Introduction

Multi-sited Ethnography: Theory, Praxis and Locality in Contemporary Research

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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,

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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'.[2] 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'.

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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[3] 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.[4] 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).[5]

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 *trans*national'. 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

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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'.[7] 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',[8] 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.

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7 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).

8 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.

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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 Riain (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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in favour of one in which '(m)eaning must be seen rather as the co-production of speaker and listener where both share in the same active power of linguistic competence' (Mueller-Vollmer 1997, 14).

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'.[11] 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 cosmologies' (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 plurivocity 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[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

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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.

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