Accessibility statement

This is an accessibility statement for the journal: Encounters.

Conformance status

The Web Content Accessibility Guidelines (WCAG) defines requirements for designers and developers to improve accessibility for people with disabilities. It defines three levels of conformance: Level A, Level AA, and Level AAA. This statement is relevant for volume 1, number 1, 2008 through volume 10, number 5, 2018. Complying with current EU regulations, articles published before September 23th 2018 are unfortunately not made accessible at this point in time. We will do our best to make them accessible as soon as possible.

Feedback

We welcome your feedback on the accessibility of the journal. Please let us know if you encounter accessibility barriers. You can reach us at:

E-mail: imvko@cc.au.dk
Address: STS Center, Helsingforsgade 14, 8200 Aarhus N

STS Encounters
Research papers from DASTS
Volume 8 · Number 1 · 2016

http://www.dasts.dk/

ISSN: 1904-4372

Texts as events – or how to account for descriptions as intervention

Sissel Olander

DASTS er en faglig forening for STS i Danmark med det formål at stimulere kvaliteten, bredden og samarbejdet inden for dansk STS-forskning samt at markere dansk STS tydeligere i nationale og internationale sammenhænge.

Texts as events - or how to account for descriptions as intervention

Sissel Olander

Introduction

A central claim in science and technology studies (STS) and constructivist social research is the assertion that texts and other non-humans have agency. Texts are not mere reflections of a reality out there; they are actors in their own right, participating in the construction of the real. I subscribe to this claim. Yet, the rather general idea that texts hold the capacity to intervene in the formation of the social, does not say much about any given particular text and how it intervenes or not, concretely and localised, during the process of research.

In this article I problematize how texts are sometimes cast as potent interventionists in cultural-analytical and constructivist social research. I engage with two different articles, which discuss texts as interventions. One is an article by Signe Vikkelsø titled: "Description as Intervention - Engagement and Resistance in Actor - Network Analyses" (2007). The other is an article by Brit Winthereik and Helen Verran titled "Ethnographic stories as Generalisations that Intervene" (2012). In both articles the authors propose strategies for writing up accounts. The purpose of these proposed strategies is to induce the text with certain qualities, in order to enhance the interventionist powers of the texts. I will inspect these strategies as particular research set-ups, and challenge how the process of writing up the analysis and account is continually conflated with claims about how the text will intervene in some more or less specified future. I do so by exploring descriptions and texts as events, and by inspecting the proposed research strategies as if they were in fact experimental research set-ups. To this end I draw on prevailing ideas about the relation between program and experiment in constructive design research, and on the work of philosopher of science Hans-Jörg Rheinberger.

On the intersection of design and the social

In the spring of 2011, the Danish Royal Academy's School of Design hosted a series of seminars titled: "Design and the Social". The title is very telling. It points to a growing fascination with the social as design material and object of design expanding from within the constructive design research milieu. Constructive design research (Koskinen, Zimmerman, Binder, Redström & Wensveen 2011), sometimes also labelled research *through* design, refers to research where the process of designing something, for example a product, system, space, media, concept, community, scenario or prototype, becomes the main way of constructing knowledge. Constructive design research then, does not refer to constructivism as theoretical or philosophical orientation, although many constructive design researchers may of course also consider themselves constructivists, as do I.

Particularly over the last decade, constructive design researchers have replaced a focus on the bounded object, system, or designed product, with a concern for complex issues, and how such issues unfold in open social arenas (Binder, De Michelis, Ehn, Jacucci, Linde & Wagner 2011; Binder, Brandt, Halse, Foverskov, Olander & Yndigegn 2011; Björgvinsson, Ehn & Hilgrenn 2010; DiSalvo 2012: Ehn 2008; Halse 2008; Halse, Clark, Brandt & Binder 2010; Jönsson 2014; Lenskjold 2015; Lindström & Ståhl 2014; Manzini & Rizzo 2011; Servalli 2012; Storni, Linde, Binder & Stuedahl 2012). This shift in focus has been nurtured by, and at the same time has led to, an increased interest in theories, concepts, and methods that circulate in the constructivist social sciences. The works of among others, Bruno Latour (1999, 2004, 2005a, 2005b, 2008, 2010) John Law (2004) Annemarie Mol (2002) Noortje Marres (2012) but also femi-

nist scholars like Donna Haraway (2007) and Karen Barad (2007) have been very influential. We may say that an inherently performative orientation and a radical constructivist understanding have pervaded constructivist design research in many productive ways, as design researchers have embraced constructivist theoretical and conceptual tools to understand settings and constellations to design for, and to analyse and report from their own design experiments and interventions.

Theoretical-analytical research and intervention

The traffic between constructive design research and the more theoretical-analytical positions in cultural and social research has mostly been travelling in one direction. From theoretical-analytical-empirical approaches in constructivist social research, it seems, design and sites of design, remain primarily an object of research and study rather than a resource for developing new ways of knowing (see for example Suchman 2011). However, questions of engaging in more experimental approaches of cultural-analytical work have been explored and problematized, for example through the notion of intervention, both in somewhat hopeful ways (Jespersen et al 2012) and in fairly more sceptical manners (Zuiderent-Jerak & Jensen 2007). In the editorial of the special issue of Science as Culture in which the article by Signe Vikkelsø is published, Teun Zuiderent-Jerak and Casper Bruun Jensen propose to replace the notion of intervention with a so-called "ethics of specificity", to overcome, what they find to be, a dichotomous understanding between descriptive and normative research. The editors are discontented with calls for "getting real" in the social sciences. They want to "unpack" what it means to intervene in STS, rather than to concern themselves with "how-to-do" action-orientated research (Zuiderent-Jerak & Bruun 2007, p. 230). It is the hope of the editors that as a result of the special issue, researchers can no longer get away with claiming success for their interventions and transformational activities unless careful qualification is given for the criteria of success, and relative gains and losses (Zuiderent-Jerak & Jensen p. 232). It seems to me, that although Zuiderent-Jerak & Jensen want to do away with professed bland either-or debates (Zuiderent-Jerak & Bruun 2007, p.229), they somehow come to re-instantiate the descriptive-normative divide, making it either a question about "how-to-do", or, a question of "what-it-means", as if these questions were somehow separate. In this paper I am not going to call for more action, I am going to call for qualification of knowledge on interventions. The discussion I wish to raise does not relate "only" to how research can or should make a difference for practice, rather, it is deeply tied to the question of what forms of knowledge research set-ups enable and favour. These explorations tie on to a broader discussion on the possible role of research in society. A debate on how social researchers working in new constellations, often with actors outside academia find themselves enrolled in new agendas, and how they, through this engagement may contribute to new insights and new configurations of realities. This is also the main theme in the guest editorial of Science Studies in which the article by Britt Winthereik and Helen Verran is published. Here the editors, Astrid Pernille Jespersen, Morten Krogh Petersen, Carina Ren and Marie Sandberg address "Cultural analysis as intervention" (2012). The editors reflect on the potential role of cultural analysis in a society marked by soft capitalism. This matter, they argue, cannot be reduced to a simple question of methods, nor is the cultural analyst someone who can simply act as witness or deliver facts to stakeholders. What they suggest instead is to examine cultural-analytical work as practices comprised by ontological tools. Practices that compose and bring forth new and possibly better worlds (ibid:6). They bring in the notion of intra-vention and the concept of the apparatus based on the work of Karen Barad (2007). If cultural-analytical work is recognized as particular material-discursive apparatuses that perform agential cuts, then it becomes increasingly

important, the editors argue, to describe the characteristics of such contemporary apparatuses.

I agree with the editors. It is important to characterize contemporary cultural-analytical work, and to take such work seriously, as particular forms of world making. It goes without saying of course, that taking this work seriously can't be reduced to a "simple" matter of methods. On the other hand, any serious attempt to characterize contemporary cultural analytical apparatuses, in my opinion, cannot exclude the question of methods either. Indeed that would be a strange agential cut, if cultural-analytical work is performed, as the editors suggest, through a material and meticulous ordering and organization of realities.

Experimental reasoning in research

Let us take cultural-analytical work seriously as a doing, and inspect the research strategies proposed in the two articles as particular apparatuses, configured in very specific ways, and enabled by distinct methodological choices and instrumental set-ups. My analysis of the two articles will be shaped by my own performative and constructivist understanding. But more importantly, it will be scrutinized from an inherently experimental position. From such a position difference and displacement is the fundamental orientating principle. In constructive design research this is commonly articulated as a process that plays out in an on-going tension between the so-called design program and the design experiment. In brief, the design program works as a sort of hypothetical worldview, a framing device, which makes a particular line of inquiry relevant (Binder & Redström 2006 p.4). The design experiment, on the other hand, with its instruments, practises, and routines, which are continually remade and reconfigured, explores the program. Through a series of events the design experiment seeks to materialise, specify, displace and even reformulate the program (Brandt, Redström, Eriksen & Binder 2011). This particular dynamic is very close to what German historian and philosopher of science Hans-Jörg Rheinberger has described through notions such as technical objects and epistemic things in his studies of the experimental practises in the life sciences (1997, 2010). Epistemic things are objects of research; they are fluctuating matter and blurry concepts in one, and it is their infiniteness that makes them operational and productive for scientific action. Just like the design program, they embody the not yet known. For the epistemic thing to be worked over and present itself, however, it must be brought into relation with the technical object. The technical object equals what could be called the experimental arrangements or the experimental conditions (Rheinberger 1997, p.29), in other words, the relatively stable identity-conditions that will allow the epistemic thing to enter into a process of operational redefinition. The important point to make here is that although the epistemic thing and the technical object are inherently and intimately related, for any researcher to engage in an experimental practise, some sort of distinction between these two elements of the research set-up needs to be upheld. Evidently, there is no way to finally separate the technical object from the epistemic thing, since all hybrids are possible. Nonetheless, in an experimental practise, as pointed out by Rheinberger, *some* platform that can uphold such distinctions, if only momentarily, is required:

> "Proper fluctuation and oscillation of epistemic things within an experimental system require appropriate technical and instrumental conditions. Without a system of sufficient stable identity conditions, the differential character of scientific objects would remain meaningless; they would not exhibit the character of epistemic things" (1997, p. 32)

This is a critical point when examining cultural-analytical work and constructivist social research as particular forms of knowing. It means, that the "problem" of lacking identities; the fact that it is

impossible to finally assign fixed identities to either technology or science, cannot be solved by simply collapsing the two. Yet for most cultural analysts and constructivist social researchers the text works first and foremost as a technology. The text constitutes a particular instrumental platform, as we shall see, which both circumscribes and gives form to the object of research. Evidently any given text can, in principle, become so much more than the instrumental and technical conditions of a research set-up, but according to Rheinberger, experimental apparatuses, and with them epistemic things, will have to be assessed by the way they are able to bring the future within close proximity. Experimental set-ups are in essence question-generating devices or machines for making the future (1997, p. 32). Technology, on the other hand, works basically to satisfy and reassure the present. It is first and foremost an answering machine. If technology gets to pervade and overspread science, then we end up with what Rheinberger characterises as an extended present; an extended present that leaves possible specific futures out of reach. This is precisely what I wish problematize in the following, because the understanding that all entities are actors, should not lead us to conclude that the text as technology is the same as some future intervention.

The role of texts in experimental knowledge practices

The experimental dynamic described above leads me to consider text as events. This is because the experimental tension between the program and experiment, or between the epistemic thing and technical object, plays out through a series of experimental events. As noted by Rheinberger, in the primarily descriptive and systematising sciences focus is on the process where the researcher extracts the object of study from what Rheinberger terms their "natural" ambiguity, and place them into a conceptual or theoretical order. In an experimental practise, on the other hand, focus is persistently on a series of events or "here-nows" strung up against each other. Through a series of forged but never fully controlled experiments the experimenter works to render visible the epistemic thing. The experimental researcher will employ many different representational techniques and tactics, and texts are one of them. Experimental set-ups need texts that account for the experimental goings-ons. Texts may be thought of as representations, but must be understood, at least in a constructivist framework, as first and foremost translations, as events, insofar as texts do not in any simple way depict a reality out-there. Experimental work, with Rheinberger, can be described as a process that operates on a continuum between vicarship, embodiment or modelling, and realisation. No research set-up can do without vicarship, representations *of* something. A text within an experimental set-up may very well change its function and position in the experimental arrangement through the process of research. Texts may also become models at some point. Perhaps as they are circulated and brought into relation with other entities. The experimenter will actively jump back and forth between different types of representation, within the system that he works, and by this he also comes to reshuffle temporalities. This is precisely why Rheinberger conceptualises experimental set-ups as potent machines for making futures. Such set-ups hold the capacity to overflow themselves, and to release contained excess. That is, they hold the ability to give form to the future not yet known and the future not yet imaginable. This is how such devices produce traces. Traces, which are recognized as new and surprising by the very way they are able to point the experimental machine in a new and productive direction. Experimenters move forward always in search for a specific difference. They are able to do so through the tweaking and twisting of experimental devices that make difference and displacement graspable from the position of the experimental system itself. This is a crucial point. Any difference thus produced must be specific and must be accounted for from the position of the research set-up itself. Otherwise there is no way for the research set-up to make

sense of its own on-going process of differentiation. This also means that difference understood as singularities in some form or other, produced through such research processes, must be able to detach themselves from the research platform. They must be able to exceed the boundaries of the instrumental set-up, otherwise they cannot be recognized as new, and they cannot be distributed in time and space. It is with these epistemic conditions for producing difference in mind, that I now turn to a closer analysis of texts and descriptions as interventions in cultural-analytical and constructivist social research.

To make a thoroughly produced actor network description usable

The article, *Description as intervention* (2007) by Signe Vikkelsø, sets out to show how an actor network (ANT) description may become useful for many if it is, in Vikkelsø's own words, made usable (Vikkelsø 2007 p.300). The main point of the article is to show that an ANT analysis, which produces symmetrical descriptions of socio-material relationships, and is written up thoroughly, holds the ability to open up a window of action. Descriptive research, according to Vikkelsø, is not a sterile and neutral activity. Insofar as we cannot *a priori* determinate which effects research engagement will produce we cannot claim that certain types of research are political and action-orientated while others are not. A good description, Vikkelsø argues, is a description that puts itself at risk by being exposed to a multiple audience, while at the same time such a description must be sensitive to the way it puts others at risk.

The article offers a guide for two different strategies that the researcher may employ to craft a good description. They come with slightly different implications. Vikkelsø sketches out a case where an ANT researcher is asked to do an ANT analysis, to assist in the development of a future electronic patient record (EPR) system in a hospital. The case is used as frame to describe the two recommended strategies of description. The first one is the so-called strategy-oriented analysis. Here the researcher follows the trail of Latour and Callon. The researcher focuses on the battles between programs and anti-programs, and describes how orders between heterogeneous actors are established and undone simultaneously. Such an analysis, Vikkelsø proclaims, will do more than inform the academic milieu. It will also inform stakeholders in the field of study on how to make their next strategic move. The analysis will make evident the specific political choices available to actors, in terms of investment and withdrawal, and point to concrete possibilities of making coalitions or drawing strategic demarcation lines (ibid, p.302). The second type of analysis, opted by Vikkelsø, is the multiplicity-oriented ANT analysis. Here the researcher explores the multiplicity of a phenomenon in line with the work of among others John Law (2004) and Annemarie Mol (2002). The implications here are that the explosion of the case into many different versions of reality will make visible the trade-offs and political choices enrolled in attempts to expand a certain practice. This visibility will raise the question of a politics to fit such an ontological multiplicity. The multiplicity-oriented analysis will, Vikkelsø argues, indirectly suggest the width and scope of a collective requirement specification for the EPR system (ibid,p. 303). It will point to a design for an EPR system that connects rather than separates or marginalizes practises. Neither of the descriptions, it is stated, will normatively identify who is victorious among actors, point out potential compromises nor change the norms or assumptions of the parties. Rather with the research strategies proposed, the object of study is elucidated and examined without employing a pre-set benchmark and universal scale (ibid, p.302). However, Vikkelsø notes, practical and political implications do not flow automatically from thick ethnographic description to an audience outside of academia. Therefore, Vikkelsø recommends that the detailed ANT descriptions of socio-material relationships are translated into "executive summaries" customized for specific audiences. She proposes for instance a presentation of a so-called SWOT analysis for

hospital management, a *pro et contra* discussion paper for the secretaries' union, or an extract of the analysis for the system developers. These texts are designed to participate in specific future "here-nows", and as such, they may be understood as very particular events that are part of Vikkelsø's research apparatus or we could say experimental set-up:

> "A thorough ANT analysis follows actors and traces performances, but shows also that there are prices to be paid and relationships that are cut for each single achievement. Accordingly, a summary based on such an analysis would not support a normative or instrumental agenda, but list the heterogeneity of networks, their nodes and extensions, their performative effects, their conflicts and dilemmas and, additionally, their others. It is a summary that does not address predefined heroes, villains and victims, but the socio-material collective. As such, it will disappoint both sides of a controversy (e.g. both the IT manager and the secretaries), but add a symmetrical specificity that affords changed political discussions and democratic decision-making." (ibid, p.304)

But how can we really know? How can we know if such summaries will not support a normative or instrumental agenda, if they will not address heroes, villains or victims, if they will in fact disappoint both sides of a given controversy, or if they will indeed afford changed political discussion and democratic dialogue? How can we really know how such symmetrical analyses, whether formed as SWOT analyses or *pro et* contra discussion papers will simultaneously perform and become enacted in some specific future "here-now", unless the research device of Vikkelsø's is configured to render these movements visible for us, from the position of the research set-up itself? To make these claims plausible Vikkelsø draws extensively on her past experience with sending different types of descriptions into circulation, and she asserts that:

> "None of these summaries were unequivocally embraced or rejected by the different stakeholders, but were employed in various ways and entered the debate. How this employment happens is not in the hands of the researcher."(ibid, p. 304)

The above retrospective re-counting of past experiences is very similar to the same general constructivist claims that the analyses started out from. The observation that descriptions, which are sent into circulation may be employed in various ways and enter the debate, is not really surprising, insofar as a performative orientation committed to seeing descriptions as actors, a commitment that I share with Vikkelsø, constitutes the whole basis for discussing the different descriptive strategies. It is the very ontological framework that makes a discussion of how to transform ANT accounts into potent interventionists relevant.

What is overlooked here, in my opinion, is how much the challenges of producing specific difference, the very accomplishment of exceeding what we already know, depends on how research activities are set up. In other words; it has to do with methods, instruments and agential cuts. It has to do with what kinds of "here-nows" Vikkelsø's particular research device is configured to handle. These conditions for producing difference are at least as important as the question of how the ANT analysis is written up, whether the researcher chooses to focus on a strategy-oriented analysis or a multiplicity-oriented analysis.

I argue here that the difficulty with reaching out to specific futures is related to the particular configuration of Vikkelsø's research apparatus. The process of research produces a trace, the text, but the text is at the same time also the very research platform that enables Vikkelsø to bring the field within reach. The text is first made in-

strumental in rendering the field of study visible for the researcher, and from here Vikkelsø attempts to shift the position and function of the text in the research set-up, when she afterwards translates the description into a symmetrical summary. She customizes the text to participate in some future "here-now", as she turns the text into a tool for future making. But before we get to the "here-now" in where the symmetrical summary is supposed to participate, the research process is brought to an end. The description is left to fight for itself in always already emerging networks.

It seems, in this process there is much care for particular ways of crafting the text, but not much care for the particular and situated future "here-now" in where the symmetrical analysis is claimed to be making a difference. And this is all the more surprising, since we remember how Vikkelsø characterized a good description; a good description is a description that puts itself at risk through its exposure to a multiple audience. A good description allows itself to become affected by the reactions from the audience (ibid, p.307). Furthermore, good research is research that acknowledges how it puts others at risk in the circulation and translations of its descriptions. Yet with the specific agential cut proposed in the quote above, the ANT researcher is not accountable to, and can neither account for, how the description differentiates, because this is already out of the hands of the researcher. It is not so hard to imagine, in this movement, how others may potentially be put at risk, while at the same time the program of the researcher, and the account, is placed in comparatively harmless surroundings, whichever way reality kicks back. We may therefore ask; how is this a risky enterprise? The particular configuration of Vikkelsø's research apparatus, I believe, is fleshed out in statements like the following:

> "Descriptions may be employed to support marginal or dominant political programs—a fact that has led to debate about the 'captivation' of research (Collins, 1990; Scott et al., 1990; Martin et al., 1991). However, the point is that such capturing is only possible by selecting certain elements of a symmetrical summary. A symmetrical summary does not forestall selective capturing, but allows competing selections to be made. " (ibid, p.304)

We are left here with the impression, that when the text establishes relations with alternative programs in the field e.g. programs that support a marginal or dominant political agenda, the symmetry of the researcher's program is challenged, and consequently the research process must be brought to an end. As soon as the description is confronted with resistance and politics from a multiple audience it seems the researcher will not commit to it anymore. The text is sent off and abandoned like an orphan. But is it not precisely this type of productive battlefield that ANT claims to be part of when it asserts that it engages in world-building activities like everyone else?

Embedding "leaning over" in ethnographic descriptions

Let us now turn to the article titled: *"Ethnographic stories as Generalisations that Intervene",* by Brit Winthereik and Helen Verran. In this article Winthereik and Verran address the capacities of ethnographic stories to work as generative interventions. The authors want to highlight the power of ethnographic analysis and storytelling for ethnographers working in multi-stakeholder situations. Inspired by Isabelle Stengers and Bruno Latour and their calls for slowing down, the authors want to show, in their own formulation "*just what is involved in doing that"* (Winthereik & Verran, p. 39). The central claim here is that the offerings of ethnographers, in the form of commentary on the everyday, will have their agencies enhanced by incorporating an irresolvable tension into the ethnographic account. By embedding in the ethnographic account a disconcerting moment, readers are actively pushed to engage with the text in a certain way.

The text will thereby perform as a loosening agent, and hopefully it will help to prevent further hardenings of the categories of the everyday (ibid, p. 39). The reason why ethnographic stories are capable of such things is because ethnographic stories are in fact re-performances, insofar as they must be understood first and foremost as makings of reality. Stories re-present, much like an index at the front of a book, as they offer some guide to somewhere or something that is boundlessly more complicated (ibid, p. 40). The story then becomes, as the authors argue, indexically implicated in a "here-now", and therefore, can be described as a generalisation. Like Vikkelsø, Winthereik and Verran stress that writing is not an act of resigning from where the action is. On the contrary, writing deals with technological cultures and futures in the making long after fieldwork has been terminated (ibid, p. 39). The field is invented again and again, as it is made available.

Ethnographic stories, according to Winthereik and Verran, have the capacity for at least two moments of generalisation; a one-many for abstracting, and a whole-part, which brings with it a situating moment (ibid, p. 38). The one-many type of generalisation represents its "here-now" as an example in a collection that adds up to form an evidence base for a general statement on e.g. human behaviour. The whole-part type of generalisation starts by first building the world. The story then becomes an emergent entity in a vague whole that will never form a complete picture of something in general (ibid, p. 40). This rather abstract scheme is illustrated in more concrete terms as the authors bring in Latour's analysis (Latour 2001) of Hans Holbein's painting *The Ambassador* (1533). The painting exhibits two men in a perfect geometrically constructed space, however, as the viewer leans over an unfamiliar brownish object stands out in the lower section of the painting. Through this composition the painting enables a dual vision or a disconcerting moment. The point here is that ethnographic stories, which incorporate different and distinct logics of generalisation, hold similar potentials.

The more operational side of this "leaning-over" effect is elaborated on through a concrete example from a field study in an environmental NGO. Here we are invited to consider the story of June, who is heading one of the development programmes in the organisation. One of June's tasks is to make sure that project partners account for how a given project is progressing by filling out and filing a so-called progress report. But her work, this is what she tells the ethnographer, is sometimes undercut by partners who fail to run their report by June before they upload the monitoring material to the central database. The ethnography highlights a specific conversation between June and the ethnographer where June discusses a particular progress report. A project worker, whom June knows, has evaluated her own performance, and in June's opinion this particular self-evaluation is rated too low. The ethnographer tells us that June's understanding of this imbalance is related to gender: June believes that this particular project worker has probably underestimated her own performance because she is a woman. We are also told that because June is so worried, the ethnographer shows an interest in the central database, and June and the ethnographer log on to the database together. But when June and the ethnographer inspect the report in question they find that all the cases of the word "modest" have been magically changed to "good". We are told how much this surprises June, and we are also told that for the ethnographer, who is really interested in following information technologies and how they participate in chains of global accountability, this is especially interesting (ibid, p. 45).

This small account is employed to exemplify how a disconcerting moment or a dual logic may be incorporated into ethnographic stories, with the purpose of enhancing the capacities of ethnographic stories to intervene. Winthereik and Verran suggest the following scheme: as a one-many generalisation the story of the database, the struggle of monitoring work and June's gendered insecurities, is a possible story of monitoring work in general. The story will exemplify the socio-material hard work of building and maintaining trans-

parent practises in aid partnerships. Alternatively, we may consider a possible story where the database, June, and the ethnographer are staged all as emerging parts. By this we accept that we do not yet know what monitoring work in fact is. As a whole-parts generalization monitoring remains a vague whole. This will be achieved, it is argued, by inappropriately emphasising the magic of the database, because this emphasis backgrounds the modern project of accuracy and accountability through the use of information technologies (ibid, p. 46). The magic database then becomes, it is argued, a memento mori directed at mapping ambitions of money flows in developmental aid. The database becomes the brownish disfigurement that pushes the reader to lean over, and see something more, just like in the Holbein painting. The point here is that the story must be constructed to allow for both the making of information exchanges and at the same time to allow for unexpected cuts in information exchanges. As the authors put it, a story that embeds this particular dual logic will be different from other stories, which feature databases as *either* cutting the world *or* as handling information correctly (ibid, p. 46).

If we compare the research strategies of Winthereik and Verran with the ones that Vikkelsø proposed, it is not difficult to see their differences. Vikkelsø proposed two different analytical strategies, for producing two different descriptions, to enact two different implications for a future development of an EPR system. Winthereik and Verran propose that the field note from the NGO is told with an emphasis on two different indexes or generalisation in the same text, in order to make the reader experience a disconcerting moment in some future “here-now”. At the same time though, we also see how these research apparatuses are similar to each other. In both articles, it is as if many different “here-nows” and identities are unproblematically conflated. We have heard how the act of writing is not an act of resigning from where the action is, because writing deals with technological cultures and futures in the making long after fieldwork has been terminated (ibid, p. 39). The field is invented again and

again, as it is made available. But who is the field made available for? The process of writing may be seen as an invention of the field, again and again, as a process of making the field available, to the researcher, using the technology of the text. It is intimately related to the fieldwork that preceded it and the finalized description that follows. The text in this extended “here-now” becomes an instrument for a particular way of knowing, a practise of momentarily conceptualizing the world on paper. In this process the researcher may imagine new orders, and the researcher's usual gaze may certainly become loosened.

Winthereik & Verran claim that the text, which was first the instrument that participated in rendering new orders visible for the researchers, now becomes a potent actor that will enable differentiation to unfold in a number of specific ways, in a series of future “here-nows”. But the ethnographic description, I will emphasize, is not the act of writing; it is an entity, which can be sent into circulation. At the time of writing, there is no way to know if this entity carries along with it the powerful reorientation that the ethnographer experienced in the process of writing. We simply cannot know if the ethnographic description will distribute and externalise a "disconcerting moment". Because the only way to reach out to the specific futures in where the description is claimed to intervene, from the research apparatus itself, in this case, is to actively slide from general constructivist claims to much more specific research claims. For example from the general claim that "it is possible to write stories that are generative for *some* of the practises that are studied, and *some* colleagues in social theory" (ibid, p. 38), to much more specific claims, like the suggestion that "the story of June and the database structured around a dual logic will feature surprising insights of day-to-day work of handling information technology in small organisations, which are of interest to the project workers themselves and their managers" (ibid, p.47). This to me is not slowing down, but rather research that travels at lightening speed.

Writing may be an infra-move, and there may not be an outside from where to intervene, but there is somewhere outside the research apparatus of Winthereik and Verran.

I want to point to a strange configuration here, between the ethnographer and the reader, which in some ways, I believe, absolves the ethnographer from worries about absencing and othering. In the first part of the article Winthereik and Verran note that an important part of understanding how ethnographic stories work as generalizations is to recognize that such stories are necessarily just one out of infinitely many possible stories that could have been written (ibid, p. 40). Many different indexes are possible. Verran and Winthereik deal with this question, which we could call the question of securing the non-arbitrariness of research, by turning analytical choices into requirements. Reflexivity, they argue, is not simply a matter of being transparent about one's analytical choices.

> "She might want to see something else than what was featured in the painting as a Newtonian, linear approach to science. Instead, she might want to see gender. Yet, it is not entirely up to her to decide what she sees as she shifts her position; the brownish object that became a magic database did not allow her the choice of focusing on the gendered aspects of monitoring. This was a specific intervention that could not be made, even if the ethnographer was committed to tell this story. Instead, telling about the database as trusted and autonomous, objective and magical, makes a basis for good faith intervention in academic discussions about aid infrastructures. Good faith here means that the ethnographer faces – not a choice – but a requirement to develop a sense of where exactly to contribute, which makes intervention a matter of writing about how the unavoidable frictions in accountability relations emerge and are handled in the environmental NGO." (ibid, p. 47)

The ethnographer's contribution is not a matter of choice, because the ethnographer faces a requirement to develop a sense of where exactly to contribute, but what about the reader then?

> "In their partiality they are radically incomplete representations, and simultaneously they take up a position in a politics. Which abstracts and which particulars, which abstracting and situated generalisations –this is radically underdetermined. Many directions are possible and many things may follow. Making the next move that is the work that readers must do. They might do nothing and they remain unmoved by the possibilities glimpsed in the double vision. To do nothing is of course to do something as it is to reconstitute current presencing/absencing/othering. Or readers might do something else, inspired by a glimpse of worlds with alternative entities and alternative modes of relating." (ibid, p.49)

There is an imbalance here, which places the burden of accountability on the reader. The ethnographic account is staged as an entity, which performs the future as radically open. The reader is left with the choice that the ethnographer did not have when she crafted the ethnographic account. If the reader choses to do nothing she is actively reinforcing the current state of presencing/absencing/othering. It is on the reader.

Concluding remarks

> "One must put forth the following question: what is a good laboratory and what is a good textual account? The latter question, far from being belated and irrelevant, becomes central to the definition of what is for us a good science of the social. To put it in the most provocative way: good so-

> ciology has to be well written; if not, the social doesn't appear through it." (Latour, 2005b, p. 124)

This quote by Latour is no longer so provoking. It may have been once, to those who believed that textual accounts were objective reports and transparent registrations of reality. But no contemporary experimenter or descriptive researcher will doubt the importance of the text, nor that it has to be well written to do the job. What is less clear however, in this extract from Latour's introduction to Actor Network Theory (2005), is that it is not enough that a text is well written; somebody has to read it as well. Rheinberger talks about the text as an event (Rheinberger 1997, p. 223). The text and the context conflate and is turned into some "here-now", and at the same time he states, such a particular "here-now", the text that is, remains and becomes a text only insofar as it is reread, rewritten, and importantly, detached from its pretext and author and recontextualised.

In this paper I have inspected texts as events. I have explored ANT research and cultural-analytical work as doings, and I have problematized how the particular event of writing up an analysis is sometimes conflated with claims about how that same analysis, in the form of a text, may intervene in some future here-now. In the articles discussed, it is as if the overflows in progress, or the disconcerting moment, the reflexive "here-now" that the researcher experiences through the process of research, is wishfully thought to be captured and contained by the description, yet with little methodological concern if this actually happens in any particular way. The text may enable a powerful reflexive moment, or a reorientation graspable for the researcher, but as I have argued, we cannot really know how the description is "re-contextualized", precisely because the particular research apparatuses presented in the two articles are configured in such a way that the "here-now" in which the text is reread, rewritten, detached from its pretext and author, happens after research has ended. Do not get me wrong; I am sympathetic to the ambition of research that may open up new stratas of worlds with alternative entities and alternative modes of relating, and I am of course not suggesting that it is up to the researchers to *decide* how readers interpret a text, nor that a researcher can claim full control of events wherein a given text participates. I claim precisely the opposite.

I appreciate how texts can be powerful actors. Indeed, the two articles that I have explored in this paper made me reflect on my own practise, and on the status of the text in cultural-analytical and constructivist social research. But taking texts seriously as always situated events that can reach out into particular futures; futures that we can actually know about, account for, and become accountable to, requires a research set-up, I argue with Rheinberger, that is solid enough to produce a series of "here-nows". From an experimental research position, the research apparatuses of both Vikkelsø and Winthereik and Verran come across as set-ups that, in one important aspect, implode on themselves: the description, the main instrument that research activities are structured around, the very platform where differential success emerges, is at the same time also the main trace; the entity that externalises the insight gained by the researchers. The trouble of telling them apart, if ever only momentarily, results in a strange kind of circularity; a very tight shifting between general constructivists claims and what ought to be highly specific research claims. With Rheinberger, we may say that we end up with an extended present that leaves possible specific futures out of reach. This, I argue, is crucial to address in a discussion on description as intervention. Experimenting with text as different kinds of particular events *within* the research process, I suggest, may be a valuable next step. This is not a "simple" matter of methods, but it certainly depends on how research is done.

References

Barad, K. (2007). Meeting the Universe Halfway: Quantum Physics and the Entanglement of Matter and Meaning. duke university Press.

Binder, T. & Redström, J. (2006). Programs, Experiments and Exemplary Design Research. Paper presented at Design Research Society Wonderground Conference 2006, 1-4 Nov 2006, Lisbon, Portugal. Retrieved from: http://www.dkds.dk/%7B1B42C0E8-DAC8-43FA-BEF3-ABD004F7488B%7D

Binder, T., Brandt, E., Halse, J., Foverskov, M., Olander, S. & Yndigegn, S. (2011). Living the (Co-design) Lab. In: Proceedings of Nordic Design Research Conference. Retrieved from: http://designresearch.fi/nordes2011/

Binder, T., De Michelis, G., Ehn, P., Jacucci, G., & Wagner, I. (2011). Design things. The MIT Press.

Björgvinsson, E., Ehn, P. & Hilgrenn P. (2010). Participatory design and "democratizing innovation". In: Proceedings of the eleventh conference on Participatory Design. (pp. 41-51)

Brandt, E., Redström, J., Eriksen, M.A. & Binder, T. (2011). XLAB. The Danish Design School Press.

DiSalvo, C. (2012). Adversarial design. The MIT Press.

Ehn, P. (2008). Participation in Design Things. In: Proceedings of the Tenth Anniversary Conference on Participatory Design. (pp. 92-101)

Halse, J. (2008). Design Anthropology: Borderland experiments with participation. IT University of Copenhagen. PhD thesis. Retrieved from:
http://nordicom.statsbiblioteket.dk/ncom/da/publications/design-anthropology%284bc96110-526b-11dd-b542-000ea68e967b%29.html

Halse, J., Brandt, E., Clark, B. & Binder, T. (Eds.) 2010. Rehearsing the Future. The Danish Design School Press.

Haraway, D. (2007) When species meet. University of Minnesota Press.

Jespersen, A. P., Krogh Petersen, M., Ren, C. B., & Sandberg, M. (2012). Cultural analysis as intervention. In: Science Studies, 25(1), 3-12.

Jönsson, L. (2014) DESIGN EVENTS: on explorations on a non-anthroprocentric in design. The Royal Academy's School of Design. PhD thesis.

Koskinen, I., Zimmerman, J., Binder, T., Redstrom, J., & Wensveen, S. (2011). Design research through practice: From the lab, field, and showroom. Elsevier.

Latour, B. (1999). Pandora's hope: essays on the reality of science studies. Harvard University Press.

Latour, B. (2001). " Thou Shalt Not Take the Lord's Name in Vain": Being a Sort of Sermon on the Hesitations of Religious Speech. RES: Anthropology and Aesthetics, (pp. 215-234)

Latour, B. (2004). Why has critique run out of steam? From matters of fact to matters of concern. In: Critical inquiry, 30(2) (pp. 225-248)

Latour, B. (2005a). From Realpolitik to Dingpolitik: or How to Make Things Public. In: Making Things Public: Atmosphere of Democracy, (pp. 4 – 31). Latour, B. and Weibel, P. (Eds.), Cambridge MA: MIT Press.

Latour, B. (2005b). Reassembling the Social-An Introduction to Actor-Network-Theory, Oxford University Press

Latour, B. (2008). A Cautious Prometheus: A Few Steps Toward a Philosophy of Design . Keynote lecture, Networks of Design, meeting of the Design History Society.

Latour, B. (2010). An attempt at writing a "Compositionist Manifesto". In: New Literary History, vol. 41, 471-490. Retrieved from: http://www.bruno-latour.fr/article?page=1

Law, J. (2004). After method: Mess in social science research. Routledge.

Leenskjold, T. (2015) Objects of entanglement and allure. The Royal Danish Academy's School of Design. PhD Thesis

Lindström, K. & Ståhl, Å. (2014). Patchworking publics-in-the-making: design, media and public engagement. Malmö Högskola. PhD thesis

Manzini, E. & Rizzo, F. (2011). Small projects/large changes: Participatory design as an open participated process. In: CoDesign: International Journal of Co-Creation in Design and the Arts. Vol. 7, issue 3-4 (pp. 199-215)

Marres, N. (2012a). Material participation: technology, the environment and everyday publics . Palgrave Macmillan.

Mol, A. (2002). The body multiple: Ontology in medical practice. Duke University Press.

Rheinberger, H. J. (1997). Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube. Stanford University Press.

Rheinberger, H. J. (2010). An epistemology of the concrete: Twentieth-century histories of life. Duke University Press.

Seravalli, A. (2012). Infrastructuring for opening production, from participatory design to participatory making? In: Proceedings of the 12th Biennial Participatory Design Conference. Roskilde, Denmark.

Storni, C., Linde, P., Binder T. & Stuedahl D. (2012). Exploring ANT in PD: reflections and implications for theory and practise. In: Proceedings of the 12th Participatory Design Conference: Exploratory Papers, Workshops descriptions, Industry Cases-Vol. 2 (pp. 145-146). ACM.

Suchman, L. (2011). Anthropological relocations and the limits of design. In: Annual Review of Anthropology, 40, 1-18.

Telier, A., Binder, T., De Michelis, G., Ehn, P., Jacucci, G., Linde, P. & Wagner, I. (2011). Design Things. Cambridge, MIT Press

Vikkelsø, S. (2007) 'Description as Intervention: Engagement and Resistance in Actor-Network Analyses'. In: Science as Culture, 16(3): 297-309.

Winthereik, B. R., & Verran, H. (2012). Ethnographic Stories as Generalizations that Intervene. In: Science Studies, 25(1). (pp.37-51)

Zuiderent-Jerak, T., & Bruun Jensen, C. (2007). Editorial introduction: Unpacking 'intervention'in science and technology studies. In: Science as Culture, 16(3), 227-235.

Biographical note

Sissel Olander is a postdoctoral researcher at the Royal Danish Academy's School of Design. She holds a PhD in design anthropology and codesign. In her research she explores participatory processes as experimental forms of knowing and making that tie together the empirical with the speculative. She is currently working with Copenhagen libraries to explore everyday prototypical practices, representational procedures and larger societal issues.