Para-Ethnography 2.0:

An Experiment with the Distribution of Perspective in Collaborative Fieldwork

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It is by now a truism that anthropology is, and has always been, collaborative by its nature. Even if there was once a time when anthropologists broadly ignored, or tried to eliminate, their influence on their research subjects, thus seeking to keep up with a positivist agenda (Davies 1999:11), the reflexive turn of the 1980s clearly established how anthropologists both act on and are acted upon by their subjects in the field. With fieldwork being the prime research strategy, anthropologists are deeply embedded in social relations and participate in the emergence of events in the field (Leach 2010), meaning that anthropological knowledge cannot but be inherently relational (Hastrup 2004:456) – and, in this sense, collaborative. Yet, in the past two decades or so, this collaborative nature of anthropology has developed into much more, and something quite different, than simply 'the subject responding to, cooperating with, and tolerating the ethnographer's more or less overt agendas', to quote Douglas R. Holmes and George E. Marcus (2008:85). Collaboration, it appears, is today an impetus – perhaps even a must or a mantra – in many fields of anthropology.

As emphasized in the call for this seminar, this is not least the case in design anthropology where participation and collaboration among various stakeholders 'have become a strong convergence point between design and anthropology'. In fact, a new criterion of success, Ton Otto and Rachel C. Smith point out, could be 'how design anthropologists are able to correspond and collaborate with people as co-creators of desirable futures and to be the facilitators of knowledge and meaningful practices that transform the present' (2013:13). But the collaborative tendency goes well beyond the field of design anthropology, as it is now broadly acknowledged that numerous of the

world's most pressing problems and themes can be neither solved nor understood by a unidimensional approach. In particular, in their highly influential discussion of 'Mode 2' knowledge production, Helga Nowotny et al. argue that processes of research have been radically transformed, from an old paradigm of knowledge production (Mode 1) characterized by being firmly anchored in distinct disciplines, to a new paradigm of knowledge production (Mode 2) considered to be 'socially distributed, application-oriented, trans-disciplinary, and subject to multiple accountabilities' (2003:179). As such, design anthropology may, in various ways, be seen as a particular instantiation of this new style of knowing.

In this paper, we wish to discuss a particular collaborative experiment, which we have conducted in, yes, collaboration with a Danish consultancy firm and a large Danish tech company. With specific focus on management as a social phenomenon rather than an individual skill, we have been involved in processes of creating – or designing – a new concept of management development which includes not only managers but also employees. Somewhat boldly, we contend that our experiment in these design processes can be viewed as a new, and perhaps more radical, version of (design) anthropological collaboration, in that we not just engaged with the managers and employees in collaborative epistemic and design processes but actively trained and urged them to become *para*-ethnographers, with basic observational and note-taking skills. Drawing on Holmes and Marcus' concept of para-ethnography (2005, 2006, 2008), developed to capture the reflexive and intellectual practices of various new subjects in contemporary fieldwork contexts, i.e. their 'preexisting ethnographic consciousness or curiosity' (2008:82), we have pushed this notion a step further in the sense that a para-ethnographer may be compared to a para-medic, who is specifically trained to deal with medical problems in the absence of a medical doctor. Our central experimental question is then: what would design anthropology look like if it took the form of para-ethnography 2.0?

In what follows, we intend to show that, in our view, the experiment served at least three different purposes. (I) It formed the basis for identifying and discussing a number of distinct managerial and organizational issues in the tech company, which, at first, was merely described to us as 'complex'; that is, it provided certain insights into how management occurs and unfolds in practice. (II) It proved a valuable means of obtaining ethnographic insights in a design anthropological context where the time

for more traditional ethnographic fieldwork was limited; and, as such, it raised critical questions concerning the nature of such insights and the method's ethical dimensions. (III) By far most importantly, we think, it served as a way of distributing the perspective during fieldwork, thus challenging a range of conventional ideas about (design) anthropological research; that is, it rendered ethnographic fieldwork a fundamentally collaborative affair – or, if you will, a collaborative formation of issues.

Radicalizing para-ethnography: from a preexisting ethnographic consciousness to trained para-ethnographers

Only a couple of minutes into the Danish radio show *Jørgen Leth's Last Journey*, the famous Danish filmmaker and author, Jørgen Leth, is asked to recite his poem entitled *Ethnography*, in which he declares that 'Malinowski is my hero' (Thouber 2012, our translation). The journalist then wants to know who this Malinowski is. Leth explains that Malinowski was a pioneering anthropologist, the first to conduct fieldwork, and that he had a particular, and highly fascinating, interest in 'the sexual life of savages'. Malinowski, Leth metaphorically elaborates, had an extraordinary ability 'to observe life through a magnifying glass' (ibid.). Then, Leth continues:

> I have found an uncle, a mentor, in Malinowski, who has guided me considerably in my exploration of life: to explore and describe what I see […] It is the essence of my work; that I am partly an observer who can explore and see and that I also include my own life in my field of observation. And I am able to look at my own life, live it simultaneously and explore it, observe it and try to use it for description. In a way, it is a cool-headed process, if you will, but it is also a demanding process. But I have made it my field: that I am not afraid of looking at my own life and of transcending the boundary between observing and participating. I am aware that I am an observer and that I always have the distance as my field; that I always have a distance between what I see and what I am (ibid.).

Leth, it ought to be clear, qualifies as a para-ethnographer in the sense laid out and developed by Holmes and Marcus in the past decade (2005, 2006, 2008). Indeed, he represents a highly reflective and knowledgeable para-ethnographer, probably far more conscious of his ethnographic inclinations, and even inspiration, than most other people who are engaged in types of reflexivity and curiosity resembling ethnographic

practices. In their work towards a refunctioning of ethnography, Holmes and Marcus precisely contend that within many contemporary field sites – such as science labs, corporate organizations, political institutions etc. – 'operate reflexive subjects whose intellectual practices assume real or figurative interlocutors' (2008:82). In this sense, they continue, '[w]e can find a preexisting ethnographic consciousness or curiosity, which we term para-ethnography' (ibid.). As the term 'para' implies, meaning 'by the side of, beside, whence, alongside of' (Marcus 2000:6), the found para-ethnographic dimensions in contemporary fieldwork contexts confront ethnographers as parallel or alternative perspectives that compel them to negotiate and meet expectations of what ethnography might mean; for 'them' as much as for 'us' (Holmes & Marcus 2008:83).

This, for Holmes and Marcus, essentially alters the conditions of ethnographic research, as it places *collaboration* at the very centre, in a sense that goes well beyond the inherent collaborative nature of ethnography. In fact, the key point is, they argue, that we, as ethnographers of the contemporary, need 'to integrate fully our subjects' analytical acumen and insights to define the issues at stake in our projects as well as the means by which we explore them' (ibid. 86). In other words, as the circumstances of ethnographic fieldwork has changed profoundly, we can no longer rely exclusively on a Malinowskian aesthetic of fieldwork which favours such distanced practices as description and analysis (ibid. 82, see also Marcus 2010). Rather, we need to engage actively with our dialogic, epistemic partners in the field, who, accordingly, turn into counterparts and co-producers of interpretations and knowledge (Holmes & Marcus 2005, 2008); that is, no longer simply 'others' serving as 'informants' in particular research endeavours defined and pursued by 'us'. Ethnography, in brief, becomes an essentially collaborative practice.

While this refunctioning of ethnography serves different purposes for Holmes and Marcus – for the former, it relates to his work on central banking and to marginal or illicit discourses within more dominant or official ones, whereas, for the latter, it serves to rethink contemporary ethnographic practices and aesthetics and, as such, to extend the so-called 'writing culture' critique (Clifford & Marcus 1986) to the new conditions of fieldwork (Holmes & Marcus 2005, 2008, Marcus 2010, 2012, 2013) – it also has a clear resonance in the field of design anthropology. Insofar as this field is broadly characterized by the entanglement of various stakeholders in design projects, not only anthropologists and designers but users, citizens and different professional

experts as well (see e.g. Halse 2013, Kjærsgaard & Otto 2012, Smith 2013), the para-ethnographic dimension is certainly pertinent. And this dimension, as emphasized by Holmes and Marcus, paves the way, and often calls, for 'partnership or collaboration with our counterparts found in the field' (2005:251). Recently, Marcus has elaborated on this condition: 'the condition of para-ethnography in contemporary fieldwork projects invites the creation of and experimentation with new forms and formats – designs within fieldwork – to meet this challenge' (2013:208).

It is in this spirit that we have experimented with a distinctive version of para-ethnography. While the found para-ethnographic, in Holmes and Marcus' terms, designates a *preexisting* ethnographic consciousness or curiosity (2008:82), the allied notion of the para-medic bears certain similar yet also slightly different connotations. 'Related to medicine in an auxiliary capacity', as it is stated in the Online Etymology Dictionary, the para-medic has developed into a distinctive profession as a subset of medicine. Referring not so much to a preexisting medical capacity as to a particular kind of *training*, the para-medic forms a central part of health care. As one textbook in *Foundations of Paramedic Care* explains:

> Although paramedicine is a relatively young profession compared to many others, it has found its footing as a unique part of the healthcare system. The Paramedic's role has evolved from simply responding to emergencies to now practicing an expanded scope of practice as a *physician extender* (Beebe & Myers 2010:4, our emphasis).

Crucially, while the para-medic thus works *in*dependently, drawing on specific skills and training, para-medic practices are simultaneously *inter*dependent, relying on the expertise of professional physicians. It is, in a nutshell, collaborative (ibid.).

Now, what would happen, we speculate, if the preexisting para-ethnographic consciousness found in many contemporary field sites, not least in design projects, were purposely moulded or developed by means of actual ethnographic training, thus giving rise to para-ethnographers on a par with para-medics? Or, to put it differently, what would happen if those subjects previously understood as informants and others, and now as epistemic partners and co-producers of knowledge, were trained in basic observational and note-taking skills – and then urged to conduct para-ethnography *on* and *by* themselves? This, we envisage, would amount to virtually the same kind of 'para-ethnographic participant observation' as so keenly explained and practiced by

Leth. Doing this in collaboration with professional ethnographers would, thus, be a more elaborate and radical version of para-ethnography.

In what follows, we present a fairly straightforward account of such an experiment, followed by our very preliminary reflections on its potentials and pitfalls.

Mobilizing ethnography for management consultancy

In 2013, we became involved in a collaborative project initiated by two management consultants, who approached us with certain expectations regarding the discipline of anthropology. The aim of the project was to develop new concepts for managerial and organizational development that took for granted the complexity and unpredictability of contemporary organizations. Referring to particular strands of complexity theory, developed with the purpose of guiding managerial decision making (e.g. Snowden & Boone 2007, Uhl-Bien et al. 2007), the consultants sought to put the complexity perspective to use in their interaction with clients. Their interest in anthropology came from the idea that the anthropological focus on 'complex social processes' resonated well with their own theoretical orientation and, importantly, that anthropology would offer a means of generating data that could shed new light on this alleged complexity.

From the consultants' perspective, the collaboration was thus imbued with the hope that it would bring about a new concept with commercial potential and scientific credibility. This, we thought, was not entirely unlikely. In fact, being anthropologists, we were quite confident that some version of ethnographic fieldwork would provide a productive starting point for the development of the new concept. As we shared with the consultants the basic tenet that management does not spring from the behaviour of *individual* managers, but that it is rather a *social* phenomenon which may, or may not, arise in particular situations, we soon agreed that the topic would lend itself nicely to participant observation. Therefore, when a management representative from a large Danish tech company expressed an interest in the project and was willing to open the doors to one of their R&D departments as a kind of laboratory for our project, there was only one problem left. How were we actually to go about it?

Time turned out to be an important issue. Like so many other anthropologists both inside and outside academia, we had limited opportunities for doing fieldwork – even for shorter periods of time. Apart from a few meetings with managers and other employees in the company, we thus decided to turn the process of ethnographic field-

work into something which would work as an organizational intervention in itself and which required more work from the 'natives' – or 'epistemic partners' – than from us. Taking inspiration from Holmes & Marcus' notion of para-ethnography, we decided to pursue our speculations described above; that is, to provide the employees and the managers of the department, a group of 15-20 engineers, with a few hours of training in ethnographic methods and instruct them to conduct para-ethnographic fieldwork at their own workplace. Their participant observation was to be recorded in fieldnotes, shared with us and eventually to become the object of a joint analytical process. In a word, it would be para-ethnography 2.0.

At least in theory, this model provided an opportunity for producing literally a larger amount of data than a brief period of participant observation conducted by two anthropologists. As such, it also appealed to the consultants who saw it as a way of systematically gathering a pool of empirical narratives about management in practice, and who also appeared quite eager to ensure the scientific foundation of the project by not ending up with 'too little data'. From our perspective, of course, the training of a group of engineers to work as para-ethnographers within their own organization was more than simply a time-saving means of generating large amounts of data. Indeed, it was first and foremost an exciting experiment which, we believed, might have certain potentials and important implications for a (design) anthropology of the contemporary (cf. Rabinow et al. 2008). But honestly, we had virtually no clue what would happen.

A kind of arrival story: encountering 'complexity'

Evidently, our first encounter with 'the field' was, by no means, as overwhelming and exotic as the Malinowskian trope of fieldwork arrival. Rather, it took a highly familiar social form, as we conducted a few meetings with a group of managers in the Danish tech company. Being in charge of an R&D department, the managers (Carsten as the Head of Department and Niels, Michael and Christian as the Team Leaders) wished to participate in the project, allegedly because they experienced some complex problems in their own social interactions and management practices. Complex problems which they had tried to solve for some time now but, apparently, without much success.

The idea of using complexity theory seemed, therefore, appealing to them. The situation in the organization in general and the department in particular was inherently 'complex', they said, for which reason they could surely need some concrete tools for

dealing with this complexity. While they told us that they were already familiar with many 'nerdy' (*langhårede*) management theories, because they had received systemic management training and, in general, enjoyed discussing such matters at length, they also reported that they experienced a 'downward spiral' where they 'talked each other down'. Despite various attempts at changing or overcoming this spiral – for instance, by moving their weekly meetings to a different time slot, as the Head of Department usually came directly from other weekly meetings in the corporate management group and, for this reason, was always in a negative mood – they nevertheless continued to experience a negative atmosphere within their group.

Although we tried hard to probe into this issue, the notion of complexity came to constitute a kind of placeholder for certain managerial difficulties which could not easily be described or identified. At one level, the four managers differed substantially in their managerial approach, as one of them appeared a bit wary of all the theorizing and 'meta-talk' and also somewhat disengaged in our discussions. Clearly, something 'complex' was going on in their social interactions. At another level, the 'complexity' came from the fact that the three teams in the department were supposed to work as a unit even though they had very different tasks and roles. Interestingly, this mirrored a paradox in the overall corporate vision: the company was seen to gain its competitive edge from being able to provide all parts and services in a 'complex' technology, but the different components in this technology were produced in different departments with diverging agendas, thus making the hoped-for 'integration' and 'smoothness' fairly illusory in practice.

In other words, the concept of 'complexity' denoted a number of aspects of the managerial practices and could be found in different shapes, types and locations. In a sense, it seemed primarily to serve as a common denominator for anything perceived to be difficult and troublesome. The pressing issue for us, then, was how to approach this 'complexity' ethnographically in a way that would allow us to delve into it and, at the same time, potentially form part of a novel concept of management development. Our para-ethnographic experiment became our proposal; not merely because it could provide us with a clearer articulation and understanding of the managerial problems – and, as such, might serve as a significant element in a concept of 'ethnographically driven management development' – but also because it would instil a new reflective gaze on the organization, from within, which might help the managers to deal with the

problems. As a particular intervention, we thus began to plan a teaching session with the purpose of developing or transforming the managers and employees' preexisting para-ethnographic curiosity, clearly present in all their 'theorizing' and 'meta-talk', into more specific para-ethnographic skills and insights.

Para-ethnographic fieldnotes: who is studying who?

In the teaching session, we introduced the managers and employees to the basic ideas and methods in ethnographic fieldwork. While this generated numerous questions and an engaged discussion, our final assignment was received with something of a mixed response. We asked the 15-20 participants to think of themselves as para-ethnographic participant observers and to write fieldnotes where they should describe, at least, two different situations in which they perceived management to occur. In the true spirit of the collaborative nature of para-ethnography 2.0, we now leave the writing to some of the participants who, despite the rather unusual assignment, took up the challenge. To focus our discussion, we present fragments of a few fieldnotes which, interestingly, zoomed in on the handling or management of the para-ethnographic experiment itself.

Gustav is an engineer who, at the time of writing, has been with the company for five years. In his fieldnotes, he describes his observations and experiences during the teaching session, focusing particularly on the behaviour of the four managers:

> *I observe that thinking/reflecting about concepts seems to be the important action (because there is never a perfect/unbiased solution/answer?).*
> *Niels (sitting in the first row) seems to be well in phase with the anthropologists.*
> *Michael (sitting in the first row close to Niels) is less active. He tries to ask a question but does not finish it.*
> *When the anthropologists take a pause, Carsten often tries to resume the situation for everybody.*
> *Niels seems to understand the concepts well and even answer some questions that were asked to the anthropologists.*
> *Christian is very passive (sitting in the last table in the back) and looks at his mobile phone and does not seem especially involved in the process.*

Niels helps to make things in perspective and tries to help building the communication bridge between the anthropologists and the engineers by using a common vocabulary or down to earth example. He is actively participating at the discussion.
Henrik is also actively participating in the discussion. I notice that he is acting in this situation like a 'non-official' leader.
Niels is helping again with common vocabulary/example.
Christian's first (and only) intervention is to make a joke. This joke is not participating actively in the discussion.
[…]
Carsten says that he would like everybody to participate even if the anthropologists have said that only people that were interested should participate (I do not really understand why he did that?).
Niels often moves his head in what I evaluate to be an agreement/acceptation when the anthropologists talk.
Niels once again takes the practical aspect into consideration. 'It is okay to take an hour off your calendar after a meeting to write down your note.'
Christian is on his mobile phone.
Niels says something, and Carsten cuts him off saying the exact same thing.
Carsten apologizes at once.
The meeting is over.

At a subsequent meeting a few weeks later, the managers encourage the employees to write and submit more fieldnotes. This, once again, leads Gustav to describe his para-ethnographic participant observation in this meeting. He writes:

Niels (standing up as always) asks how the experience is going. He is pointing out the fact that the anthropologists did not receive many observations.
Michael is sitting next to him and seems to agree with him.
Christian is on the other side and seems to stare at the floor.
Some people declare again that they have had no time to do it and will rather let the interested people take care of it (I think that this is a fair statement).
[…]

Eventually, we agree that it takes time to be an anthropologist and that it is okay to deliver something that is not perfect. We just have to do our best.
During the discussion, Niels and Michael are doing a good job as facilitators. Christian is still looking at the floor.
At this point, the discussion continues and begins to take a weird angle. Many people are talking but no one is really paying attention.
At this point, Christian stands up and begins to summarize the situation:
'This is what we were supposed to do, this is what we have done, and this is what we expect you to do now… It is okay to send an observation that is not perfect. The important thing is to give a bit of a puzzle so that the anthropologists have something that they should try to put together and can have an idea of the whole picture (I am not 100 % sure that it was Christian who said the last sentence, but I think that it was a nice way of seeing it)'
I think that it was good that Christian had this intervention, and it was good to see that he also has a positive point of view on the subject.
Niels and Michael agree, and it seems that most of us are thinking that it is an important task.

Also for Thomas, another engineer, the same meeting apparently illustrates a situation in which management occurs. He observes and describes:

It quickly developed into a discussion about why it may be that it is so difficult to write some ethnographic fieldnotes. This was a discussion characterized by the 'usual suspects'; NVB who mentioned the wish to write a whole thesis as a kind of showstopper, ESO who always presents an intellectual analysis, JG who thought that our management is so unrealistic that it cannot possibly be interesting to analyse, and myself who thought that an unanalytical observation must be the easiest thing to provide ☺ During the meeting, there was traffic back and forth: one employee chose to leave the meeting in order to answer his phone at his desk (in the same open office space) after which he returned, and two employees chose to leave the meeting a few minutes before its closing in order to resume their work at their desks.

Interestingly, the three local managers Niels, Michael and Christian decided to conclude the discussion with Christian saying 'should we just leave it here then…', Michael commenting 'and let people give it a few thoughts…', and Niels sarcastically concluding 'let's see, then, if Carsten is going to write to all of you that you MUST write some fieldnotes' ☺ In other words, no clear ACTION-points were determined but rather a more loosely 'Grundtvigiansk' [democratic, optimistic] *perspective (yes, that is my analysis…).*

While a couple of the fieldnotes thus describe certain para-ethnographic observations of the management of the para-ethnographic experiment itself, other fieldnotes place emphasis on more humdrum activities and managerial practices; for instance, how the managers articulated a particular frustration with another department, how a concrete meeting unfolded in practice, or how a specific complaint from a client was discussed and taken care of. In combination, the fieldnotes provide different perspectives on a number of situations in which management is perceived by the para-ethnographers to occur in noticeable ways. What appears to be a defining characteristic, in this sense, is that it becomes highly unclear and diffused who is in fact studying who. Everybody or everything is potentially an object of one or more para-ethnographic gazes, which not only implies that a particular (and new) kind of attention and reflection on managerial processes is instilled in the organization but also, and crucially, that the Malinowskian aesthetic of fieldwork is critically challenged. For this reason, it seemed obvious to us that the para-ethnographic approach ought also to include the more analytical part of the collaborative formation of issues.

Analytical collapse? Or two anthropologists in deep water

When we met with the para-ethnographers three weeks after the teaching session, our plan was to engage in a process of collaborative analysis. During these three weeks, it had occurred to us, however, that the 'cool-headed' and 'demanding' process, as Leth described it, might be too much of a challenge for a group of busy engineers. In fact, we began to worry if our intervention, particularly our call for transcending the line or boundary between observation and participation, might affect the social climate in the workplace in such a negative way that it would actually contribute to the 'downward spiral' rather than changing or solving it. Indeed, it all seemed quite unpredictable.

Perhaps we did not have to worry too much about this. The atmosphere among the managers and employees was already tense, and they had been through a range of events and sessions which seemed, at least, equally demanding and challenging. More importantly, we received a total of only 12 documents from nine different employees (two of whom were managers), so the active participation, particularly in the writing of fieldnotes, was far from overwhelming. Nevertheless, we were told more than once that the para-ethnographic experiment was object of intense debate in the department and that it had preoccupied the minds of many employees, even if it had not materialized in written notes. Consequently, we had to rethink the analytical processes and, in collaboration with the consultants, we decided to conduct a more traditional workshop with the hope of producing a multitude of para-ethnographic oral accounts – or 'para-ethnographic oral fieldnotes', if you will – in lieu of a larger body of written material.

The workshop, however, was not a good experience. Despite the use of several process tools, as well as the facilitative efforts of one of the management consultants, the workshop seemed primarily to reproduce discussions and arguments which were all-too-familiar to the participants. Quite a few of them did not say anything at all, and the insights provided by the fieldnotes, as well as the alleged para-ethnographic observations that had not materialized in written notes, were never given any significance. Indeed, the workshop tended to reproduce precisely the vague sense of organizational issues which, at the beginning, had merely been labelled as 'complex'. For us, then, the intended collaborative analytical process turned more into ethnographic fieldwork, allowing a certain kind of participant observation that gave insights into the everyday tensions and dynamics in the department.

As anthropologists, however, we felt hopelessly ill equipped to deal with this type of situation. Somewhat ironically, we had been so concerned with how – and if – we would be able to teach the managers and employees to become para-ethnographers that we had totally ignored what para-ethnography 2.0 would require from us. This, to be sure, is a critical question to be further discussed. As we returned to the fieldnotes, however, we found that they contained various interesting insights which could hardly be achieved through more traditional ethnographic fieldwork and which could surely, with appropriate preparation and facilitation, be productively explored and debated in collaborative epistemic processes. Most importantly, we think, the fieldnotes provide descriptions of the social dynamics among managers and employees *as experienced*

and perceived from different positions and perspectives within the organization; that is, the para-ethnographic experiment distributed the perspective on management into a larger part of the organization.

Towards para-ethnography 2.0?

Now, what can be learned from this experiment? In terms of ethnographic research, it might be argued that nothing new or different has come from it. Anthropologists have always collaborated with their research subjects and asked them to perform various activities such as, for instance, to write diaries, take photos, or even serve as research assistants in the field. Indeed, as Tim Ingold contends, anthropology is not a study *of* but a study *with* people, in that anthropologists are immersed in environments of joint activity, learning other ways of perceiving the world by working and studying *with* people, not only by gaining knowledge *about* them (2008:82). Yet, in our view, there is more to our experiment in para-ethnography 2.0 than this acknowledgment of how 'the world and its inhabitants, human and non-human, are our teachers, mentors and interlocutors' (ibid. 83). By way of closing this paper, we give this a few thoughts in relation to anthropology in general and design anthropology in particular.

First, the basic training in ethnographic methodology introduced the managers and employees to the central tensions and perspectives involved in para-ethnographic participant observation as so eloquently described by Leth. Moreover, we specifically instructed them to reflect upon their own positions in different situations, presented the idea of a continuum between participation and observation, asked them to describe situations from different positions along this axis and urged them to write descriptive fieldnotes with no evaluative or analytical content (as far as this is actually possible). In this way, the point was *not* to leave the managers and employees on their own but to make the practice of para-ethnography both *in*dependent from, and *inter*dependent on, ourselves as professional ethnographers (like the in- and interdependent nature of paramedics). Hence, the experiment not only dissolved the commonplace distinction between ethnographer and informant but provided the basis for a rich and analytically potent ethnographic material, which differed substantially from, for instance, diaries, log books or interview transcripts.

Second, it follows that the fieldwork perspective becomes broadly distributed and, as such, essentially collaborative. While anthropology is inherently relational (as

argued above) and, therefore, fundamentally concerned with the intersubjective field between anthropologist and fieldwork subjects, para-ethnography 2.0 seems not to fit this argument so easily; or, at least, it makes it far more 'complex'. In this respect, for design anthropology, a significant point is that the distribution of perspective renders the ethnographic dimension a key part of the design processes, not only to be pursued, practiced and defended by the often lone anthropologist but, hopefully, by the various para-ethnographers as well. As such, it instils an essential ethnographic dimension in the pursuit and design of new futures, in that the para-ethnographers come to serve as a kind of 'anthropologist extender' (cf. the paramedic as a physician extender).

Third, the para-ethnographic training and the following process of 'fieldwork' can be viewed as an intervention in the very organizational and managerial processes that we set out to examine. Even if, along the way, we became concerned about the implications of the experiment for the social interaction and climate in the work place, it was clear that the experiment served to strengthen the preexisting para-ethnographic reflexivity among the employees (indeed, the experiment appeared to have a stronger impact on the employees than the group of managers). Several employees expressed that they found it quite useful to think about their own (and others') positions in the organization in terms of the participation-observation axis, and that they saw an interesting potential in the para-ethnographic skill of 'transcending the boundary between observing and participating' (Leth, quoted in Touborg, op.cit.). At the same time, of course, the experiment raised a range of ethical questions and concerns: what happens to a social environment in which everybody is (or at least could be) a dedicated para-ethnographer, observing while participating, and writing detailed fieldnotes during or after meetings, lunch breaks, informal conversations etc.? Which types of consent are required, and how may it be obtained? How do we, as anthropologists, deal with the knowledge obtained, and how do we ensure that it does not negatively influence our para-ethnographic collaborators? These are questions which require more thought.

To sum up, we believe that the particular methodological and analytical potentials of para-ethnography 2.0 merit further discussion and experimentation, not least in relation to the focus on 'collaborative formation of issues' and design anthropology more generally. We are particularly interested in discussing how (or if) the collaborative formation of issues might become informed and shaped in new ways by drawing on para-ethnography 2.0., a prime concern being not just what this type of experiment

requires from our epistemic partners, but even more importantly what it takes from us. Surely, para-ethnography 2.0 is no simple route to design anthropological knowledge.

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