

The fieldsite as device

“Oceanography and ethnography have much in common. The convergences came to me most vividly when I was at sea one day in Monterey Bay. I was on a trip with scientists using a remote controlled robot to gather microbial samples. Our presence at sea, it occurred to me, was fieldwork for both marine scientists and myself. [...] The more I thought about it, though, the stranger fieldwork seemed as a word for what we were doing: marine biologists were assaying an underwater environment in motion; I was following activities that would take me to labs and classrooms not always so clearly fenced off from my academic life.” (Helmreich 2009:21-22)

Is a fieldsite a device, as defined by the editors of this special issue, namely a ‘patterned teleological arrangement’? This question has a rather different resonance if asked of a behavioural ecologist and of a social anthropologist. The present article starts from an account of a biological fieldsite, the Kalahari Meerkat Project (KMP), which could be described as a careful patterned arrangement whose main purpose is to collect observational data on meerkat behaviour and ecology. In line with recent historical and social scientific accounts of field science, the KMP could be described as providing an alternative, emplaced, observational counterpoint to the classic model of laboratory science: a particularly robust device for producing knowledge about a particular species located in a particular place.

Or that at least, is how I first understood it after undertaking ethnographic fieldwork there in 2008. Based in part on my first visit to the KMP, and on ongoing interviews and documentary research, I published an article on human-animal relations in scientific research which focused amongst other things, on the researchers’ ability to maintain a detached, distanced form of co-existence with the animals they studied (Candea 2010). While fans of a television show filmed at the site, dismayed at the death of their favourite furry stars, urged the researchers to intervene, the researchers stuck to their ethic of non-interference. The KMP was a device for ‘letting nature take its course’ while documenting it as closely as possible, with the minimum disturbance possible.

But you never step twice into the same fieldsite. When I returned to the KMP in 2011, what I saw was more akin to a large-scale experimental project, involving a range of carefully controlled human interventions into the meerkats’ lives and behaviour. Partly the project itself had shifted somewhat in direction and structure. Partly my own understanding of animal behaviour science, and my relationship to the project, had changed in subtle but far-reaching ways, which meant that aspects of the KMP which had seemed distant or secondary in 2008 had now come into full view, while aspects which had forcefully claimed the foreground back then, had now receded into the background, become obvious and no longer snagged my attention. New things, in other words, had become interesting.

On the face of it, then, this is a story about the KMP as a biological fieldsite, a particular scientific device whose aims and objectives are negotiated through time on the contested borderlands between field and lab, between observation and experimentation. But running through the above description is another device: the KMP as *my*

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ethnographic fieldsite – or rather, as one of the fieldsites in an ongoing multi-sited research project on the translocal world of behavioural biology.

The KMP as anthropological fieldsite only partly overlaps with the KMP as biological fieldsite, but it too has its patterns and arrangements, and, perhaps – albeit more problematically – its teleology. Through a series of empirical descriptions and vignettes, this paper examines side by side these two fieldsite devices, 'mine' and 'theirs'. In conclusion I return to what these two devices and the differences between them can contribute to ongoing discussions in science studies and beyond about the nature of field science.

RE-PLACING NATURE: THE SCIENTIFIC FIELDSITE AS DEVICE

The past two decades have seen a growing number of writings on scientific fieldwork (Kuklick and Kohler 1996; Kuklick 1997; Kohler 2002; Rees 2009; Kelly 2012; Gieryn 2006; Radick 2007). After being for a long time focalised on the laboratory, studies of scientific practice turned to the fieldsite as a distinctive scientific device for engaging with the world. In this literature, as perhaps also in the experience of many practising field scientists, the field emerges very much in the shadow of the lab. Field science exists, as Kohler puts it, "in a world of labs and experiments" (Kohler 2002:ii).

For some commentators, the field is a sort of 'anti-lab', the mark of a radical break with the classic model of placeless fact established under inquisitorially controlled conditions. Isabelle Stengers, for instance, contrasts the model of the theoretico-experimental sciences, in which the scientist has the power to stage his own questions, and the "irreducible uncertainty [...] of the field sciences" (Stengers 2000:144). Indeed, she argues, "No field is valid for everyone, no one can authorise the "facts" in the experimental sense of the term. What one field-site allows us to affirm, another field-site can contradict..." (Stengers 2000:140-141; See also Despret 1996; Despret 2002) If the lab acts out the apotheosis of the subject's power over the object, the field, the acme of an 'object' which talks back, puts researchers to the test of its own interests and agendas. Beneath the skin of these contrasts pulses the hope that field scientists might echo, finally, from within the scientific establishment, the science studies dream of a truth which accepts itself as irredeemably, joyfully, local and multiple – situated and partial, in other words, located, rather than universal (Haraway 1989).

More often, however, historians and sociologists of the field sciences have painted a picture of the lab-field boundary as an ambivalent border-zone, in which fieldwork is as often as not marked by attempts to extend and adapt the norms and expectations of laboratory life into a necessarily imperfect and challenging medium, rather than by a positive embracing of 'irreducible uncertainty'. For the science of behaviour in particular, Amanda Rees (Rees 2009; Rees 2006) has given an excellent historical account of the development of the field-site itself as a form of "scientific instrument" in primatology which resonates strongly with the situation in field behavioural biology more generally. On the one hand, the field-site (at least in its ideal version) allows behavioural biologists to claim a direct access to undisturbed natural behaviour which laboratory science cannot aspire to – in one form or another, this has been a feature of ethology's claim to distinction against lab-based behavioural psychology since the early days of Lorenz and Tinbergen (Burkhardt 2005; see also Radick 2007:253-256). The claim to the unique value of naturalistic observation as an alternative to the 'royal way' of laboratory experimentation is still upheld by many behavioural biologists (e.g. Dawkins 2007).

On the other hand, Rees also notes that, just like the ideal laboratory, the ideal field-site is precisely that: an ideal, and thus dogged in practice by shortcomings and ineradicable uncertainties. Rees points to the difficulty of isolating the field from threats

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to its "physical and epistemological integrity" (Rees 2009:8), the difficulty of deciding on what constitutes natural behaviour when animals have to be habituated in order to tolerate observer presence to begin with, debates over the abilities, qualifications and representativeness of observers. All of these aspects build up to what Rees terms 'fieldworker's regress' (ibid.): findings from a particular fieldsite are always open to challenge, particularly under conditions of scientific controversy.

At the KMP the potential danger of 'fieldworker's regress' was dealt with in two ways, which mark out its specificity and its claim to fame in the world of behavioural biology. The first was through the sheer scale of its effort: the multiplication of observers and measurements, and the longevity of the project. The second was through a close integration of observational and experimental approaches which, effectively, brought the lab to the field. In this respect, KMP researchers were the inheritors of an ethological tradition which had made a particular virtue of combining fieldwork and experimentalism (Radick 2007: 253 and *passim*; see also Rees 2009:22-24). I will focus on these two aspects – scale and experiment – in turn.

AN ENDURING DEVICE

Meerkats (*Suricatta Suricatta*) are mongooses who live together in groups of around 10 to 50 individuals, headed by a dominant male-female pair who monopolise reproduction, while subordinate members of the group help to keep watch for predators while the group are foraging, to feed and babysit the pups, and in a range of other ways. In the language of behavioural ecology, these type of animal societies are known as 'cooperative breeders', and they constitute an ideal 'system' for enquiring into cooperative behaviour, a topic which has fascinated the discipline since the sociobiological turn of the 1970s (Wilson 1975; Krebs and Davies 1997).

The KMP was set up in the early 1990s to study meerkats in their natural environment, habituating them to human presence to enable observers to follow them and record their natural behaviour (for the early history of the KMP, see Clutton-Brock 2008). The seeming contradiction in the previous sentence does not escape the behavioural ecologists: how do you transform an animal's behaviour in such a way that it accepts the close presence of human observers, without transforming it in such a way that you have troubled the very phenomenon you wanted to study (Rees 2006)? The KMP could be seen as a large and particularly well-designed device for managing precisely that problem. KMP researchers undergo a painstaking procedure, which can take up to two years, through which a meerkat group which strays within the grounds of the project is slowly habituated to the presence of an observer – at 50 meters, then progressively closer, sitting, then standing, immobile then moving and finally walking along with the group as they forage (for a more in-depth discussion of concepts and practices of habituation which also considers their theoretical relevance for anthropology, see Candea, Forthcoming).

In this respect what the KMP is doing is broadly in line with other behavioural ecology fieldsites, and the method of habituation, with its attendant concerns about detachment and non-interference is shared broadly (Rees 2007). One aspect in which the KMP particularly stands out, however, is in its sheer scale and staying power: the site has managed to follow a population of around 200 meerkats for nearly twenty years. In a world in which the statistical analysis of quantitative data rules, this is a non-negligible achievement.

Part of this staying power is economic. Somewhat uncharacteristically for such field projects, the KMP owns a large part of the land upon which it operates, two adjacent farms purchased in the early 1990s and fused together into a Reserve. This initial

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independence speaks of an impressive and ongoing success in securing research funding, but it also enables a set of other economic benefits. The KMP has been extremely successfully networked with a series of lay interests in meerkats. Thus some of the project's funding comes from documentary and other film-makers paying for access to a habituated population of meerkats, through a regular stream of paid visits by eco-tourists via the association Earthwatch, and through membership fees of an association of meerkat enthusiasts called 'Friends of the Kalahari', many of whom became enamoured of the furry critters at the site by following their adventures on the show Meerkat Manor (cf. Candea 2010).

Part of this staying power is technologically enabled. Each new meerkat group which forms or wanders into the area is coopted into the project not simply through habituation, but through a multilayered socio-technical bind. If habituation is slow and painstaking, technological cooptation on the other hand is quick and sharp: it involves collecting a blood sample from each individual, inserting under their skin a micro-chip bearing their unique ID code, and fitting the lead male or female of each group with a radio-collar. These various technical fixes mean that groups can be traced, and individuals recognized consistently through time.

Technical fixes alone cannot however stabilise inter-species relationships, and habituation has to be maintained, as well as set up. Just as importantly, data must flow continuously into the database, at the risk of interrupting the ongoing life-history of the meerkat population. To these intertwined ends, a rolling population of volunteer observers, mostly biology graduates from UK universities, provide a constant human presence at the site. In this respect too, the KMP stands out, by its reconfiguration of the widespread division of labour in scientific fieldsites. In many if not most contemporary animal behaviour field stations, the actual work of observation and basic data collection either falls to local technicians, or is done directly by doctoral students who are working on their own research projects. Lorraine Daston has traced the emergence, at least as early as the work of Charles Babbage, of a "division of labor in science between the lowly work of 'recording precisely the facts which nature has presented' and the 'higher task' of revealing 'the undiscovered laws by which nature operates'" (Daston 1994:196, n148; citing Babbage 1830). Divisions of labour between foreign researchers and local participants is a widespread feature of contemporary biological field research, and the mediation of these relationships, particularly in the often complex post-colonial contexts in which much field science operates, has been an important focus of research (Kelly 2012).

The Kalahari Meerkat Project volunteers, however, represent a third category, in between data technicians and fledgling researchers. In many respects, KMP volunteers play the role of data technicians: while they are given training in observational methodology, they are not involved in designing the research, or in selecting what counts as relevant behavioural data.

On the other hand, unlike most data technicians, KMP volunteers only stay at the site for a year, and are nearly all university graduates from the UK with degrees in biology or conservation. Some of them see the KMP as the beginning of a career in scientific research, perhaps at the KMP itself. Indeed, while the volunteers form the bulk of the KMP's population (around fifteen to twenty at any one time), the site also includes more transient doctoral and post-doctoral researchers (the 'independents') who stay at the site for a few months working on their own projects. Every year, some of the volunteers stay on at the fieldsite and become involved in its management, while others return to the site as research assistants to independents, or as PhD students, 'independents' in their own right. Thus, for instance, the data manager in 2011 had been a volunteer during my

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previous visit, as had his girlfriend, who had now returned as an assistant on one of the research projects.

Other volunteers see their time at the KMP rather as an opportunity to build up skills and an experience of extended, demanding fieldwork, as a first step towards a career in animal conservation. The majority in my experience had considered both of the above options, and saw their year at the KMP as an opportunity to decide on a choice of career, to test their aptitude and interest.

Either way, given the project's high profile and status within the discipline, volunteering at the KMP was a highly sought-after opportunity, and the project has not only a steady stream of volunteers, but can interview and select amongst its applicants those who have prior field experience and seem most likely to be able to get along with others to keep the site running smoothly.

This again sets the KMP somewhat apart from other such fieldsites. Kuklick and Kohler have argued that close collaboration in the field between 'professionals' and 'amateurs' makes "cultural translation [...] a persistent and pervasive possibility" (Kuklick and Kohler 1996:4), and contributes to the more general porosity of field-sites, unlike laboratories which "rapidly become exclusive places [...] to which access is restricted and in which a specialized enterprise is pursued by a narrow range of social actors." (ibid). The KMP, by contrast, has rewired this situation in such a way that the technician role is delegated to a carefully selected population of fledgling scientists, who are themselves in the process of being trained to become the next generation of project managers and senior researchers. The same process which achieves a certain kind of closure and autonomy, also contributes to the projects' self-perpetuation.

Habituation doesn't simply require constant human presence, it requires this presence to be carefully managed on a day-to-day basis. At the KMP, a centralised rota ensures that each meerkat group is followed by one observer just enough to keep them habituated, but not so much that their actual exposure to predators, or their sense of vigilance is affected. It ensures also that different volunteers, with different levels and kinds of observational skill, are rotated around the groups, providing the observational equivalent of randomisation. Strict procedures are in place to ensure that volunteers don't interact with the meerkats more than is necessary (cf. Candea 2010).

Of course, keeping meerkats used to human presence isn't the volunteers' only or indeed main task – it is a means to the end of data collection, each volunteer following a group of meerkats for around six hours a day, six days a week, to feed the project's database of behaviours which contains millions of lines of code, each line indexing one behaviour undertaken by a meerkat individual whose parentage and social position in its group is known, as is their weight and reproductive history.

FROM OBSERVATION TO EXPERIMENT

The above description portrays the KMP as a large-scale, internally heterogeneous, yet strongly patterned and single-mindedly purposeful device for the collection of behavioural data. It draws primarily from my first stint of fieldwork at the site in 2008. My initial understanding of the KMP, and the paper I subsequently wrote (Candea 2010), were centrally concerned with the project's ability to build 'interpatient' social relations between humans and meerkats, and more broadly, with the ways in which a care for this relationship was combined with a 'care for the data' (cf. Fortun n.d., Walford, forthcoming). In both respects, detached non-interventionist co-presence was, to my mind, the key feature of the KMP 'device'.

I was thus somewhat surprised when, some time after my initial field research at the KMP, I increasingly encountered clues to the fact that the massive production of

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observational data, while important bedrock of KMP activity, was not perhaps what the KMP was most famous for in the behavioural ecology community. Rather, judging by interviews and incidental conversations, what other behavioural ecologists picked out as particularly significant about the KMP was its unparalleled ability for hypothesis-testing, including by some active experimental intervention in the population it studied.

Of course, much hypothesis-testing can operate simply through the scale of the project and size of the database, which allows internal comparisons between different groups and types of individuals at the site: do males help more than females, old more than young? Is there a positive advantage in living in a group of a certain size? As Amanda Rees (Rees 2009) has argued in the case of primatology, the value of long-term field studies of individually known animals is related to the rise of sociobiological theory in the 1970s. Together, these two developments marked a particularly significant moment in the biology of behaviour:

[A]nimals were known as individuals and their unique histories were recounted in site records, and sociobiological theory made these records meaningful. It allowed researchers to ask tightly focused questions about the reproductive consequences of individual behavioral "decisions," often phrased in terms of a "cost-benefit" metaphor that reflected both the importing of game theory into evolutionary biology and the increasingly statistical analysis of the quantitative data it was now possible to collect. Description had been replaced by investigation: natural history, it appeared, had now become science. (ibid.: 205)

But, the KMP set-up also allows for actual field experiments. These include 'presentations' in which animals were exposed to a sound, smell or object – such as a faecal sample from another group – and their reaction assessed; 'feeding experiments', in which particular animals' diet was supplemented for a period of time and the change in their behaviour and/or morphology assessed; and, occasionally – subject to appropriate ethical approval procedures – hormonal treatments either to temporarily suppress reproduction, or to assess the hormonal correlates of particular types of behaviour.

Seen from this angle, in other words, the KMP is very much a device geared to the 'theoretico-experimental' model of science. The KMP is, in Amanda Rees' formulation, not simply a place that produces data, but more pointedly, 'a place that answers questions' (Rees 2006; for a sense of the range of questions answered both experimentally and observationally based on KMP data, see for instance Clutton-Brock 1998; 1999; 2002; Clutton-Brock et al 1998; 2000; 2001; Scantlebury et al. 2002; Carlson et al. 2006; Thornton 2008; Madden 2009; English et al. 2010). I slowly began to realise this sometime after my first visit to the site, through conversations and interviews with senior KMP researchers and other behavioural biologists back in the UK. When I returned to the KMP in 2011, its status as a 'theoretico-experimental' device seemed blindingly obvious. Why had I not seen this the first time around? Had the KMP changed? Had I? At this point my account of the KMP as biological fieldsite starts to morph into an account of the KMP as *ethnographic* fieldsite – another, different device for producing knowledge.

HERE AND THERE: THE ETHNOGRAPHIC FIELDSITE AS DEVICE

Certainly, the KMP had changed somewhat between 2008 and 2011. In the intervening years, the project had grown: more volunteers were following more groups and taking more and different types of data as part of their day-to-day job. The KMP now had a dedicated data manager, Al, who kept a close eye on the data-productivity of

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the volunteers. Al recalled that during his own volunteer days (at the time of my last visit), no one used to ask him how many 'lines' (shorthand for observed behaviours) he brought back from a field session. These days, he kept track of how many lines volunteers return on average and would raise concerns if a volunteer consistently returned fewer than average. But Al also explained that his and the project's aim in striving to habituate more meerkat groups was to expand the base for experimental purposes: his ideal target of twenty fully habituated meerkat groups, would allow for experiments to be run on ten groups with the other ten as control, thus providing greater statistical validity than was possible at present with only fourteen fully habituated groups.

Of course, field experiments were not new. Indeed, as Radick (2007) has argued, they formed one of the distinctive legacies of Tinbergen's ethology – behavioural ecology's parent discipline. Nor were they new at the KMP: feeding and presentation experiments had been run pre-2008. None of them, however, were running at the time of my visit, and they had in any case been entirely the province of the 'independents'. By contrast, in 2011, the volunteers' instructions and routine had changed. They now did not simply collect data on behaviour, weights and life-history, but also participated in a couple of ongoing feeding experiments. The project had also recently run a relatively large-scale hormonal experiment which the volunteers were still discussing at the time of my arrival. Would this impact on meerkat behaviour or transform the 'natural' shape of the population? Would this mean that the life-histories which were still being painstakingly amassed in the database would now lose their meaning? Perspectives on such questions differed amongst the volunteers I spoke to. Some of the independents, with a somewhat longer-term experience than the volunteers, were arguing more generally that the project's outlook was increasingly shifting from the observational towards the experimental. One in particular contended that this was a necessary step forward: the project had amassed nearly two decades' worth of observational data on natural behaviour and population structure, and the questions which could be asked on the basis of observation alone had now mostly been answered. New questions required experimental manipulation.

In other words, the field itself had slightly changed. But so had my own outlook: I was in a position in 2011, to attend to aspects of the KMP which I had little grasp on in 2008. For my first trip, I had intentionally gone into the KMP relatively 'cold', with only a rather basic understanding of the conceptual issues at stake. I had thus mainly come away (as was my aim), with a sense of the levels of the research which most mattered to the volunteers at the KMP – hence my focus on the pragmatics of detached interpatience and on the details of 'care for the data' (Fortun, n.d.; Walford, forthcoming). In the intervening years, more reading, discussions with the researchers who ran the project in Cambridge, ethnographic attendance at behavioural ecology talks and seminars, helped me fill out the picture. I came to understand the broader logics of what counts as an interesting question in behavioural ecology. I came to see also that while the more senior scientists involved with the KMP cared about the production of good, reliable data, this was encompassed by a broader teleology: answering specific questions. I was thus ready, in 2011, to see the theoretico-experimental side of the KMP, in a way that I was not in 2008.

Concomitantly, the sense of 'where' my fieldsite was, also shifted. While I had initially hoped to return quickly to the KMP for a much longer stint of fieldwork, the strictures of a new academic post and difficulties in securing funding meant that I was unable to do this. Over time, my 'monographic' ambitions faded, and I came to see the KMP rather as only one site in a broader constellation of a multi-sited research project which was more broadly about the biology of behaviour. While my fieldwork at the KMP was far from the archetype of classic, long-term immersive field-work in one locale which is still often

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invoked as a disciplinary ideal in anthropology, I nevertheless found support and comfort in an emergent counter-ideal of multi-sited ethnography. The anthropological fieldsite too, has been changing rather radically in the past two decades.

If field science exists, often ambivalently, in a world of labs and experiments, the anthropology of science was born in an anthropological world of long-term, immersive fieldwork in a particular place, and has contributed to change this world. Emily Martin was amongst the first to point to the tension between classic notions of fieldwork location and the peculiarly discontinuous, complex and transitory geographies of science (Martin 1998; Martin 1994). Her work and that of others, fed into the arguments for the value of 'multi-sited ethnography' articulated most famously by George Marcus (1995).

Marcus' proposal for multi-sited ethnography was written in a context when the traditional methodological resources of the discipline seemed on the point of being exhausted. Multi-sitedness, promised a new language of relevance and a new form of authority for ethnographic knowledge which would straddle both the intimate (the traditional province of the ethnographer) and the large-scale. Most crucially perhaps, multi-sitedness opened up the term 'site' to a range of meanings beyond that of a mere geographical location: a 'site' could be an archive, 'the media', or a geographically dispersed population of practitioners). The vigorous, reflexive reconsideration of ethnographic fieldwork practice which resulted (see, amongst others, Gupta and Ferguson 1997; Marcus 1999; Englund, Leach et al. 2000; Amit 2000; Hage 2005; Author 2007, 2010; Falzon 2009) left the archetype of the fieldsite as a single, neatly bounded location which correspond to a named human community or culture, thoroughly in tatters.

THE ANTHROPOLOGIST RETURNS

Nevertheless, upon finally managing to return for a second 'leg' to the KMP in 2011, I felt some trepidation. The ghost of Malinowskian long-term fieldwork came back to haunt me, perhaps because of the overdetermined definition of the KMP as, precisely 'a site' – surely, this was the community which I should be gaining a 'thick', contextual understanding of? Could one really claim this through fieldwork done in two relatively brief stints, three years apart? And whereas anthropologists often speak fondly of their return to a familiar fieldsite, what sort of 'return' could this be, when (given the yearly turn-around of volunteers) nearly all of the present residents of the field-site would by now be strangers I had never met before?

My first intimation that something had changed at the KMP came in a discussion with Al, the data manager, shortly after my arrival. I had assumed that I would, as upon my first visit, shadow experienced volunteers for the training period which new volunteers undergo upon arriving at the site. During my previous stay, this had provided invaluable first-hand experience of the process of field-data collection. This time, however, Al had to decline my request: the site was far too busy, scheduling was far too tight, for him to be able to spare an experienced volunteer for the time it would take to 'train' me again. My tagging along would have had a noticeable negative impact on research productivity.

Instead, Al offered to bring me along on less run-of-the-mill trips, such as those built around capturing meerkats for different purposes: to change the radio collars of adult meerkats; to take blood samples and measure the meerkats, under anaesthetic; to implant, into the back of pups' necks, the micro-chips which would stay with them for life; or, in one occasion, to euthanize a meerkat who had developed advanced signs of TB, which was endemic amongst some of the groups in the population. These were all practices which I had heard about during my previous trip, but had never witnessed – and neither had most volunteers. As a result, in 2011, I got a much more direct, first-

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hand sense of the extent to which even the basic observational work at the KMP was underpinned and enabled by some prior hands-on intervention: drawing blood, changing radio-collars, holding the meerkats firmly yet carefully until they passed into anaesthetic sleep.

Thus on the one hand, Al was channelling me towards activities in which, being off-rota and with no direct repercussion on data-gathering, my presence could cause no disturbance. On the other hand, these trips were rather more 'advanced' than most of what standard volunteers got to experience, and indexed to some extent a shift in my own persona as a returnee.

During my first visit, like most social anthropologists upon first arrival to an unfamiliar field-site, I had been a somewhat shadowy, ambivalent, and probably at times irritating guest. I didn't know anyone, was inexperienced and out of my depth, had to be told everything, both about data-gathering and about social living in a remote isolated site. Furthermore, the informal and protean methods of ethnographic research – 'hanging out', 'joining in', informal conversations and rambling recorded interviews – looked at best odd and unconvincing to a group of people whose main day to day work was also research, but research of a strenuously well-defined quantitative sort.

In 2011, I returned as a rather different persona. This time, I knew some of the short-hand references for practices and procedures, I understood the basics of data collection, I knew some of the histories and foibles of individual meerkats and groups. This time, I also knew two people locally, as I mentioned above, but just as importantly, I had in the intervening years met and interviewed researchers associated with the project in Cambridge. The KMP was part of a broader community of practice which reached back to Cambridge, where every volunteer had been interviewed before being cleared for the field, where some of them hoped to study in future, and where many independent researchers were based when not in the field. Last but not least, I had some sense of the proprieties of social interaction at the field-site, the subtle ways of letting people have their own space, and being available for social interaction without intrusion, or the somewhat labyrinthine written and unwritten rules of collective food consumption.

Of course, in none of these areas were my competences such that they could straightforwardly compare with those of an experienced volunteer. However, a crucial feature of the KMP social landscape which made me a far less uncanny presence during my second visit, was the pattern of volunteers eventually returning as independents, combined with the fact that any population of volunteers consisted of overlapping 'generations' including new arrivals, and old-timers who had been there for a year. This meant that it was a familiar occurrence at the KMP for an independent researcher to arrive whom only some of the older residents had met, but whom the others had heard about – and most of the volunteers at the time of my second visit had heard of the anthropologist who had written a (rather ludicrously-titled) paper about the site.

This in itself was significant. Having written a peer-reviewed article, while it clearly did not make me a 'scientist', was a locally recognised currency which made me a bona fide researcher. This meant that I could more comfortably be pigeonholed with the 'independents'. Most importantly perhaps, as a bona fide researcher, I fell categorically on the right side of an implicit but powerful distinction drawn locally between real researchers and various tourists, film-makers and other visitors. The line was drawn particularly strongly in relation to the Earthwatchers and 'Friends of the Kalahari' (or 'Friends' for short) who occasionally visited the site. The latter were often held up as negative exemplars amongst the volunteers for what were seen as their overly sentimental views towards meerkats, which were negatively contrasted to the volunteers' own striving for scientific detachment and self-control (Candea 2010). In retrospect, I came to see that some of the emphasis on detachment and inter-patience which had been

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so prominent in my experience in 2008 reflected the cautionary tone in which both new volunteers, and, more pointedly, Earthwatchers and Friends are admonished upon first arrival at the site. The meerkats might look 'tame', but they are merely 'habituated'. They are wild animals, not your pets! Etc... When I returned, it was assumed I understood these ground rules of (non-)interaction: people could thus take that as read and focus on other things – including the many ways in which, carefully and guardedly, interventions did in fact take place.

THE ANTHROPOLOGICAL FIELDSITE AS EXPERIMENTAL DEVICE

The fact that my initial concerns about returning turned out to be groundless could be used to vindicate the deeply-held conviction of pioneers of the anthropology of science, such as Emily Martin (Martin 1998) – later echoed by proponent of multi-sitedness in other fields – that anthropologists can study multi-sited entities, objects which are 'here and there' (Marcus 1995), without a radical loss of context. The moment when, upon returning to the fieldsite, I found that I was partly recognised and felt that I even 'fitted in' a little bit (but in to what?), was a moment in which the non-spatial continuity of something like a KMP 'worlding' (Tsing 2010) – a partially connected cloud of places, but also of names, of knowledges, of commitments, which are deployed by KMP actors to outline their own 'contexts' – became evident to me as it had never before.

But then again, the very brevity and intermittence of my presence at the KMP, backed up by the kind of multi-sited fieldwork I have described above, was precisely what brought home to me something about the gaps which are compatible with this sense of context and wholeness. I am echoing here a point made by Stefan Helmreich concerning his dive alongside oceanographers in the *Alvin* module: since many scientists themselves only get one dive in the *Alvin*, there is little point in asking whether the dive described by Helmreich is sociologically representative. But this very question of representativeness, which "bedevil[s] both anthropology and oceanography" is precisely what echoes across the practices of Helmreich and those he studies (2009:226).

My own 'dive' into the KMP has helped me see, in the way that continuous residence might not have done, the fact that the KMP world is precisely a world of discontinuous co-presence, of comings and goings, of friends-of-friends, of people one has heard of suddenly materialising with their own slightly different take on the local terminology or the feel of the place. It is a 'world' which self-consciously extends to the hinterland of Cambridge, but while for some this is another home for some, for others it is a distant prospect, and for others still, a partially known centre of power whose agendas and teleologies shape their day-to-day.

It is a world whose temporality is marked by the constant periodicity of the monthly trips to the airport in Uppington, each trip promising slightly ambivalent arrivals (new volunteers, always a bit of an unknown quantity, but also film-makers, eco-tourists, etc.), of departures, some unnoticed ('have the film crew left?'), some emotionally charged (as when one's friend of nearly a year, or indeed someone who has become one's boyfriend or girlfriend, leaves the Kalahari for good), a world also of unexpected returns. It is a world whose sociality is woven ambivalently out of these different temporalities. It is thus common for volunteers to be closest to people who have arrived in the months shortly before and after them, and to have a quasi-sibling-like relationship with people who came in on the same trip. Volunteers who are nearing the end of their stay, on the other hand, are usually less keen to commit to building new friendships and to getting to know new arrivals whom they will only overlap with for a month or so. The group of volunteers as a whole is thus internally traversed by complex patterns of closeness and distance –and that's before we add the extra complexity of volunteers' relations to the

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site managers (often as we have seen, themselves former volunteers), to the independent researchers, and to the far more transitory population of film crews and 'Earthwatchers'.

Multi-sitedness calls for the reconfiguration of the ethnographic fieldsite not just in terms of space, but also in terms of time. 'How short can fieldwork be?' (Marcus and Okely 2007) remains a highly provocative question in the context of an ongoing disciplinary ideal of 'thickness' achieved through long-term 'immersion' into a specific context. But increasingly, the recognition that if there is 'immersion', it is into a 'worlding' which is often itself multi-sited means that short times spent in different places can be seen to add up in complex ways.

Good anthropology will always take time. Yet, I can see no reason for concluding that the time it takes must in every case be spent in its bulk in a physical field site . . . The ethical profile of the good anthropologist, in short, yields no methodological a priori concerning the appropriate duration of a project. Everything hinges on the terms and requirement of the question of research itself. (Faubion 2007, cited in Marcus & Okely 2007)

In my own case, the research which informs this paper has taken three years, even though the majority of that time has been spent in the UK, in seminars, in libraries, in interviews – and non-negligibly, working in an anthropology department roughly half of whose staff are themselves trained in animal behaviour, ecology, zoology, or primatology, and who count the KMP researchers as colleagues. These different, partially connected sources of insight all feed into the above account.

Critics have expressed concern that multi-sitedness, by spreading the ethnographer too thinly across space, jeopardises anthropology's commitment to depth and thick description (Pedelty and Hannerz 2004), or more pointedly, that multi-sited anthropology's new roaming aspirations might undermine its regard for subjects' own understandings of the importance of context and emplacement (Englund, Leach et al. 2000).

One could reply, perhaps, that thickness is not so much lost as it is reconfigured/reconfigured (Falzon 2009; Horst 2009): where traditional Malinowskian fieldwork achieves thickness through the slow, continuous adding-up of time spent in one place, multi-sited fieldwork can produce a different kind of thickness which comes of the layering of partly incommensurable experiences in different places through time, and tracing the connections and disjunctions between them. But then again, that contrast is rather lessened when one turns up the magnification on what traditional single-sited ethnography actually involves. My previous doctoral work on identity and belonging in Corsica might seem in contrast to what I am describing in this paper, to have been classically single-sited, focused as it was mainly in and around a village in the north of this Mediterranean island. And yet, as I have written elsewhere (Candea 2007), my research then was just as multi-sited as my research now. Indeed, far from playing the role of a neatly bounded face-to-face community, the village contained its own multiplicities and partial connections. Fieldwork in that small place was already inherently multi-sited: every encounter, every story, spoke of a different vector, a different temporal and spatial direction. Depth, once again, came partly from a layering of these different moments.

Note that I am not for a second disputing the fact that spending a year at the KMP would have produced different kinds of insight into many aspects of this particular place and the experience of its volunteers. But what is at stake here is a shift in emphasis, from the question of sheer quantity (How long? How much? How many? How far 'in' did you get? How 'representative' was your experience?) to viewing the fieldsite as a device for experimenting with time and space – and more precisely, as a device for the echolocation

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(cf. Jean-Klein and Riles 2005) of others' temporal and spatial concerns. For instance, Helmreich notes that the ethnographer's increasingly partial, fragmentary, technologically mediated, multi-sited and multimodal presence in 'the field' echoes the changing nature of oceanographic fieldwork too (2009:233). But equally, insight can come from the disjunctures, rather than the echoes, between the field practices of the anthropologist and those she studies.

Either way, the shift from sheer quantity to a more experimental approach in which "[e]verything hinges on the terms and requirement of the question of research itself" (Faubion 2007) is reminiscent of the shift I have described for the KMP and which Amanda Rees has described for primatology. Is this a mere play on words (after all 'experimental' can mean many things...), or does it suggest some deeper connections between what has been happening to biological and ethnographic fieldwork?

CONCLUSION

In conclusion, let me try to parse these interwoven discussions of the fieldsite in biology and anthropology, through the editors' notion of the device. What do these two kinds of fieldsite have in common, beyond their remote joint origin in 19th century scientific exploration (Kuklick 2011)? Can they even both be described as 'devices' in the sense outlined by the editors of this special issue – as 'patterned teleological arrangements'? That might sound like a fairly unchallenging claim, since the editors themselves suggest in the introduction that nearly everything of human interest could perhaps be seen as a device. And yet there are deeply ingrained methodological reasons for balking at the description of anthropological field-sites, at least, as either 'patterned' or 'teleological'. Let us take these two characteristics in turn.

Patterned arrangements, first. The KMP is without doubt a patterned arrangement, even though its pattern shifts through time. It holds together and even reproduces itself, despite its material heterogeneity and the many internal differences it encompasses. Keeping it alive involves a constant process of re-configuration, tinkering, and invention, and yet it holds its shape enough to be recognisable from year to year, even when most of its components have changed.

What about my fieldsite? If one defines it strictly as the KMP, then the question is answered in the above paragraph: my fieldsite is a patterned arrangement because theirs is, since my fieldsite is 'parasitic' upon theirs: the KMP is there to return to. But as we have seen, this would be a problematically literalist interpretation of sitedness. The problem was succinctly phrased by Michael Herzfeld: "The term 'multi-sited ethnography' [...] suffers from the same oversimplification of the notion of fieldwork *location* as does the term 'globalization'. When are sites separate, different, or otherwise distinguishable?" (Herzfeld 2004:216, n258). If multi-sitedness is understood as the threading together of distinct sites, this brackets the crucial question of what if anything makes something *a* site in the first place. The question can be posed temporally: is the KMP I visited in 2008 the same site as the KMP I visited in 2011? From the point of view of the researchers, certainly. From mine, however, these two could rather logically be seen as two sites in tension. "What one field-site allows us to affirm, another field-site can contradict..." (Stengers 2000).

The question can also be posed spatially (cf. Hage 2005). The KMP researchers have established a friendly relationship with the owners of surrounding farms, who allow them to range over their land in pursuit of the meerkats. In the volunteers' day-to-day discussions of place, however, the land surrounding the research site is not mapped out primarily in terms of boundaries of ownership, but in terms of ecological spaces and home ranges. If a meerkat crosses a fence, so does the volunteer who is following it. Few

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if any of the latter, however, have visited the neighbouring farmhouses, and their relations with farmers and their workers are scant. But should the KMP as *ethnographic* site map the same self-contained entity (the one which is primarily meaningful in terms of a KMP 'worlding'), or should it stretch out to the neighbouring farmhouses, to encompass questions of landownership, race or politics?

To most social anthropologists, the answer might seem to be a no-brainer – but it actually speaks to a profound tension at the heart of the ethnographic endeavour, between mapping the contours of the world one's informants inhabit, and analysing or contextualising their experience by including elements which informants themselves might consider to be outside the story – the type of tension Tsing seeks to capture through her use of the notion of 'worlding' (2010). In sum, an ethnographic 'fieldsite' emerges at the intersection of the localising processes of the people studied, and of the interests, decisions and commitments of the anthropologist. If the fieldsite is a patterned arrangement, its pattern is a negotiated outcome of this process.

This is not such a bad description too of a biological fieldsite, come to think of it: the field is wherever the meerkats roam – within reason. There is a boundary beyond which the volunteers will not stray, and meerkats which move too far out simply drop out of the population. But it is significant that when referring to places in the surrounding landscape, the volunteers mostly refer to them by the names of meerkat groups they are going to visit ("where are you going today? I'm going to Commandos..."). On a larger scale, the KMP emerges at the intersection of the interests and ecologies of meerkats, and the interests and conceptual worlds of the researchers.

The difference then, is perhaps primarily one of emphasis. Most contemporary discussions of sitedness in anthropology have put the focus on the former aspect: the discovery of the inherent contours of an object of study through a flexible, open-ended approach to location. Pattern and arrangement are out there in the world, this emphasis implies, and our job is to find them. By contrast, with their focus on painstaking research design and clearly motivated selection of places and species, biologists give a more explicit account of the fact that the pattern and arrangement of the field is first and foremost a feature of their own interests.

Which brings us neatly on to teleology. The KMP is a large-scale, heterogeneous, yet strongly patterned and single-mindedly purposeful device for behavioural research. Of course, as we have seen, the KMP is also many other things: a training camp for budding animal behaviour scientists and conservationists, an unparalleled location for film-makers to get close to habituated meerkats, a place for eco-tourists to try their hand at field science and meet the furry stars of their favourite tv shows. But the site is administered and centrally coordinated by Al and the other managers, to hierarchise these different aims. As field-based natural history gives way to theoretico-experimental science, the fieldsite becomes a place which is there first and foremost to answer the questions which the researchers want answered.

By contrast, one of the most enduring methodological injunctions of anthropological fieldworkers, one which has survived the many transformations of the Malinowskian archetype of fieldwork, is precisely to suspend one's own goal-directedness in order to register the aims and purposes of others (Kuklick 2011). Indeed, the Malinowskian principle that ethnography involves "using one's whole personal life as a scientific instrument" (Thornton and Skalník 1993:26 cited in Kuklick 2011:23) grounds both the sense of the discipline's methodological distinctiveness amongst other social sciences, and the explicit commitment to open-endedness in research 'design'. As Jean-Klein and Riles wrote: "[i]f the ethnographer willingly serves as a kind of tool, she is a tool for the 'echolocation' of knowledge (Wagner 2000), for allowing others to practice their knowledge on and through her." (Jean-Klein and Riles 2005:186).

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There is again a partial overlap here. Radick notes that as biologists take their experiments to the field, the nature and aims of these experiments change: field experiments take researchers into the animals' world to find out what matters to them (2007:256, 364). And this shift is key to certain field experiments' ability to achieve an "unbeatable combination of moral and epistemic authority: the authority of experiment over observation or speculation, and the authority of nature over artifice" (ibid, 368).

Yet the key difference remains that the value of these field experiments is still measured in terms of the questions it can answer. For anthropologists, as for biologists, the mark of successful fieldwork is usually that one returns with different questions to the ones one had set off with. However, for anthropologists, this is not (or very rarely) because our questions have been answered, but usually rather because the field has somehow shown them to have been badly put. A good fieldsite device in anthropology is thus often one which succeeds in redirecting, rather than straightforwardly fulfilling, the ethnographers' aims, interests and purposes. That is, precisely, the strangely self-suspending teleology of ethnographic fieldwork: the fieldsite is a device for producing the unexpected. Which – unexpectedly – is perhaps precisely what makes anthropology, in its own way, *experimental* (cf. Rheinberger 1997).

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