scales and sites, we have to derive the research objects and research fields from theoretical questions. Marcus (1995, 1998) has suggested several ‘modes of construction’ for spatially dispersed objects of ethnographic study such as following people, things, metaphors, plots, stories, allegories, lives, biographies, and conflicts. The ‘chains, paths, threads, conjunctions, or juxtapositions of locations’ (Marcus 1995, 105) of a multi-sited ethnography must be held together by ‘an explicit, posited logic of association or connection among sites that in fact defines the argument of the ethnography’ (ibid.). In other words, construction of the field must be guided by theory. In spite of this call for theoretical considerations, Gille and Ó Riain (2002) criticise the multi-sited approach for privileging the ethnographer’s imagination and logic of association over the character of social relations within and between sites and for ignoring dynamic processes and external connections that transform sites. Positing
a ‘focus on dynamic social relations rather than static sites’ (ibid., 288) gives us a general direction, but still no clear algorithm for defining fields. After all there are at least as many social relations to choose from as there are people, things, stories, conflicts, or other objects linking potential fields.

Ethnographers studying processes and actors across multiple sites have conceptualized their fields in various terms. However, these efforts are generally situated on a highly abstract level and cannot help us with the very practical problem of ‘dirty’ fieldwork: given a specific research question, what do we observe? Even though the value and epistemological status of ‘being there’ has been questioned with regard to the more fragmented and fleeting fields of global, transnational, or multi-sited ethnography, we contend that establishing some form of physical presence and participation – some form of ‘prolonged co-presence of observer and events’ (Amman and Hirschauer 1997, 21) – is still the hallmark of ethnography. And, as Spradley (1980, 39) reminds us, ‘Wherever the ethnographer may go and whatever the size of the social unit …, all participant observation takes place in social situations’. We cannot directly observe social systems, capitalism, globalization, the entrepreneurial self, or other abstract concepts, but only ‘a place, actors, and activities’ (ibid.). Or to quote another authoritative voice: ‘All social systems, no matter how grand or far-flung, both express and are expressed in the routines of daily social life’. (Giddens 1984, 36) But the question remains: which social situations, places, actors, and activities do we choose? The answer can only be found in a specific social theory.

We argue that symbolic interactionism with its emphasis on actors, situations, and processes, and its capacity to link agency and structure, micro and macro levels of analysis (Nadai and Maeder 2007; Fine 1992), offers some useful conceptual tools with regard to constructing fields for multi-sited sociological ethnography. In our study, we conceived of our fields as ‘social worlds’ (Strauss 1978, 1984). Social worlds are formed by ‘sets of common or joint activities or concerns bound together by a network of communications’ (Kling and Gerson, as cited in Strauss 1984, 123). They comprise a set of actors focused on a common ‘going concern’ (Hughes 1971) and acting on the basis of a minimal working consensus (Clarke 1991). Social worlds are not inherently existent, but become visible to the researcher in their practical consequences (Strübing 2007, 84).

Our research topic consisted of the discourse in action of the entrepreneurial self, and its implications regarding social exclusion – in particular of institutionalized practices and ideas pertaining to this discourse. This suggested that we seek this elusive object out in its quasi ‘natural habitat’, that is in the economy and specifically in the labour market, where according to pertinent analyses it actually originated (Boltanski and Chiapello 2001; Bröckling 2000). In modern societies the labour market is also one of the main sites for the production of inequality and exclusion. Obviously the labour market is not a field constituted by a geographically located space with more or less clearly delineated

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3 Nowadays this includes forms of virtual presence in cyberspace (see Wittel 2000).
boundaries. Rather it is a concept about the negotiation of labour demand and supply. This concept is put into practice by people in organizations deploying labour such as public or private enterprises as well as organizations steering people into and out of the labour market such as headhunters, outplacement agencies, welfare programmes for the unemployed, and the like – each of them constituting a social world of its own. We opted for businesses as an entry point for fieldwork. Concretely, our research sites in the economic field were a multi-national company, a bank, and a large nationally-operating retail company. These organizations provided the ‘locus of study’ without being themselves the objects of study (see Geertz 1973, 22). Nor are organizations necessarily co-extensive with social worlds (Clarke 1991, 131), which means that we cannot simply equate organizations with fields. Organizations may participate in a number of social worlds, and social worlds may be composed of more than one organization. To identify a social world we have to start with its going concern(s). In the context of our research topic one of the core activities is ‘evaluating and ranking people as to their work capacities and abilities’, that is, determining their ‘performance’ or ‘employability’. Boltanski and Chiapello (2001) call this kind of evaluation ‘paradigmatic test’, that is, institutionalized competitions based on agreed-upon resources for determining the social worth of the involved actors.

In business organizations important paradigmatic tests are job interviews and performance appraisals. The latter provided the observable social situations for our ethnographic work, including the actors and actions pertinent to the process of evaluation (primarily supervisors and employees). Starting with performance appraisals we soon discovered connected situations and internal and external actors involved in the process such as managerial meetings and training, round tables regarding a particular case, human resources managers, social workers, doctors, personnel of the social insurance systems, representatives of labour unions, and so on. Thus it was the trajectories of cases observed in social situations and the organizational processes related to the technologies of evaluation that guided us as to which situations and actors we followed within the field of business organizations.

Castel’s (1995) suggestion to study processes of exclusion by looking at mechanisms ‘in the centre’ helped us identify strategic locales for exploring issues of exclusion regarding labour market participation. This perspective led us first into the management ranks of private enterprises, where decisions regarding layoffs are taken, thus triggering careers of exclusion. At the same time discourse-analytic studies have described this social context as a stronghold of managerial ideology and the belief in the entrepreneurial self. Probing the alleged omnipresence of the model of the entrepreneurial self on the one hand, while taking the idea of exclusion as a process seriously on the other, we then proceeded to organizations which deal with those who have been excluded from the labour market, namely work programmes for the unemployed. The official mandate of these programmes, which operate within the legal and institutional framework of the unemployment insurance, is to enhance their clients’ ‘employability’, so these people will find a job as quickly as possible.
This task necessitates an assessment of the clients’ ability and willingness to work. Just as in the business organizations of the economic field, the going concern of unemployment programmes involves evaluating and ranking people with respect to their position in the labour market. Pertinent social situations to be observed were for instance one-to-one coaching sessions, role-plays (for example rehearsing a job interview), meetings, courses, and the like. The most important external actors anchoring the social worlds of the programmes in a wider arena were the regional placement offices of the unemployment insurance, welfare offices, and the invalidity insurance.\textsuperscript{4} Our research sites in the field of welfare were three work programmes catering to different groups of unemployed people: a virtual trading company for skilled clerical workers, a workshop for unskilled workers, and a programme for young people without formal occupational credentials.

As a further heuristic to connect the fields of economy and welfare, which at first glance appear to be worlds apart, we used Goffman’s concept of ‘cooling the mark out’ (1952). In a society where ‘many … are called but few chosen’ (Goffman 1952, 456) there is a need for institutionalized practices to reconcile people who have been deprived of their position with their fate and provide them with a new identity framework. From a perspective of cooling the mark out, four aspects of exclusion come to the fore:

- Institutional procedures and trajectories;
- Cooling agents;
- Those who are being cooled out;
- Legitimation of processes and outcomes (which in our case may or may not be based on the ideal of the entrepreneurial self).

Thus, using a precise instrument for observation derived from sociological theory, we can counterbalance the fuzziness of our highly complex fields.

A Multi-sited Search for the Entrepreneurial Self: Selected Findings

Since this is a methodological treatise we cannot present all our findings or give detailed descriptions (see Nadai and Maeder 2006). But we think our results support the argument that the complexities of our multi-sited research design are worth the effort. We present only three conclusions here to demonstrate the usefulness of a multi-sited approach: first, the social differentiation of the norm of the entrepreneurial self; second, similarities between economy and welfare system regarding their technologies; third, the import of human resources management concepts by welfare programmes.

\textsuperscript{4} Contrary to what we might expect there were very little institutionalized contacts to employers.
The Entrepreneurial Self

Governance studies and Boltanski and Chiapello’s (2001) analysis of the new spirit of capitalism both posit the rule of the entrepreneurial self as a justificatory regime pervading all societal spheres. Both lines of research rest on discourse analysis, a considerable body of respective data consisting of management guidebooks. In the literature this entrepreneurial self is usually characterised by terms like flexibility, disposability, mobility, polyvalence, relentless self-improvement, orientation to market opportunities, and the like. Our ethnographic study of economic and welfare organizations corroborates the omnipresence of this normative model to some extent. The entrepreneurial self is not only found in its natural habitat, the world of business, but also roams the institutions of the welfare state, in the case at hand, in work programmes for the unemployed. However, we also found important contextual variations of the model and a social differentiation in the application of the norm.

In the field of economy we observed differences across businesses and within each of them according to employee category. The three businesses studied adhere to different cultural models of performance, which approximate the ideal of the entrepreneurial self to different degrees: from an almost paradigmatic adoption and respective implementation in human resources tools in the multinational company to a superficial lip service to the model in addition to a slightly modernized form of disciplinary labour control in the retail company and the bank respectively. Generally, the enforcement of the norm correlates with the position of actors in the social field: those with higher status and more social and cultural capital are expected to conform more closely to the model of the entrepreneurial self, while at the same time profiting most from it. This social differentiation is also reflected in the differences between the three work programmes for the unemployed. The programme for skilled office workers adopts and applies the norm to a fuller degree than the workshop for unskilled, mostly immigrant workers or the youth programme for adolescents with very poor educational and social resources.

However, in spite of informing practices in economy and welfare alike, the discourse of the entrepreneurial self is not automatically absorbed by those exposed to it. Neither the so called ‘low performers’ in business companies, nor the unemployed we met during our fieldwork fully accepted the attributions or explanations derived from this framework. They did not unequivocally hold themselves accountable to live up to the norm. We assumed that those profiting from this ideology (the ‘high performers’ and ‘winners’ of the labour market) are more likely to accept it, while the ‘losers’ (‘low performers’ and people with low chances in the labour market) are more sceptical. Yet, both the sceptics and the true believers cannot help but adopt the primary virtue demanded of actors in the labour market as well as those excluded from it: the art of self-marketing. The techniques used to assess employees and unemployed alike (see below) amount to selling oneself as a product. Those having a job must present their (most often routine) work as special and personally attributable ‘performance’ to
keep or better their positions. Those who have lost their job are primarily taught to package their occupational career and their person according to the supposed expectations of prospective employers. In the end, self marketing is actually the core of employability rather than any substantial skills.

Thus, our multi-sited approach allows us simultaneously to capture variability and uniformity. While discourse analysis postulates a single model of the entrepreneurial actor dominating all social spheres, multi-sited ethnography reveals important contextual variations that are based on local organizational cultures on the one hand, and social structural factors on the other hand. At the same time, the findings of the (multi-sited) research caution against overrating the uniqueness of local cultures. There may be organizational adaptations of and individual resistance against the entrepreneurial self, but in the end businesses as well as social services, managers as well as workers, employees as well as the unemployed are affected by the radicalization of the market principle in the labour market.

**Similar Technologies of the Self in Economy and Welfare**

The manager’s prime task is to mobilize commitment to the job and good performance. Social workers dealing with the unemployed aim at ‘repairing’ their supposed deficits regarding ‘employability’ and at coaching them back into the labour market.\(^5\) Both groups hold the same basic belief, namely that success or failure in the labour market (if not in life generally) is a result of an individual’s own effort and ultimately their own responsibility. Although they act in different social worlds there is another striking parallel in their activities: at the core of their work we can identify similar ‘technologies of the self’ (Foucault 1988) which are used to transform their employees’ and clients’ behaviour according to the ideal of the entrepreneurial self. Basically both managers and social workers follow a three-step sequence consisting of exploring, improving, and marketing the self.

In the economy the formal interactional context for bringing up the subject of performance are yearly reviews of employees by their supervisors, during which goals, deficits, measures, and possible sanctions are discussed. The employee is supposed to propose individual goals, judge their own performance, stress their success or confess failures to reach goals and to agree with the supervisor on measures to improve. In the programmes for the unemployed more or less sophisticated forms of ‘assessments’ are used to lead the programme participants to recognize their own weaknesses, strengths, and goals and to devise strategies to enhance their employability.

Interactional scripts like the employee appraisal interview and the coaching session are structurally homologous to the institution of the religious confession.

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\(^5\) Here we use the term ‘social worker’ only as a shorthand designation for the employees of the three work programmes. In fact, few of the employees were professional social workers.
The topics and questions of these institutionalized quasi-confessions provide individuals with ‘maps for the landscape of his soul’ (Hahn 2000, 207), that is, they show them which aspects of their selves they ought to analyse and to mould according to the norms dominant in a given context. Thus, the macro-social discourse of the entrepreneurial self is imposed on individuals and becomes effective in everyday life. Discourse analytic research provides convincing evidence for the prevalence of the entrepreneurial self on an interpretive level and there are also studies showing how the norm translates into concrete (social) politics. Nonetheless, especially works focusing management textbooks do not answer the question as to how the discourse reaches an audience beyond managers and other elite groups. As our study demonstrates, ethnography has the capacity for discovering how such a discourse may be implemented in everyday life. Moreover, a multi-sited research strategy encompassing the social worlds of managers and social workers, that is occupational groups with very different, if not incompatible professional belief systems (Strauss et al. 1963), discovers shared points of reference beyond apparent disparities. Finding similar professional technologies, based on the same ideological foundation, thus provides another avenue for assessing the impact of a normative ideal in everyday practices.

**Human Resources Management**

In a similar vein, multi-sited research which crosses the borders of social worlds uncovers how technologies travel between social spheres. Our study shows how people-processing techniques developed in the economic realm, based on the model of ‘homo oeconomicus’, colonize social work practices. The secular forms of confession are but one technology used by economy and welfare alike. While our study cannot answer the question of historical precedence – where this interactional format was first used – other technologies and concepts clearly have their origins in the economy from where they infiltrated the welfare system. On the macro-social level of social politics and the administering of welfare programmes this development is debated under the heading of ‘managerialism’ (for example Clarke et al. 2000). Again ethnography is especially well suited to spell out what this means on the ground.

**Conclusions: Risks and Gains of Theory-based Multi-sited Ethnography**

Multi-sited ethnography is a complex research strategy confronting the ethnographer with methodological and practical problems. The question remains: is there a reasonable balance between the extra effort and the returns? In conclusion we want to discuss the advantages and the specific problems of multi-sited sociological ethnography, in six arguments.

First, ethnography spanning multiple fields is confronted with the need to give good reasons for selecting more than one field and exactly those fields rather than
others. We have argued that fields have to be constructed, and that this construction process must be guided by theory on the one hand and by the imperatives of fieldwork on the other. At the same time field construction is an ongoing process throughout the whole research project. We first have to define our research object theoretically, then to find locations and social situations where according to theoretical assumptions this object may be found, and finally to be prepared to follow the leads of the field and extend our research as far as we can. We have suggested that symbolic interactionism offers valuable tools for a non-arbitrary construction of ethnographic fields. The concepts of ‘going concern’ and ‘social worlds’, that formed the basis for our research, ensure that field construction does not just follow the whims and interests of the researcher but is actually in line with the constructions of the ‘natives’ of the field(s). Furthermore, we have resorted to another theoretical concept to ground our observational strategy, namely the concept of ‘cooling the mark out’. The focus of ethnographic fieldwork must correspond to important going concerns of the field members. Evaluating people was a central concern of the social worlds we studied, and the significance of this activity was apparent in the considerable effort and resources, which were dedicated to this task and the ensuing cooling out procedures. While the going concern assembles a set of actors in observable social situations, social worlds still have centrifugal tendencies: they have porous boundaries, intersect with other social worlds and split into segments. This gives the ethnographer the opportunity to follow links between social worlds that arise out the preoccupations of the field(s). For instance, in the field of economy the going concern of handling ‘low performers’ quite often transcended the boundaries of the respective organization and involved organizations and actors of the medical or welfare system. This was the case whenever the responsible actors had come to the conclusion that the problem was not merely insufficient performance but illness, disability, addiction, or the like. However, simply following hints from the field cannot be a sufficient substitute for the ethnographer’s own discretion. For research objects such as structures, systems, and discourses, are often invisible to those affected by them. There was no tangible link between managers of private businesses and social workers of unemployment programmes on the level of the discourse we were interested. They were not aware that they acted on the same premises and used similar technologies. Thus it takes ‘sociological imagination’ (Mills 1959) to sniff out such a link and then investigate it.

Second, multi-sited ethnography is comparative by nature, but it is more than that. As Marcus (1995, 102) notes, conventional comparisons in traditional ethnography ‘are generated for homogeneously conceived conceptual units’: one compares communities, locales and people and looks for contrasts and similarities. In multi-sited ethnographic research the object of study is inherently fragmented.

Unfortunately even the most theoretically advanced and empirically sophisticated ethnographic research is soon confronted with the very practical barriers of funding and time restrictions.
and multiply situated. Therefore comparison is an integral dimension of such a research design, but these comparisons take on, to quote Marcus (ibid., 102) ‘the form of juxtapositions of phenomena that conventionally have appeared to be (or conceptually have been kept) “worlds apart”’. To demonstrate the difference between comparative and multi-sited research with an example from our own research we refer to a previous project. In this project we analysed the functioning of public welfare in Switzerland conducting five ethnographic case studies in five different welfare administrations in five different cantons (Maeder and Nadai 2004a,b). In Switzerland there is little legal regulation on a national level concerning entitlement to welfare benefits. Welfare laws are the responsibility of the 26 cantons individually, while the political communes are in charge of the social services handling welfare. However, cantonal laws are based on a few common principles. Thus we had one research object, namely the administering of welfare, and we used five different sites to study local variations thereof. The comparison served to obtain a finer-grained picture of similar processes in different political contexts. In contrast, in the research used as an example here the findings from the different fields provided us with answers to different questions like elements of a puzzle that are put together to form a complete picture. Since the object of the study spans more than one social world, it cannot be reconstructed by exploring only one field. Thus, a multi-sited approach becomes indispensable.

Third, in anthropology multi-sited research strategies for ethnography have raised a number of logistical and/or methodological questions regarding the duration of stays in the field, the transformation of field relations and roles, the degree of immersion in the field, the distinction between home and field, and so on (Amit 2000). We want briefly to address only two problems, namely length of stay and field roles. Traditional ethnography has always stressed the importance of a prolonged stay in a chosen field. Classically cultural anthropologists used to spend one, two or more years in the field before they dared claim having reached some understanding of the culture they studied. If you used this approach in multi-sited research one project would probably take up a decade, not to speak of the problems of funding such a long-term venture. There are a number of arguments why sociological ethnography in general and especially multi-sited ethnography cannot and need not follow the classical model. In a multi-sited research the depth of focus will vary from site to site due to problems of differing accessibility and the nature of the field itself. For example, while we conducted a classical form of participant observation in the three programmes for the unemployed by hanging around for a chosen period of time, participating in various self chosen events, informal talk and interviews, formal interviews, and such, we faced quite different conditions in the private businesses. In these organizations we were summoned up to attend certain clearly defined situations and events such as managers’ meetings, performance appraisal interviews, team meetings, and the like. In order to understand what was going on we had to rely more extensively on formal interviews than in the unemployment programmes or other fields of our previous ethnographic projects. We argue that this merely reflects the terms on which also the members of this field meet and interact: the problem we
were interested in (issues of evaluating employees and taking action in cases of ‘low performance’) was tackled in narrowly-defined social situations, which happened regularly once a year (appraisal interviews) or sporadically at unpredictable intervals. At any rate, our research topic constituted the going concern for a changing set of actors who otherwise did not permanently work together as a team, let alone live together. They normally just met for the occasions we observed and in narrowly circumscribed functional roles. Hence we argue that the ethnographer has to accept the social forms and practices of his or her field. This acknowledgement of course influences the form of the fieldwork to be carried out. If in the economic organizations the relevant practices consisted of occasions and situations that were not connected to stable social groups, we would not have gained much by hanging around with other actors and for a longer period of time. In a similar vein the field limits the choices of the ethnographer regarding field roles. In complex organizations where field members interact mainly in specialized professional roles requiring expert knowledge and respective credentials the ethnographer’s role is limited to that of an observer. This applies to single site research as well, but the problem is multiplied if we would have to craft a field role involving active participation in several fields.

Fourth, one consequence of shorter stays in the field is a loss of descriptive detail in different contexts, which are part of the whole study. However, this must not be considered as the result of a rushed ethnography and therefore as bad research. Rather, it follows from the theoretical decision to restrict the description to central concepts and omit descriptive details, which in a sense would add colour to an already understandable black and white picture. The question then arises: How problematic is this loss of context description? If we think of famous sociological ethnographies conducted with the use of multiple sites, for instance Becker’s studies on marijuana smoking and jazz music (1973), the work of Strauss and his associates on nursing practices (Strauss et al. 1963), John Irwin’s research (1985) on adaptation to the jail or Eviatar Zerubavel’s (1979) analysis of patterns of time in hospital life and others, the reader has to concede that they all gave us poor descriptions of everyday life in their fields, but rich insight into important social phenomena. Sociology works with a different concept of culture from classical cultural anthropology. A concept of culture as a theory of society cannot in our view be a goal of sociological ethnography. Therefore sociological studies based on participant observation have always been restricted in this sense. The argument for this abandonment is theoretical. On a theoretical level functional differentiation, pluralistic lifestyles and individualization take their toll. Our ‘natives’ live in many places, perform many roles and cannot easily be put into one single category or group. In other words: Sociological ethnography does not equate culture with society. Culture in this view consists of shared webs of meanings in language and interaction. But the concept of society adheres to the emerging social forms thereof, like social roles, class, institutions, or, in our example, exclusion. Once again the object of the study is not a particular field and all its culture, but some theoretical concept, which supposedly can be best studied in a certain context or field.
Fifth, the argument against the need for contextual detail seems to diminish the relevance of a given field. From this evolves a difficult question. If sociological ethnography does not aim at analysing a field in its entirety, that is as a unique web of meaning and an ensemble of equally unique structural features, what exactly remains the function of the field(s)? If the ethnographer decides beforehand to limit his or her observation and analysis to questions derived analytically from sociological theory, is there not the risk to single out arbitrarily certain aspects of the field compatible with this interest at the expense of other dimensions which may be much more important to field members? The cornerstone of ethnography has always been to understand a culture from an ‘emic’ perspective and to translate from one ‘cultural idiom’ into another (Werner and Schoepfle 1987). In our view the yardstick for an ethnography which, despite a certain detachment from concrete fields still asserts the claim to understand a given field in its singularity, has to be whether the sociologist’s research questions make sense at all in the eyes of the ‘natives’. While quantitative survey sociology assumes that asking questions is just a matter of adequate wording and structure, the starting point of ethnography is to learn which questions are actually understandable in a given culture. Does a research question strike relevant issues of the field? Does the research tackle a problem with some significance for the members’ everyday lives? And, of course, in the end field members should also recognize themselves at least partially in the findings of such a study. This attitude includes the readiness to adapt one’s research to new questions and issues emerging from the field. From this perspective, analysing, understanding, and describing the specifics of a given field is necessary as far as it contributes to an adequate comprehension of the phenomenon under scrutiny (see also Lauser 2005). And of course the argument raised by Clifford (1986) applies even more to this kind of ethnography than to more traditional ones. We are producing ‘partial truths’ in the strict sense of the term.

Finally, we would like to conclude this chapter by pointing out one major gain of such a research strategy. In our view the main advantages lies in the potential for generalization. By using multi-sited ethnography we can enlarge the traditional ‘single tribe, single scribe’ way of doing ethnographic research and contribute to sociological questions that cut across the boundary of a single traditional field. We are searching categories of social practice that can be generalized to a higher level and reach beyond a single social group. In the case of our research on the entrepreneurial self and exclusion, switching between seemingly disparate fields allowed us to contrast the elegant findings of discourse analysis to the ‘dirty’ practice of everyday life in organizations. It also enabled us to examine the correspondence of discourse and everyday life and trace the limits of the discursive concept. Thus multi-sited ethnography is a powerful tool for building truly empirical grounded theory.
References


Chapter 14

Traversing Cultural Sites: Doing Ethnography among Sudanese Migrants in Germany

Cordula Weißköppel

Introduction

When I designed my project on Sudanese migrants in Germany¹ I was very much inspired by the publication of George Marcus’s landmark paper (1995) and motivated to experiment with a multi-sited style of research. After my PhD in a multicultural school-class in which I consistently adopted the stationary (single-sited) style of participant observation for a year (Weißköppel 2001), I was curious how a different approach would work. In diaspora studies it had already become common to combine research localities in order to look at ethnic and/or religious transnational networks. It seemed plausible that a mobile research style would be appropriate for a project which was interested in different ‘diasporic’² formations (our aim was to compare the Egyptian diaspora with the Sudanese in Germany). As we were mainly interested in the question of how migrants from different African countries could sustain aspects of their original cultural identities, we were also prepared to conduct parts of our research in the home countries.

It was obvious, of course, that the mobile and flexible mode of fieldwork among Sudanese migrants in Germany (mainly political or religious refugees, academics and labour migrants) would confront me with a number of unpredictable problems. Besides the struggle to find appropriate solutions I felt the need critically to reflect

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¹ This project was funded by the German Research Foundation from July 2000 to June 2003. It was situated in the Department of Geography at Bayreuth University, within the framework of the collaborative research centre ‘Local Action in Africa in the Context of Global Influences’ (which ran from 2000 until 2006). Many thanks to Prof. Dr. Fouad Ibrahim who initiated our comparative project on the Egyptian and Sudanese diasporas in Germany, and who became my trusted supervisor and colleague during the whole process of fieldwork.

² There has been much theorizing and disagreement about the term ‘diaspora’ (Brubaker 2005); I use the term throughout this chapter in its descriptive sense, in order to maintain awareness of the way in which Sudanese men and women are scattered all over the world. Diaspora as the formation of a specific identity or community needs to be analysed and discussed in each individual case, see Weißköppel (2004).
about my experiences with multi-sited ethnography (Weißköppel 2005b), because at that time (2003) scepticism, rather than a productive debate, was the rule of the day about this new methodology in anthropology. Fortunately in the interim a number of researchers who adopted the multi-sited style as enthusiastically as I did have also deliberated on it (for example Hage 2005; Hannerz 2003; Dorsch and Scholze 2005; Lauser 2005). I am therefore able, in this chapter, to consider my experiences in a wider context and not only in relation to the theoretical postulates of Marcus (1986; 1995; 1998). The increasingly critical but nevertheless productive debate can contribute to clearing up misunderstandings between those who tend to be enthusiastic (Hannerz 2003; Dorsch and Scholze 2005; Lauser 2005; Weißköppel 2005b) and those who are rather less so (for example Hage 2005; Candea 2007), and can help to differentiate rather than dichotomize the discourse. With this in mind I would like to draw attention to two important aspects before stating my own case.

I think a polarized opposition of stationary versus multi-sited field research is no longer plausible, particularly in light of the historical facts. Stationary research over a long period, with participant observation as its central instrument, was developed at the beginning of the twentieth century in reaction to ‘arm chair anthropology’ and to the expeditions of travellers and explorers that were the main source of knowledge at that time (see Kohl 1993).3 Precisely because the latter allowed only a limited and brief acquaintance with ‘foreign’ cultures, and merely led to theoretical speculation (for example Freud 1986; Durkheim 1981; Eliade 1998), it was a new discovery that living in a society for a longer period enabled the researcher to obtain greater insights into that society (notably as an unintended consequence of Malinowski’s [2001] forced stay on the Trobriands). The empirical attitude on the one hand and the special method of participant observation with a ‘foreign’ people on the other, resulted in more systematic practices of data collection and analysis, which set cultural theories on more solid foundations. The new academic discipline of anthropology thus gained greater credibility – one reason why stationary field research in one place, in one society has become the empirical ideal in anthropology.

At the same time it should be remembered that mobile research, in the sense of a multi-sited strategy, existed alongside stationary fieldwork. It even flourished in the first half of the twentieth century as anthropologists, especially cultural relativists, required comparative data in order to back up their arguments with empirical evidence (for example Mead 1968; Benedict 1955). Later, Lévi-Strauss’s (2001) many short trips to various countries and continents helped him demonstrate the universal character of the structures underlying patterns of behaviour and meaning systems. We should thus talk of a co-presence of both research styles (see for

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3 On this kind of ‘mobile research’, which was widespread in the nineteenth century and which is currently becoming fashionable again, see Greverus (2002), Clifford (1997), and Spittler (2001, 3–4).
instance Schlee 1985), even if stationary fieldwork was the preferred model in the second half of the twentieth century.

A second important point in the general debate is the frequently uncritical way in which ‘multi-sited’ is equated with ‘multi-local’ (for example Werthmann and Hahn 2005). To my mind this has been encouraged by the discursive polarization of stationary and mobile research styles, as though a multi-sited strategy would only refer to fieldwork in more than one geographical locality, and that is seen as the central criterion why the researcher needs to become mobile.

However, simultaneous to these methodological questions, an understanding of culture has entered many areas of anthropology (not in the least confirmed by empirical results from the anthropology of globalization and the anthropological migration and diaspora studies) which focuses on social-communicative processes for the production of cultural meaning that takes place beyond a geographical and/or political belonging. In this way, ‘culture/s’ can separate geographical-territorial or spatio-material localizations from one another, but can also connect and transcend them. This central theoretical notion of the deterritorialization of culture has also entered into the concept of multi-sited ethnography, and constitutes an important shift in terms of how research topics in anthropology are constructed nowadays. There are almost no limits when analysing, for example, discursive phenomena like environmentalism (Krauss 2001) and weather reports, or following the worldwide distribution of luxury goods like sugar (Mintz 1985) or medicine (Whyte 2002). For such kinds of research it is not at all clear how many ‘fields’ one needs to study, but the object of the study and the concrete research question will show the researcher where to move to and where to stay. The alternative term of ‘site/s’, introduced by Marcus in 1995 (see also Fog Olwig and Hastrup 1997), is much more appropriate here because it opens up new horizons for anthropological understanding of how cultural meaning systems are created, distributed, appropriated and changed in a global era of increasing cultural contact and exchange (Schneider 2006). I therefore set forth a central message in my 2005 article, that ‘sites’ are not the same as ‘fields’, as a consequence of which multi-sited ethnography cannot be reduced to multi-local research. I shall elaborate on this later.

Using my research on Sudanese in Germany as an example, I will explain why I visited so many different places during my fieldwork. The constant movement between these places forced me to think more intensively about the conscious construction of my research ‘field’, and this helped in the process of progressively discovering which cultural sites were most relevant to my research interests as outlined above. To my mind, this intense methodological reflection within the research process has become a special feature of multi-sited ethnography, because it evokes a process-related generation of knowledge which remains sensitive to methodological decisions and epistemic limitations and thus leads to the generation of ‘partial truths’, as was pointed out by Clifford already in 1986 (see Candea 2007).

Following this epistemic postulate I switch between the reconstruction of my research process with its different phases and seek to connect each section
with the theoretical foundations of the multi-sited style. Following in particular the methodological decisions in each of the six phases, I will discuss specific advantages and disadvantages of the multi-sited style from the perspective of my key question: How do Sudanese feel, act and manage their lives in *ghorba* [life away from home, see Abusharaf 2002, 128–9]? To conclude this close examination of the relationship between choice of method and results obtained, I will finally sketch two of the paths of understanding which point to specific existential feelings and practices amongst Sudanese men and women who manage their ‘life away from home’ and/or in transnational spaces.

**Phase One: Explorative Research on Sudanese Migrants in Germany**

At the time of my research from 2000 to 2002 the Sudanese were only a small group among all the African migrants in Germany. According to the Federal Office of Statistics, in 2001 there were 4,113 Sudanese registered in Germany, out of 303,018 immigrants from Africa (Stat. Bundesamt Wiesbaden 2002). Thus they formed just one per cent of all African immigrants. Sudanese were most likely to be found in large metropolitan areas with essential infrastructure such as international airports, universities and immigration authorities, and this was a significant criterion for the selection of Hamburg and Berlin as my main places of research.

In 2000, 233 Sudanese were registered in Hamburg and 466 in Berlin. If one compares these figures with migrant groups from other African countries, such as Ghanaians (5,712 in Hamburg, 1,971 in Berlin), one of the demographically largest groups in Germany, or the Egyptians (1,568 in Hamburg, 1,678 in Berlin), it is clear that the Sudanese are rather marginal in the African diaspora in Germany.

My task at making first contacts was more like looking for a needle in a haystack than fieldwork in the conventional sense, where the anthropologist is surrounded by the society to be studied (see Kokot 2000). It was this explorative work that forced me to go to all kinds of different places. At first, I contacted a political

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4 Abusharaf emphasises that the concept of *ghorba* equates ‘the physical state of being distant from one’s home and the psychological state of mental pressure’ (2002, 129).

5 Although the majority of Sudanese migrants to Europe are young, unmarried men, this does not mean there are no women. Women may come as newly married wives, or to study for doctoral or post-doctoral degrees; women who have suffered political persecution may seek asylum, or they may be born as the daughters of Sudanese living in Germany (Abusharaf 2002) – and such women were included among my contacts.

6 See Statistische Landesämter Hamburg and Berlin (2001), demographic data from 2000. Some Sudanese were surprised at these figures, and estimated that there are 1–2000 Sudanese in Berlin alone – which may serve as an indication of how high the number of unregistered individuals probably is.

7 Egypt is mentioned here because our project was conceptualized as a comparative study of two diaspora groups, Egyptians and Sudanese, see SFB-FK 560, sub-project A7, report (2003).
scientist who was the spokeswoman of Sudan Focal Point Europe, a network for all non-governmental organizations which lobby for the Sudanese opposition in exile, whose activities have to be coordinated from abroad. For many Sudanese, my contact with her signalled potential loyalty to the opposition circles in and outside the Sudan and this created a certain amount of trust, at least within the opposition’s camp. But public institutions, such as the municipal ombudsmen for foreigners [German: Ausländerbeauftragte], were also important sources of information, in addition to advice given by colleagues at German universities. These preliminary enquiries meant that communication by telephone was an important medium of my research, and frequently it continued long after making my first contacts. My ethnographic activity thus became a mixture of journalistic investigation, visiting potential contact zones (Pratt 1986), and making selected first contacts, which I could use to further explore my intended object of study: Sudanese men and women mainly living in Germany. But this national framework of my research did not mean that they could be found at a favourite spot or that they would establish one ‘community’ throughout Germany as it is sometimes presumed in sociological concepts of incorporation into the nation state (for example Bommes and Halfmann 1998; Alba 2005; cf. Nieswand 2008). From the very outset my ‘field’ consisted of many places, numerous networks and communities, and this multi-local character meant that the concept of ‘field’ as a limited research area was no longer appropriate.

I nevertheless used it in a metaphorical sense: the idea of a field implies an area that needs to be marked out. It was necessary to clarify in advance which society or which actors I was interested in. Where could I meet them, where were opportunities to be amongst them? What was the aim of my research? Anyone who has written applications for research funding is familiar with these preparatory constructions (Amit 2000). That these conscious constructions also continue during and after the research work (Emerson et al. 1995), and that the permanent construction of the ‘field’ is carried on by the researcher, was for a long time not spelled out as such.

The Permanent Construction of the ‘Field’

In this respect, the edited collections by Fog Olwig and Hastrup (1997) and Amit (2000) mark a growing awareness in anthropology of the many decisions and shifts of focus that are made in the course of the research process, which are a part of the interactive cooperation between participants and researcher, and which ultimately determine the design of a research project, its modes of conduct, and thus the production of results. Moreover, both books consistently argue that the limits of the ‘field’ or area to be studied can no longer automatically be drawn along geographical lines or along lines of ethnic identity, because ethnic ‘units’ are regarded as ideal constructions or historically transmitted inventions (Hobsbawm and Ranger 1992), and today increasingly overlap or compete with other national or non-ethnic identities.
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(see Hylland Eriksen 1991). This applies especially to mobile groups or actors in diasporas (Kokot 2000). The consistent abandonment of the idea of a group that is resident primarily in one place has contributed to the development in contemporary cultural studies of the notion of communication ‘spaces’ and their inherent dynamics: ‘… instead of taking local cultural entities for granted, we want to explore the siting of culture as a dynamic process of self-understanding among the people we study’ (Fog Olwig and Hastrup 1997, 3, my emphasis).

The English word ‘site’ opens up a broad semantic field, and can be translated by a variety of words in German (my mother tongue), from Lage [location] and Gelände [ground] to Schauplatz or Einsatzort [scene/stage or place of action]. Interestingly, the German translations refer very much to the geographical connotations of site in the sense of territorially bounded space. Only the latter terms, Schauplatz and Einsatzort reflect what is intended to be emphasised by using ‘site’ as an alternative to the earlier term ‘field’: as a cognitive and interactive source of thought and action, culture is not tied to places but should be regarded as something potentially flexible, which can be drawn upon or staged at very different places and through different media. This situative siting of culture can be found wherever people negotiate meanings, experience similarities and differences, and form their cosmologies (Weißköppel 2001, 56). The researcher no longer aims to mark out one or more territorial fields, as in multi-local research, insofar as these different sites, or centres of cultural meaning, first have to be identified, and often have to be ‘dis-covered’ (Amann and Hirschauer 1997, 13). In some cases this makes it necessary to be highly mobile in a geographical sense, but above all it requires mental and intellectual flexibility: Where do thematic connections to a central meaning reveal themselves? Where can other sources of conspicuous ascription of meaning be tapped? What chains of meaning do the actors suggest and how can one follow them appropriately?

During my explorative phase it became very clear that carrying out research in several places must be regarded more as an effect and less as a requirement of the multi-sited strategy. Many sources of information are nowadays available directly at the researcher’s desk, so that there may be no need for geographical mobility. The multi-sitedness of a research project can refer to various media sites, such as newspapers, archives, websites or the radio. It is not just a question of using these media to complement the fieldwork data – they can even constitute the main focus of study.

The constructivist position (currently gaining broader recognition) that ‘culture’ is a socially- and communicatively-produced phenomenon, always embedded in historical processes of power relations and their negotiation, permanently obliges the anthropologist to decide which of the various scenes of interaction and negotiation are relevant for their research. More and more they have to carry out their construction of the ‘field’ through the selection of relevant sites, and therefore the siting becomes part of the data. Clear indications of this methodological turn, from fields that were clearly marked
out right from the beginning to flexibly localized sites of cultural production of meaning, were contained in the programmatic essay on multi-sited ethnography by Marcus in the *Annual Review of Anthropology* (1995). In retrospect, it is remarkable that the debates about recording ethnographic data in writing were more controversial (see for instance Geertz 1990) than Marcus’s appeals to methodological changes in the design and practice of fieldwork (1986). Yet, as he pointed out in the preface to his *Ethnography Through Thick and Thin* (1998), this was his intention: to design a new imaginary (Marcus 1998, 6) for anthropological research.

**Phase Two: Practical Experience with Multi-sitedness**

My main reason for geographical mobility was not so much the idea of following the movements of Sudanese migrants between Berlin, Saudi Arabia and Khartoum, but rather the wish to know where Sudanese lived and how they shaped their life in ‘German society’ although this abstract national framework of the host country mainly played a role concerning their legal status. For this reason the decision in favour of the cities of Hamburg and Berlin was a pragmatic one. The multi-sited character of my fieldwork began in these cities, where I commuted between different meeting places: a Protestant church, a Sudanese club and a German-Sudanese Sufi brotherhood located in a renovated factory. In the first phase I spent one month in Hamburg and a second in Berlin. This meant that I had little chance of becoming immersed for longer periods in the individual lifeworlds, or getting involved in interactive relationships. I did not aim at *dichte Teilnahme* [thick participation; in analogy to Geertz’s (1987) ‘thick description’], a term proposed by the German anthropologist Spittler (2001, 19) in order to stress the qualities of participant observation in the sense of social nearness and a holistic, material participation in other people’s lives. In these short research periods it was impossible to form constant relationships, such as are recommended in ethno-psychoanalysis in particular (Nadig 1987), and in ethnography in general.

Moreover, I realized that my mobile existence, at times with three addresses, was diametrically opposed to the life led by Sudanese immigrants. Many of them had been settled in Berlin for years and were glad to have a flat they could afford; others were asylum seekers who were accommodated in peripheral regions, and who had to submit an application to the immigration authorities every time they needed a train ticket to go to Berlin. Even within Berlin, many did not attend the Sudanese Club at the Ostbahnhof because they felt it was too far away from where they lived. I quickly learned that urban lifeworlds were shaped by daily habits and necessities, and that trips far from home were a special event or were undertaken for a particular reason. It is important to realize that migrants tend to be sedentary for long periods and that they use
their financial resources strategically in order to make sporadic geographical mobility possible.

Finally, I became aware of another problem, namely that my social identity as a researcher sometimes appeared diffuse and nebulous rather than trustworthy and reliable. While the actors strongly identified as Muslims or Christians, and therefore attended either mosques or churches in their free time, I switched between these communities. I was a hybrid, and it was only when I felt myself under pressure from the current Sheikh who wanted me to convert to Islam that I explained I was a Christian; this bailed me from the role confusion I found myself in and was not really a profession of faith (Weißköppel 2005c).

The large number of places of research and my frequent change of place, the short periods available for my fieldwork and the doubts as to my political and religious identity made it difficult to gain the confidence of my informants and showed that a multi-local strategy was not the best for establishing good long-term relations with Sudanese immigrants and refugees. Viewed from afar, my experiences were not different from those of other anthropologists in complex fields, for example cities (Kokot et al. 2000). In addition, the ethnographic methods could not be applied in a preconceived sequence: ‘It is the circumstances which defined the method rather than the method defining the circumstances’ (Amit 2000, 11).

Despite problems linked to my presence at a number of different places, these initial proceedings helped me gain first insights into the character of the Sudanese diaspora/s. The diverse Sudanese networks, which I became familiar with more or less simultaneously, seldom overlapped. Even if individual actors turned up in more than one of them, the dominant observation was that networks were formed according to political groupings, cultural occasions, religious affiliation, or linguistic and ethnic identity. At each place there was a prevailing core, but otherwise there was great fluctuation among the participants. The different sites frequented by Sudanese migrants, for example cultural clubs, political lectures, and religious congregations of different individual confessions were thus an expression of the partial reappropriation of place where certain aspects of their cultural identity could be practised with compatriots. At the same time these social spaces were often only accessible in cooperation with the host society’s

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8 With this term I refer to the work of Jenkins (1996) who emphasises the interactive basis of identity constructions.

9 This popular attribution is used here deliberately as an exaggeration and a provocation, for there are good reasons for avoiding such an individualized use of the word. See Weißköppel (2005a).

10 I use this term in the plural since other current studies of Sudanese in the diaspora confirm the heterogeneity of the transnational social spaces which must be characterized very differently depending on the host country and the motive for migration. See Fabos (2002), Abusharaf (2002), Assal (2004), DeLuca and Bruch (2005), and my own publications (Weißköppel 2004; 2005a-d; 2006; 2008).
institutions, so that strong ethno-national in/exclusion was not observable, but they retained their character as contact zones and meeting points.

**Phase Three: Focused Ethnography at Selected Places**

In many respects my subsequent, situation-bound method could be labelled ‘focused ethnography’ (Knoblauch 2001, 126). In complex, and confusing, research situations, the pressure is greater to select parts for observation. But how can one select a part when one has not seen the whole? Here it was necessary to uphold the empirical premise of ethnography that ‘cultural phenomena are yet to be discovered’, as the sociologists Amann and Hirschauer (1997, 13) put it. This was a case for ‘groping one’s way’ forward, as suggested by Marcus (1995, 98). To do this, I had to visit different ‘points’ of data production (Amann & Hirschauer 1997, 16); in Marcus’s terms, different cultural sites which were relevant for Sudanese in the diaspora.

What my research approach boiled down to was an ethnography of events. This led to an even more serious problem: there was not enough time available to take field notes. This problem was aggravated by the fact that the majority of my interview partners refused to be taped; many of them had fled the Sudan for political reasons and had had negative experiences with tape recordings, either during interrogations in the Sudan (Weißköppel 2006), or as part of the documentation procedure when applying for asylum in Germany (Scheffer 2001).

In my view, these different events did not add up to a ‘period’ of data gathering. Rather, the multi-sited style, while freeing me from the habitus of an orderly ethnography carried out within a delimited space, exposed me to a self-imposed chaos and produced doubts. Should I stick to my chosen research style, and have the courage to make situational and ‘thin’ descriptions, while continuing to keep an eye out for the much invoked ‘connections’ Marcus was speaking of (1995, 98)? The vision of a target oriented ‘trajectory’ referring to my research topic seemed very distant; in reality I had countless pieces of a puzzle, and with each interview or visit to an event I only produced more. Was this ethnographic procedure a reflection of the ‘life in fragments’ which Zygmunt Bauman (1995) sees as a characteristic feature of post-modern life, or was I in danger of projecting my own research style onto other people’s lives?

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11 I use ‘thin’ here in contrast to Geertz’s (1987) ‘thick description’, but not in the sense of Ryle (1971) who used it to mean a detached description of behaviour, which according to Geertz is hardly possible for ethnographers. As ethnographers we select information, and thus already interpret what is going on; the systematic or ‘thick’ observation of recurring scenes or activities was largely impossible for me in the course of the fieldwork considered here, but see Weißköppel (2001).
The Challenge to the Ethnographer’s Self-image

Interestingly, Marcus (1995) points out these uncertainties with respect to the self-image of the fieldworkers, either as a prediction or from his own experience. He refers to one of these potential ‘methodological anxieties’ as ‘attenuating the power of fieldwork’ (1995, 100): one would need to allow a weakening of former ethnographic skills through the changed conditions, which puts the anthropologist into a conflict between goals and reality.

Devereux’s *From Anxiety to Method in the Behavioural Sciences* (1988) showed how the ethnographer’s own fears with respect to the object of study can be controlled through the use of appropriate methodological tools. While participant observation has become famous as an open and therefore humane method (Malinowski 2001; Kohl 1993; Abu-Lughod 1991), for anthropologists it metaphorically represents a safe ‘haven’ in which many ‘thundering waves of the wild ocean’ can be calmed. In other words, emotional involvement or feelings of foreignness, which easily lead to emotional stress and an inability to cope with fieldwork experiences, are ‘sat out’. Ethnographers talk of lengthy learning and understanding processes or of the need to digest particular experiences, often continuing long after the field trip. The challenge of multi-sited ethnography to approach the foreign culture through successive short visits, to abandon the tried and trusted habitus of gradually going native (with the corresponding process of resocialization on returning home), and to learn something new, is therefore linked to specific uncertainties. I felt these uncertainties during my fieldwork, although I was at no point tempted to give up my experiment in multi-sited ethnography. My unfamiliar procedure became more obvious in the regular evaluation phases at my university, where I met colleagues who had followed the stationary principle, doing fieldwork at chosen places for between six to twelve months. Here I had to justify my method as being genuinely ethnographic and to defend the quality of my data, even if it was of a different quality.

Nevertheless, Marcus’s intention was not to suppress moments of methodological uncertainty when using new strategies, but to use them as productively as possible in order to gain new insights (1995, 99) – and I explore below what I learned during the struggle to traverse cultural sites. The customary self-image of the ethnographer comes, as in other professions, from the specific training within our discipline, that lasts for many years and which we cannot set aside from one day to the next. But the uncertainty of this self-image opens up opportunities for cultural and professional criticism12 which in the final analysis is aimed at being able to perceive and represent other people’s realities more appropriately through variances in data production.

12 Here we need to refer not only to the ‘Writing Culture’ debate (Clifford and Marcus 1986), but also to the diverse proposals arising from ‘action’ or ‘applied’ anthropology (see for instance Seithel 2000), and feminist anthropology (see for instance Moore 1988).
As Candea (2007, 178) aptly reflects, a multi-sited strategy can also lead to grand illusions in this respect by following a new, global, holism, which meets its limits in no less than the functionalist approaches that claim to be able to describe societies in all their complexity. Multi-sited research can thus lead to a subconscious attitude of omnipotence and a wish to ‘get to grips’ with the whole world (Devereux 1988). Candea therefore proposes ‘self-imposed limitations’ (2007, 180) during fieldwork, his central argument being to return more or less to stationary research at a single site. In the process of my research these limitations came up mainly for pragmatic reasons.

**Phase Four: Pragmatic Solutions, Return to Research Sites**

After having seen to some validations of the first phase of my research in Hamburg as well as in Berlin, I decided to return to those places where I already knew Sudanese people. This time I went only to Berlin, where I was able to participate in three networks that were relevant to my research topic: a Sudanese cultural association, which received funding within the framework of the integration of foreigners in Germany; a Protestant church which ran an open café for non-immigrants and immigrants; and a German-Sudanese Sufi brotherhood (Weißköppel 2005c; 2005d). A characteristic of all of these sites was that Germans and migrants from elsewhere were present in addition to Sudanese migrants, so that these were ethnically mixed spaces of interaction. This is an aspect that was neglected in migration research in the past (see Glick Schiller et al. 2006) and which was particularly interesting in the light of my initial research question, which was whether and how Sudanese in the diaspora are able to continue or resume practices that are part of their home cultures (see Weißköppel 2004). Here I learned that one strategy was to transcend certain aspects of their original social identities because in these mixed spaces it was more relevant to stress an African or black identity (aspects of race and genealogy), being a woman (aspects of gender), or belonging to an Arab-speaking group (aspects of linguistic and ethnic identities).

On the whole it was possible to visit these three sites at times that did not clash, so that I was able to make more continuous observations and produce ‘thicker descriptions’ – more detailed accounts, as it were. My relationships with certain individuals improved because of my repeated presence, so that during this time I was able to collect a number of biographies. It was particularly significant that the places where we met were of secondary interest to many of my interview partners, because there were many other places and activities in their lives.

**Phase Five: Travelling to the Migrants’ Home Country**

In addition to my selected sites in Germany, I travelled to Khartoum, as many of my interview partners still had family members and relatives in Khartoum-Omdurman.
Although my original plan was to visit the families of individual Sudanese living in Germany, my stay in Khartoum-Omdurman resulted in a constant movement between various contact visits, expert interviews, and religious or political civil society events (for example southern Sudanese theatre groups, northern Sudanese women’s groups, activists in the local human rights movement). Besides, I got in touch with a number of university academics, and this generated a new focus group of informants – academic return migrants who had studied in Germany and come back to Sudan to pursue professional careers.\textsuperscript{13}

The day-to-day transnational life of my landlady, whose three sons lived abroad, was a small eye-opener, as was the daily small talk with taxi drivers, vegetable hawkers or young academics, who all wanted to talk about how they could organize emigration. It became obvious that migration was widely accepted among the northern Sudanese middle and upper classes as a strategy for maintaining the family’s standard of living, meeting specific needs with respect of medical care, education or technology, and thus potentially achieving a higher status. The experience of attending a middle class wedding in Khartoum-North made clear to me the gain in prestige derived from having among the guests a large number of relatives who had come from abroad (not only from western countries) – and conversely how the pressure is increased for all Sudanese present to splash out on a lavish celebration, which often can only be financed by the remittances and savings of the groom. The video recordings of these weddings would eventually become visual icons of connection in German or Sudanese living rooms. These rituals of seeing and being seen represent access to the flows of wealth and the presence of a productive and vibrant way of life. Their visual conservation seals the moral obligation of emigrants towards those who stay at home, as emigrants assure the remittance of funds or the family’s chances of going to Europe (see Lauser 2004).

My stay in Khartoum therefore served to generate ideas on how the ‘pieces of the puzzle’, all the fragments of data I had gathered at different sites, might be meaningfully concatenated. I realized that the real challenge of the multi-sited method consists in explicating (yet) unseen connections between different research sites and their inherent data. I refer again to Marcus (1995, 98): to discover those connections or concatenations between different cultural sites was his central vision of the multi-sited concept (see also Hannerz 1996; Cooper 2001). According to Marcus, opening up such connections generally requires an associative ‘groping your way’ and searching for ‘tracks’ which can become a ‘path’ of understanding, and finally help to create the ‘trajectory’ in the sense of an overarching thesis or a plausible interpretation about the object of study.

Although I warmed up to this bundle of metaphors I often felt confused during phases of validation and analysis. What kind of data would ‘tracks’ be, which ones

\textsuperscript{13} Their perspective became quite relevant to understand more about how transnational spaces were sustained especially by return migrants who educated and prepared the young generation for going abroad.
would shape a ‘path’ of understanding and at which stage of interpretation should I speak of a ‘trajectory’?

**Phase Six: Production of Results, Shaping Paths of Understanding**

The longer I kept moving among different cultural sites and the more often I returned to places I had been before, the more I developed a feeling for what Marcus may have meant by ‘tracks’: recurring details, topics of conversation or expressions and symbols of emotion, which gradually form a ‘path’ of understanding. The following are two examples.

**The Discourse on Airlines**

A recurrent topic of conversation in a variety of informal contexts was the Lufthansa airline. Two of my informants, a German secretary and a Sudanese engineer had met at the Lufthansa office in Khartoum. Cheap flights had made it possible for them to conduct their married life between Hamburg and Khartoum for a number of years, and finally to organize family reunification after the divorce. In one of the Sudanese Associations in Germany, I happened to be at a large meeting at which the results of a committee of inquiry about the murder of a Sudanese asylum seeker were discussed. He had been suffocated by Federal Border Police on a Lufthansa flight (see Amnesty International 2000, 158; Dahlkamp and Mascolo 2001, 42–4). Although this tragic event had occurred two years before my research, I could feel the empathy of all those present for their dead fellow countryman; one of the older men commented that his death had become a symbol for all the risks and imponderables of their situation: regularly overcoming long geographical distances, and getting used to uncertainties concerning their legal status in immigration countries.

As the time approached for my journey to Khartoum, the more irrational I became in my planning, for the events of 11 September 2001 were still fresh in my mind. I decided in favour of travelling with Lufthansa, although it was more expensive. In Khartoum I met a Sudanese woman from Berlin who was visiting her family, and she immediately told me about Lufthansa’s new price offers which meant she also could afford to fly with Lufthansa; in her view it was simply the safest airline.

Something that I had experienced in various situations during my fieldwork, not least by traversing cultural sites, was suddenly clear to me: In the colloquial speech of different diaspora agents, the ‘Lufthansa airline’ had become a metonymic expression of the overcoming of geographical distance, the cultivation of social connections between Germany and the Sudan, and also a cherished security in the face of dangers and threats which were thought to inhabit transnational space.
In/visibility of Violence

A second ‘track’ I occasionally encountered in the course of traversing my sites was the sudden change from the invisibility to the visibility of experiences of violence. In Germany, political refugees from the Sudan are protected by the asylum laws, but before being granted asylum they have to go through the various bureaucratic steps of the German asylum procedure (Scheffer 2001), which can mean many years of uncertainty about their situation and the future of their families. An exceptional case was that of a family man who wept when he told me of his anxieties about his insecure status as asylum seeker. It would have been an unthinkable situation to do that in front of his family, for as a good Sudanese husband and father he had to embody strength and security. Another young man was granted asylum after waiting four years; and in his euphoria he showed me his (successful) application documents during our interview (Weißköppel 2006). In a previous conversation he had described to me in graphic detail the methods of persecution used in the Sudan, and had taken his shirt off to show me the scars from torture using corrosive liquids. From this encounter I got an insight into how refugees’ experiences of violence have to be legitimized, inscribed on the body and/or documented in files – otherwise they will not be recognized in Germany.

Just prior to my second stay in Sudan in 2002–2003, I read about violent clashes at Khartoum University between students and young, often untrained, military police: beatings, flying bricks, plundering of professors’ offices, tear gas, closure of the university for months on end. When I met people at Khartoum University five weeks later little evidence remained of the violent excesses on campus; so, in order to get more information I visited the surgery of a doctor in Khartoum who treated victims of torture and political persecution. During our interview he showed me a series of digital photographs which had been sent to the headquarters of Amnesty International in London: bloody stripes on young men’s backs, and plaster covered holes in shaved heads. ‘Europeans need naked evidence, otherwise they don’t believe us’ was the unspoken message. Once again, this encounter, which somehow connected different sites of knowledge production, enhanced my understanding of how the international movement for human rights works nowadays: There is a flow of digital information between victims, legitimized experts and professional multipliers.

But I also realized that there were different ‘visual’ cultures at play to which the refugees had adapted simultaneously (see Levitt and Glick Schiller 2004). In their home society they were forced to veil experiences of (political) violence, otherwise they would risk becoming persecuted. In contrast, in western countries they had to explicate or to embody the violence as a visual fact in order to be acknowledged as bona fide asylum seekers.
Conclusion: Traversing Cultural Sites to open up New Horizons of Understanding

To summarize, the multi-sited style implied a compromise between an actor-centred ethnography and conscious decision-making by the ethnographer. On the one hand multi-sited practice enabled me consistently to follow up relevant sites for Sudanese migrants in Berlin and Hamburg so that I could get a rough overview of the socially, religiously and politically fragmented ‘landscape’ of different networks situated in Germany, which were nearly all connected to other countries of destination and/or to the home societies in the Sudan.

On the other, it was obvious that I could not keep in touch with all meeting places and that I had to concentrate on selected networks or contact zones. I therefore made a clear decision for religious sites because the main thrust of my original question was that Sudanese migrants split into networks of different religious interest referring to their identities in their home society.

To keep in touch with different religious localities meant getting to know different cultural practices and meaning systems which either referred directly to traditions from Sudan (for example in the German-Sudanese Sufi Brotherhood) or to mixed sources like the international culture of humanitarian activism of Christian churches and NGOs. Therefore my task was not only to participate in these different practices but to concatenate the experiences ‘from being there ... and there ... and there’ (Hannerz 2003). This more or less conscious habit of traversing sites instead of plunging in deeply enhanced my interpretative capacity to explicate meanings articulated latently by Sudanese migrants in transnational spaces.

Beyond this specific knowledge about existential feelings of refugees in the Sudanese diaspora/s and their splitting into differentiated networks, I also got more insight into the general dynamics and problems of living in transnational spaces. Much of my ethnographic data serve as a ‘contingent window into complexity’ (Candea 2007, 179) of the Sudanese diaspora/s. Basing my arguments on individual biographies (Weiβköppel 2004; 2006) or specific contexts of agency (Weiβköppel 2005a; 2005d; 2008), I understand my further analysis as ‘ethnographies of the particular’ (see Abu-Lughod 1991), connecting it with theoretical questions concerning cultural ‘hybridity’ (Weiβköppel 2005a) or ‘simultaneous incorporation’ (Levitt and Glick Schiller 2004) between two or more societies (Weiβköppel 2008).

Further, due to my concentration on religious contexts of migrants’ activities I was able to open up new horizons of understanding in the scientific debate on the dynamics of religious identities in immigration countries (Adogame and Weiβköppel 2005; Lauser and Weiβköppel 2008). In my case studies I can demonstrate a wide range of motives for migrants to become active in religious congregations, for example the transnational expansion of a religious space as I could observe in a Sudanese-German Sufi brotherhood, (Weiβköppel 2005a; 2005c) and by contrast, in a Protestant church, getting access to material and social resources which enable the organization of further refuge for family members who have survived the civil
war in southern Sudan by fleeing to camps in the borderlands of Egypt or Kenya (Weißköppel 2008).

Although I sometimes struggle to get my fragmented data ‘ready’ for representation, I am convinced by now that the multi-sited approach was the most appropriate one for getting an idea about the complex struggle Sudanese migrants face.

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Once upon a time (and I am old enough to remember it) anthropological fieldwork was basically pedestrian – not in the sense of being stupid or trivial, but quite literally carried out on foot, because that was the way people in the field moved around (although in some places there were horses or camels to ride on, and in others perhaps canoes). It might take the field worker some time and effort to get to those usually distant fields, but once in place they might need, not least, apart from notebooks and quinine pills, a good pair of shoes. The assumption of ‘best practice’ was then to remain on site for a year, or two if funding allowed it, before coming home to write up.

Then, at some point in the 1990s, I found myself partly surrounded in my university department (in Stockholm) by colleagues, mostly younger, and doctoral students who went about their work in another way. The time of multi-site fieldwork had arrived. Some of them studied migrants, refugees or diasporas, and defined their extended fields primarily in ethnic, national or religious terms. For such multi-site work there were already models, particularly in migration studies (see especially Watson 1977). Several others took on the study of transnational working life, although focusing on a set of rather unusual occupations, and this perhaps became for a time the department’s most notable contribution to the emergent trend. Christina Garsten (1994) studied the organizational culture of Apple, the computer company, in three locations in Europe and America. Tommy Dahlén (1997) studied the ongoing professionalization of the anthropology-near field of intercultural communication, as applied in consultancy and training, not least in business and education. Helena Wulff (1998), doing her research on classical and modern ballet companies where they were resident and also accompanying them as they toured, made the point that ballet had actually been transnational for centuries, although the circumstances had certainly changed. Soon enough I joined this cluster, with a study of newsmedia foreign correspondents which took me to Jerusalem, Johannesburg and Tokyo, and more briefly to some other sites (for example Hannerz 2004).

Among the ten or so researchers who were thus having somewhat parallel experiences, although going about their studies in different places and in rather varied ways, we could fairly quickly identify some of the issues, challenges and choices which are more or less intrinsic to ethnographic multi-site studies and
which are also debated in illuminating ways in this volume.\textsuperscript{1} Personally, giving some more organized thought to the possibilities of multi-site studies in this context, I could also look back at how a couple of my earlier projects could conceivably already have moved me in such a direction. In the early 1970s, when I was in the Cayman Islands doing a study of local politics (Hannerz 1974), I could stand in the harbour of George Town, the capital, watching the fishing boats and the sea, and think of the historical network of small Anglophone communities on islands around the western Caribbean of which the Cayman Islands had also been a part. Unlike the Cayman Islands, still a British colony, most of them belonged to various Central and South American mainland nations. Their interconnections may largely have faded away in recent times, yet it might even now be an intriguing endeavour to search for what remains of them, in local cultures and collective memories. I did indeed toy with the idea, although never quite seriously, of spending field time on more or even all of them for such purposes, but then I went on to other matters.

Later on, in the 1970s and 1980s, as I was doing research in a Nigerian town and began to think about its cultural processes in terms of an analogy with sociolinguistic understandings of creolization, I envisaged a spatially-extended cultural continuum stretching from metropolitan centers such as London, by way of larger Nigerian cities, to towns such as mine (and beyond, to villages in ‘the bush’, the ultimate periphery) (for example Hannerz 1987; 1996, 65–78; 2006a). The point was to get away from what was the still regnant tendency in anthropology to delimit fields as bounded local units, and also to criticise the common public assumption that greater global interconnectedness would necessarily lead to cultural uniformity. It was hardly difficult, on the basis of fairly general knowledge of West Africa and the world, to recognize what other kinds of sites could be located along the continuum, and what kinds of relationships and cultural processes occurred between them. Yet some concrete ethnography from such locales and strategic groupings in them could no doubt have allowed some further elaboration of the idea – and helped make clear that the root metaphor of creolization is something more than a fancy way of saying ‘mixing’.

But mostly, among those multi-site ethnographers in Stockholm, we did not look back, and while we were aware that some of the complex, combinatorial fields we studied had a considerable historical depth, we could also see that the new wave of multi-site research was a methodological expression, perhaps the most conspicuous one, of an overall late twentieth century shift in anthropology: toward interests signified by such keywords as world system and globalization,

\footnote{Indeed one result of our experiences and discussions was perhaps the first book devoted entirely to multilocal, or translocal, field research in anthropology – although since we wanted to use it for our own teaching purposes, and give our local public a sense of what anthropologists could nowadays be up to, we published it in Swedish (Hannerz 2001). An edited version of the introduction to that volume, however, has also appeared in English (Hannerz 2003).}
transnationalism, multiple modernities, diaspora, hybridity and creolization, and cosmopolitanism.

Certainly there was a shift, evident not only in the academic discipline but in public consciousness as well. The word ‘globalization’, now everywhere, hardly came into general use until the 1990s. Yet here, briefly, I will allow myself to be a contrarian. Taking the long view from that pedestrian, and classical, mode of doing field work, I am perhaps less inclined than some to see only discontinuity. I see rather a long march of anthropology (those good shoes may be useful again), not necessarily entirely straight but with false starts, dead ends and forgotten detours along the way. Elsewhere, I have already once described the expanding understandings of anthropological fields over time in terms of ‘studying down, up, sideways, through, backward, forward, away and at home’ (Hannerz 2006b). Here I would like to point to just some of the ways in which current concerns, directly or indirectly linked to multi-site studies, have had their forerunners in anthropological thought and practice.

Regions, Continua, Levels, Civilizations

Let us remember a few of the leading mid-twentieth century figures of anthropology. They happen to be Americans. Robert Redfield had one kind of evolutionary perspective, stretching from ‘little communities’ to civilizations, linked in what he described as the ‘folk-urban continuum’. The very idea of a continuum may at times suggest the possibility of a series of field sites for its ethnographic mapping – again, I think of my own notion of a creolization continuum – and one part of Redfield’s work on such matters was a major project involving field studies in the tribal village of Tusik, the peasant village of Chan Kom, the town of Dzitas, and the metropolitan city of Mérida. *The Folk Culture of Yucatán* (1941) was the major book resulting from the effort. Redfield’s own ethnographic involvement was mostly concentrated in Chan Kom, but as a ‘big man’ in the anthropology of those times, with appropriate access to funding, he led several fieldworkers, posted to the different sites. So is this study not a part of the history of multi-site studies? If so, it raised early on the alternative possibilities in such studies of single researchers and research teams, alternatives which also come up elsewhere in this volume.

Some time later, the large volume edited by Julian Steward, *The People of Puerto Rico* (1956), also drew on the work of a team, notably including Sidney Mintz and Eric Wolf, coming leaders in the discipline, in early ethnographic efforts, focusing on different types of Puerto Rican communities. Like the Redfield project, the study was thus defined in terms of a wider regional entity, and certainly this gave the projects one kind of overall coherence, even if linkages between field sites did not have the prominence they usually do in recent explicitly multilocal studies. The point I would want to make, in any case, is that there were in mid-twentieth century anthropology some attempts to move beyond the study
of the local and small-scale: Redfield, like Alfred Kroeber, developed points of view toward civilization (which I find still relevant at a time when that concept is again in circulation in geopolitical discourse), Steward, with another evolutionary perspective, had his notion of ‘levels of sociocultural integration’. Even if Steward did not himself take this scheme beyond a national level, one might for example take note that it inspired Alvin Wolfe (1963) to make an early attempt to conceptualize a ‘supranational level of integration’, with special reference to the African mining industry, even before we knew of a world-system theory.

Earlier yet, there had been the Boas student Melville Herskovits’s pioneering attempts to trace African components in the African-American cultural heritage. His *The Myth of the Negro Past* (1941), a somewhat hurried synthesis, was an early instance of American public anthropology, hardly entirely successful but enduringly influential as well as controversial. However, Herskovits did not only bring together available written materials on black life in the New World. He also undertook field studies in Surinam, Haiti, Trinidad, and Brazil, as well as in an area of West Africa which had been prominently involved in the Atlantic slave trade. Perhaps – taking the long view of much of one scholarly career – this could be seen as one multi-site research endeavour, not only a number of separate field projects? One might just possibly be reminded of Herskovits’s movements within a diaspora as one learns, in the present volume, of Karen Leonard’s extended work on the Hyderabadis. Anyway, understanding cultural connections between Africa and Afro-America remains a challenge to anthropologists. The ethnographic and historical work of Lorand Matory (for example 2005) in Brazil and Nigeria is perhaps not so often identified as part of the growing corpus of multi-site studies, but it has contributed intriguingly to a view of cultural processes in both continents, and in the traffic across the Black Atlantic.

**Culture Distributed**

A notion of knowledge as distributed, differentially yet in a more or less organized fashion, seems now to become recurrent in multi-site field studies; people in different locations are linked in sometimes intricate ways by thinking in different ways, knowing different things. It is a kind of understanding which also has a longer history in anthropological thought than is perhaps generally realized.

At times this distributive view may be seen as contrasting with an old (and rightfully criticised) conception of culture as something homogeneous, static, and well-bounded. One may or may not want to use the term ‘culture’ itself, in view of dubious past and present uses. I am not sure that ‘knowledge’ is necessarily a much less ambiguous alternative. In any case, distributive understandings of culture go back at least to another of Franz Boas’s students, Edward Sapir (1938), and his ponderings on the differences in views between a yet earlier ethnographer’s Amerindian informants. They have then showed up, in different places at different times, used by different authors for different purposes, so that it is far from easy to
assemble an overall view of them (but see Hannerz 1992; Rodseth 1998). Although his own particular view is quite micro-oriented, I have found Anthony Wallace’s (1961) broad contrast between ‘replication of uniformity’ and ‘organization of diversity’ perspectives toward culture particularly striking, since I first encountered it almost half a century ago. Since then, Roberts (1964), Goodenough (1971), Schwartz (1978) and others have offered other more or less elaborated analytical constructs for understanding culture distributively.

Mostly these writers did not concern themselves much with any spatial dimension, and thus with knowledge distributed between separated sites, although it is hardly difficult to take their work in such a direction. Ideas of civilization, however, with their assumptions of centre-periphery structures and refined and simple versions of religions and other symbolic orders, have often quite obviously involved understandings of differential cultural distributions between sites. This was evident, for example, in the contrast drawn by Robert Redfield (1956) and others between ‘great’ and ‘little’ traditions, particularly in studies of Indian civilization. As Barbara Ward (1965) discussed the varied ‘conscious models’ of Chineseness which she found among the ‘boat people’ in an island village on the outskirts of Hong Kong, it was also clear that they maintained a distributive understanding of the ways in which they were like and unlike other Chinese.

Scholarly Generations and Views of the Past

My point in bringing up these writings from earlier periods of twentieth-century anthropology is certainly not to claim that everything ‘has been done before’. I do think that anthropologists in the last few decades have faced a set of new circumstances, and have responded to them creatively. Yet there has been a tendency to ignore the diversity of past anthropology, and therefore probably to make less than full use of opportunities to identify what might be good to think with in the work of predecessors. ‘History of anthropology’ becomes another somewhat marginal speciality within the discipline, rather than a living, more widely shared resource.

Current tendencies in the formal and informal organization of academic life may well contribute to such intellectual amnesia. Within the rather inward-turning academic marketplace, attentiveness to current fashions and vocabularies may go with an inclination to claim newness. The growing dependence in academic management on devices such as citation indices may push the consciousness of the career-minded in the same direction. Taking a slightly longer view of shifts in the discipline, however, it seems to me that one might usefully look at some of the discontinuities in thought and practice in terms of generational cleavages (not least in American anthropology).

George Marcus (2008, 10), one of the prominent innovators in late twentieth-century anthropology and a contributor to this book, has recently reminisced about his own graduate student days at Harvard University in the early 1970s. Although
he points out that anthropology always had exciting potential, he thought that at the time, the teaching of it was stale. So instead there had formed, not least among anthropology students (although it had counterparts elsewhere on campus), an ‘invisible college’ where other readings were discussed: Foucault, Barthes, Habermas, Althusser, and the ‘literary turn’. And so these sources of inspiration from outside the discipline left a mark on the new anthropology which gathered strength and took over at the centre of attention in the 1980s.

Marcus’s generation, at Harvard and elsewhere, would seem to have been more or less that of the ‘baby boomers’ (to the perhaps slightly earlier Swedish counterpart of which I happen to belong myself). Large in number and perhaps collectively self-confident, it was capable of redefining the situation, and deciding for itself what was worth reading and what was not. Here I am reminded of a recent brief consideration of another strong academic generation, earlier and with the anthropology department at Columbia University as a base. The members of the Mundial Upheaval Society (there was perhaps some sense of irony even in that generation), again a sort of informal reading network, were in large part World War II veterans, coming to the university on the ‘GI Bill’ which allowed ex-soldiers to further their education. Among them were, for example, Eric Wolf and Sidney Mintz. It was a time when the department, notes William Peace (2008, 147), the recent historian of this grouping, was still ‘reeling from the death of Franz Boas’. The type of softly humanistic anthropology exemplified there by Ruth Benedict, a rather mediocre teacher, had little appeal for these worldly, hard-nosed, often radical, not so young students, mostly inclined toward materialism and evolutionary thinking, and issues of class and power. So they formed their somewhat subversive, and apparently rather macho, reading group. The perspective of Julian Steward, newly-hired faculty member, was a great deal more to their liking; several of them later joined his Puerto Rico project. Peace, their historian, does not say a whole lot about their outside sources of intellectual inspiration, but it is clear that they read some amount of history and archaeology with an affinity to their views. It is likewise clear that they engaged in a somewhat systematic campaign to expand their influence in the discipline.

Columbia University around 1950, or Harvard University in the early 1970s, were not all there was of anthropology, or even of American anthropology, in these periods, but they seem to exemplify the tendency of strong generations to distance themselves from existing scholarly universes which may seem to be yielding diminishing returns, and to offer little scope for their own ambitions. When Sherry Ortner (1984), in a well-known article, surveyed dominant tendencies in anthropology between 1960 and 1980, she identified a period at the beginning of which an earlier set of prominent American anthropologists had just left the scene (including some of these mentioned above), and when we may see that it was the veterans of the GI Bill generation who were taking over. (Elsewhere on the same scene, Clifford Geertz had also once been a GI Bill student.) And then toward the end of the period, as George Marcus’s generation assembled, it was with the various preoccupations of that group it became impatient.
I am not convinced that the discontinuities were equally marked in every anthropology department even in the USA in those periods. (In the early 1970s, I spent a year teaching in one of them which was quite unlike that of Harvard at the same time.) Nor do I quite believe that even if American anthropology has become increasingly dominant in the global ecumene of anthropology, the discipline elsewhere moved entirely in step with the same drummers. Nonetheless, there are at least relative discontinuities and trend shifts, and with these have evidently gone a widespread inclination mostly to ignore the past of the discipline, and to represent it in stereotyped and impoverished forms. My extremely sketchy references here to some midcentury anthropologists are certainly not intended to imply that they made up a cohesive, harmonious group. Indeed some of them were evidently adversaries with regard to some of the important issues of their times. The point, then, is precisely this: for a rather long time, anthropology has been internally quite diverse, nationally or internationally, and any attempt to explore the traces of that long march – of a fairly loosely interconnected crowd of coteries and loners, innovators and stragglers – may result in a richer sense of previous problem definitions, experimentations, solutions, failures and remaining loose ends. What one might wish for, then, is a combination of that openness to current and emergent realities, and to sources of inspiration in the surrounding intellectual landscape, which is needed to keep the march going, with some more curiosity about what might still be instructive from the past.

Multi-site Collaborations

As I have suggested, that past includes studies which can be taken to point forward to the more recent interest in multi-site ethnography, and to conclude, I will take up one particular issue which the team research organized by Robert Redfield and Julian Steward also raises. I will approach it by drawing on my own study of newsmedia foreign correspondents which I mentioned before. This does not have so much to do with the way I did my own study, but rather with an instance of news work.

The World Trade Center in New York, it may be remembered, was a target of terrorism not only on 11 September 2001, but also before that, with a bombing in 1993. The man who was suspected of having planned that bombing, Ramzi Ahmed Yousef, was only caught in early 1995, in a rooming house in Islamabad, Pakistan. The way the New York Times, with its sizeable network of correspondents, covered that story in the days which followed seems to me interesting not least for ethnographers. The paper’s South Asia correspondent was quickly in place in

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2 My comments on multi-site collaborative research here remain somewhat parallel to those I made in an overview of transnational research a decade ago (Hannerz 1998).

3 The details of the story, with precise references to the reporting in the New York Times, is in Hannerz (2004, 202–3).
Islamabad, to report on the circumstances of the arrest from there. But almost at the same time, the Southeast Asia correspondent could report from Manila that the suspect had previously spent time in that city, almost getting caught when a bomb he was preparing exploded—and on the laptop computer he left behind when he fled, there was evidence that he had planned to bomb American passenger airlines, and also to assassinate the Pope (who was about to visit the Philippines). It then turned out that when Yousef was caught, it was after a tip-off by a South African Muslim who studied in Islamabad and moved in the same circles. And the Africa correspondent of the paper, based in Johannesburg, could then quickly report on the reactions of this informer’s relatives and friends in South Africa to the news that he had betrayed a fellow Muslim.

The reader of the *New York Times*, then, got a kind of rapid multi-site coverage of a news event. And it could be orchestrated in this way because the large news organization had correspondents who were, if not exactly in place, able to get there (and there … and there) at a moment’s notice. This suggests a kind of argument for team research in multi-sited studies which is also put forward elsewhere in this volume, particularly in Mazzucato’s chapter on studying Ghanaians at home and abroad. There are times when simultaneity matters, when research in interconnected sites should preferably go on at the same time, to offer the best view especially of quicker-moving processes. When the single researcher moves over time from one site to the next, they are a bit less likely to get a clear and detailed understanding of such phenomena.

Another argument for multi-site collaborative efforts obviously has to do with the circumstances of distributed knowledge. The point is made often enough that a single, mobile fieldworker, covering several sites in the time usually allotted to an anthropological field project—again, that year or so—cannot acquire the depth of knowledge that a more sedentary, all-pedestrian study of a single site allows. The argument may sometimes be a bit beside the point: multi-site studies have foci which should not be compared to holistic local community studies. It is true, however, that in practice, single-researcher multi-site studies may tend to be somewhat constrained in the choice of topics and sites by the researcher’s capabilities, in cultural and linguistic terms. In his early review of such studies, Marcus (1995, 101) concluded that most of them had been conducted in monolingual, mostly English-speaking settings.

The team research directed by Robert Redfield in Yucatán and by Julian Steward in Puerto Rico was mostly carried out by young field workers from the USA, and so apparently did not match particular cultural and linguistic skills to particular sites—although notably, much of the ethnographic work in the village of Chan Kom was actually carried out by the local school teacher Alfonso Villa Rojas, who then went on to be trained as a professional anthropologist at Redfield’s department in Chicago. Clearly, however, the possibility of putting together a research team of members with different linguistic and cultural competences, matching the diversity of sites, should greatly expand the range of potential multi-site projects. In his contribution to this volume, it is true, George Marcus argues that ethnographic
projects in anthropology are likely to remain ‘resolutely individual’ because this is programmed into the making of professional anthropologists. Yet if this goes for projects which are part of a graduate education, there may still be a professional life beyond the dissertation project, and it does not seem entirely obvious that other, later projects have to take the same form. (Come to think of it, in earlier writings Marcus has indeed argued that much of the innovation in late twentieth-century anthropology came out of such later, post-dissertation research.) Perhaps there is room here, in other words, for more rethinking of the varieties of anthropological field research. The work of the Matsutake Worlds Research Group, as described in this volume, is certainly suggestive in this context.

I admit that my own multi-site research experience, from the foreign correspondent study, is indeed of the single-researcher type, so I can only imagine what a multi-site team project might be like – apart from such evidence as a couple of the contributions to this book offer. It would appear to me, in any case, that the conditions for such collaborative efforts are nowadays very different from what they were in Malinowskian times, or even a couple of decades ago. Ethnography in the era of laptops and email need not be entirely a lone-wolf enterprise. The ease of communication between dispersed collaborators, in the field and during writing up, would seem nearly complete in technological terms. If there are difficulties, they are probably mostly of other kinds, involving in part such rather less tangible factors as commitment and trust.

The question of the difference, and the relationship, between ‘anthropology at home’ and expatriate anthropology is very likely to come up here. In much of the world, a large part of anthropological research is now obviously of the former kind, in the sense of involving scholars in field study in their own nations. Sometimes these local scholars have sharp criticisms of the work of visiting expatriate colleagues, but the latter may have other advantages, and contribute other perspectives. The possibility is readily at hand of a kind of multi-site team research where team members make their ethnographic contributions to the project in large part or entirely in sites where they already have local knowledge. This may help deal with the challenge of having the skills to match the internal diversity of the multi-site field, and also, again, with a need for interconnected data simultaneously collected in several sites. It is also likely to allow for a more longitudinal view than the traditional one-year-for-field travel type of project. On the other hand, no doubt, such collaborative attempts can generate problems concerning the intellectual division of labour – to put it bluntly, who brings the

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4 I recently had a conversation with one anthropologist who began working closely, and co-authoring, with a colleague when both of them resided in the same European country. Then one of them moved to Scandinavia, and the other to the South Pacific. Yet my conversation partner said their collaboration could now work even more effectively, due to the difference in time zones. When one of these two had finished work for the evening and emailed the results to the other, it was morning on the other side of the world, and the colleague could begin attending to that mailing immediately.
high-flying theory, and who brings the pedestrian ethnography? Once we can look back at more such experiments, I suspect, we may come to find interesting tensions and spectacular failures, and also instructive successes, models for further work. Let the long march of anthropology continue, in its changing diversity of styles.

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