

LSE ANTHROPOLOGY

PROVOCATIONS #1

Radicalising Anthropological Research

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Preamble: From ‘innovate methods’ to ‘radical research’

After accepting an invitation to write a position paper on ‘innovative research methods’, I realised that this was drawing me into a game I did not want to play. The ‘innovation’ framing too easily leads to a focus on the latest, newest, and ‘most advanced’. Seen that way, I do not think that our department will ever be recognised as the most ‘innovative’ centre of anthropological research, and frankly we may not want to pursue such a dubious moniker. Instead, I propose to exploit what I understand to be our actual strengths: to pursue classical anthropological research, doing so by building on the explicit *and* implicit strengths of ancestors, while radicalising its potential in a world that is very different from that of a century ago. In fact, this line of thinking surfaced not long ago when Laura Bear launched the ‘first book competition’ of the LSE Monographs on Social Anthropology (in 2015), stating in the accompanying blurb that ‘The competition seeks to reward monographs that use ethnography’s radical commitment to a nuanced empiricism to explore central aspects of human experience’.

The term ‘radical’ surely has its own issues. Calling our research (or ourselves) ‘radical’ may sound pretentious, perhaps unduly political. But I suggest this is outweighed by several interrelated reasons for discussing ‘radical’ or even ‘radicalising’ research practices. For a start, dictionaries define ‘radical’ not only as ‘extreme’ and ‘far-reaching’, but also as ‘thorough’ and as relating to ‘the root or roots’ and as ‘going to the root or origin’ of that which is of concern (Oxford English Dictionary).¹ Applying ‘radical’ to anthropological research, it is profitable to think of ‘radical commitment’ to the *potential* of our roots. Much may be gained by pursuing ‘radical’ (as in far-reaching) interpretations of original agendas, doing so by adapting them to the world we live in today. This is not just of academic interest; it may also have pragmatic relevance for staying competitive in the academic environment.

¹ I thank Ikenna Acholonu for pointing out the irony of quoting the Oxford English Dictionary at the start of this paper, an irony which of course highlights the double meaning of ‘radical’.

This is not to deny the value of crossovers with quantitative and experimental sciences, or with say literary studies, but it does suggest that these are not radical in the sense implied here. 'Radicalising' is a way to stay true to original potentials, while considering that these radicalisations may well be transformative. In this position paper I do not have the pretension to satisfactorily pinpoint all the forms this will take. I merely attempt to sketch some starting points for what I hope will be fruitful debate. But before getting there, a brief historical excursion is vital.

Radical Roots

One of the curious things about our department is that it managed to produce the single most influential methodological text in the entire history of anthropology (this, obviously, being Malinowski's 'Method and Scope of Anthropological Fieldwork' (1922)), only to remain virtually silent on methods in the many decades thereafter. Apart from one staff member having co-written a methods handbook for development workers (Pratt and Loizos 1992) and a current member of staff having published valuable reflections on her fieldwork experiences while employed elsewhere (Gardner 1999), it was only in the 2010s that publications from *within* the department directly addressed key issues related to anthropological methods (Astuti 2017; Shah 2017).² It is telling that even these latter contributions were responses to prompts (specifically, Ingold 2014), rather than having emerged organically. And ironically, the last time the department collectively embarked on a project with broad methodological potential (our research afternoon on Critical Comparisons), the resulting edited volume *How People Compare* focused on analytical and empirical (ethnographic) aspects of comparing, but by and large ignored methodological implications for our discipline (Pelkmans and Walker 2023).

There appear to be several intersecting reasons for the relative paucity of writings on methods in our department. I think it is fair to say that the department's emphasis on empirical findings combined with the prestige of theory in the discipline have prevented methods from reaching the top of our agendas. In fact, until about fifteen years ago, a commonly held opinion in the department was that methods – including teaching them – could be left to non-Russell universities; it was not a subject that elite institutions should bother themselves with. Equally relevant is 'the relative homogeneity of the tradition of fieldwork' within British social anthropology, deriving its 'practice and approach from

² I should emphasise that I did *not* carry out a systematic search of methodological publications by members of the department. I thank colleagues for reminding me of some additional contributions, though these did not refute that the department has had a relatively low profile in methodological debates for most of its history.

a common ancestry', with the result that 'differences in fieldwork method are not marked' (Watson 1999: 17) and are left unelaborated. Perhaps it is also fair to say that the 'Malinowskian methodological revolution in ethnography' (Roldán 1995: 144) was simply ahead of its time. Not only is the text still being taught to new generations of students, but several of its principles continue to be referred to as having radical, even revolutionary, potential (Shah 2017). Helpful for keeping Malinowski's agenda alive has been the fact that his aspirational vision could never be fully realised, including by himself. As Jonathan Spencer put it at the end of a discussion on imagination and uncertainty in studying the political: 'The potential of Malinowski's obsessional empiricism is as radical (and impossible) as ever' (1997: 15).

'Radical potentials' offer useful aspirational guidance, but instead of straightforwardly embracing ancestral lessons, I suggest these should be considered in view of ongoing conversations within the discipline. To do so productively, I briefly reflect on what the 'original agendas' were about, to then consider how the associated values developed over time, and how they translate to the 'radically' changed world in which we carry out our research projects. I start each section by juxtaposing an old and a more recent statement pertaining to the methodological issue under review, hoping that this will provide a sense of how to chart directions of possibility for 'radical' anthropological research.

Imaginative Encounters

Imagine yourself ... alone on a tropical beach [with] nothing to do, but to start at once on your ethnographic work. ... with nothing to guide you and no one to help you (Malinowski 1922: 3).

[W]e need to move outside the zone of 'being-in-the-world' of fieldwork ... and into the 'being-in-the-armchair' of speculation (Willerslev 2011: 518).

The classic image is crudely clear. It presents the anthropologist as awkwardly alone in the field. Well-known examples include Hortense Powdermaker's description of panic when she found herself 'alone in a native village' (1966: 51, 58), Evans-Pritchard's sense of frustration or 'nuerosis' (1940), and Margaret Mead's insistence that one had 'to dive into the field and never come up for air until it's all over' (1977). Such accounts certainly contributed to the image of the anthropologist who stuck it out by heroically overcoming difficulties, doing so all alone, driven by determination.³ But once we shed

³ I guess that we should not be surprised that Willerslev's armchair anthropologist comes across as an equally lonely figure, even if not a particularly heroic one (2011).

this unhelpful mythologised image, what remains is the notion that 'being there' matters, that it generates potential for understanding the world in unanticipated ways (e.g. Watson 1999).

Presumably 'being alone' or even 'being there' merely provides conditions of possibility, with the potential for new understandings still needing to be ignited, for example by the disorientation and confusion that inhere in encounters with difference. As Seymour-Smith puts it, a 'state of disorientation is perhaps necessary, and is in the long run a productive one, since like a rite of passage it prepares the ethnographer for the imaginative leap involved in coming to terms with an alien culture or way of life' (1986: 117). Disorienting encounters, when approached with the requisite 'openness', allow us to question, reveal, and suspend our own subjective and sensory knowledge, even if only imperfectly. This potential has been usefully highlighted by Malkki, who suggests that the 'capacity to be surprised indexes a profoundly important methodological possibility, one that requires imagination' (2008: 175, 182). The point of imagination is key, but it is not necessarily obvious what the sources of imagination are.

Rane Willerslev may have been on to something when he suggested that 'anthropological insight ... begins not from what we conventionally regard as the solidity of actual empirical observation' (2011: 505). But he takes this in a curious direction when stating that 'on the contrary,' this insight stems 'from the scholar's speculative imagination'. While we could certainly acknowledge that empirical observation is never 'solid' or analytically generative in and of itself, we might still want to maintain that speculations are only anthropologically productive when they connect with the lived reality in which we do fieldwork. Imagination, after all, cannot be entirely manufactured from within, but is always dependent on connections with the outside world. Relevant here is Mittermaier's work on dreaming. Critically observing that in Western academia dreams tend to be understood as the product of the unconscious, she suggests that even if only indirectly, 'dreams ... always come from an elsewhere' (2012: 260). Analogously, and without minimising our own 'dreamwork', we could postulate that imagination arises in the middle, between anthropologist, the writings they engage with, and their fieldwork encounters.

If we accept that 'anthropological imagination' is situated in the middle, then it might prove difficult to radicalise its potential, given that pushing in one direction will likely pull imagination out of the middle. Still, it might be useful to reflect on the various directions. Willerslev's armchair suggests that what matters to him is not just *any* pondering, but rather that concrete encounters with written sources trigger speculative imagination. It is a valuable point, one that we are all familiar with, even those who enjoyed the perhaps unfair remark that 'American anthropologists think they can solve any problem by re-arranging their reading list'. I personally like armchairs, but it also appears to me that

encounters with literature are most generative when they are brought in conversation with the reality of fieldwork (even if this conversation takes place after the fact in an armchair). Especially illuminating in this regard is the recent volume *Philosophy on Fieldwork* in which twenty-seven authors take their favourite philosopher on a fieldtrip to see how philosophies can be mobilized in analysing concrete ethnographic cases (Bubandt and Wentzer 2022: 2).

Pushing in another direction, we could attempt to expand our own imaginative capacity, our embodied ability to make 'imaginative' connections and inferences. We may not need to do a 'Carlos Castaneda' and use stimulants to hallucinate the teachings of our interlocutors (1969) (though perhaps someone should do this, properly this time!), while still acknowledge that we all have techniques for stimulating our brain. Thinking instead of our imaginative work as a form of 'abductive reasoning' (sometimes happening in hazy states of consciousness), suggests that our common fieldwork practice of trying out, of considering multiple links and connections, requires an open research process (for a discussion see Timmermans and Tavory 2012). Hence, there continues to be tremendous value in resisting any artificial separation between data collection and data analysis, to embrace our messy engagement with field materials throughout the research process.

None of these reflections are particularly novel. Perhaps, then, we have all along been involved in a radical balancing act? Still, let me add some thoughts on the unsettled and unsettling characteristics of fieldwork encounters, and their generative potential. To allow ourselves to be unsettled requires openness, which for Paul Stoller is an attitude that can be cultivated, requiring ethnographers to 'open themselves up to others and absorb their worlds' (cited in Pink 2009:64). Musante and Dewalt point out that while we 'should be consciously aware of observing all the time ... sometimes it is best to just sit back and experience' (2010: 92), to thereby allow our peripheral vision (or other sensory perception) to generate insights that were not prefigured. In a video on ethnographic epiphanies, Alpa Shah, Hans Steinmüller, and Harry Walker reflect on how it was in unexpected moments that they came to realise unanticipated aspects of labour migration, political complicity, and debt peonage.⁴ Such chance encounters can perhaps not easily be provoked, and yet we may think of fieldwork as a form of 'structured serendipity' (Berdahl 1999) that activates the potential of such encounters to emerge. This may be especially useful in contexts where 'difference' hides below the surface of familiarity. As a technique for 'de-familiarisation' it pushes us to take on multiple roles and to switch between positions, to thereby offer new perspectives on familiar issues.

⁴ The video features in the course AN486 Research Methods in Anthropology (Moodle, week 7).

No matter how they are prompted, our surprises, confusions, wonderings, and hallucinations still need to be anchored in fieldwork contexts to ensure that they are more than fleeting, ephemeral moments. If we agree that one of the central aims of anthropological research is to ‘overcome prejudices and unexamined assumptions while simultaneously taking alternative realities seriously’ (Kapferer 2001: 342), then we need to think about our various engagements with those alternative realities. What forms this takes is the subject of the next section.

Engagements against objectification

I did not ask questions about attitudes towards white people. These were given spontaneously, as informants talked about working for them and about their other contacts (Powdermaker 1966: 157)

[T]o observe is not to objectify; it is to attend to persons and things, to learn from them, and to follow in precept and practice (Ingold 2014: 388).

In his discussion of ethnographic authority, James Clifford points out that in many classical ethnographies, such as Geertz’s famous essay, ‘we are seldom made aware of the fact that an essential part of the cockfight’s construction as a text is dialogical’ (1983: 132-3). He continues by pointing out that this dialogical quality is too often erased in the process of ethnographic writing. At most this quality used to be presented in separate fieldwork reflections. A good example of such separately published fieldwork reflections is Powdermaker’s *Stranger and Friend* (1966), which shows that her most penetrating insights were based on informal and embodied conversations, such as during a car ride with a black male interlocutor while being watched by a flabbergasted white audience, triggering largely non-verbal conversations that focalised and revealed the reality of racial division and violence in 1930s Mississippi (1966). Anthropology may have come a long way since Clifford made his critique – most published ethnographies no longer present generalised, impersonal truths. Still, there may be room for optimising or indeed radicalising the dialogical quality of anthropological fieldwork, thinking of this as linked to collaborative processes of attending, learning, and relating.

Extending this line of thought, and moving beyond dialogue, we find Tim Ingold’s vision of participant observation as a ‘*practice of education*’ in which the anthropologist is called upon to ‘attend to what others are doing or saying and to what is going on around and about; to follow along where others go and to do their bidding, whatever this might entail and wherever it might take you’ (2014: 389). Taking this lead serious has ‘radical potential’ not only in generating penetrating insights into issues at hand, but also in fostering collaborations with research participants to potentially overcome

problems of objectification and othering (see also Fabian, especially 1996). As is the case with dialogue, a practice of education is potentially 'democratic' in that it approaches interlocutors as equals, engaging with them on their terms.

This democratic or equalising potential has been actively pursued in what can be referred to as 'participatory action research' (Hemment 2007), in 'decolonial strategies' (Bejarino et al 2019), and can be found in active forms of participation (sometimes confusingly referred to as 'auto-ethnography' (Hine 2015)), in the 'go-along method' (Kusenbach 2003), as well and in crossovers between anthropology and psychoanalysis.⁵ Without pretending to do justice to these various initiatives, let me offer a few examples. In a video on 'participatory methods' (AN486 Moodle week 8), Catherine Allerton reflects on her activities with children – designed to engage with them as full human beings. By asking children to make drawing titled 'the road of my life' and to take 'photos of my life', entry points were created that allowed for productive dialogue. In the same video, Mayanka Mukherjee discusses how she collaborated with artists in her research on housing and emptiness in Chelsea, not only to address aspects of gentrification not usually captured, but as a way of making research more democratic. While making the research process more mutual, it also generated new challenges, including how to ensure that participants were recompensated for their efforts. After all, if implemented improperly, participatory research would boil down to asking others to do (part of) the work *for you*, resulting in new forms of extraction and exploitation.

Beyond mutuality, efforts to erase the line between researcher and researched emphasise the ability to produce embedded and embodied forms of understanding that are rooted in everyday experience (Hine 2015: 115). They also claim to be able to relate what people say with what they do in a more organic way than research practices that separate observation from conversation.⁶ Moreover, they link in with analytical techniques that inhere in our interlocutors' meaning-making practices 'that are not predominantly focused upon (academic) research', often done so with a view on how activist research may facilitate change and transformation (Hui 2023: 1080).

Overcoming otherness, collaborating with interlocutors, and democratising our research practices is essential, and yet we need to do so without abandoning our own 'dissenting position' (Shah 2020). Identifying completely with others (in research as elsewhere) inhibits attending to the plurality that inheres in all groupings. It could, as Ortner (1995) points out in her discussion of resistance,

⁵ Relevant here are the analogies between anthropology and psychoanalysis: using similar techniques of listening and being the only academic fields in which line between observer and observed is (partly) dissolved.

⁶ For example, in the go-along technique, 'ethnographers are able to observe their informants' spatial practices in situ while accessing their experiences and interpretations at the same time' (Kusenbach 2003: 463).

amount to a form of 'ethnographic refusal' that fails to understand the complex ways in which people engage with dominant structures. Here it is useful to link back to Clifford's emphasis on dialogue, not just between researcher and interlocutor, but to acknowledge with Bakhtin that 'dialogical processes proliferate in any complexly represented discursive space' (Clifford 1986: 15). Collaboration may turn out to be a double-edged sword in another sense as well: when applied not to marginalised people, but say missionaries, militaries, policymakers and other such ethnographic subjects, collaboration acquires a very different taste.

Unstable Anchoring

The ethnographer has in the field ... the duty before him of drawing up all the rules and regularities of tribal life; all that is permanent and fixed; of giving an anatomy of their culture, of depicting the constitution of their society. (Malinowski 1922: 11).

Never mind explanation and interpretation The most basic task for anthropology must be conceptualization ... anthropology, if you like, as painting by concepts (Holbraad 2020: 497).

This section briefly addresses the scientific ambitions of early anthropology. Not, of course, in the way envisioned back in the 1920s, when anthropologists such as Malinowski and Boas were enamoured by the promises of positivistic science. Consider Malinowski's advice to draw up 'all the rules and regularities of tribal life; all that is permanent and fixed' (1922: 11). Such a diction hardly leaves room for uncertainty, irregularity, ambiguity, and chaos, aspects that nowadays even natural scientists would emphasize. The challenge would be to find ways to think of systematising parts of our research in ways that captures the instability of 'rules and regularities'.

A useful counterpoint might be Martin Holbraad, when he suggests that anthropology is akin to the practice of art, its strength lying in 'painting by concepts'. His art analogy looks primarily at conceptualisation as an imaginative practice and creative form of expression (2020: 519), but we might extend it by thinking of painting as a *craft*, involving skilful techniques. Let's not forget that we are able to paint creatively with concepts only because these become destabilised in fieldwork encounters. We might even want to systematize this potential by, say, drawing on elicitation techniques to tease out the nuances, dimensions, contradictions, and connotations that concepts and categories have in lived experience (see for example Longfield 2004; Werner 2000). It may help to destabilise categories and concepts, to tease out their shifting properties, and better understand their epistemic, moral, social, and political lives.

Even though anthropology's aspirational horizons may have shifted, producing a chasm between drawing up rules on the one hand and artsy brushstrokes on the other, it is still all about categorization and conceptualisation. This is relevant since one of our primary interests is to tease out the nuances, contradictions, and ambiguities in how people (including anthropologists) engage with and make sense of the world. Could we move forward by drawing on Malinowski to experiment with how to tease out rules and regularities, and on Holbraad to destabilise and re-imagine concepts? The resulting scientific-art product could show how lines and contours come together with shades and smudges.

The Site Multiple

The events at Malungwana bridge ... must be related to a system at least part of which consists of Zulu-European relations (Gluckman 1940: 11).

To limit ourselves to arbitrary locations, geographic or otherwise ... gives us something to strive against, a locus whose incompleteness and contingency provide a counterpoint from which to challenge the imagined totality of [cultural formations] (Candea 2007: 180)

Few anthropologists will contest the importance of studying small places to address large issues (to paraphrase a bestselling textbook). Indeed, this is at the heart of a Matei Candea's article in defence of the bounded field site (Candea 2007). In it, the author argues that a village is complex enough as it is, in other words, that almost by definition it includes multiple field sites. No disagreement here, but perhaps the article also reveals a limited imagination in conceptualising the 'field site'.⁷ I contend that the issue is not, ultimately, about single versus multiple sites but rather about constructing field sites in such ways that coherence is ensured – it is about connecting the multiple with the singular, which can be done geographically as well as relationally, materially, temporally, or conceptually.

In fact, one could easily argue that Malinowski was an early adopter of the multi-sited approach when he embarked on his study of the *kula* ring, travelling with the argonauts from place to place, placing their journeys within the single 'site' of *kula* (as a practice of exchange) as it moved from island to island, affecting the people through which *kula* moved, all the while being transformed through movement. Another classic will do equally well: in Max Gluckman's 'situational analysis' the locality referred to as 'the bridge' was not so much a place as a 'point of interception' and 'entrance

⁷ Candea (2007) acknowledges that locations can be 'otherwise' than geographic, but leaves this largely unexplored.

point' for studying social relationships. That is, the connections constituted the 'site' to be studied – a site that should be conceived relationally rather than geographically. Indeed, starting with a tiny place (the bridge) allowed him to resist the 'boundedness' of anthropological research to instead offer an extended analysis – without pursuing the problematic ideal of 'completeness'.

My point is that the integrity of a research project can be preserved even when spatial bounding is let go. Although Candea's intervention got off at the wrong exit ramp, his search for 'methodological asceticism' still points in a valuable direction. As he puts it, 'the best way to think about and participate in a complex world was precisely to define self-imposed limitations, to look for some methodological asceticism' (2007: 174-75). Perhaps more so than the text, his visual imagery drives the point home. Contrasting two cinematographers, he invokes Peter Jackson's rendering of *Lord of the Rings*, which recreates an entire world with the help of thousands of actors, computer animation, special effects, and the skilled work of hundreds of technicians. As seductive as the film may be, anthropologists would be advised to instead compare themselves to Lars von Trier, who in his early days produced minimalist films such as *Dogville* that were based on Brecht's epic theatre in which 'a play never needs to be more complicated than a man on a street corner re-enacting an accident for a crowd'. The features of improvisation and limited means while focusing on the essence, offers a vision of what 'self-imposed limitations' can look like in anthropological practice.

It will always remain valuable to study small geographic places – squares, schools, pilgrim sites – but 'methodological asceticism' may equally be attained by thinking of fluid sites that are moving, that flash up, extend through time, or that fold into others. This, of course, is something that many of us do. For example, one of my PhD students started her fieldwork inside a refrigerator, using this as a vantage point to explore how people relate to food, and how food relates to a good life (Tongyue Zhu). Another example is a former student whose dispersed yet coherently interlinked field site was the 'assembly line' in which illegality came to be produced (Andersson 2014). If a single site always contains multiple sites, then the challenge is to see how multi-sited fieldwork can be imagined and construed to form a single site in at least one dimension (be it spatial, virtual, temporal, relational, or conceptual).

Following these lines of thought, let me offer one 'extreme' example. In an article inspired by participatory pedagogy, Arjun Shankar (2019) starts with a video produced by one of his students. It features mostly the bottoms of walking goats, but this is overlaid with a sugary Bollywood love song. Although initially making little sense to the author, this video scene exploded the act of goat-herding, not only in demonstrating that the soundscape of goat-herding have changed with the availability of mobile sound devices, but also that goat-herding itself is not confined to walking dusty paths, but

extends into the romantic (or otherwise) desires that are part of menial tasks. By tracking the goat herder and the music they listen to, the reader follows lines of flight into different dimensions of human existence, as well as the sociopolitical and mediatized landscapes in which it is embedded (2019: 239). The shape of this field site may not be directly obvious, but it chimes in with Burrell's work on digital ethnography, which engages with the field by seeking entry points rather than to define one-dimensional sites; to follow stories and intercept them; to see the field site as a network that incorporates physical, virtual, and imagined spaces (Burrell 2009).

Given the proposed fluidity – and the conceptual stretching involved – one might ask why retain the notion of 'field site' at all? My reason is that 'field site' combines the boundlessness of 'field' with the concreteness of 'site'. And importantly, while field sites are ultimately constructs, as sites they are spaces of co-occurrence where life happens in all its messiness, which allow for adopting varying positions and vantage points from which to address the issue at hand. As such, the field site offers a bulwark against one-dimensionality or flatness. One-dimensional flatness is offered by other scholars, whether they doggedly mine their databases to find patterns, or carry out experiments to test for specific correlations (scholars without sites are either flat or unhinged). By contrast, a site can always be approached from multiple angles, allowing for combining different data collection techniques to engage with its multiple denizens and do justice to the multidimensionality of existence.

Radicalisations?

Radicalisations that draw on our roots can offer means for revitalising our research, but potentially also for regaining a confident voice in discussions with other disciplines, and with funding bodies. Instead of allowing ourselves to be pushed to the margins – as providers of illustrative 'case studies' or of qualitative flavour – we should aim at recentring methodological debates, emphasising that we deliver results that no other discipline can, in ways that are deeply entangled with people's lives, and do so by taking ethics beyond formal bureaucratic procedures.

This is not a fetishisation of fieldwork *per se*. While close-up engagements with interlocutors will continue to be the hallmark of anthropological research, there is no need to stubbornly insist that fieldwork should *always* be long-term (even if for PhD students it probably should) or that research is limited to a spatial field (as traditionally conceived). Rather, we thrive in messy encounters that enable us to learn from interlocutors, collaborating with them to unearth unseen and overlooked aspects of human existence. Embracing a dissenting position that queries and teases out contradictions,

ambivalences, and hidden diversities deepens understandings of complexity, but with the aim of using our penetrating methodology to get to the heart of large issues.

While there is tremendous scope for experimentation, foregrounding radical potentials suggests that not all experiments are equally promising, be it because their neglect of our 'roots' unhinges them from lived reality or because they end up being one-dimensional. Auto-ethnography can be powerful when taking the 'ethno-' (as in 'social') part serious, but narrow applications may end up revealing little beyond the personal experiences of the author (Pink 2009). There is value in thinking of participant observation as a learning device, but this remains too limited if the learning remains stuck in the classroom, or largely takes place on the couch as in *some* psychoanalysis informed approaches (e.g. Hollan 2017). Making use of specialised techniques can probe and deepen insight into key aspects of human experience, but this strength is lost when it gives in to the pull of formalisation and quantification (such as Ryan and Bernard 2006). Imagination is central to our interpretive and analytic world but should not remain stuck in the armchair (pace Willerslev 2011).

Ultimately, the messiness of anthropological research should never be traded away. New insights emerge through friction, enabling us to be 'surprised, of reaching knowledge not prefigured in one's starting paradigm' (Willis in Malkki 2008: 174). Messiness should be embraced not just because it provokes theorising, but also because it does justice to human experience. Perhaps it is exactly in this messiness that we find the potential for democratising our research practices. Indeed, if we want to stay true to democratic or equalising potential, we need to engage in dialogue with all sides, and without giving up our sceptical, critical, or dissenting position. Instead of seeing our commitment to incorporate multiple perspectives as a problematic 'both sides' position, we can present it as a 'radical no sides' research attitude, leaving open the possibility to take sides once we are confident to have captured the relevant 'sides' that hide within complex realities. There is value in the unspoken disciplinary ideal of almost 'going native' (in multiple of directions) but refraining from going all the way. Anthropology thrives when it remains uncomfortable.

The acknowledgment that as anthropologists we study 'small places / large issues' need not translate into a fetishization of place, whether village, factory, or even moving circus. We can use our penetrating methodology to move beyond notions of Euclidian spacetime to explore how virtual and actual spaces, how future and past, inner and outer worlds, the here and elsewhere are combined and folded in complex and often surprising ways, while still holding on to methodological asceticism. Here we might also take inspiration from 'negative methodology' (Navaro 2020). Its attendance to 'traces' as indexing absences and presences can be fruitfully linked to 'incompleteness as a theorem of practice' (Marcus 2009: 28). Our approaches already resist closure and formalisation, and refuse to

retreat from complexity. It is a position that, when confidently presented, should be seductive not only to ourselves but to others that matter.

In a methodology paper that attempts to think through the messiness of fieldwork, an obvious final question would be: is there 'method to this madness'? Without reiterating the various points about anthropological research, I would hope that the answer is a resounding yes. But it is equally important to ask if there is enough 'madness to our methods'. Let me end with a wink to Michael W Scott who recently 'dreamt' his way through the very public William Fagg lecture, as this made me think (logically so?) of Sigmund Freud and his words: 'You worthy critics, or whatever you may call yourselves, are ashamed or afraid of the momentary and passing madness which is found in all real creators, the longer or shorter duration of which distinguishes the thinking artist from the dreamer' (1997 [1899]: 17). Without feeling beholden to Freud, the point is that there is nothing straightforward to our subject matter, nor indeed to the way we engage with it. While committing to radical potentials, what to do with these potentials remains very much open. And that, as always, is how it should be.

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